An Examination of Dose in Mindfulness-Based Programs and Mindfulness Practice through a Dose-Response Meta-Regression and Randomised Controlled Experiments.

by

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ABSTRACT

Mindfulness research has grown exponentially in recent years including research with various doses related to mindfulness-based programs (MBPs) and mindfulness practice. This PhD thesis aimed to further understanding of the effectiveness of different doses related to MBPs and practices through a comprehensive review and experimental studies.

A large-scale dose-response meta-regression including 203 randomised controlled trials (both, compared to inactive and active controls) was completed with 15 dose variables related to MBPs and practice. The outcomes were depression, anxiety, stress, and mindfulness at post-program and follow-up. The meta-regression showed significant dose-response relationships between doses related to actual program use, face-to-face contact, and program intensity and the mindfulness outcome. No robust significant dose-response relationships were found for psychological distress outcomes.

Actual amount of mindfulness practice was frequently not consistently and reliably recorded in the studies included in the dose-response review. Additionally, the review did not support causal conclusions. Therefore, a randomised controlled experiment examined the relative effectiveness of longer (20-minute) and shorter (5-minute) mindfulness practices in a general population sample of novice practitioners. Although both doses were found effective at reducing psychological distress and increasing mindfulness compared to control, results showed that shorter practices had a significantly greater positive effect on mindfulness and stress than longer practices.

Additionally, the effectiveness of a single-dose mindfulness practice was assessed. An online-delivered randomised experiment, with a general population sample, examined the effects of a mindfulness induction on state hope and gratitude. This induction had significant positive effects on both outcomes, and state mindfulness statistically mediated the improvements in state hope and gratitude.

Overall, thesis findings have contributed to the field of mindfulness research by showing that higher and lower MBP and mindfulness practice doses are helpful, but that for novices, lower mindfulness practice doses may be more effective, especially in self-help MBPs without an experienced teacher present.

DEDICATION

This PhD is dedicated to everyone who has been told by people and circumstances that they cannot do something, that this is just not how it is usually done (mostly because it is not how *they* did it), that this will be too much work, or who has heard the word “No” one too many times. This is from me, to you, saying you *can* do this, you *do* have what it takes, and it *is* going to work out. And if you fail, you can try again. Imagine all the amazing results and findings if we are mindful of and trust in the abilities of people. Look what we can do!

And of course, this PhD thesis is for all mindfulness practitioners (novices, experienced, and in-between) to show that mindfulness is something that can, and should, be enjoyed by all, no matter what dose.

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LIST OF PUBLICATIONS RESULTING FROM PHD

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This publication refers to the dose-response meta-analysis and meta-regression and encompasses Chapters 2, 3, 4, and 5 of the PhD thesis.

Strohmaier, S., Jones, F. W., & Cane, J. E. (2021). Effects of length of mindfulness practice on mindfulness, depression, anxiety, and stress: A randomized controlled experiment. *Mindfulness, 12* (1) 198–214. https://doi.org/10.1007/s12671-020-01512-5.

This publication presents the face-to-face study examining effects of different mindfulness practice lengths and refers to Chapter 6 of the PhD thesis.

Strohmaier, S., Jones, F. W., & Cane, J. E. (2022). One-session mindfulness of the breath meditation practice: A randomized controlled study of the effects on state hope and state gratitude in the general population. *Mindfulness.* https://doi.org/10.1007/s12671-021-01780-9.

This publication presents the online study examining effects of a mindfulness induction and refers to Chapter 7 of the PhD thesis.

LIST OF CONFERENCE PRESENTATIONS AND COMMUNICATIONS RESULTING FROM PHD

Strohmaier, S. (2021, April 19). Five common myths of mindfulness – debunked. *WorkLifePsych blog.* https://www.worklifepsych.com/five-common-myths-about-mindfulness-debunked/

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CHAPTER 1

Thesis Introduction and Contextualisation:

Selective Review of Mindfulness Foundations, Theory and Literature, Thesis Aims and Methodological Position

1.1 Chapter 1 Overview

This chapter introduces and contextualises this thesis, which examines the concept of dose in mindfulness and mindfulness-based programs (MBPs). Due to research on mindfulness and MBPs having grown exponentially (Goldberg et al., 2018), a literature review including the entirety of research and theoretical accounts published on this topic would be impossible within the confines of this thesis. Therefore, only the theory and literature relevant to the topic of this thesis, namely dose in mindfulness-based programs and practices, are discussed.

Firstly, an introduction of the foundations and conceptualisations of mindfulness is given to contextualise the thesis topic including its Buddhist background and application in Western Psychology followed by an exploration of various mindfulness definitions until arriving at the definition of mindfulness utilised in this thesis. Based on various mindfulness definitions, several theories of mindfulness have been devised. With a focus on dose, the next section considers and critically compares the four most relevant mindfulness theories which have informed thinking in this thesis in terms of dose. Mindfulness theories have been informed by and have formed research. Therefore, in the next section, relevant mindfulness research literature is reviewed. Since it is impossible to review all foci of the vast mindfulness literature within the scope of this thesis, mindfulness research is reviewed with the purpose of providing a focused review of research as it currently stands and which the topic of dose in this thesis builds upon. The review draws on the relevant broad research areas in the field identified in a recent special issue on mindfulness (Bernstein et al., 2019) and considers how these relate to dose. These areas are research of dose in mindfulness for physical health conditions, mental health conditions, the general population, other mindfulness research areas, as well as mechanisms of action and criticism of mindfulness related to dose.

Based on mindfulness foundations, theory, and literature as it currently stands, this thesis sought to explore and answer several research questions with regards to dose in MBPs and mindfulness practice. These are outlined in the section that follows the literature review. In particular, this section considers the three broad parts to this thesis and the research aims and questions which are explored in each of these parts. Next, since there are different philosophical and methodological positions which can be taken when researching mindfulness, a rationale of the philosophical approach of post-positivism within the critical realist ontology adopted in this thesis is provided. In the final section, this chapter concludes with a chapter summary and orientation of chapters that follow.

1.2 Foundations and Conceptualisations of Mindfulness

1.2.1 Buddhist Roots of Mindfulness

Mindfulness has its roots in Buddhist traditions. Since a comprehensive review of the Buddhist background of different meditation practices is beyond the scope of this thesis (and has already been provided elsewhere, e.g. Shonin et al., 2015; Sun, 2014), a more succinct overview is given. One form of meditation in Buddhist traditions is mindfulness practice, which refers to a bare attention and consciousness towards the world as it is (Nyanaponika, 1973). In the Pali language, the word mindfulness (=*sati*) translates to "to remember" (Bodhi, 2000; Nyaniponika, 1973). According to Bodhi (2011), *sati* can be understood in two related ways: firstly, the original meaning of *sati* as an aspect of memory, of remembrance and calling to mind, which is thought to facilitate greater awareness and a sense of purpose. Secondly, *sati* is understood as a lucid awareness of the present moment, with *sati* manifesting a presence of mind which allows one to be awake in the present moment. This wakefulness in the present moment in turn supports the first meaning of *sati* as the recollective function (Bodhi, 2011; Brown et al., 2007). In Buddhism, mindfulness practice is associated with spirituality (Carmody et al., 2008) and is seen as a necessary step towards spiritual enlightenment and thus the end of suffering (Das, 2009), resulting in a calm, balanced, ethically- and psychologically-well self (Gethin, 2001). Additionally, mindfulness meditation in Buddhist traditions is considered as the "heart" of meditation (Thera, 1962) and the practice of mindfulness is believed to be grounded in all Buddhist teachings (Hanh, 1999). Fundamentally, mindfulness meditation in Buddhism is thought to be achieved by firstly focusing on one aspect, most of the time the breath, and then gradually, with practice, expanding one's attention, aiming to ultimately incorporate all physical and mental sensations. This includes experiencing feelings and thoughts exactly as they occur, thus paying conscious attention to all aspects of life without interpretation or assigning judgment in a process of non-judgemental investigation of continuous experience (Bodhi, 2000; Kabat-Zinn, 2003; Nanamoli & Bodhi, 2000). Since attention tends to wander, the purpose in mindfulness meditation lies in bringing back the attention to the breath or object of meditation in the current moment (Chiesa, 2012; Hasenkamp et al., 2012). The path towards spiritual enlightenment as well as in-depth understanding and knowledge of mindfulness in Buddhism is thus thought to be gained through persisting with a large amount of practice over a long period of time (Chiesa, 2013; Gunaratana, 2002; Thera, 1973). Within the wider context of Buddhism, mindfulness is situated within the Dharma, which has been understood as Buddhist teachings or, more widely, the natural law of the world (Krägeloh et al., 2019). The Dharma provides, amongst others, an ethical framework and foundation to mindfulness meditation which furthers compassion and wisdom (Stanley et al., 2018). Caution needs to be exercised when taking mindfulness meditation out of this ethical framework, since, although some of the benefits of mindfulness meditation may still be experienced, the intention and insights gained from meditation may be less clear thus deviating from their intended purpose (Dorjee, 2010; 2016; Marx, 2015). Arguably, within secular MBPs, care thus needs to be given to ensure the original intention of practice within the Dharma is maintained while remaining accessible for secular applications and environments (Cheung, 2018). According to Marx (2015), it can often be helpful for mindfulness to be taught and MBPs to be designed by teachers who are aware of the conceptual framework of mindfulness in Buddhism.

1.2.2 Applications of Mindfulness in Western Psychology

Mindfulness has been adopted by Western psychology by adapting its processes for inter- and cross-cultural application (McMahan et al., 2008; van Gordon et al., 2015a). In Western psychology, mindfulness has commonly been defined as "paying attention in a particular way: on purpose, in the present moment and non-judgementally" (Kabat-Zinn, 1994, p.4). Numerous articles have been written on the cultural differences and the move away from spirituality and focus on dualism, particularly in comparison with Buddhist traditions (Dunne, 2011), but also other religions and cultures (e.g. Thomas et al., 2017; Tomasino et al., 2014; Trammel, 2017; 2018), and how secular adaptations of mindfulness practice and measurement have been de-contextualised. Research has also started to appreciate the value of re-conceptualising mindfulness within the context of Buddhist spirituality and religion (e.g. Lomas et al., 2017). This is presented in more detail, elsewhere (e.g. Kabat-Zinn, 2011; van Gordon et al., 2015).

Kabat-Zinn (1982) was one of the first and particularly influential pioneers of mindfulness meditation in the Western world. He first included mindfulness as part of an 8-week, self-regulatory stress reduction and relaxation program helping patients to live with their chronic pain (Kabat-Zinn, 1982). This stress reduction program was later termed Mindfulness-Based Stress Reduction (MBSR; Kabat-Zinn, 1990). Within this program, mindfulness meditation is considered as a self-regulatory activity, which is learned via directing ones' attention consciously, from moment to moment, without judgement, in detached observation, thus reducing the experience of suffering from pain and mood disturbance (Kabat-Zinn, 1990). Although the practices in the MBSR programs are similar to those in Buddhist traditions, mindfulness according to Kabat-Zinn (2003) is free from religious, spiritual, or cultural barriers. Specific features of mindfulness teaching which were deemed important by Kabat-Zinn (1990; 2003) include mindfulness being taught in a face-to-face group format with an experienced teacher present to allow participants to discuss their experiences and support each other in their practice, which in turn was thought to also enhance motivation and compliance. Nevertheless, according to Kabat-Zinn (2003), mindfulness needs to be learnt via continued practice, whereas other elements (such as reading materials and group discussions) are considered supplementary to practice.

Mindfulness practice consists of both formal and informal practices, with formal practice taking place when individuals consciously set aside time, often with the help of an audio recording of a guided practice (c.f. Birtwell et al., 2019; Kabat-Zinn, 2003). Informal practice on the other hand occurs when participants apply mindfulness skills during their everyday lives (Langer, 2014); for example, mindfully washing dishes (e.g. Hanley et al., 2015). Both formal and informal mindfulness practices have been associated with improved wellbeing (Birtwell et al., 2019); the effectiveness of practice is reviewed in more detail in Chapters 2, 3, 5, 6, and 7.

Kabat-Zinn firstly came to the notion of applying mindfulness meditation as an intervention to change individuals' relationship to their chronic pain since Buddhist meditators had reported mindfulness as a helpful approach to working with intense pain during long hours of sitting meditation (Kabat-Zinn, 1982; Krishnamurti, 1979). This was supported by research stating that Zen meditation practitioners had a significantly lower sensitivity to pain compared to non-meditating controls (Kornfield, 1977; Nyanaponika, 1962; later also found by Grant et al. (2010)). According to the Gate Control Theory (GCT; Melzack & Wall, 1965), cognitive and emotional interpretations of pain have been found to contribute to the experience of pain just as much as sensory occurrences of pain. The GCT has an underlying biological basis where bottom-up sensory information can be modulated by the top-down interpretation of pain via descending pathways at the spinal cord, which acts as a gate, thus impacting the severity of the sensory pain experience (Melzack & Wall, 1965). Therefore, if regarded simply as an event without any value or judgement, pain has been found to lose considerable power in that the physical presence of pain may still be felt but emotional and cognitive components of the pain experience are reduced through awareness meditation (Banth & Ardebil, 2015).

Additionally, Kabat-Zinn (1982) argued that patients' beliefs in therapy outcomes can be maximised by being told that mindfulness is effective for pain relief with continued practice. This positive placebo effect, though mostly employed in pharmaceutical studies, has also been found beneficial in psychotherapy interventions (Rosenthal & Frank, 1956). Participants are expected to be responsible for the effectiveness of their pain relief without expectations of striving towards specific goals, but rather through developing their practice over time; continued regular practice is therefore seen as very important in mindfulness meditation, as understood by Kabat-Zinn (2003). In fact, Kabat-Zinn (2005) has compared mindfulness meditation to scaffolding on a building where the ability of being mindful is built up over time through continuously longer and informed practice.

Based on Kabat-Zinn's MBSR program with its primary focus of physical pain and stress management, Segal et al. (2002) developed Mindfulness-Based Cognitive Therapy (MBCT), which combines mindfulness with cognitive therapy and thus is often described as part of the third-wave of cognitive therapies (Hunot et al., 2010). MBCT follows a similar format to MBSR, i.e. it is typically delivered over eight weeks via face-to-face groups, but has the treatment of depression, with its original focus on relapse prevention in recurrent depression that is in recovery or remission, at its focal point. Through MBCT, participants are able to learn how to respond to their cognitions in an adaptive way, thus minimising the occurrence of relapse to depression (Segal et al., 2013). According to research by Kuyken et al. (2019), MBCT has been found effective for relapse prevention in recurrent depression, especially for evident residual symptoms. The efficacy of MBPs is considered further in Section 1.4 and in Chapters 2-5.

Both MBSR and MBCT have been found to have transdiagnostic qualities since both have been adapted to suit populations with different health conditions. This includes individuals with cancer (e.g. Johannsen et al., 2016), dementia (e.g. Churcher Clarke et al., 2017), or an eating disorder (e.g. Kristeller et al., 2014). Adjustments for these populations included different levels of support or foci as needed (e.g. gentler exercises for cancer patients, simplified instructions for persons with a dementia, a focus on avoidance of overeating, etc.). Similarly, mindfulness has also been found helpful when integrated into different therapeutic approaches, including mindfulness-integrated cognitive behavioural therapy (e.g. Bahrani et al., 2017), mindfulness-based smoking cessation (e.g. Goldberg et al., 2013), or mindfulness-based childbirth and parenting programs (e.g. Pan et al., 2019), amongst others. Furthermore, MBSR and MBCT programs have also been identified as helpful for individuals from the general population, for participants with a low, but what has generally been considered a non-clinical, level of physical or psychological distress (e.g. Jansen et al., 2017; Shahar et al., 2010).

Although traditionally, MBSR and MBCT (including adaptations) have a primarily clinical focus, further adaptations of MBPs have been developed, which include different modes of delivery (e.g. self-help, online) and different program or practice lengths; these MBPs also often have a broader application of mindfulness in everyday life for the general population (also see Chapter 2, Section 2.2). Furthermore, different psychological interventions have been developed which incorporate some aspects of mindfulness, but where the main focus is not on formal mindfulness practice. For instance, Acceptance and Commitment Therapy (ACT), based on Relational Frame Theory (Hayes, 2004a,b), focuses on fostering acceptance of unhelpful thoughts and emotions followed by committing to behaviour-change strategies resulting in increased psychological flexibility (the ability to stay connected to the present moment regardless of unpleasant thoughts and flexibly responding to circumstances and situations in pursuit of personal values and goals) and deterrence of experiential avoidance (Hayes et al., 2006; Hayes & Feldman, 2004). Similarly, Dialectical Behaviour Therapy (DBT), originally devised for individuals with borderline personality disorder, focuses on the relationship between understanding and changing maladaptive behaviours, emotions, and thoughts (Linehan, 1993a).

The language often preferred in the field of mindfulness, and to better align with its Buddhist roots, is to refer to courses involving mindfulness as “programs” as opposed to “interventions” or “therapies” (cf. Crane, 2017; Kang & Whittingham, 2010); the term “program” is therefore used throughout this PhD thesis.[1] Although some authors (e.g. Baer, 2003; Chiesa & Malinowski, 2011) have included psychological interventions such as ACT and DBT in the group of MBPs, in this thesis, only programs where the majority of content is mindfulness practice are regarded as MBPs, following the definition by Crane et al. (2017). This definition of what constitutes an MBP is further outlined in Chapter 2, Section 2.3.1.

[1] Referring to mindfulness courses as “programs” was also the view of editor B. Khoury (2020) representing the journal *Mindfulness,* which is a highly valued journal in the community of mindfulness researchers (Mindfulness, 2021) and has been considered the “flagship journal of the area” (Krägeloh et al., 2019, p.1), during the publication process for the dose-response meta-regression review (Strohmaier, 2020).

1.2.3 Different Conceptualisations of Mindfulness

Drawing on the foundations and applications of mindfulness, several definitions of mindfulness exist, which have been devised over the years of its increasing popularity (Krägeloh et al., 2019); this lack of a single, operational definition in the literature has been criticised (e.g. Chiesa, 2012; Levit-Binnun et al.; 2021; Malinowski, 2008). Buddhist definitions of mindfulness have been described above and, in more detail, elsewhere (e.g. Anālayo, 2019; 2021). Western mindfulness definitions include those from insight meditation teachers (e.g. Boorstein, 1997; Goldstein, 1987; Kornfield, 2007), where attention and awareness of inner experiences is emphasized, and who define mindfulness from the perspective of Buddhism as the quality of mind bringing enlightenment and developing wisdom in the form of wise attention. Other, more recent ways that mindfulness has been described by secular researchers include a focus on non-judgemental observation and awareness (e.g. Baer, 2003; Cardaciotto et al., 2008; Kabat-Zinn, 2003; 2005), individuals' capacity for attention and awareness (e.g. Brown & Ryan, 2003; 2004; Brown et al., 2007), a particular way of paying attention (e.g. Kabat-Zinn, 1990; 1994; 2003), present-moment awareness (e.g. Marlatt & Kristeller, 1999; Shapiro, 2009), and/or curiosity, openness and acceptance (e.g. Erisman & Roemer, 2012), to only name a few. These secular definitions of mindfulness are not necessarily contradictory, but rather are considerably consistent with a certain degree of overlap, and differ mostly in the placement of emphasis on different aspects.

Although there may also be some overlap of secular MBPs with Buddhist definitions, some have argued that there are considerable differences between the different Buddhist and Western definitions of practice (Dorjee, 2010; 2016). For instance, mindfulness as defined outside of Buddhism has been criticised for its focus on short-term stress relief (termed "here-and-now-ism"; Brazier, 2013; Dorjee, 2016; Purser, 2015; see Section 1.4.6.2) rather than the longer-term path towards enlightenment as outlined in Buddhism. Arguably, Western definitions do not fully capture the meaning of the Pali term *sati* due to translational and linguistic issues where language can point to the concept of *sati*, but it is impossible to fully capture it (Choi et al., 2020). The disparities between definitions of mindfulness in Buddhism and Western psychology have been further evidenced in qualitative research exploring the conceptualisation of mindfulness, where considerable differences between how mindfulness was defined by senior ordained Buddhists and how it appeared in mindfulness questionnaires commonly employed in Western psychology research were found (Feng et al., 2018).

Nevertheless, Western definitions of mindfulness have been thought to have been devised in a way that is befitting Western psychological theoretical frameworks which are thus also more easily understood by researchers and clinical practitioners alike (Chiesa, 2012). Additionally, the secular definitions of mindfulness not being fully conceptualised provides the opportunity for scholars to develop different emphases for specific contexts and different research strands (Hart et al., 2013; Kabat-Zinn, 2011). In fact, Kabat-Zinn (cited in Cullen, 2011) mentioned that the focus of mindfulness definitions can differ according to participants and nature of MBPs.

Since in this PhD thesis, the aim was to further understanding of mindfulness meditation and the effectiveness of secular mindfulness programs and practices as applied in Western contexts (see Section 1.5 for thesis research aims), the definition most commonly cited in Western mindfulness research papers (Black, 2011; c.f. Chiesa, 2012; Hart et al., 2013) namely Kabat-Zinn's (1994, p.4), formulation of mindfulness as "paying attention in a particular way: on purpose, in the present moment and non-judgementally" is adopted.

1.2.3.1 Trait and State Mindfulness and their Measurement

Further dismantling the concept, mindfulness has been described as a disposition or habitual trait, other times as a current state of mind, both, a trait- and state-like quality, or a skill to be learned. More specifically, mindfulness as a trait has been described and measured both as a multifaceted (e.g. using Five Facet Mindfulness Questionnaire (FFMQ; Baer et al., 2006)) and single-faceted construct (e.g. using Mindful Attention Awareness Scale (MAAS; Brown & Ryan, 2003) or Freiburg Mindfulness Inventory (FMI; Buchheld et al., 2001; Wallach et al., 2006)). Additionally, mindfulness has been characterised as a state-like quality following a mindfulness meditation (e.g. using Toronto Mindfulness Scale (TMS; Lau et al., 2006)). Mindfulness has also been described as a skill or process to be refined through practice (e.g. using Mindfulness Process Questionnaire (MPQ; Erisman &

Roemer, 2012)). In research, distinct measures have been employed to assess participants' trait, state, or mindfulness skill; this is explored in more detail elsewhere (e.g. Hildebrandt et al., 2017; Malinowski, 2008). Measures of mindfulness have generally shown high reliability and validity (cf. Bergomi et al., 2013; Goldberg et al., 2019). Additionally, research has continuously refined measurement of different mindfulness constructs for different populations (e.g. Padmanabham et al., 2021; Sweeney et al., 2021) and measures have been translated into different languages and assessed for validity and cultural appropriation (e.g. Hou et al., 2013). However, the research field has been criticised since no explicit definition of what constitutes a mindful person, as assessed with a single valid measure, prevails (Grossman, 2011). Nevertheless, although various conceptualisations of mindfulness exist, the different classifications of mindfulness frequently co-exist harmoniously (Chiesa, 2012) and often several measures assessing different mindfulness qualities are employed in a single research study. Indeed, research has found that increased state mindfulness through continued practice contributes to greater trait mindfulness (Bravo et al., 2018; Kiken et al., 2015). For a more detailed consideration of mindfulness measures included in the dose-response meta-regression, see Chapters 2, and 3. The rationale behind which measures are chosen for studies in this thesis are in Chapters 6 and 7.

1.3 Consideration and Critical Comparison of Relevant Mindfulness Theories in terms of Dose

Based on the various mindfulness definitions, several theories have been developed. Due to the many different facets of mindfulness, it has been argued that it is impossible for a single theoretical model to incorporate all phenomena of mindfulness (van Dam et al., 2018). Therefore, a wide range of potentially relevant theories exists within the literature, and these have been reviewed in detail elsewhere. For example, Brown et al. (2007) provide a comprehensive review on theoretical foundations of mindfulness and books have been written exploring theoretical underpinnings of mindfulness in general and within specific populations (e.g. Brown et al., 2016; Mace, 2008; Schonert–Reichl & Roeser, 2016). However, due to space constraints, the focus here is a selective review of theories that are either the most prominent in the field or the most directly relevant and most drawn upon within the topic of this thesis and on which the research on dose in mindfulness-based programs and practices presented in the following chapters builds. Some theory included in this thesis concerns mindfulness as a mechanism of action for positive psychological concepts and mindfulness incorporated in positive psychology theory (e.g. Broaden-and-Build theory (Fredrickson, 2004; Garland et al., 2015)). This is discussed in Section 1.4.6 below.

Other theories less relevant to this thesis (e.g. Self-Determination Theory, Relational Frame Theory, Theory of Planned Behaviour, etc.) are not considered in detail; however, for more information on these theories, see Deci and Ryan (2012), Hayes et al. (2001), and Ajzen (1991), respectively. Additionally, there are other, more general theories which are relevant to dose in MBPs, for example theoretical rationales behind MBPs often being conducted in a group format and related group processes occurring during therapeutic programs (Yalom, 1983). These are not considered in detail here but will be picked up in the thesis at relevant points.

The four most influential theories that have informed thinking in this thesis are Mechanisms of Mindfulness (Shapiro et al., 2006), Metacognition (Teasdale, 1999), Modes of Mind (Williams, 2008), and Liverpool Mindfulness Model (Malinowski, 2013). These are reviewed with research and the implications of how these theories relate to dose are considered.

1.3.1 Mechanisms of Mindfulness (Shapiro et al., 2006)

This theory focuses on the mechanisms underlying mindfulness practice and how these affect change while emphasising their theory to be only one of many potential theories around mechanisms of mindfulness. According to this theory, there are three axioms or “fundamental building blocks” (Shapiro et al., 2006, p. 375) of mindfulness, termed intention, attention, and attitude (IAA), which are all incorporated in the above-introduced definition by Kabat-Zinn (1994). These three components are not viewed as separate stages of mindfulness, but rather as corresponding facets of a simultaneously occurring process (Shapiro et al., 2006).

Intention relates to the purpose of practicing, thus setting the context for practice. With continued practice and insight, intentions are set to develop towards higher-order intentions. Research has demonstrated that having set intentions prior to practice significantly related to improved outcomes (Bishop et al., 2004; Shapiro et al., 1992).

Attention as the second axiom is thought of as being beyond attention as detailed in cognitive theories such as switching of attention (Posner, 1980) or working memory (Baddeley & Hitch, 1974), where attention is perceived as merely paying attention non-mindfully through understanding or judgement. Alternatively, mindful attention is thought to be linked more closely, though not entirely, to theories such as Gestalt's integrative awareness, where present moment awareness and focus in itself are deemed interesting to interpret (Perls et al., 1958). As opposed to Gestalt theory however, mindfulness focuses on purposeful rather than relaxed attention (Brown et al., 2007). Research has supported the importance of mindful attention, with benefits such as changes to behaviour and motivation (Papies et al., 2015) and social connectivity (Quaglia et al., 2015).

Thirdly, the axiom attitude relates to the particular quality with which attention is paid in mindfulness meditation. A mindful attitude should hereby not be seeking hedonic wellbeing, in the sense of pleasure attainment and pain avoidance (cf. Ryan & Deci, 2001), but rather adopting an accepting, kind, and non-judgemental manner towards all thoughts and experiences (both positive and negative) with the aim of striving towards equanimity (Shapiro et al., 2006). Mindful attitude differs from approaches in CBT where the focus is typically on assessing and changing rather than accepting negative thoughts (Beck, 1979). Research applying Shapiro et al.'s theory has found beneficial effects of mindful attitude as this was inversely correlated with habitual worry (Verplanken & Fisher, 2014).

According to Shapiro et al. (2006), through engaging in a high dose of ongoing mindfulness practice, individuals are able to embody the three axioms IAA. Practicing with all three axioms, individuals are able to intentionally disengage from the contents of their thoughts and focus on present moment experiences (attention) with greater clarity and objectivity (attitude). This is theorised to lead to a shift in perspective from a subjective to an objective point of view, termed "reperceiving". Reperceiving is thought of as a "meta-mechanism of action" (Shapiro et al., 2006, p. 374) overarching other mechanisms of practice. Reperceiving has been compared to other concepts of shifting perspectives such as decentering, detachment and deautomatization (Safran & Segal, 1990; Deikman, 1982; Bohart, 1983, respectively). However, through a high dose of engagement with mindfulness, ones' personal perspective to internal processes is hypothesised to widen towards greater objectivity and awareness of all thoughts and emotions as they appear, thus continuously developing the self as opposed to detaching oneself from, or feeling indifferent to, negative or maladaptive thoughts (Shapiro et al., 2006). In a qualitative study, participants' accounts were consistent with the notion that mindfulness practice improved their ability to reperceive (Schussler et al., 2019) and research incorporating reperceiving has found mindfulness to be associated with lower levels of malicious envy (Dong et al., 2019). Research supporting Shapiro et al.'s theory has also been conducted on the psychological (Chiesa et al., 2014) and pain-related mechanisms of MBPs (Day et al., 2020), and mechanisms of mindfulness in the general population (Burzler et al., 2019), among others. Therefore, although Shapiro's theory does not explicitly state the dose needed for the meta-mechanism reperceiving to occur, it is theorised that higher doses of engaging with mindfulness are needed to result in this shift in perspective.

Other models on the mechanisms of mindfulness that have built on Shapiro et al. (2006) include: the Buddhist Psychological Model by Grabovac et al. (2011), which incorporates Buddhist foundations into mechanisms of mindfulness; models of mechanisms from a conceptual and neural perspective, which focus on the process of developing mindful states (Hölzel et al., 2011); and Monitor and Acceptance Theory, which focuses in particular on monitoring attention and accepting momentary experiences (Lindsay & Creswell, 2017). However, these are not covered in detail for reasons given in the introductory section above.

1.3.2 Metacognition (Teasdale, 1999)

In contrast to Shapiro's mechanisms of action, Teasdale's (1999) theory on metacognition focuses on a specific population, namely on the treatment of depression, particularly relating to avoidance of relapse and recurrence. Teasdale conceptualises metacognition in relation to the Interactive Cognitive Subsystems framework (Barnard & Teasdale, 1991) according to which distinct mental codes represent different aspects of experience. With regards to depression, there are two relevant meaning codes; one is propositional and one implicational (Teasdale, 1999). By this account, metacognitive knowledge resides within the propositional level and refers to the theoretical and actual knowledge that individuals have stored through, for instance, schooling or common world knowledge (Flavell 1979). Metacognitive knowledge is often adopted in individuals with depression where thoughts are understood as facts as opposed to being subjective themes (Quiles et al., 2015), in this way akin to Shapiro's (2006) subjective point of view (see above). However, a key limitation of metacognitive knowledge is that it is not directly connected to emotional experience and is thus generally insufficient to lead to emotional change (Teasdale, 1999). Rather, according to this model, it is an implicational meaning that connects directly with emotion. Therefore, Teasdale (1999) argues that for individuals to recover from depression and prevent relapse and recurrence, there is a need for change at an implicational level, which he refers to as metacognitive insight. Such metacognitive insight could include, for example, experiencing thoughts as being merely thoughts and not in fact reality (cf. Flavell, 1979), which can help break ruminative negative thought cycles (see Chapter 2, Section 2.2.4 on theory of rumination and worry). Metacognitive insight is theorised to be achieved through greater dose of mindfulness practice (Teasdale, 1999).

MBCT seeks to generate such metacognitive insight through both, relatively high-dose mindfulness practices and cognitive therapy exercises aimed at changing participants' relationship to their experience (Segal et al., 2002; Teasdale et al., 1995). Within Teasdale's model, mindfulness is therefore thought to act on the deeper level of meaning (implicational level), which directly connects with the emotional experience. In practice, the focus of MBPs is on changing ones' relationship to depressive thoughts rather than changing their content. Depressive thoughts are therefore experienced to a greater extent as events happening in the mind as opposed to being regarded as a true reality which therefore reduces the probability of relapse (Barnard & Teasdale, 1991). Therefore, it is implied by Teasdale (1999) that a higher dose of mindfulness practice is required in this population of individuals with depression. Additionally, MBPs are thought of as helpful in reducing relapse and recurrence of depression by building up mindfulness skills and metacognitive insight during a non-depressed state to allow participants to have the necessary tools available when potential relapse threatens to occur (Teasdale et al., 2000). Research has supported Teasdale's theory, particularly in relation to MBCT for depression (e.g. Teasdale et al., 2002) and higher dose of practice over the long-term (e.g. Mathew et al., 2010).

Based on Teasdale's theory, other metacognitive models have been developed, for instance incorporating neuropsychological evidence in metacognition of mindfulness (e.g. Jankowski & Holas, 2014) and the Metacognitive Processes Model of Decentering (Bernstein et al., 2019). Building on Teasdale's work, Dorjee (2016) has developed a broader model of metacognitive self-regulatory capacity as a mechanism applied to a wider range of different meditations and described various modes of existential awareness achieved through meditation. Similar to Teasdale's shift from metacognitive knowledge to insight, a shift between different states of being is achieved through the high dose of ongoing meditative practice with the ultimate aim being a pristine state of existential awareness. By this account, this represents the ultimate understanding of the self, where acting with compassion is a natural state (Dorjee, 2016). However, Dorjee's model is not focused on in detail in this thesis, since it includes different types of meditation other than just secular mindfulness, which is the focus here.

1.3.3 Modes of Mind (Williams, 2008)

Drawing on Teasdale (1999), the Modes of Mind theory was devised (Segal et al., 2002; Williams, 2008), according to which individuals operate within two different cognitive modes of mind: on the one hand, a discrepancy-based mode which is characterised by habitual rumination, avoidance, perfectionism, and maladaptive processing, also referred to as a constant "doing" mode of mind where the aim is to achieve a desired state and minimise any undesired states (Williams, 2008). On the other hand, the higher-order mode of mind is characterised by intentionally acknowledging and experiencing every moment without judgement, also termed "being" mode of mind, thus recognising and interrupting negative thought patterns in depression. The being mode of mind has also been found to be associated with greater trait and/or state mindfulness and the resolution of the discrepancy between a desired and actual state of mind where instead of aiming for a desired outcome, the person is simply aware of the present moment (Williams, 2008). Through mindfulness practice, individuals are able to recognize, disengage and switch from the former to the latter mode of mind (Marx, 2015; Segal et al., 2002; Williams, 2008).

Similar to Teasdale's theoretical account of metacognition, according to Modes of Mind theory, the ability to switch from the doing to being mode of mind is thought to be improved with greater dose of mindfulness practice, though again, specific details with regards to dose have not been included by Williams (2008). Though initially developed for depression, the Mode of Mind model has also been applied in, for instance, organisational contexts (Lyddy & Good, 2017) and education (e.g. Hyland, 2014). More details on the application of this model to research in this thesis is specified in Chapters 2, 5 and 6.

1.3.4 Liverpool Mindfulness Model (Malinowski, 2013)

The next theory considered with regards to dose in MBPs and mindfulness practice is the Liverpool Mindfulness Model (LMM). Within the LMM, attention and the development of attentional skills is understood as a central component, where mindfulness practice with motivational factors such as the right intention is theorised to enhance attention (Malinowski, 2013). Though the LMM does not make any explicit statements about the dose required for any individual practice, it does postulate that relatively ongoing, repeated doses of mindfulness practice are necessary for the core process of attentional control mechanisms to interrelate with flexible emotional and cognitive responding (Malinowski, 2013; Moore 2012). Mindfulness practice is thought to further enhance the benefits of and stabilise increased attention resulting in the mental stance of non-judgemental awareness. The combination of flexible attention and non-judgemental awareness is then theorised to predict positive behaviour changes as well as physical and mental wellbeing (Malinowski, 2013). The importance of non-judgemental awareness and practicing with the right intention and attention as described in the LMM thus echoes that of previous theories described (Shapiro et al., 2006; Williams, 2008) and the definition of mindfulness given by Kabat-Zinn (1994), but with a particular focus on attentional processes and functions. In particular, the LMM focuses on attentional control within mindfulness meditation to foster the benefits of mindfulness. Additionally, similarly to Teasdale (1999), the importance of cognitive and emotional flexibility is outlined in the LMM where with mindfulness practice, flexibility over thoughts and feelings is exercised thus helping individuals to relate to their thinking differently. This is then associated with reduced cognitive fusion of rumination on maladaptive thoughts and translates to individuals approaching experiences more flexibly. The author also describes the neural processes, networks, and stages of attention regulation during mindfulness practice (see Malinowski, 2013). This model has been supported by research demonstrating that attentional control processes strengthen after regularly engaging in relatively brief but ongoing doses of 10-minute mindfulness practices over several weeks (Moore et al., 2012).

1.3.5 Summary of Mindfulness Theories in terms of Dose

In summary, there were four main theories which have informed thinking in this thesis with regards to dose in MBPs and practice. Overall, authors of all four mindfulness theories have suggested that greater doses of mindfulness in terms of ongoing mindfulness practice is beneficial for positive outcomes. In addition, while not explicitly stated by these theories, there is an implication in some theories (e.g. Shaprio et al., 2006; Teasdale, 1999; Williams, 2008) that doses of longer formal individual practice and engagement with MBPs may be helpful for outcomes whereas this is not implied in the same way in the LMM (Malinowsk, 2013). However, it is worth noting that none of the above-considered theories are decidedly specific with regards to the best dose in MBPs and practice. Relevant theory is also covered in depth in subsequent chapters, relating to dose as considered in each chapter, where appropriate.

The above and other theories of mindfulness have been informed by and have formed research; this is reviewed with regards to dose next.

1.4 Selective Review of Mindfulness Literature in terms of Dose

In order to explain why research on dose in mindfulness is important, the broader field of mindfulness literature is briefly considered. Research, including quantitative, qualitative, and mixed methods, on mindfulness and MBPs has grown exponentially over the past four decades (Goldberg et al., 2017; Shapiro & Weisbaum, 2020). In fact, the term “mindfulness” appeared four times as often in 2007 compared to 1980 in published books (Valerio, 2016) and before the year 2000, 39 peer-reviewed research papers on mindfulness were published compared to over 6,000 since 2000 (Bernstein et al., 2019). According to a bibliometric analysis by Baminiwatta and Solangaarachchi (July 2021) on the development and trends in mindfulness research, from the period of 1966 to 2021, 16,581 papers on mindfulness were published. Even when considering that psychology research has been growing on the whole (Krampen et al., 2011), mindfulness research has still increased considerably across different areas and disciplines and has also sparked booming interest outside academia (van Dam et al., 2018). Since it is impossible to review all the extensive mindfulness literature within the scope of this thesis, the context of mindfulness research is reviewed with the purpose of providing a focused review of research as it currently stands and which the topic of dose in this thesis builds upon.

The selective review of mindfulness research that follows draws on the relevant broad research areas in the field identified in a recent special issue on mindfulness (Bernstein et al., 2019) while recognizing that not all foci of the vast mindfulness literature could be included. The areas explored within this special issue are used as a template for the research overview provided in this thesis and explores these in relation to dose.[2] The areas are research of dose in mindfulness for physical health conditions, mental health conditions, the general population, and other mindfulness research areas, as well as mechanisms of action and criticism of mindfulness in relation to dose. Literature related to each of the specific parts and research questions of this thesis is reviewed separately in subsequent chapters (Chapter 2 for research on doses in mindfulness, Chapter 6 for research on mindfulness practice lengths, and Chapter 7 for research on mindfulness inductions).

Prior to reviewing relevant mindfulness literature in terms of dose, it is worth outlining common research limitations identified across the literature which apply across studies. To avoid repetition, these will be covered first since they apply to many of the included studies in the subsequent literature review.

[2]Please note, some of the areas included in the special issue, such as historical and conceptual foundations, and mindfulness theory, have already been introduced in Sections 1.2 (foundations) and 1.3 (theory). The list of mindfulness research as outlined by the special issue is by no means exhaustive, but for purposes of brevity, only areas most closely relevant to the topic of this thesis are explored. Areas not closely related to the thesis, but which have started to research dose-effects, i.e. related to neuropsychology, cognitive processes, and compassion, are only introduced briefly in terms of dose.

1.4.1 Key Limitations of Mindfulness Research

Research on mindfulness has been criticised, including issues associated with methodological quality. In a large-scale review examining the quality of mindfulness research published over 16 years, no statistically significant improvements in quality over time were found for a number of factors (Goldberg et al., 2017). Specifically, there are several key methodological considerations within the mindfulness literature. This includes the fact that generally, the majority of published studies have employed inactive controls, thus not controlling for possible demand characteristics, which are lessened to an extent when including active control groups (Baer, 2003; also see Chapter 2, Section 2.3.3.2). Although including active controls improves quality, the issue of blinding still persists since it is near impossible to fully blind participants as to their group allocation in psychological studies (see Chapter 3, Section 3.4.3). Additionally, when comparing to active controls, there often tends to be a much smaller or no difference in effect. There are different possible explanations for this: i) although MBPs may be effective for reasons theoretically given (see Section 1.3), there may be other interventions/programs which are effective on other grounds and might in fact have a similar level of effectiveness; ii) simply participating in an intervention/program and the expectations this generates can be found helpful. In other words, the question arises to what extent the mindfulness element of MBPs has an effect rather than simply taking part in a program/group. Any effects may thus be due to the common factors experienced when participating in an intervention/program rather than the specific ingredients of the intervention/program itself (=therapy effect) and comparing MBPs against active controls exposes this possibility; iii) effects could be Type II errors, especially where studies are not powered to the level needed to find an effect since when comparing any two or more active interventions/programs, the difference is likely to be smaller due the points raised above and thus greater power is needed. For more detail on active and inactive controls, see Chapter 2, Sections 2.3.3 and 2.3.4.

Next, a methodological issue commonly reported is that studies often lack long-term follow-up assessments and thus no conclusions with regards to the long-term effectiveness of a given MBP is possible (Bishop, 2002). According to Goldberg et al. (2017), program fidelity assessment to assess program validity is often missing as well as intention-to-treat analysis to account for possible program drop-outs. Information on teacher' training and experience to establish credibility is also often not recorded and reported sufficiently, this is detailed further in Chapter 2, Section 2.3.4.2. Finally, methodological limitations in mindfulness research also relate to measurement since this can be ambiguous at times due to possible overlap with other constructs (Fried, 2017; Chapter 2, Section 2.3.5.4).

Some of these methodological limitations and issues are due to lower-scale and pilot research being conducted, which often examines new areas, in addition to the smaller number of higher-quality and well-funded research studies; a mix of quality is therefore to be expected. Nevertheless, there is a need to be aware of any limitations of research and to draw conclusions tentatively. Methodological concerns more specifically relating to dose in mindfulness are also discussed in subsequent chapters. These limitations and others minimise the confidence we can have in the results of mindfulness research studies. Additionally, implications of lower quality research has the potential negative effect of false conclusions being drawn, both within academia and in practice, which can then form incorrect foundations for research and practice to follow. It is worth holding the above limitations in mind for the literature review which follows since these apply across a large number of research studies within the literature.

1.4.2 Doses of Mindfulness for Physical Health Conditions

Research on different doses of mindfulness has been completed for physical health conditions. As outlined in Section 1.2.3 above, MBSR was originally developed and found effective for individuals with chronic pain (Kabat-Zinn, 1982). Since its commencement, researchers have endeavoured to further understand this program (e.g. Bishop, 2002; Greeson & Chin, 2019) with more and larger-scale research being completed with different doses of MBSR and MBPs for chronic pain and physical health conditions, which has been outlined in more detail elsewhere (e.g. Carlson, 2015). This includes a selective review of randomised controlled trials (RCTs), where various doses of MBPs (varying between three days and eight weeks) have been found effective for pain management in adults with various physical health conditions. (Creswell et al., 2019). These MBPs were found superior to health education and social support applications, though Creswell et al. (2019) argue that more research into doses for physical health benefits needs to be completed. This review needs to be considered with some caution due to various physical health conditions and MBP doses being grouped and reviewed together rather than each condition, population and MBP dose grouped separately. Several reviews and/or studies have also been completed on the effectiveness of different doses of MBSR and MBPs for distinct physical health conditions, including long-term physical health conditions (e.g. Crowe et al., 2016), chronic lower back pain (e.g. Day et al., 2020), cancer (e.g. Cillessen et al., 2019; Zhang et al., 2018), multiple sclerosis (e.g. Simpson et al., 2014), fibromyalgia (e.g. Haugmark et al. 2019; Lauche et al., 2013), rheumatoid arthritis (e.g. DiRenzo et al., 2018), cardiovascular disease (e.g. Scott-Sheldon et al., 2020), sleep difficulties (Shallcross et al., 2019), and HIV/AIDS (e.g. Scott-Sheldon et al., 2019; Riley & Kalichman, 2015), among others. Findings of the above research with physical health conditions generally not only show significant positive changes to pain and illness management, but also improvements to psychological distress and life satisfaction for patients. Nevertheless, these results need to be regarded somewhat tentatively due to studies often having relatively small samples and methodological limitations, for instance RCTs with inactive control comparators, and lack of examination of long-term effects of MBPs (see Section 1.4.1 above for details).

Additionally, although there has been a start in exploring the effectiveness of doses of MBPs other than MBSR and of programs delivered via online doses (e.g. Toivonen et al., 2017) for physical health conditions, a level of ambiguity related to the optimal doses regarding amount and type of MBP for particular physical health conditions still exists in research (Creswell et al., 2019). Despite growing research on MBPs for physical health conditions, the literature as it currently stands is not as comprehensive as research of MBPs for mental health conditions (Creswell, 2017; Creswell et al., 2019); this is reviewed next.

1.4.3 Doses of Mindfulness for Mental Health Conditions

Similar to physical health conditions, research on different doses has been completed for mental health conditions. As introduced above, MBCT was first developed as a relapse prevention intervention for those prone to relapse to depression (Segal et al., 2002; 2013), and MBCT, MBSR and other doses of MBPs have been offered for other mental health conditions and mental wellbeing more generally (e.g. Keng et al., 2011). Many research studies, systematic reviews, and meta-analyses, all of which cannot be reviewed within the scope of this thesis, have since been conducted to examine the effectiveness of different doses of MBPs for individuals with mental health conditions, most notably depression, anxiety, and stress. For instance, in a meta-analysis of MBSR and MBCT doses offered to clinical populations, a significant, positive, moderate effect on depression and anxiety at post-program was found (Hofmann et al., 2010). This result is of particular value since the authors had previously expressed that they did not expect these doses of mindfulness to have any, or if, only a very small, effect (Hofmann & Asmundson, 2008) and are therefore unlikely to have been influenced by expectancy bias in their review. Additionally, in a large-scale comprehensive meta-analysis of over 200 studies with more than 12,000 participants, different doses of MBPs (ranging from four to 31 treatment hours) were found beneficial for a range of psychological distress outcomes, but especially for depression, anxiety, and stress (Khoury et al., 2013). This effectiveness of MBP doses for mental health outcomes has also been demonstrated in more recent research (e.g. Enkema et al., 2020; Goldberg et al., 2018; Goyal et al., 2014; Potes et al., 2018). However, caution again needs to be exercised with regards to key research limitations (see Section 1.4.1). Other studies have shown dispositional mindfulness to be associated with improved mental health (e.g. Tomlinson et al., 2019). However, although individuals with measured higher levels of dispositional mindfulness have been found to have higher levels of day-to-day mindfulness, the benefits of mindfulness are not limited to these individuals (Brown & Ryan, 2003). Details on the effectiveness of doses of mindfulness in addressing underlying mechanisms of depression, anxiety, and stress are outlined in Chapter 2, Sections 2.2.4 and 2.2.7.

Moreover, with research being extended across other forms of psychological distress, doses related to MBPs have been found helpful for other mental health conditions, for instance post-traumatic stress disorder (PTSD; e.g. Boyd et al., 2018; Goldberg et al., 2020; Lang, 2017), obsessive-compulsive disorder (OCD; Leeuwerik et al., 2020), and psychosis (e.g. Chadwick, 2019; Jansen et al., 2020), among others. Doses of MBPs have also been discovered as beneficial for eating disorders (e.g. Barney et al., 2019), with some MBPs specifically designed to treat eating disorders (e.g. Kristeller et al., 2014). Similarly, MBPs have been found helpful for substance abuse and addiction (e.g. Chiesa & Serretti, 2014), with the dose of MBPs adapted and designed for the treatment and prevention of specific addictions (e.g. Alizadehgoradel et al., 2019; Maglione et al., 2017; Sancho et al., 2018; Witkiewitz et al., 2005). MBPs were identified similarly as effective as other psychotherapy treatment programs including cognitive and dialectical behaviour therapies for substance use disorder in adults (e.g. Lo Coco et al., 2019). In a study comparing effectiveness of mindfulness-based addiction treatment (MBAT) and CBT to usual care for smoking cessation, both MBAT and CBT had similar positive effects on smoking cessation whereas MBAT participants showed greater perceived control over deciding to smoke and lower occurrences of anger than both, CBT and controls (Spears et al., 2019).

For individuals with the above and other mental health conditions, different doses relating to types and lengths of programs have increasingly been developed, including briefer (e.g. Blanck et al., 2018; Howarth et al., 2019 Schumer et al., 2018) and online doses of MBPs (e.g. Flett et al., 2020; Sevilla-Llewellyn-Jones et al., 2018; Spijkerman et al., 2016). In fact, Segal (2011), one of the developers of MBCT, has predicted that the future of MBPs was likely to be via online doses. This notion has since been echoed by other mindfulness researchers (e.g. Mrazek et al., 2019). Further details of such doses of programs are outlined in Chapters 2 and 6. Although most mindfulness research so far has focused on physical and mental health problems research has also been conducted with the general population.

1.4.4 Doses of Mindfulness for the General Population

Extensive research has found positive effects of various doses relating to MBPs in the general population including for improved wellbeing (e.g. Galante et al., 2018; Lomas et al., 2019), reduced distress (e.g. Pascoe et al., 2017; Querstret et al., 2020), quality of life, and social functioning (e.g. de Vibe et al., 2018).

Furthermore, mindfulness and MBP research with different doses has been completed in different settings and with different general population samples. This includes qualitative research with participants in the workplace, where participating in an MBP was thought of as helpful for emotional awareness in leaders and greater understanding of work-related stressors thus improving leadership responsibilities (Dix et al., 2021). Similarly, in several reviews with employees in different workplace settings, beneficial effects for employee wellbeing, psychological functioning, and job satisfaction, as well as reduced burnout and psychological distress, was observed with effects maintained up to 12 weeks post-program (Bartlett et al., 2019; Janssen et al., 2018; Vonderlin et al., 2020). This suggests that MBPs can successfully be implemented in the workplace. However, research suggests it is worth adapting the dose of mindfulness training to the context of a specific organisation and employment level (Rupprecht et al., 2019). Additionally, various doses of MBPs have also been identified as helpful for wellbeing, stress management, and job performance for healthcare professionals (e.g. Rudaz et al., 2017; Scheepers et al., 2019; van der Riet et al., 2018), whereas various doses of MBPs, including lower-dose versions of more intense MBPs (e.g. shortened to four-week doses), were found to be advantageous (Kriakous et al., 2021; Lomas et al., 2019). MBP doses have also had beneficial effects for different demographics within the general population, such as older adults where regular mindfulness practice was found to improve sleep quality (Hazlett-Stevens et al., 2018), and promote healthy aging (Klimecki et al., 2019) and cognitive processing (Malinowski et al., 2015; 2017). Additionally, MBPs have had positive effects in education settings, where research conducted with university students and educational professionals has found positive changes to distress and anxiety, whereas no discrepancy in effectiveness existed between different doses of programs (Dawson et al., 2019; Lomas et al., 2017).

Despite the above presented research generally having discovered favourable effects of MBPs for different general population participants, in a recent large-scale review and meta-analysis of different doses of MBPs (ranging from four to 30 contact hours) for mental health promotion, although participating in an MBP significantly improved common mental health difficulties and wellbeing in non-clinical populations when compared to inactive controls, no such effect was observed when participating in an MBP was compared to active controls, such as physical exercise (Galante et al., 2021). See Section 1.4.1 above with regards to this as well as other key limitations in mindfulness research. A limitation with regards to dose to be noted is that the dose of online MBPs was not included in Galante et al.'s research and conclusions can therefore not be drawn outside of doses of face-to-face programs. Additionally, limitations identified by Galante et al. were that mindfulness was not found effective in every general population setting given the large heterogeneity of included studies and high risk of bias present in several included trials. In addition to this evidence for positive effects of doses of in-person MBPs (at least when compared to inactive controls), beneficial effects of MBPs offered by several other means have been observed. Specifically, different doses of MBPs are available for general populations, which often include different forms of delivery, such as smartphone apps (e.g. Bostock et al., 2019; Champion et al., 2018; Economides et al., 2018; Howells et al., 2016), online MBPs (e.g. Jatawardene et al., 2017; Querstet et al., 2018), and abbreviated versions of longer programs such as MBSR or MBCT (e.g. Demarzo et al., 2017; Klatt et al., 2009), which all commonly identified promising findings for general population samples.

1.4.5 Dose in Other Mindfulness Research Areas

The above selective review covers mindfulness research most closely related to the topic of this thesis. However, other research areas concerning mindfulness exist, which have started to research dose-effects. Some of these research areas are now briefly covered in terms of dose.

1.4.5.1 Neuroscience and Cognitive Processes in Doses of Mindfulness

Another area of research has explored the neural mechanisms of different doses of mindfulness, which has been presented in more detail elsewhere (e.g. Hölzel et al., 2011; Tang et al., 2015). Reviews of neuroimaging studies observed improved insular cortex activity following participation in relatively high doses of MBPs (between seven and eight weeks) resulting in enhanced awareness and internal momentary reactions (Young et al., 2018) as well as significant brain structure changes to volume and density, particularly in the prefrontal cortex and regions associated with body awareness (sensory cortices, insula) in meditators (Fox et al., 2014). Additionally, significant alterations in EEG theta networks, especially relating to smaller path lengths and increased network clustering, have been observed even after a lower-dose, short-term meditation (Xue et al., 2014). A review of the neuroanatomy of long-term meditators engaging in higher doses of mindfulness practice found that, across different studies, long-term meditators generally showed larger global, regional, and local anatomical measures including grey matter volume, brain structure, and cortical thickness across several regions in the brain compared to controls, suggesting that prolonged, high-dose mindfulness meditation has the potential to alter the physical structure of the brain (Lüders & Kurth, 2019). However, it was unclear whether the brain structure, personality or mental capacities of long-term meditators were already different prior to commencement of meditation practice and how much of the neuroanatomy was a consequence of having engaged in a dose of long-term practice rather than a pre-existing characteristic. Further research is therefore needed to examine neuroscientific effects of mindfulness, and comparing different doses of mindfulness practices and lengths, for different participant groups.

Mindfulness research has also focused on cognitive processing and memory performance in terms of dose. Engaging in different doses of mindfulness practice ranging from a few days to a few weeks has been found to be related to enhanced selective, executive, and sustained attention, and improved working memory capability in a systematic review (Chiesa et al., 2011). However, the findings of this review need to be interpreted with caution due to lower quality and heterogeneity of included studies and effects across different doses having been examined. In a recent meta-analysis and meta-regression reviewing effects of different MBP doses (between three and 18 sessions), a significant small effect was only found on executive function but not on attention, working memory or long-term memory (Im et al., 2021). Again, substantial heterogeneity of included studies was observed, and meta-regression analyses revealed significant moderating effects of the MBP-type dose on attention and executive function (Im et al., 2021). Nevertheless, in another review, participating in a relatively high-dose MBP, namely MBSR and MBCT, although enhancing working memory, cognitive flexibility, and meta-awareness, was not found to be significantly associated with increased attention nor executive functioning, leading to calls for further research and theoretical clarification (Lao et al., 2016). Levi and Rosenstreich (2018) suggest vigilance when examining working memory processes changing resulting from engagement with doses of mindfulness practice since this may depend on the type of memory system assessed. Finally, although a start has been made (e.g. Kaunhoven & Dorjee, 2021; Lykins et al., 2012), generally, research often examines the immediate cognitive effects of a single and relatively brief lab-based mindfulness practice dose, rather than long-term cognitive effects of doses such as repeated practice over time, which would be important to explore in future research.

1.4.5.2 Doses of Mindfulness for Compassion

Another research area which has enjoyed considerable attention in the literature is the study of different doses of mindfulness for compassion; this has been presented thoroughly elsewhere (e.g. Germer & Barnhofer, 2017; Gilbert & Choden, 2015; Gilbert, 2019). For instance, participants who received a relatively low-dose of a three-week app-based MBP responded with greater compassion and prosocial behaviour towards a person in pain than controls (Lim et al., 2015). Similarly, in a meta-analysis, participating in different doses of MBPs (ranging from six to fifteen weeks) was found to enhance empathy and compassion in children and adolescents (Cheang et al., 2019). Engaging in different doses of mindfulness practice has not only been found to engender compassion towards others, but also towards the self. For instance, in a review with healthcare professionals, participating in a variety of different MBP doses (ranging between six and 40 sessions) was found to improve self-compassion (Wasson et al., 2020). The same was found for participants in MBSR, where improved self-compassion along with empathy were observed post-program (Birnie et al., 2010). However, different doses of mindfulness for compassion have so far not been assessed comparatively, which would be valuable to do in the future.

A variety of other meditation programs have also been found helpful in fostering compassion, such as compassion-focused therapies, which, although incorporating some components of mindfulness, do not have mindfulness as their main focus (cf. Gilbert, 2014; 2019; Luberto et al., 2018). For details on the definition of what constitutes an MBP from the perspective of this thesis, see Chapter 2, Section 2.3.1.

1.4.6 Mechanisms of Action of Mindfulness in terms of Dose

Another area worth exploring is mechanisms of action of mindfulness and MBPs in terms of dose. Mechanisms of mindfulness are only briefly introduced since this is covered in more detail in subsequent chapters, in particular relating to different doses of MBPs and practice (Chapters 2-5), as well as state and trait mindfulness as mechanisms of mindfulness practice (Chapters 6 and 7).

In a systematic review of doses of MBCT and MBSR mediation studies, the findings were consistently that there were several psychological mechanisms (as statistical mediators) underlying MBCT and MBSR (Gu et al., 2015). These mechanisms included cognitive and emotional reactivity, as well as mindfulness, rumination, and worry, which were found as significant statistical mediators between participating in an MBSR or MBCT program and mental health outcomes (Gu et al., 2015). Furthermore, in a meta-ethnography of qualitative studies examining experiences of the process of participating in an MBSR or MBCT program in individuals with chronic depression, mindfulness emerged as an underlying mechanism of action in these MBP doses, identified by participants who felt better able to develop an understanding of their difficulties (Malpass et al., 2012). Additionally, in a meta-synthesis of group-based doses of MBPs, several mechanisms of action of MBPs were described by participants with mental health difficulties, including awareness of maladaptive habits, acceptance and non-judgement of thoughts, and change in attitude towards the self (Wyatt et al., 2014). A qualitative review with healthcare workers also identified overcoming challenges related to engaging in doses of mindfulness practice and changing one’s relationship to the self as mechanisms of action of mindfulness training (Morgan et al., 2015). Mechanisms of different meditations within contemplative science with a particular focus on the meta-cognitive self-regulatory capacity have been described in detail elsewhere (Dorjee, 2016).

1.4.6.1 Mechanisms of Action of Mindfulness Doses for Positive Psychological Outcomes

Another key area of mechanisms of mindfulness explored in this thesis is the effect of mindfulness doses on positive psychological outcomes. Higher doses of mindfulness programs and practices have been found to act as a mechanism for increasing positive psychological variables in previous research. For instance, a recent meta-analysis of higher-dose MBPs at work has found these to be effective for increasing compassion, empathy, and positive wellbeing (Lomas et al., 2018). Additionally, participating in an 8-week online-delivered MBP significantly predicted increased levels of optimism and affect in direct-care employees (Heckenberg et al., 2019). Mindfulness practices have also been found to act as a mechanism for hope and gratitude outcomes. For instance, Bluth and Eisenlohr-Moul (2017) showed that participating in a higher-dose MBP of 8-weeks has been associated with increased gratitude, and a study examining the effects of mindfulness meditation delivered in a relatively high dose of face-to-face sessions over 12 weeks, found increased hope compared to controls in university students (Sears & Kraus, 2009).

Mindfulness as a key mechanism of action for positive psychological resources has also been supported by theory. For instance, according to Fredrickson's (2004) Broaden-and-Build theory, increasing positive psychological resources broadens one's awareness, encourages positive thoughts and actions and builds personal resources; mindfulness practice is thought to aid this process of broadening awareness (Garland et al., 2015). According to hope theory, Snyder (1994) theorized that meditation acts as a mechanism to help calm the mind which in turn allows greater deployment of attentional resources by focusing on moving towards hoped for goals and thus increasing individuals' current sense of hope (Munoz et al., 2018). Additionally, mindfulness has been theorised to act as a mechanism for individuals experiencing greater gratitude, since gratitude has been defined as individuals' mindful awareness of the positive things in life (Emmons & Mishra, 2012).

Furthermore, although some evidence and theory exists in support of mindfulness as a mechanism of action in higher dose MBPs, mechanisms of mindfulness with respect to low and single-dose practice has yet to be examined, in particular whether single dose mindfulness practice is sufficient to change state hope and state gratitude. Single-dose mindfulness practices have been utilized due to providing the possibility of tightly controlling length, dose, and type of practice, resulting in researchers being able to draw more specific causal inferences (Tang et al. 2015). Previous research has found single-dose mindfulness practices to act as a mechanism in reducing psychological distress state outcomes (e.g. Johnson et al. 2015; Leyland et al. 2019). Based on above mentioned theory and research, we might therefore expect that a single dose of mindfulness practice also has an effect on positive psychology state outcomes; however, it remains unclear whether a single dose would be sufficient to change these outcomes. This is covered in more detail in Chapter 7. Therefore, in this thesis, mechanisms of action of mindfulness are explored by examining the effectiveness of different doses related to MBPs and practice for various psychological outcomes.

1.4.7 Criticisms related to Dose of Mindfulness: Implementation and Adverse Effects

1.4.7.1 Criticism of Implementation of Mindfulness including in relation to Dose

Furthermore, with the topic of mindfulness enjoying increasing popularity both within academia and mainstream media, exaggerated positive claims of different doses of mindfulness not based on empirical research have been made due to societal pressures, which have been criticised for painting mindfulness as a panacea to heal all ills (Gunderson, 2016; van Dam et al., 2018). An issue associated with mindfulness believed to be a panacea is that it invites the outlook that individuals are solely responsible for their own wellbeing rather than societal systems or maladaptive organisational contexts (Arthington; 2016). Mindfulness being viewed as a panacea thus misappropriates the foundations of practice since it is being separated from its ethical context, and oftentimes does not address underlying maladaptive or unethical circumstances (Purser & Milillo, 2015; Purser et al., 2016). Mindfulness has also been described as a “hype” (e.g. van Dam et al., 2018) with it often understood that a single-dose practice can heal the most dysfunctional environments without anything else needing to change (Marx, 2015; cf. Purser & Loy, 2013; Purser, 2018).

Furthermore, some researchers have criticised the field for increasingly disregarding the Buddhist roots of mindfulness and becoming too secularised. It has been argued that this loses sight of the intention of flourishing and Buddhist ideas on existential awareness, and has been described as “McMindfulness”, and a quick fix solution (Purser, 2013; Purser 2015). In particular, Dorjee (2010; 2016) has criticised the secular applications of mindfulness and argued that the intention of meditation should be about improving one’s self-understanding and cultivating an abiding state of existential awareness and enlightenment, rather than a temporary decrease of depression and anxiety. Thus, the intention of practice differs between different applications of secular and Buddhist meditation. It has been argued that a need therefore exists for the ethical and socially responsible integration of mindfulness, while at the same time being aware that certain limitations exist when a concept is taken out of its spiritual and is implemented within a secular context (cf. Dorjee, 2010, 2016).

Another criticism of mindfulness literature is the inconsistencies of conceptual definitions and absence of a single, all-encompassing definition resulting in ambiguity and confusion (see Section 1.2.3). This was evidenced in a research study where diverse conceptions of mindfulness were understood in university student participants (Hitchcock et al., 2016). What is known and not known about mindfulness therefore needs to be communicated accurately to the public to avoid confusion and incorrect interpretation of research results (Hanley et al., 2016; van Dam et al., 2018). This includes the difference between statistically significant findings and practical/clinical significance (van Dam et al., 2018; more on this in Chapters 2, 4, and 5).

1.4.7.2 Adverse Effects of Mindfulness including in relation to Dose

Despite the large literature base on its salutary effect and researchers having argued for the benefits of mindfulness across different causes for human suffering (Kabat-Zinn, 2005; see Chapter 5, Section 5.4.3), participants have also reported experiences of harm and unpleasantness potentially associated with different doses of mindfulness practice; expectations therefore need to be managed appropriately (e.g. Baer et al., 2019; 2021; Dobkin et al., 2012). Researchers have criticised mindfulness as having a non-monotonic effect where boundaries to the benefits of mindfulness may exist with increased doses (Lindahl et al., 2017; Lindahl & Britton, 2019). In other words, mindfulness practice can be experienced as too intense for some people, which in turn can have an inverted U-shaped effect where initial positive effects of practicing mindfulness turn negative with increased practice doses, also termed a too-much-of-a-good-thing effect (Britton, 2019). Non-monotonicity has in particular been found in the dose amount of mindfulness practice participants engage in. In fact, according to Britton (2019), the inverted-U-shaped and non-monotonic principle applies to mindfulness practice and MBPs since negative effects of mindfulness practice are often more likely with greater doses of practice or MBP intensity (this question of dose and effectiveness of different practice lengths is further explored within this thesis). For instance, in research by Britton et al. (2010; 2014), briefer mindfulness practice doses were related to increased sleep duration whereas longer practice doses (of more than 30 minutes) were associated with decreased sleep duration and depth.

Furthermore, despite the many beneficial outcomes of mindfulness practice explored above, engaging in mindfulness practice may not always be helpful, and in fact can result in adverse effects (Sahdra et al., 2017). For instance, in an RCT examining the effectiveness of a brief MBP-dose compared to relaxation training in chemotherapy patients, practicing mindfulness was related to increased distress and lower quality of life suggesting that mindfulness may not be helpful during acute stages of physical illness (Reynolds et al., 2017). In qualitative research with interviews of over 100 meditators, several negatively valanced and distressing experiences of mindfulness meditation were found (Lindahl et al., 2017). Similarly, in a large online survey, 25% of respondents reported unwanted effects such as panic/anxiety, headaches, and dissociation/depersonalisation after mindfulness practice (Cebolla et al., 2017); the difficulty persists to identify the dose of mindfulness practice needed to achieve the optimal level of distance/decentering from the self without resulting in dissociation (cf. Britton, 2019). From a neuropsychological perspective, high levels of insula activation and interoception within the context of mindfulness meditation have been found to be associated with increased emotional intensity, which can result in negative outcomes such as panic, stress, anxiety, and depression (cf. Britton, 2019; Craig, 2009).

In research, positive results are generally overrepresented, while negative outcomes are often not published, or if published, are frequently subject to post hoc analyses to present a positive effect (Coronado-Montoya et al., 2016). Additionally, missing data due to dropouts and lack of follow-up data, which is where negative effects are most likely to have occurred, are often not examined (Morone et al., 2017, see Section 1.4.1 above). Similarly, long-term meditators with mental health conditions are often excluded from studies, thus resulting in limited research with meditators with mental health conditions and how/whether this is related to their dose of mindfulness practice (Britton, 2019). Therefore, it is important for researchers in the field to understand the possibility of mindfulness having non-monotonic qualities, and some have called for more precise research into the boundaries and negative effects of mindfulness and the difference in MBPs effectiveness for different groups (Grant & Schwarz, 2011).

1.5 Research Aims and Questions explored in this Thesis

Based on the theoretical background and literature as it currently stands, this thesis sought to explore and answer several research questions with each consequent part of the thesis building on findings from the previous chapter(s). Since MBPs of different duration, delivery, and format, and with different amounts of practice (both in-class and outside-class), have been found beneficial, it is of interest to examine dose in MBPs and mindfulness practice.

There are three broad parts to this thesis. Firstly, this thesis aimed to explore dose-response relationships in MBPs by conducting a large-scale meta-analysis and meta-regression analysis of different MBPs exploring 15 different doses related to MBPs for four outcomes (depression, anxiety, stress, and mindfulness) at post-program and follow-up timepoints. This review sought to answer the question of whether dose-response relationships exist between different doses related to MBPs and outcomes and what these look like. Where possible, separate analyses are completed by population group and outcome measure employed. The rationale, methods, results, and discussions of this large-scale review encompass four chapters (2-5).

Secondly, since the practice of mindfulness has been considered the most important component in learning mindfulness (see Chapter 6, Section 6.2), and since dose-response relationships relating to practice were ambiguous in the dose-response meta-regression (see Chapters 4 and 5), the question arises whether there is a discrepancy in effectiveness of different lengths and amounts of mindfulness practice. An experimental manipulation, which allows stronger causal conclusions to be drawn, was therefore needed. This was examined by comparing two different mindfulness practice lengths to an active control group, with participants assigned to groups randomly. All participants in this study were novice practitioners from the general population. To further control the examination of the effectiveness of practice only, practice was isolated from other components typically present in MBPs, such as post-practice discussions with a teacher or peers. Additionally, participants were asked to refrain from practicing outside of face-to-face sessions. An experimental randomised controlled design was used assessing effectiveness for the same outcomes included in the dose-response meta-analysis and meta-regression, as well as underlying mechanisms; this is presented in Chapter 6.

Thirdly, as detailed above in the literature review in Section 1.4.6.1, another question relating to dose of mindfulness is the effectiveness of a single mindfulness practice, also often termed mindfulness induction, on state outcomes (see Chapter 7). While the effectiveness of such mindfulness inductions has previously been explored for psychological distress outcomes, this had not yet been examined for positive psychological states, and hence the outcomes state hope and gratitude as well as underlying mechanisms of trait and state mindfulness were examined. This question was explored in an online-delivered randomised controlled experiment, where a single mindfulness practice was compared to an active control group, in a general population sample.

More specific research questions and related hypotheses for each respective part of this thesis are presented in Chapters 2, Section 2.2.8, 6, Section 6.2.6, and 7, Section 7.2.5. More detail on prospective research opportunities following on from the research completed in this thesis is provided in Chapter 8. Although the specific methods and processes employed differ for each of the separate parts of this thesis, the methodological approach and research paradigms applied are the same throughout. These are outlined next.

1.6 Thesis Methodological Position

There are different philosophical positions that can be taken when researching mindfulness. In this thesis, the philosophical approach of post-positivism within the critical realist ontology is taken. Post-positivism follows on from positivism, which resides within the naïve realist ontology, where it is thought that the absolute truth existing within a single reality can be understood through a dualist/objectivist epistemology. Here, researchers are thought of as detached and independent of the object of study aiming for value-free hypothetico-deductive inquiry by quantifying reality through structured and systematic scientific experiments to understand cause-and-effect mechanisms (Guba, 1990; Lincoln & Guba, 1985).

In post-positivism, ontology shifts from naïve to critical realism, where understanding of reality is limited in that it can only ever be understood to an extent, but never fundamentally, due to extraneous and contextual variables, which cannot be controlled fully by the researcher (Guba, 1990). This paradigm is often applied to research within the social sciences (de Souza, 2014). The objectivist epistemology is therefore modified in post-positivism to exploring knowledge as much as possible, rather than absolutely, with both researcher and participant' biases thought to influence the research process (Guba, 1990), though efforts are made to minimise bias by for instance employing RCTs (see Section 1.6.1) and adopting reliable and valid measures (cf. Guba & Lincoln, 2005). Therefore, in post-positivism, the understanding exists that not everything in research can be controlled and that although objectivity is sought, an awareness of bias and limitations on what can be concluded from research is always present; findings are thus generalisable only to an extent (Miller, 2000). The background on the scientific study of psychology has been summarised more comprehensively elsewhere (e.g. Dienes, 2008).

While recognising that research has also employed other positioning when researching mindfulness from the background of other worldviews (e.g. Crowder, 2016), the position of the post-positivist paradigm within the critical realist ontology is adopted in this thesis using primarily quantitative research methods to answer research questions. Therefore, though the position taken in this thesis is that the absolute truth can only be understood to an extent, examining the effectiveness of MBPs is helpful in furthering knowledge and providing a basis for future research. Strengths and limitations of this positioning will be returned to in the thesis discussion (Chapter 8).

All parts of this thesis employ the same methodological stance of post-positivism with the ontological position of critical realism as outlined above and the reader is asked to keep this in mind in the chapters that follow. The specific methods employed in each part of the thesis are listed separately in Chapters 2, Section 2.3, 6, Section 6.3, and 7, Section 7.3. To enhance scientific rigor and examine the effectiveness of MBPs, RCTs were employed throughout this thesis.

1.6.1 Randomized Controlled Trials (RCTs)

RCTs are research studies where participants are randomly allocated to either an intervention/program or a control group to reduce potential bias as much as possible. RCTs are often referred to as the gold standard of research (e.g. Andrews, 1999) and have been considered as the most reliable and valid way of assessing effectiveness of interventions (Akobeng, 2005). However, there has been an ongoing debate over the years where some have claimed this to be an exaggeration and that RCTs are simply "good experimental designs" rather than gold standards (e.g. Grossman & Mackenzie, 2005, p. 516) and others have argued RCTs to be the most appropriate way to examine the effectiveness of a program (e.g. Hariton & Locascio, 2018). Nevertheless, in MBP research, RCTs with active controls are increasingly referred to as "the golden standard" (Dorjee, 2016, p. 2). For reasons of cohesiveness, RCTs were exclusively included in the dose-response meta-regression (Chapters 2-5) and randomised controlled designs were also employed in the empirical studies (Chapters 6 and 7). However, it is worth noting that within the critical realist paradigm, some limitations to knowledge resulting from findings of RCTs still exist, which are discussed in subsequent chapters along with strengths and limitations of characteristics of research methods employed in this thesis. Finally, the quality of RCTs included in the dose-response meta-regression was measured and controlled for using the Cochrane Risk of Bias Tool (see Chapter 2, Section 2.3.4) and RCTs applying these high-quality elements were employed for experimental studies (Chapters 6 and 7).

1.7 Chapter Summary and Thesis Orientation

This chapter has provided an overview and contextualisation of this thesis by introducing the foundations and definitions of mindfulness, covering relevant theoretical frameworks and empirical literature, and outlining the research aims and philosophical position. Despite the large volume of research and theoretical exploration already completed in relation to mindfulness, there is a gap regarding our understanding of dose effects in mindfulness and MBPs. Therefore, this thesis firstly presents an extensive dose-response meta-analysis and meta-regression, followed by two experiments examining different aspects of dose related to mindfulness.

This thesis is presented in eight chapters. The next four chapters focus on the dose-response review, specifically its rationale and method (Chapter 2), descriptive statistics and meta-analysis results and discussion (Chapter 3), meta-regression results and discussion for psychological distress outcomes (Chapter 4), and meta-regression results and discussion for the mindfulness outcome (Chapter 5). Next, two randomised controlled experiments examine the effects of different mindfulness practice lengths over several sessions (Chapter 6), and the effectiveness of a single-session mindfulness practice (Chapter 7). This thesis concludes with a discussion and synthesis of findings from all parts of this thesis and the thesis contribution to the field (Chapter 8).

CHAPTER 2

Dose-Response in Mindfulness-Based Programs: Rationale and Methods

2.1 Chapter 2 Overview

The previous Chapter (1) reviewed relevant literature outlining the theoretical framework and context of the effectiveness of mindfulness and mindfulness-based programs. This chapter describes the rationale and methods employed for the dose-response meta-regression analysis. Firstly, the rationale for completing a dose-response meta-regression analysis is outlined in line with previous research, theory, and current gaps in the literature, culminating in research questions and hypotheses to be addressed. Secondly, mindfulness-based programs are defined, followed by illustrating the stages of study selection based on the Prisma flow diagram (Moher et al., 2009). Next, the process of extracting relevant data and information and calculation of dose variables is demonstrated. Quality assessments with regards to study bias are described. This chapter concludes by outlining approaches and methods used for data analyses and ends with a chapter summary.

2.2 Rationale for Dose-Response Meta-Regression, Research Aims, and Hypotheses

2.2.1 Summary of Mindfulness-Based Programs

As introduced in Chapter 1, mindfulness-based programs (MBPs) are empirically based psychological programs adopting practices and techniques of mindfulness meditation. A range of different types of MBPs exist. In the Western world, Mindfulness-Based Stress Reduction (MBSR; Kabat-Zinn, 1982; 2013) and Mindfulness-Based Cognitive Therapy (MBCT; Segal et al., 2002; 2013) are currently the most commonly employed MBPs with a standardised length of eight weekly 2 to 2.5-hour group sessions which often include an all-day retreat and daily homework practices of 40-60 minutes. Furthermore, in addition to standardised MBSR and MBCT, in recent years, different

MBPs have been developed which not only differ in program intensity (Klatt et al., 2009) or amount of home practices, but also in mode of delivery, with an increasing number of MBPs being delivered by means of self-help materials (Hazlett-Stevens & Oren, 2017; Taylor et al., 2014) and online (e.g. Kvillemo et al., 2016). Self-help and online programs are generally delivered in lower "doses" than standard MBSR and MBCT not least since these programs typically do not involve face-to-face contact with facilitators or attendance to group sessions (Spijkerman et al., 2016). A more detailed definition of the different MBPs included in this research are given in Section 2.3 below.

2.2.2 Effectiveness of MBPs

As discussed in Chapter 1, a large evidence-base on the effectiveness of MBPs for a number of different outcomes exists. Due to the many potential benefits associated with different types of mindfulness programs, there has been an increase in MBPs both for clinical (Keng et al., 2011), as well as non-clinical (e.g. Dane & Brummel, 2014) populations which vary in quality.

Standardised MBSR and MBCT programs have been found to have beneficial effects, including reduced symptoms of stress, anxiety, and depression. For instance, in clinical populations, in a recent overview of meta-analyses and systematic reviews, Gotink et al. (2015) have identified significant improvements following participation in MBSR and MBCT programs for depression, anxiety, stress, physical functioning, and quality of life. Somewhat differently, a meta-analysis of participants with a diagnosis of anxiety or depression found a significant effect of MBCT, but not MBSR programs, for depressive, but not anxiety symptom severity when compared to inactive control groups (Strauss et al., 2014). This may be due to the fact that the review by Strauss et al. (2014) had exclusively included studies employing participants with a diagnosis of depression or anxiety, whereas Gotink et al. (2015) included meta-analyses and systematic reviews including studies with participants from both, clinical and non-clinical populations. Additionally, lower power was found in the meta-analysis by Strauss et al. (2014) compared to the review by Gotink et al. (2015), which had included 23 reviews with a total of 115 RCTs. Alternatively, for professional populations, in a review of MBSR-based programs, Lamothe et al. (2016) have found positive effects of MBPs in healthcare providers with regards to burnout, stress, anxiety, depression and empathy. Likewise, in a meta-analysis for healthy adults, MBSR programs have been found to have moderate to large positive effects on stress, anxiety, depression, distress, and quality of life as well as a small positive effect on burnout (Khoury et al., 2015).

However, MBCT and MBSR with their scheduled in-class as well as daily home practices can be time-consuming programs and although they have been found to be beneficial for some populations, the question arises whether they may not be suitable for all and can at times even be counterproductive for some (Dobkin et al., 2012). This has for instance been suggested by the relatively high levels of attrition in these MBPs. For instance, Kabat-Zinn and Chapman-Waldrop (1988) have observed a 24% dropout rate in MBSR programs and previous research mentioned participant attrition of up to 17% in MBCT programs (Kuyken et al., 2008; Ma & Teasdale, 2004; Teasdale et al., 2000), with participants withdrawing from the program before having received what is considered an adequate dose of mindfulness (Crane & Williams, 2010). Reasons for dropping out of mindfulness programs are often cited as difficulties in adhering to the expected time commitment recommended in MBSR and MBCT, not only during the sessions but also when completing daily home practices (e.g. Chang et al., 2004; Shapiro et al., 2005). In addition, recent qualitative studies have explored barriers associated with practicing mindfulness. For instance, in a recent qualitative study with healthcare staff who have engaged in a self-help MBP, the large time commitment of longer mindfulness practices as well as self-criticism associated with not being able to fully engage with the recommended practices were identified as key barriers of engaging with mindfulness (Banerjee et al., 2017). For example, a participant in Banerjee et al.'s (2017) research stated that "had there been less number of things (practices), I might have continued practice" (p.1658, lines 20-21). Additionally, challenging thoughts and feelings were often found difficult to be present with during mindfulness practice (Lomas et al., 2015). Therefore, participants often preferred shorter mindfulness practices (Boggs et al., 2014).

To address issues such as time constraints when participating in MBPs, previous research has examined potential benefits of lower dose MBPs. For instance, Klatt et al. (2009) have found that a shortened MBSR program with reduced session time and abbreviated daily home practices, adapted to suit healthy working adults, has reduced perceived stress and improved mindfulness with a relatively high rate of adherence in a randomised controlled trial. In the program evaluation, participants rated the reduced time commitment, both in class as well as for recommended home practices, as the most useful aspects of the program (Klatt et al., 2009). Additionally, previous reviews have found lower-dose, briefer MBPs to be beneficial for several outcomes, including improving mental health (e.g. Schumer et al., 2018; Spijkerman et al., 2016) and self-regulation (e.g. Leyland et al., 2019). Even engaging with brief mindfulness exercises as stand-alone programs without introductory or discussion elements as part of a therapeutic framework have been found helpful in reducing anxiety and depression compared to controls in a recent meta-analysis (Blanck et al. 2018). The evidence for the effects of brief MBPs is strongest for the general population. For instance, in a meta-analysis of studies with working adult participants, findings suggested briefer versions of MBPs to be equally as effective as their higher-dose counterparts for psychological distress (Virgili, 2015). Nonetheless, brief MBPs have also been found valuable for populations with low levels of mental wellbeing, for instance for individuals with acute depression (Costa & Barnhofer 2016). Additionally, in previous research of a brief MBP with university students, who were mostly novice mindfulness practitioners, it was discovered that participants preferred engaging in shorter mindfulness practices since no difference in actual time spent practicing was found for participants who were asked to practice mindfulness at home for different lengths of time (Berghoff et al., 2017). Differences in doses and delivery methods have therefore led to considerable variations in MBPs offered, which may be more or less appropriate for individuals with different needs. From a theoretical perspective, some predictions regarding what the optimal dose of mindfulness could be, have been made.

2.2.3 Theoretical Foundations of MBPs in relation to Dose

As was outlined in Chapter 1, various theoretical models of MBPs and mindfulness exist. In the mindfulness literature, a general consensus exists that more mindfulness instruction and mindfulness practice (i.e. a greater dose) are likely to be associated with better outcomes since these are generally viewed as significant components of MBPs (Beblo & Schulte 2017; Crane et al., 2014). Additionally, specifically for the prevention of relapse for people with a history of recurrent depression, a greater amount of mindfulness practice has been found to be associated with better treatment outcomes in MBCT (Kuyken et al., 2016).

The different theories of mindfulness and MBPs fit with the notion that more and regular mindfulness practice and greater MBP dose is helpful. For instance, continuously practicing mindfulness with the right intention and attitude is thought to enhance individuals' ability of attention processing and thus learning mindfulness resulting in positive outcomes through non-judgemental awareness (Malinowski, 2013; cf. Shapiro et al., 2006). Additionally, according to Mode of Mind theory, more practice of mindfulness should strengthen individuals' ability to disengage from the 'doing' mode of mind and to switch into a 'being' mode, which in turn should reduce rumination and in turn depression (Williams, 2008).

2.2.4 Theory of Rumination and Worry

Rumination is thought to be a key mechanism of depression (Nolen-Hoeksema & Morrow, 1993). It involves individuals being stuck in a loop of negative self-focused thoughts they believe to be true (Joormann et al., 2005). In a longitudinal study, exposure to stressful life events predicted increased levels of rumination (Michl et al., 2013), which in turn was associated with greater levels of depression, anxiety, and stress. Relatedly, worry has been defined as an anxious expectation of potential negative future events (Borkovec et al., 1983) and has been identified as one of the mechanisms in depression, anxiety, and stress (Keogh et al., 1998; Starcevic, 1995). To reduce psychological distress, rumination and worry therefore need to be targeted. And from a theoretical perspective of depression, rumination and worry were identified as one of the underlying mechanisms addressed in MBPs in a previous review on meditation studies in MBPs (Gu et al., 2015). Another previous review of empirical studies evidenced rumination and worry as mechanisms of depression, anxiety and stress through escalating repetitive, negative thoughts deriving in maladaptive emotional and information processing predicting negative mood states (Segerstrom et al., 2000).

To tackle rumination and worry and thus also depression, anxiety and stress, mindfulness practices contain exercises that help disengage from the doing mode and engage in the being mode, such as breathing or paying attention to specific parts of the body (body scan), while being aware that negative, worrying, and stressful thoughts do still occur (Segal et al., 2002). However, instead of ruminating on their content, through mindfulness, individuals practice to simply acknowledge thoughts hereby experiencing thoughts for what they are (thoughts) as opposed to assigning a value (Segal et al., 2002; Teasdale, 1999).

One prediction which theories of mindfulness and rumination often share in common is the implication that greater dose relating to MBPs may be associated with greater response for psychological outcomes (Chapter 1, Section 1.3). However, to examine the veracity of this prediction, empirical work on dose-response relationships in MBPs needs to be examined. Additionally, it is possible that such research might reveal optimal doses relating to MBPs for specific populations, for instance for individuals suffering from depression. To the best of the PhD researcher’s knowledge, given the comprehensive nature of the literature searches that have been completed and similarly, based on responses from reviewers of this research for publication in a peer-reviewed journal (Strohmaier, 2020), no previous comprehensive dose-response meta-regression analysis has been completed for MBPs and this therefore needs to be addressed (Creswell, 2017; Thomas, 2017).

2.2.6 Dose-Response in Other Psychological Programs

It is valuable to be informed by other psychological programs that have conducted dose-response relationships, to determine which dose variables may be most useful to examine. Most previous research into dose-response relationships has focused on pharmaceutical treatments and medical interventions to determine the specific quantity or combination of medications leading to a desired outcome (e.g. Dekker et al., 2005). However, in some psychological treatments, optimal dose of the treatment program has been examined. For instance, for different psychotherapies, significant dose-response relationships with regards to amount, frequency and intensity of the program and outcomes (Cuijpers et al., 2013) as well as patient attendance and adherence to homework for CBT for anxiety (Glenn et al., 2013) has been examined. In Cuijpers et al.’s (2013) meta-regression analysis of doses of psychotherapy for depression, although a significant effect of intensity (as measured in number of sessions a week) was found, no dose effect between total contact time or length of the psychotherapy program was found. Although a comprehensive exploration of dose-response, as has been completed for other psychological programs presented above, is missing for MBPs, a start has been made in exploring some aspects of dose in mindfulness programs.

2.2.6 Previous Research Evidence for Dose-Response in MBPs

At time of writing, there are mixed findings as to whether there is a dose-response relationship between aspects of mindfulness programs and outcomes (Lloyd et al., 2018) and different researchers have discovered different aspects of MBPs being associated with positive outcomes (e.g. Bondolfi et al., 2010; Grossmann et al., 2010; Klatt et al., 2009; Manigault et al., 2021).

On the one hand, in a previous randomised controlled trial examining the preventative effects of MBCT on depressive relapse, although relapse occurred later in the MBCT program group, this was not found to be correlated with practice amount (Bondolfi et al., 2010). Additionally, Jain et al. (2007) did not find a significant relationship between total number of hours of mindfulness practiced and changes to psychological distress and rumination in university students. Similarly, in a quasi-experimental study comparing the standard 8-week and an abbreviated 4-week version of the MBSR program, no difference to improvement of outcomes was found in a general population sample (Demarzo et al., 2017). And a review comparing traditional and adapted (shorter) MBSR programs has not found a significant relationship between length of sessions and mental health outcomes (Carmody & Baer, 2009). However, a limitation of Carmody and Baer's (2009) review was that only MBSR and adapted MBSR programs were included. Findings are therefore not necessarily generalizable to other MBPs such as MBCT or other adaptations, warranting further research incorporating different variations of mindfulness programs.

Furthermore, in a review of 98 studies of home practices in MBPs, only roughly 50% of studies found beneficial effects of home practice amount with half of included studies finding no effect (Vettese et al., 2009). On the contrary, a lower-dose MBP may not be beneficial for all participant groups and previous research has been inconsistent regarding the beneficial effects of regular daily practices in mindfulness programs (Nyklíček & Kuijpers, 2008). For instance, in a previous study examining the relationship of practice and outcomes, amount of time spent completing formal meditation exercises at home was significantly related to increased levels of mindfulness and decreased levels of distress (Carmody & Baer, 2008).

However, the above listed studies have significant limitations with regards to power as well as the research designs employed. For instance, some of the above listed studies had recruited only a small sample of participants (e.g. Bondolfi et al., 2010; Jain et al., 2007) therefore implying a potential Type II error, or did not include the more robust randomised controlled trial (RCT) design (e.g. Carmody & Baer, 2008; Demarzo et al., 2017; see Chapter 1, Section 1.6.1 on benefits of using RCTs in research). Additionally, Vettese et al. (2009) reviewed studies only narratively as opposed to conducting a more systematic meta-analysis. These findings therefore need to be interpreted with caution. More recent research by Parsons et al. (2017) has addressed such power issues by conducting a larger scale meta-analysis. This more robust meta-analysis included 43 MBSR and MBCT programs of different designs (RCT, non-randomised studies, and pre- and post-designs) with a total of 1,427 participants, finding a small but significant association between the amount of participants' self-reported formal mindfulness practice completed at home and positive program outcomes (Parsons et al., 2017). However, one limitation with Parsons et al.'s (2017) research was that only one aspect of dose relating to MBPs was examined, namely amount of home practice, but not other aspects of doses relating to mindfulness programs, such as amount of contact with the mindfulness teacher, length or intensity of the program, length of sessions, etc. Additionally, the meta-analysis by Parsons et al. (2017) only included MBCT and MBSR programs, but not further adaptations of MBPs such as online or self-help MBPs which do not strictly follow the MBSR or MBCT guidelines. Therefore, while Parsons et al.'s (2017) findings provide valuable information about dose-response relationships with respect to the amount of mindfulness practiced in the context of MBSR/MBCT, their findings are not generalizable across different MBPs and other doses relating to MBPs.

Outstanding questions about dose relating to MBPs across a broader range of programs and doses exist. In the dose-response meta-regression presented in this thesis, not only different types of MBPs are therefore included, but also other aspects of MBPs are examined such as program length and duration, amount of facilitator contact, duration of sessions and intensity of the program. To the best of the PhD researcher's knowledge, there has been no previous analysis that has examined "dose effects" in a systematic review or meta-analysis including the full range of types of MBPs and a range of different doses relating to MBPs. It is therefore helpful to examine these dose-response relationships to test theory and advance current knowledge. Additionally, secondary research techniques such as meta-regression have the potential to overcome at least some of the limitations of previous research outlined above, with reasons of the inconsistent pattern of findings including potential measurement difficulties, risk of bias and low power, by aggregating data from across studies and examining whether factors such as the quality of measurement of dose moderate dose-response relationships in MBPs.

Furthermore, research to determine whether a most effective dose and type of MBP exists is essential, especially with regards to the role of practice associated with different MBPs since practice is generally viewed as a significant component of mindfulness programs (Crane et al., 2014). Additionally, since many have found mindfulness practice challenging (e.g. as mentioned by Kabat-Zinn, 2003) and can often cite this as a reason for dropping out (Kabat-Zinn & Chapman-Waldrop, 1988), it is particularly important to establish if there is a dose-response relationship for different types and doses relating to MBPs and to understand the nature of this relationship, if there is one. For instance, if participants are likely to drop out of longer programs, it may be unnecessary for individuals to sign up to longer MBPs if the same benefits can be found with briefer/less frequent practice and dose of MBP. Since different types of MBPs exist as outlined above, the moderating effects of mindfulness program type is being assessed as part of the analysis.

Given the variation in MBPs, there are several different ways in which the dose of program offered and/or received differs between MBPs. More specifically, MBPs vary in terms of how much mindfulness practice they recommend, the extent to which participants engage with this practice, how much face-to-face contact with a mindfulness teacher they provide, how many sessions participants receive, their proximity and duration as well as the total length of the MBP. In this thesis, these will all be considered aspects of the dose of an MBP. There may be some discomfort for some in the community of mental health practitioners and/or mindfulness teachers to refer to psychological programs in terms of dose, perhaps due to this seeming an overly reductionist or medical way to examine mindfulness. However, the PhD researcher is of the impression that there is currently no widely accepted alternative terminology to describe the amount of program offered or received. Although dose does seem to be colloquially mostly a medical and pharmaceutical term, for instance to determine the specific quantity or combination of medications leading to a desired outcome (e.g. Dekker et al., 2005), perhaps it is time to extend the understanding of this towards mindfulness-based therapeutic and psychological programs, which has already been completed for other psychological programs (e.g. Cuijpers et al., 2013; see above).

Therefore, to be able to now better understand the relationship between different doses of MBPs and outcomes, a dose-response meta-regression analysis is required. A meta-regression analysis is defined as a meta-analysis with trial-level covariates, in this case the dose of MBPs, added as predictors to the model to determine whether dose of mindfulness practice and program predicts effect sizes of outcomes across a number of different studies (Thompson & Higgins, 2001). Further details on the method of meta-regression are outlined below, since one of the questions that need to be addressed first in a meta-regression including a diversity of programs is that of which outcomes to examine. This is outlined next.

2.2.7 Rationale for Outcomes

As introduced above and in Chapter 1, the strongest evidence-base for MBPs is in relation to depression. For instance, MBCT has primarily been developed and adapted from MBSR with the aim of it being employed as a treatment method for depression (Segal, et al., 2002). MBCT has arguably been found to be the MBP with the most rigorous research evidence for effectiveness for depression (MacKenzie & Kocovski, 2016). MBCT is underpinned by cognitive theories of depression (Segal et al., 2002), so that mindfulness theoretically can be seen to target some of the mechanisms thought to underpin depression, such as rumination and self-criticism (see Section 2.2.4 above for more detail). Research evidence suggests that this is how MBPs might work (e.g. Gu et al., 2015), which is why depression as the primary outcome follows. Additionally, a large number of trials of MBPs have included depression as one of their outcomes (MacKenzie & Kocovski, 2016) due to depression being a growing issue. This is for instance demonstrated in depression having been found to considerably impact on both individuals as well as society with extensive costs to healthcare services globally (Bock et al., 2014; Greenberg et al., 2015). Additionally, according to the World Health Organisation (WHO; 2020), close to 300 million individuals of all ages in the world currently experience a form of depression. In 2019, roughly 17.3 million American adults over 18 (7.1% of all American adults) suffered from at least one major depressive episode in the previous year (National Institute of Mental Health, 2019). In the UK, according to mental health statistics in 2020, 24% of women and 13% of men are diagnosed with depression in their lifetime and many more undiagnosed cases are believed to exist (Mental Health First Aid England, 2020). In this dose-response meta-regression analysis, depression was therefore chosen as the primary outcome, to be able to assess whether different doses relating to MBPs predict the degree to which MBPs have a beneficial effect on depression.

Although the strongest research evidence for MBPs has been found for depression, which is why it has been included as the primary outcome in this dose-response review, some evidence for MBPs associated with beneficial effects on anxiety and stress exists (e.g. Chiesa & Serretti, 2009; Hoffman, 2010); anxiety and stress have therefore been added as secondary outcomes. As well as depression, anxiety and stress have also been found to negatively impact on individuals' lives globally. Anxiety disorders were found to affect 19.1% of the US population every year (Anxiety and Depression Association of America, 2021) and in the UK, 8.2 million cases of anxiety were diagnosed in 2013 (McManus et al., 2016) with up to 10% of individuals in England expected to suffer from anxiety at some point in their life (Nopanic.org, 2021). According to the World Health Organisation, stress has been referred to as "The Health Epidemic of the 21st Century" (Fink, 2016). Finally, in their comprehensive meta-analysis of MBPs, Khoury et al. (2013) found mindfulness to be most effective for depression, anxiety, and stress.

In addition to mindfulness practice being a helpful skill for building up resilience to psychological distress and decrease rumination and worry as presented above, mindfulness as a trait has also been considered as being beneficial for individuals. For instance, previous research suggests that more mindful individuals show increased levels of interpersonal abilities, intrapersonal awareness, and effective emotion regulation (Davis & Hayes, 2011). Mindfulness was therefore added as an additional secondary outcome to assess whether the dose of mindfulness practice and programs predicted increased levels of mindfulness. Additionally, the primary intention of MBPs is to increase trait mindfulness (Kabat-Zinn, 1990), as this is the mechanism of MBPs that is thought to lead to/facilitate changes in other mechanisms (rumination and worry) and ultimately to improvements in mental health (Gu et al., 2015). Therefore, in terms of understanding dose-response in MBPs, it is expected to see a dose-response relationship especially between mindfulness practice and mindfulness outcomes as this is considered the proximal treatment target of MBPs. The research aims and hypotheses resulting from the above-presented rationale with the outcomes outlined above are detailed next.

2.2.8 Research Aims and Hypotheses

In summary, due to variations in length, frequency, and intensity of different MBPs and home practices outlined above and based on analyses of dose effects completed for other psychological programs and a proportion of some types of mindfulness programs, there is a need to examine whether a dose-response relationship of MBPs exists and what this looks like (Creswell, 2017; Ribeiro et al., 2018; Thomas, 2017). Additionally, since MBPs have been administered to different populations, both clinical and non-clinical, the moderating effects of population group is being assessed as part of the dose-response meta-regression. Therefore, based on previous literature and theory outlined in the previous chapter and the above rationale, this research presents a comprehensive dose-response meta-regression of randomized controlled trials of MBPs that aimed to examine whether MBPs show dose-response relationships for doses including program length, number and duration of face-to-face contact, amount of recommended mindfulness practice, recommended and actual use of the program, and intensity of the MBP, and responses including the primary outcome depression, and secondary outcomes anxiety, stress, and mindfulness. Depression was selected as the primary outcome due to the strong evidence-base of MBPs for depression (e.g. Lu, 2015, see Section 2.2.7 above). It was hypothesized that greater doses would be predictive of better outcomes. More specifically, the following research aims were explored:

1) Prior to examining dose-response relationships, a meta-analysis is conducted on the included studies to confirm that in this dataset, MBPs do indeed show beneficial effects on the included outcomes, as would be predicted by previous research as presented above.

2) To examine whether MBPs show dose-response relationships for doses including the number and duration of face-to-face contact, program length, frequency and duration of recommended home practice, amount of face-to-face contact with a facilitator, amount of recommended and actual use of the MBP, intensity of the MBP, and responses including the primary outcome depression and the secondary outcomes of anxiety, stress, and mindfulness.

3) If/when a dose-response relationship is found, it is planned to identify moderators of this relationship with regards to study characteristics including (1) the population receiving the program (depression, other mental health condition, long-term physical health condition, and general population), (2) the type of program (MBCT/MBSR or close variant [as these are the MBPs with the strongest research evidence for effectiveness] vs. other MBPs), (3) study quality (potential risk of bias) and (4) actual practice quality reporting (memory and social desirability bias).

The meta-analysis tested the following hypothesis:

1) Based on meta-analysis, participating in MBPs significantly relates to decreased depression, anxiety and stress and increased mindfulness compared to both inactive and active controls based on previous research.

The dose-response meta-regression analysis tested the following hypotheses:

2) A dose-response relationship of greater doses of mindfulness practice and programs predicting more beneficial effects on all included outcomes (depression, anxiety, stress, and mindfulness) is hypothesized to be found based on previous research and theory.

3) Effect sizes of outcomes of depression, anxiety, stress, and mindfulness differ when conducting sub-group analyses of moderators. The strength of the dose-response relationship will be moderated by participant population groups, demonstrated by significant moderator effects of population group found for significant dose-response relationships. It is hypothesized that greater doses of mindfulness predict larger effect sizes of depression, anxiety and stress in clinical populations compared to general

population since previous research has argued that mindfulness predicts greater changes in clinical levels of depression, anxiety and stress (e.g. Goldin & Gross, 2010; Kuyken et al., 2015; Speca et al., 2000).

4) It is hypothesised that significant dose-response relationships are moderated by MBP type with standardised MBSR and MBCT programs hypothesised to find a larger effect than other MBPs as has been suggested by previous research (e.g. Carmody & Baer, 2008; Parsons et al., 2017).

5) It is hypothesised that significant dose-response relationships are moderated by study quality (both in general and in terms of actual practice quality reporting) with lower quality studies hypothesised to find a larger effect and for effect sizes to be smaller in higher quality studies due to previous research suggesting effects may be inflated in lower quality studies (Savovic et al., 2012).

The following sections in this chapter outline the methods used to address these research aims and test these hypotheses. However, due to the risk of Type I and II errors, it is impossible to confirm or reject the above hypotheses completely, though measures to control for Type I and II errors as much as possible have been employed (for more detail, see methods below). This is addressed in the discussion sections of Chapters 4 and 5.

2.3 Methods of the Dose-Response Meta-Analysis and Meta-Regression

2.3.1 Definition of MBPs

To determine whether a program was an MBP, and therefore potentially eligible for inclusion in this meta-regression analysis, the definition developed by Crane et al. (2017) was adopted. According to this definition, MBPs are defined by essential characteristics (termed ‘warp’), which are based on the MBSR program, as well as variable elements, which can be adapted for different populations or contexts (termed ‘weft’) for instance adaptations such as program structure and length (Crane et al., 2017). Essential characteristics are that MBPs need to be based on theories and practices informed by a concurrence of contemplative traditions, sciences and disciplines and focusing on the human experience and relationship to experiences (Crane et al., 2017). Furthermore, the aim of MBPs needs to be to support the growth of individuals’ self-regulation and development of positive qualities including compassion and equanimity, which is achieved through intensive meditation practice and inquiry-focused exercises (Crane et al., 2017). In line with this definition, programs can include both a range of different mindfulness practices or consist of a single practice only, for instance the body scan. However, to allow for comparisons between programs, MBP studies needed to surpass one session to be included in this review. This is due to the fact that some MBP studies are experiment-based where participants have been asked to engage in mindfulness meditation for a short amount of time (often no longer than five to ten minutes) after which they are asked to perform a task often related to memory performance (e.g. Brown et al., 2016). Since the focus of these studies is on short-term results of mindfulness meditation which is measured through performance on a task (e.g. Geng et al., 2011; Kee et al., 2013) as opposed to the learning of mindfulness skills to develop positive qualities and self-regulation to be incorporated into everyday life which is included as the aim in Crane et al.'s (2017) definition, one-session lab-based experiments of mindfulness meditation were excluded. However, the effectiveness of a single-dose mindfulness practice is examined in Chapter 7.

Additionally, although some programs such as Dialectical Behaviour Therapy (DBT) and Compassion Focused Therapy (CFT) may include some practices which are informed by mindfulness, these programs are not included here since mindfulness meditation is not considered as the central practice and what the program is mainly based on (Crane et al., 2017). These programs therefore do not meet Crane et al.'s criteria and were excluded for this review.

Nevertheless, programs which do include mindfulness practice as their core element can differ in terms of structure, length and frequency of sessions and home practices and can be adapted to best suit the target population and context. MBPs can range from standardised programs such as MBSR (Kabat-Zinn, 1982, 2013) and MBCT (Segal et al., 2002) and including adaptations of MBSR or MBCT to meet participants' specific needs. For instance, previous research argued for the effectiveness for both shorter (Carmody & Baer, 2009) as well as longer (e.g. Abholgasemi et al., 2015) duration of programs to be accommodating for its participants.

2.3.1.1 Intensity of MBPs

Additionally, mindfulness programs can involve more or less intensive sessions and practice. For instance, a more intense MBP with twice-weekly sessions over five weeks had reduced levels of stress and anxiety in parents of children with special needs in a previous RCT (Benn et al., 2012). Less intense MBPs with shorter sessions, a shorter overall program and briefer recommended practices on the other hand have been found helpful in populations who find it difficult to concentrate over long periods of time, such as patients with traumatic brain injuries (Bédard et al., 2014). Not just session length, recommended home practice assignments in mindfulness programs can also vary. Most standard protocols recommend 40-60 minutes of formal home mindfulness practices for six days each week over the course of the program in addition to informal practices, which focus on including mindfulness in everyday activities such as eating, brushing teeth, walking, etc. (Segal et al., 2002). Other MBPs recommended 5-15 minutes of daily formal practices to fit in with busy schedules (e.g. Wolever et al., 2010).

2.3.1.2 Facilitation of MBPs

Furthermore, MBP studies not only differ in accommodating the sessions to fit its participants, but also often differ in the person who teaches the session itself. Mindfulness teachers can differ to a great degree in the amount of formal training they have received, whether they have a personal practice and the number of weeks, months, or years they have practiced mindfulness prior to facilitating the program. Differences in instructors also include how regular their own practice is as well as whether facilitators knew participants to some extent before the begin of the program. For instance, in a study of mindfulness to improve mental health in students, medical student leaders were elected from their year groups at university and had to first learn how to facilitate a mindfulness program which they then taught to their peers (Moir et al., 2016). According to Crane et al. (2017) the teacher facilitating an MBP is required to have specific competencies and qualities to be able to deliver mindfulness practice(s) effectively. These competencies include coverage of relevant mindfulness practices, appropriate planning and structuring of the session curriculum, suitable interpersonal skills to interact with individual participants as well as the group as a whole, being able to inquire as to individuals' experiences and guide participants through exercises (Crane et al., 2013). Additionally, a mindfulness instructor is required to have engaged in appropriate training and to commit to ongoing personal practice. Finally, it is necessary for a good mindfulness teacher to participate in the learning process of their students and actively engage in sessions (see Crane et al., 2017).

2.3.1.3 Modes of Delivery

However, increasingly, solely person-based, digital and other self-help delivery methods of mindfulness practices, such as bibliotherapy (e.g. Hazlett-Stevens & Oren, 2017), are of interest and are included in Crane et al.'s (2017) definition. For instance, this includes programs based on self-help books (e.g. Moritz et al., 2015) where participants receive reminders from researchers to complete exercises but other than that do not hear direct instructions from a facilitator. Additionally, text- and phone-based MBPs are increasingly employed for easier access and convenience to its participants (Dufau et al., 2011; Miller, 2012). Smartphone applications are increasingly used (Mani et al., 2015; Flett et al., 2018), and significant differences in terms of positive affect have been found (e.g. Howells, et al., 2016). Finally, online programs often with video content and audio recordings are increasingly popular (e.g. Cavanagh et al., 2013; Moore et al., 2020) and often preferred by participants due to its privacy and flexibility since there are no set times and locations to adhere to (Segal, 2011).

Programs included in this dose-response meta-regression analysis therefore varied in duration (number and length of sessions, number of weeks and length of recommended mindfulness practices to be completed outside of sessions), frequency (frequency of session and frequency of recommended home mindfulness practices) and mode of facilitation of sessions (face-to-face group settings, via self-help books, mobile phones or presented online). However, all programs selected had to meet the criteria for MBPs as proposed by Crane et al. (2017).

2.3.2 Review Registration

The dose-response meta-regression was pre-registered online on the Prospero International Prospective Register of Systematic Reviews (PROSPERO 2017: CRD42017056864; see full statement in Appendix 2.3.1), which is the recommended prospective register for systematic reviews by the Prisma Group (2020).

According to the Prisma guidelines (Moher et al., 2009), protocols for systematic reviews and meta-analyses should be registered at inception to avoid unplanned duplication of research. Registration and publication of planned review methods and pre-specified outcomes is a step towards ensuring quality control and avoid publication bias (Moher et al., 2009). What follows adheres to the pre-specified study protocol unless otherwise indicated.

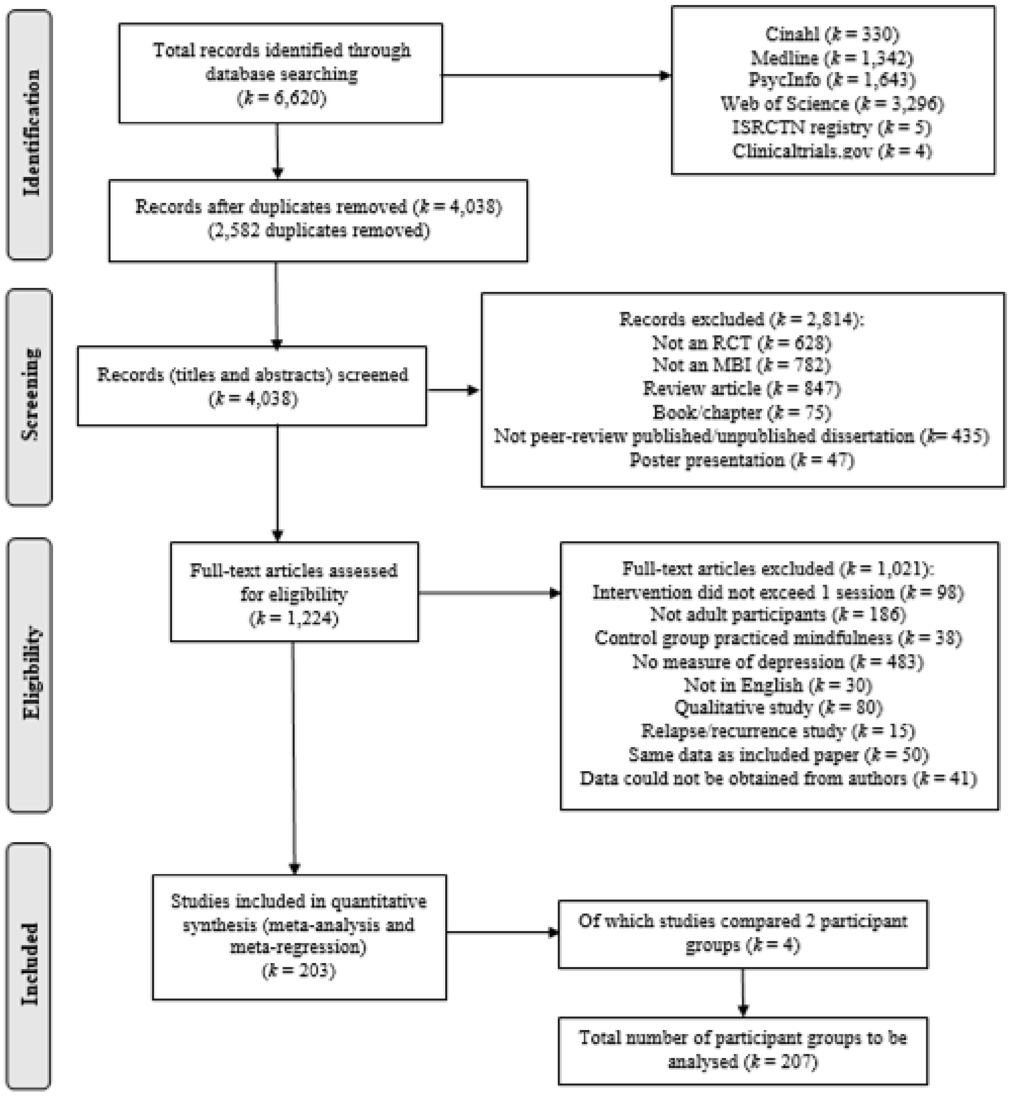
2.3.3 Study Selection

The Prisma statement was adopted for selection of studies to be included in the dose-response meta-regression (Moher et al., 2009)[3]. According to the Prisma checklist and flow diagram (Figure 2.1), the following steps need to be pursued: Identification of relevant studies by database searches and removal of any duplicates identified by different databases, initial screening of titles and abstracts based on inclusion and exclusion criteria, determining eligibility of included studies by assessing full-text articles and finally detecting the final number of included studies (Moher et al., 2009).

[3]Although the researcher is aware that the Prisma diagram was updated in 2020, at the time the searches were conducted, reviewed for publication, and then published, the guidelines had not yet been updated. However, statements do not differ on essential characteristics (cf. Page et al., 2021).

Figure 2.1

Prisma flow diagram outlining identification, screening, eligibility, and included stages of study selection (k=number of studies)



2.3.3.1 Stage 1: Identification of Relevant Studies – Search Strategy

Multiple sources were searched to identify studies. First, the electronic databases containing peer-reviewed journals PsycInfo (from 1860), Web of Science (from 1950), MEDLINE (from 1902) and CINAHL (from 1963) were searched from their inception until 19th June 2019. Additionally, clinical trials registration sites, including clinicaltrials.gov (USA) and ISRCTN Registry (international) were also searched on 20th June 2019 for any clinical trials that may not yet have been published but where data had been collected already. Finally, reference lists from already included papers as well as of current systematic reviews and meta-analyses were searched to identify any additional papers not obtained through databases or trial registration sites. However, no additional studies were identified in the reference lists of included studies or published reviews not already selected through electronic databases. The systematic searches including all following analyses were completed a total of three times (initial searches in 2017 at commencement of PhD studies, January 2018, and June 2019) with the updated searches in June 2019 being in response to requests from the editor and reviewers of the journal *Mindfulness,* to ensure that searches were as up to date as possible for publication. Findings presented in this thesis result from the final update.[4] A combination of four key search terms was used. The search terms [(mindful* OR MBCT OR MBSR) AND (random* OR RCT)] were searched for in the title, abstract and keywords (please note, * indicates truncation in search terms). From these searches, 6,620 studies were identified from all databases and registration sites. After exact and close duplicates were removed, 4,038 studies were screened. Studies identified by databases and clinical registration sites were imported to the reference managing tool RefWorks (www.refworks.proquest.com) to commence with screening based on inclusion and exclusion criteria.

2.3.3.2 Inclusion and Exclusion Criteria

For studies to be included in the dose-response meta-regression analysis, certain characteristics had to be met. Studies needed to (1) be randomised controlled trials (RCTs); (2) be in the English language; (3) be published in a peer-reviewed journal or registered on a clinical trials registration site; (4) contain an MBP for adults as defined in Section 2.3.1; (5) be a program which involves more than one session; (6) include a quantitative, continuous measure of depression symptom severity as an outcome (depression could either be the primary outcome or one of the secondary outcomes); and (7) contain any type of control condition (active or inactive) so long as the control condition did not include any mindfulness practice. The rationale for only including studies which employed a randomised controlled design is that RCTs are seen as the most valid way of

[4]Results did not materially differ between various updates since studies added only made up approximately 10% of included studies due to the large body of studies already included. Therefore, an update of the studies included would not be valuable until a larger number of studies has been conducted, which is likely not for several years. Each update involved repeating all meta-analyses and meta-regression analyses for all control groups, timepoints and doses including all additional analyses of controlling for baseline, moderator analyses, population-specific analyses, clinical significance and severity analyses and measure-by-measure analyses. The findings presented in this thesis are the ones that were peer-reviewed and published and provided a basis on which decisions of subsequent studies in the thesis were made. After consultation with the University, the PhD researcher was advised that it was appropriate to leave the date to the date in the publication of the paper.

assessing effectiveness of programs (Akobeng, 2005; Chapter 1, Section 1.6.1). Furthermore, peer-review is still considered important as a form of quality assessment of published work from peers in the field (Gaillet & Guglielmo, 2014), which is why only published or soon-to-be published studies or trials registered on an accredited trial registration site were included. The reason for including studies with active control groups as well as inactive control groups is that according to the U.S. Food and Drug Administration (FDA; 2018), higher quality studies should increasingly employ active control groups. Dose-response relationships of MBPs are looked at separately for RCTs compared to inactive and active controls since effect sizes tend to be smaller when compared to active controls due to participants actively engaging in a program as opposed to receiving no program, waiting for their program, or receiving treatment as usual (Wampold, 2001). This is discussed in more detail in Section 2.3.4 below. Additionally, many program studies which have included an inactive control group in the form of waitlist or treatment as usual (TAU) as opposed to an active control group have recognised this as a limitation of their study since therapy effects of the MBP could not be controlled for in an inactive control program (e.g. Asuero et al., 2014; Bogosian, et al., 2015). Where a study included an active control group (both, in addition to an inactive control group or as the only control group of the study), active control groups where the nature of the program was similar were planned to be grouped carefully and analysed separately, provided enough studies with similar control groups were available.

Papers were excluded if (1) they were a laboratory experiment examining the effects of mindfulness rather than testing a program. For instance, in an experiment by Waiter and Dubois (2016) the effects of a 10-minute single-session mindfulness exercise was assessed on the outcomes of attention and memory. This study was excluded from the dose-response meta-regression analysis since the mindfulness task did not exceed one session and tested the effect of a short mindfulness activity as opposed to building up a mindfulness practice over a longer period of time with repeated exercises and discovering how this relates to mental health outcomes. (2) Studies were excluded if the same data as in another included paper were used. However, if a separate paper was published on follow-up data of an already included study, follow-up data of this study was extracted and added as data points to the already included study without adding the newer paper to the list of included studies. (3) If all control programs in a study included a mindfulness component, this study was excluded, since it would be impossible to determine the effect of mindfulness if both groups practiced mindfulness. For instance, a study which compared a traditional MBSR program to a MBSR program combined with animal-assisted therapy (using a therapy dog) was excluded since both programs included components of mindfulness (Henry & Crowley, 2015). Similarly, programs which compared group to individual MBPs were excluded for the same reason (e.g. Schroevers et al., 2016). (4) Studies which focused on measuring relapse prevention and recurrence of depression reported as diagnostic status only (i.e. presence of depression: yes or no) were excluded since these studies removed individuals for having clinical symptoms of depression at baseline. This exclusion criterium was added since studies which only selected participants with low depression scores at baseline would artificially deflate effect sizes of the dose-response meta-regression analysis. However, relapse prevention studies that included a continuous measure of depression symptom severity at post-program were included in line with above listed inclusion criteria.

2.3.3.3 Stage 2 and 3: Screening and Assessment of Studies for Eligibility

In accordance with the Prisma diagram (Moher et al., 2009), after identification of studies and removing duplicates, the second stage involved screening titles and abstracts to remove studies which obviously did not fit the inclusion criteria. Excluded studies comprised books or book chapters, unpublished dissertations, poster presentations and review articles, and totalled 2,814 out of the 4,038 studies. For the third stage, full texts of the 1,1,224 remaining articles were screened for eligibility to determine whether the inclusion criteria were met. This process is further outlined in the Prisma diagram in Figure 2.1 where reasons and numbers for inclusion and exclusion of papers are listed. A further 1,021 studies were excluded through full-text eligibility checking at stage three. For piloting of the study selection process, titles and abstracts of all identified studies were screened by the PhD researcher with studies not meeting the criteria being excluded. As a reliability check, a random 100 eligible full-texts of papers were screened by the first and second supervisors of the PhD student at the time and a third colleague from the University of Sussex against inclusion and exclusion criteria. The level of agreement between all members was sufficiently high (>90%) and the remaining full-texts were screened by the PhD student with any areas of uncertainty resolved through discussion before continuing with extraction of relevant data and information about study characteristics.

2.3.3.4 Stage 4: Inclusion of Studies

The final stage of the Prisma diagram is inclusion of studies (Moher et al., 2009). At this stage, studies which met the inclusion criteria but contained the same data as an already included study were removed. For this review, 203 studies were included and subject to quantitative synthesis (in the reference list, references indicated with * refer to these included studies). Four of the 203 studies incorporated two different participant groups, namely Kubo et al. (2019), who included cancer patients and caregivers; Schellekens et al. (2009), who included patient and partner participants; Williams et al. (2008b), who included participant groups with unipolar and bipolar disorders; and Zautra et al. (2008), who included participants with and without depression. Rather than combining the data for these groups, participants with different conditions were included in the analyses separately, resulting in a total of 207 separate participant groups to be analysed. To test whether the results concur when only including one participant group per study, meta-analyses with the primary outcome depression at the post-program (primary) time point compared to both inactive and active controls were repeated with only using one sample per study choosing the larger of the two participant groups for each of the four studies concerned. Please note, it was not possible to include the total participant sample for each study since this data was neither available in published papers nor from contacting the authors of the four studies in question. Results of this analysis as well as strengths and limitations of this approach are detailed in Chapter 3.

2.3.4 Data and Information Extraction

Where available in the published articles, immediately post-program and follow-up means and standard deviations for each condition (MBP and control group(s)) as well as the number of participants in each group were extracted for the primary outcome depression symptom severity. Additionally, where they were available, means, standard deviations and participant numbers for each group (MBP and control group(s)) were also extracted for the secondary outcomes anxiety, stress, and mindfulness, at immediately post-program and follow-up time points. Data for three different follow-up time points were extracted, namely 1-4 months post-program, 5-10 months post-program and 12-15 months post-program.

It seemed plausible that dose-response effects could be influenced by control condition type (active versus inactive), given that trials with active controls are likely to show smaller effects than comparable trials with inactive controls (Karlsson & Bergmark, 2014). Where a study included an active control group (both, in addition to an inactive control group or as the only control group of the study) data were extracted for each type of control group and analysed separately. As recommended by Karlsson and Bergmark (2014), two separate analyses were conducted, one involving program effect size calculated relative to inactive control groups and another calculating program effect size relative to active control groups rather than collapsing control groups into one generic comparison group. Therefore, if a study included more than one control group, both control groups were selected, and inactive and active control groups were examined in separate analyses. In other words, if a study included an inactive and an active control group, each study was only added and counted once in each separate meta-regression. This approach of inactive and active control groups in separate analyses appears to be common in other meta-analyses (e.g. Hofmann et al., 2010; Piet & Hougaard, 2011; Sevilla-Llewellyn-Jones et al., 2018). The comparison between MBP and inactive control groups was the primary analysis given that the majority of studies were likely to have inactive control groups, which also reduces heterogeneity. According to the Cochrane Handbook for Systematic Reviews of Interventions (Higgins et al., 2011) inactive control groups either do not engage in an intervention/program, are waitlist controls, or receive care as usual, whereas active control groups can include a variation of the intervention/program group (not including mindfulness practice for this review) or contain a different intervention/program.

2.3.4.1 Extraction of Participant and Study Characteristics

The following information was also extracted from studies to situate their samples: (1) age, (2) gender, (3) country the study was set in, (4) the population the study was sampled from to gain a good understanding of included studies. Additionally, the type of MBP adopted (e.g. standard MBCT or MBSR; adapted MBCT/MBSR; different kinds of MBP for instance online or smartphone/app-based programs) and the program the control group participated in (if any) or whether it was an inactive control group were obtained from included studies.

2.3.4.2 Extraction and Calculation of Doses and Variability of Doses

In line with the Prospero statement, information about the program dose was extracted and incorporated in the analysis as primary dose variables, which included "total number of face-to-face sessions", "duration of a face-to-face session (in hours)", "program length (in weeks)", "frequency of recommended mindfulness practice (number of recommended practices per week)" and "duration of a recommended practice (in minutes)". Although it was originally planned to analyse different types of mindfulness practices, this proved difficult to extract reliably from published papers since this information was generally not clearly enough specified and was therefore not included as a dose variable.

In addition to the primary (pre-specified) dose variables, theoretically derived composite dose variables were calculated; these being "total amount of facilitator contact (in hours)"; "total recommended use of the program (in hours)", "total actual use of the program (in hours)" and "program intensity". The approach taken to calculate composite dose variables is detailed next.

The dose total amount of facilitator contact was calculated by summing the amount of in-session face-to-face contact with the program facilitator in hours. For online and other self-help programs, e.g. bibliotherapy, where there was no face-to-face contact with a facilitator, these studies were noted as having zero hours for this dose. As an additional and separate dose, studies with no face-to-face facilitator contact were excluded and the dose-response relationship between total amount of face-to-face facilitator contact and outcomes was assessed with remaining studies only to determine whether a dose-response relationship existed when excluding studies that did not have any face-to-face contact with facilitators.

Total recommended use of an MBP was calculated based on attending all sessions (where applicable) plus completing all recommended home practices and were summed in hours. This dose therefore included in-session practice as specified by mindfulness meditations as well as in-session work and amount of home practices that were recommended to be completed. Mindfulness practice was defined in this review as engaging with all aspects of an MBP, including formal mindfulness practices completed in-sessions (where applicable) and at home, as well as engagement with related learning activities (e.g. noticing present moment experiences of pleasant and unpleasant events). The amount of recommended use of the program was included in this meta-regression analysis since it may be of interest in its own right, separately from the actual engagement with the MBP, since this could predict (and has predicted) how much individuals actually engage with and practice mindfulness. As outlined in the rationale, according to findings of a meta-analysis completed by Vettese et al. (2009), participants did not necessarily engage in the amount of practice that was recommended and in previous research, participants have reported feeling exhausted after having participated in longer meditations which was related to increased likelihood of dropping out (Dobkin et al. 2012), which has also been reflected in previous qualitative research (e.g. Bannerjee et al., 2017). It is therefore possible that longer recommended practices could seem too overwhelming a challenge resulting in participants being more likely to cease practicing altogether than when asked to practice for a shorter amount of time. Therefore, recommended use of the program has been included as a dose in this paper in addition to actual use of the program.

Information on the actual use of the MBP dose was summed from both actual session attendance (where applicable) and actual completion of home practices in hours. Attrition rates with regards to session attendance and compliance to home practices were retrieved from studies to determine how much mindfulness materials and practices participants actually engaged in as opposed to how much was recommended. Information on compliance to home practices was generally collected via participants completing daily logs or diaries of their practices (e.g. Cherkin et al., 2016). For online and self-help MBPs, there was often no differentiation between reported session attendance and practices outside of the sessions since all practices were completed at home (e.g. O'Leary & Dockray, 2015). Similar to the recommended use of MBP dose, an inclusive position was taken on mindfulness practice since in-session work and home practice would frequently be a combination of formal mindfulness practice as well as discussion of practices and educational materials. From the data available, it was not possible to separate out these different elements, therefore the recommended and actual use of the MBP doses considered these together as doses of recommended and actual formal mindfulness exercises completed during sessions (where applicable) and at home as well as engagement with learning activities. Strengths and limitations of this approach for the calculation of dose variables are addressed in Chapter 3.

Although planned, recommended and actual amount of informal mindfulness practice, defined as “weaving mindfulness into existing routines through engaging in mindful moments and bring mindful awareness to everyday activities, such as mindful eating or mindfully washing dishes” (Birtwell et al., 2019, p.90, lines 24-27), could not be added as part of the recommended and actual use of MBP dose variables since informal practice was generally not reported in published articles as this was usually not recorded at all or not collected reliably. This has been added as a limitation in Chapter 3.

The dose intensity of program was calculated by the total number of sessions divided by the total number of weeks in the program (i.e. the number of sessions per week). Two different doses of intensity where calculated: intensity when excluding all-day retreats (where applicable) and intensity when including all-day retreats (where applicable) hereby regarding all-day retreats as a session. This distinction within the intensity dose was made since not all included studies incorporated all-day retreats as several different program designs were included, such as online and self-help MBPs. Furthermore, to ensure that intensity of programs was sufficiently measured, the above dose variables facilitator contact (both when including and excluding zero hours), recommended use of the program and actual use of the program were divided by weeks of the program and added as separate dose variables.

In addition to the above-specified dose variables, information about MBP teachers’ years of experience was planned to be extracted for face-to-face MBPs. However, when regarding the data available more closely, information on mindfulness teacher experience was not of a sufficient amount or quality and the type of information provided in published papers varied substantially. For instance, in some papers and from conversation with authors, years of teaching mindfulness was reported (e.g. van Dam et al., 2014; Wong et al., 2018), others reported years of personal mindfulness practice of the MBP teachers (e.g. Black et al., 2015; Nyklíček et al; 2014), some studies had employed a measure assessing how closely teachers followed the mindfulness program guidelines (e.g. Johns et al., 2015), some only referred to having had an "experienced teacher" without further explanation as to what this referred to (e.g. Arif et al., 2017; Koszycki et al., 2007) and others employed university students as instructors where the level of previous mindfulness practice and teaching experience was unclear and not collected (e.g. Moir et al., 2016). The inability to reliably calculate and include MBP teacher experience as a dose variable is discussed in Chapter 3.

2.3.4.2.1 Variability of Doses of Included Studies.

Due to most papers included likely being standardised MBSR or MBCT programs with the same doses, as these are generally the most common programs employed and researched (Hofman & Gómez, 2017; Khoury et al., 2013), the question arises whether papers varied enough to find significant and meaningful dose-response relationships. To address the subject of variability in included studies, the average number and percentage of MBCT and MBSR programs and of other programs was calculated and compared. Additionally, histograms for each dose were created to visually illustrate the range of studies and thus the doses included. These are presented in Chapter 3. Variability was also addressed by including type of program as a moderator in the model, this is outlined further in the data analysis Section 2.3.5.2.2 and results are presented and discussed in Chapters 4 and 5. Additionally, to determine whether type of program (MBSR/MBCT or close variants vs. other MBPs) was confounded with doses, independent samples t-tests were conducted to compare means between different MBP types. Where all or all but one or two included studies in a model had the same value for a dose, meta-regression analyses were not completed since a reasonable degree of variability is needed for meta-regression, as recommended (Thompson & Higgins, 2001). However, this was only the case rarely for doses involving intensity at follow-up and is discussed in Chapters 3 and 5.

Although most information needed for calculation of dose variables could be extracted from published papers, for some studies, the relevant information needed was not published in the journal articles and therefore, authors of papers were contacted.

2.3.4.3 Contacting Authors

Where insufficient outcome data, incomplete study and participant characteristics or not enough information to calculate dose were reported in the published article, authors were contacted via email to ask for this data and information. Corresponding authors of included papers were contacted via email up to three times. According to previous research into the impact of contacting authors of studies to acquire additional data not published in the article, all authors who were willing and able to send their data did so by the third request (Selph et al., 2014).

Corresponding authors of papers were contacted and asked to send their data and information and were given a deadline of two weeks. For authors who did not reply to the first email, a follow-up email was sent copying in the first email. Another deadline of two weeks was given. Finally, a third email similar to the second was sent to those who had not replied and given another two-week deadline (see Appendix 2.3.2 for email templates). Where authors did not respond to the third email, no further follow-up emails were sent as is the norm in meta-analyses and systematic reviews (Selph et al., 2014). Authors of all studies where data and information was missing were researched prior to being contacted to ensure that the contact information and author title published in their journal article were up to date especially in articles published several years ago. For instance, where authors moved to a different university than listed in the published paper or were promoted from Doctor to Professor, author information was amended accordingly.

Out of contacted authors, 80 of 179 responded to the first email, 26 of 94 responded the second time and 8 of 68 responded to the third request. However, many authors declared that they had not collected relevant information on dose-related characteristics, especially data on participants' compliance to home practices. Overall, 114 authors responded to emails at either the first, second or third enquiry with 65 authors not replying to emails.

2.3.4 Quality Assessment

2.3.4.1 Cochrane Risk of Bias (RoB) Tool

Studies were rated against the Cochrane Risk of Bias (RoB) tool (Higgins et al., 2011), an evidence-based tool for assessing bias in RCTs (Lundh & Gøtzsche, 2008). A previous review evaluating the tool based on comments of researchers who had worked with it found it was used in all Cochrane reviews and was the most frequently used tool in all non-Cochrane reviews of RCTs (Jørgensen et al., 2016) thus being the most commonly adopted quality assessment tool in reviews currently. Although the risk of bias tool has previously shown low reliability of scoring between independent reviewers (Armijo-Olivio et al., 2014; Hartling et al., 2013) variations in scoring have been found to be due to misinterpretation of the domains of the tool rather than mistakes in extracting relevant information from studies. Instructions to the tool have since been updated and reworded and additional domains have been added for ease of scoring (Higgins et al., 2016). Additionally, the tool has shown high internal validity for reviews of RCTs (Hartling et al., 2012).

The Cochrane RoB tool contains seven domains to examine risk of bias. For each of the domains, the reviewer searches for evidence for and against bias in the published article and gives a rating of low, unclear, or high risk of bias. Previous reviews, for instance Cuijpers et al. (2013), only used a selection of three or four domains of the tool to judge articles on. In the present review however, included studies were judged on all seven domains to gain a better understanding of different biases which could have affected results. Studies were rated using the Review Manager (RevMan) software version 5.2. (Cochrane Collaboration, 2012). The domains assessing bias are random sequence generation and allocation concealment (selection bias), blinding of participants and personnel (performance bias), blinding of outcome assessment (detection bias), incomplete outcome data (attrition bias), selective reporting (reporting bias), and other sources of bias (other bias).

2.3.4.1.1 Domains of the Cochrane Risk of Bias Tool.

2.3.4.1.1.1 Random Sequence Generation.

In this domain, studies are judged by the method used to generate the randomisation sequence to allocate participants to either the program or control group thus assessing selection bias. Studies judged as having a low risk of bias for this domain needed to have described a random component in the sequence generation process, such as using a computerised random number generator, a random number table, throwing dice, etc. (Higgins et al., 2011). A high risk of bias would be assigned if the allocation sequence was decided based on a non-randomised component, for instance allocation based on date of recruitment or allocation decided by a researcher. The importance of adopting an appropriate randomisation sequence is to minimise selection bias thus ensuring greater robustness of studies employing a truly random design to best assess the effectiveness of programs.

2.3.4.1.1.2 Allocation Concealment.

The domain allocation concealment also assesses selection bias due to inadequate concealment of which group participants are allocated to. An example of a low-risk judgement in this domain would be if participants received knowledge of their allocation through a sequentially numbered, opaque, and sealed envelope. A judgement of high risk of bias would be if an open allocation schedule were employed where participants or researchers could potentially foresee allocation to groups during enrolment (Higgins et al., 2011). The importance of allocation concealment is to ensure allocation to groups was truly random and concealed and thus cannot be predicted by participants or researchers.

2.3.4.1.1.3 Blinding of Participants and Personnel.

This domain assesses performance bias by judging the risk of participants or instructors/researchers being aware of the groups participants have been allocated to. If there is a high risk of bias, the possibility exists that participants perform differently on outcome measures based on their knowledge of group assignment. This can be avoided through blinding and employing active control groups. However, in psychotherapy programs particularly with waitlist, treatment as usual or no-program controls, blinding is often not possible to achieve. This is discussed in detail in Chapter 3.

2.3.4.1.1.4 Blinding of Outcome Assessment.

This domain examines risk of detection bias resulting from outcome assessors being aware of participants' group allocation. Low risk of detection bias can for instance be achieved by employing a separate researcher to complete assessments, who is not otherwise involved in recruitment, allocation, instructing or other aspects of the program. The importance of blinding of outcome assessments is to ensure that those rating outcome measures are not biased towards group allocation when analysing data (Higgins et al., 2011).

2.3.4.1.1.5 Incomplete Outcome Data.

Judgement of the incomplete outcome data domain examines potential attrition bias by assessing how missing data, occurring for instance from participant dropout, was handled. A judgement of low risk of attrition bias would for instance result from missing data being balanced across groups and where appropriate methods for imputing missing data and intention-to-treat analyses having been completed. A judgement of high risk of attrition bias would be assigned for studies employing per-protocol analysis but not intention-to-treat analysis and where reasons for missing data is likely to relate to outcomes (Higgins et al., 2011). The importance of employing appropriate methods, such as intention-to-treat analysis, to reduce attrition bias is to ensure unbiased comparisons among participants who were originally assigned to groups (Gupta, 2011).

2.3.4.1.1.6 Selective Reporting.

This domain assesses reporting bias due to selective reporting of outcomes in studies. To assess risk of selective reporting, trial registration on official registration sites of included studies was checked for those that had preregistered their studies and whether this was done before data collection commenced. Judgement on this domain is used to assess whether researchers reported all outcomes they had originally planned to report to ensure consistency in outcome reporting and to avoid authors omitting non-significant or unexpected outcomes. According to the Cochrane Handbook (Higgins et al., 2011), a low risk of reporting bias should be assigned for studies which had pre-specified outcomes by publishing their study protocol on preregistration sites and then reporting all pre-specified outcomes in the published paper. Where no study protocol was published or published after commencement of data collection, this was judged as having an unclear risk of reporting bias since it is ambiguous whether the reported outcomes had all been planned prior to data collection.

2.3.4.1.1.7 Other Bias.

Finally, the other bias domain assesses risk of bias not specified by another domain. This includes studies which report concerns for fraud or any potential conflicts of interest (Higgins et al., 2011). Potential conflict of interest could be problematic since it introduces a possible bias relating to the reason for conducting the study.

2.3.4.1.2 Inter-Rater-Reliability Analysis of Judgement on RoB Tool.

For a reliability check of rating papers against the Cochrane RoB tool, 20% of papers (k=41) were rated independently by the PhD researcher's first supervisor (FJ) and were subjected to inter-rater reliability analysis using Cohen's kappa. Papers for the inter-rater reliability analysis were chosen randomly using the Excel random number calculation function. Cohen's kappa was between κ=.84 and κ=1 for each of the seven domains of the tool with an overall kappa of κ=.92. According to McHugh (2012), Cohen's kappa between .81 and 1 represents almost perfect agreement. The remaining papers were rated by the PhD researcher with any uncertainties discussed and agreed. Whether study quality moderated dose-response relationships is addressed in Chapters 4 and 5.

2.3.4.2 Publication Bias

Although the Cochrane RoB tool is considered a good quality assessment tool (Lundh & Gøtzsche, 2008), according to researchers who have employed the tool in their studies, the reporting bias domain has not been considered as working well for the assessment of publication bias of selectively reporting only studies with significant effects (Jørgensen et al., 2016). Since research with no statistically significant results is much less likely to be submitted and published in academic journals, this can have a serious effect on the validity of meta-analyses as several studies are considered to be missing due to publication bias (Sutton et al., 2000). This effect was termed the "File Drawer Problem" by Rosenthal (1979), where studies with nonsignificant results are believed to be stored in file drawers as opposed to being published and shared with peers. Publication bias is considered an extensive problem in academia, which can actively alter findings particularly in meta-analyses, where the combined effects of a number of studies may be too optimistic (Dickersin, 2005) and thus needs to be assessed properly with the use of more than one assessment tool (Thornton & Lee, 2000). Therefore, publication bias is assessed through several methods in this meta-analysis to ensure that it is sufficiently addressed. In addition to examining whether published articles had preregistered their trial on an open registration site as part of the Cochrane RoB tool, funnel plots, which plot program effect estimates of each study against a measure of its size (Higgins et al., 2011), were also used to assess publication bias. Examination of funnel plots was chosen as a visual assessment of whether effect sizes of included studies were distributed symmetrically around the mean effect size as well as whether studies with less power are spread more widely around the mean due to larger random error (Fragkos et al., 2014).

Furthermore, the trim-and-fill method was employed to address asymmetry in the funnel plot as a result from publication bias (Duval & Tweedie, 2000a,b). The trim-and-fill method is a non-parametric method with a first step of trimming smaller studies which cause asymmetry in the funnel plot so that remaining studies are minimally impacted by publication bias, followed by the fill step, which imputes missing studies based on bias-corrected estimates in the funnel plot (Shi & Lin, 2019). In addition to providing an estimate of the number of missing studies, an adjusted effect size estimate is obtained by performing meta-analyses which include missing (filled) studies (Higgins et al., 2011). The trim-and-fill method is generally perceived as a helpful method for detecting and adjusting for publication bias and has been increasing exponentially in popularity since it was first introduced (Shi & Lin, 2019). Particularly, the trim-and-fill method has been praised for being more intuitive and efficient than other methods, since it not only identifies publication bias but also adjusts biased results (Murad et al., 2018). However, the trim-and-fill method has previously been criticised for not considering other possible reasons for funnel plot asymmetry other than publication bias (Mavridis & Salanti, 2014). Additionally, the trim-and-fill method has been found to perform less well in meta-analyses with greater heterogeneity of included studies (Peters et al. 2007). The results of adjusted meta-analysis therefore should be interpreted with caution. However, the main purpose of the trim-and-fill method is to assess the susceptibility of the meta-analysis for one mechanism of study selection, i.e. one form of publication bias, rather than generating a definitive estimate of the effect (Viechtbauer, 2010). Therefore, although trim-and-fill has been criticised by some, it was employed in this meta-analysis as a more careful consideration of publication bias in addition to the above outlined methods and because it is more sophisticated than other methods, such as Rosenthal's Failsafe N. This was also the view of a reviewer during the publication process.

The PhD researcher initially considered employing Rosenthal's Failsafe N to assess publication bias, a method whereby the number of additional studies with a null effect required to produce a non-significant overall result (p>.05) are calculated for meta-analyses with significant outcomes using the formula Failsafe N>5k+10 with 'k' being the number of included studies (Tang et al., 2007). However, there has been substantial criticism around the use of Rosenthal's Failsafe N as a method for assessing publication bias in meta-analyses. Although Rosenthal's method was the second most frequently used tool to assess publication bias, after the visual inspection of funnel plots, in meta-analyses (Ferguson & Brannick, 2012) and is still being used (e.g. Hopwood & Schutte, 2017; Wasson, 2020), this method has been criticised for being outdated since it focuses on the level of statistical significance of p-values rather than the substantive significance of effect sizes, which are more commonly used in meta-analyses today and which are examined in trim-and-fill analysis (Becker, 2005; Borenstein, 2009). The argument is made that Rosenthal's File Drawer analysis is limited as it focuses on the number of studies required to arrive at a non-statistically significant outcome rather than focusing on how many hidden studies are required to reduce the overall effect (Borenstein, 2005). Additionally, some critique Rosenthal's method for assuming missing studies have a zero effect thus not considering that missing studies could have a small, but not zero, effect, which could also be in the opposite direction (Hsu, 2002). Therefore, the trim-and-fill method was adopted for this meta-analysis in combination with looking at funnel plots and judgements on the reporting bias domain on the Cochrane RoB tool to explore and address possible publication bias.

However, it is worth noting that due its nature, it is impossible to determine with complete certainty the mechanism of publication bias (Higgins et al., 2011). Therefore, as mentioned above, results of trim-and-fill-adjusted meta-analyses need to be interpreted with caution. Furthermore, if publication bias across studies is suggested as a result of meta-regression analysis, the potential impact of this on dose-response relationships can be difficult to ascertain since it is impossible to determine how the file drawer effect is distributed across different levels of dose. Therefore, if publication bias is suggested, any dose-response relationship finding needs to be treated with even greater caution.

2.3.4.3 Actual Practice Quality Rating Tool

To assess the quality of how actual mindfulness practice was recorded in studies, a bespoke "actual practice quality rating tool" was developed in similar style to the Cochrane RoB tool. In this tool, studies were judged from low potential risk to high potential risk of memory and social desirability bias, both of which have been highlighted as being problematic when aiming to understand how much mindfulness practice participants actually engaged in (Lacaille et al., 2018). Memory bias was estimated based on how often practice was recorded by participants, for instance whether practice was reported in daily or weekly logs/diaries or was reported at the end of the program. Daily reporting was taken to suggest low risk, weekly practice recording to indicate medium risk and retrospective reporting at the end of the program to indicate high risk of memory bias. Secondly, social desirability bias was rated based on who collected the information on practice amount from participants. If practice amount was recorded anonymously, for instance either online, by mail or recorded automatically by computer software, this was judged at low risk of social desirability. Where a member of the research team who was not the main instructor or clinician, for instance a research assistant, collected the data, this was judged at medium risk. A rating of high risk of social desirability bias was given to studies where the clinician or researcher who facilitated the MBP themselves collected practice data directly since this could have influenced response bias (Booth-Kewley et al., 2007; Richman et al., 1999; van de Mortel, 2008). Only studies for which there was information to rate memory bias and social desirability of home practice recording were rated.

2.3.4.3.1 Inter-Rater-Reliability Analysis of Actual Practice Quality Rating Tool.

Again, a random 20% of these studies (k=11) were independently rated on the actual practice quality rating tool by the first supervisor and subject to inter-rater reliability analysis, with the result of an overall Cohen's kappa of κ=.91 (memory bias: κ=1; social desirability: κ=.82). Due to excellent inter-rater reliability, the remaining papers were rated by the PhD researcher.

2.3.5 Meta-Analysis and Meta-Regression Analysis

The R software for statistical computing was used for both, meta-analysis, and meta-regression analysis since it is one of the most widely used free professional statistical software packages available (R-bloggers, 2019). For meta-regression, R was the only freely available software for this analysis.

2.3.5.1 Meta-Analysis

For meta-analyses, to calculate effect sizes for each study for each outcome, between-group effect sizes on all outcome measures at post-program and follow-up(s) (where available) were calculated using the ‘`metafor`’, ‘`meta`’ and ‘`ggplot`’ packages (Viechtbauer, 2010; Schwarzer, 2007; Wickham, 2009) of the R statistical software versions 3.4.2. to 4.1.0 (The R Foundation for Statistical Computing, 2017-2021). Univariate meta-analyses were conducted for each of the outcomes depression (primary outcome), anxiety, stress and mindfulness (secondary outcomes) at the post-program and follow-up time points compared to inactive and active control groups. Standardised between group effect sizes (SMDs) were produced in a random effects model using the below formula with group one as program group and group two as control group. In the formula, spi represents the pooled standardised deviation of the two groups.

$$SMD = \frac{m_1 - m_2}{spi}$$

Effect sizes were computed using Cohen’s *d* statistic along with 95% confidence intervals (C.I.). The standardized mean difference estimator automatically corrects for its slight positive bias within the R functions used (Hedges & Olkin, 1985; Viechtbauer, 2010). Random effects models were used since sample populations in the MBP designs differed and effect sizes were expected to vary within groups (Borenstein et al., 2009) and variations of MBPs were included. Heterogeneity was examined using the I^2 statistic to assess the percentage of between-study heterogeneity and the *Q*-statistic examining whether between-study heterogeneity exceeds that expected by chance alone thus indicating whether moderator analyses are warranted. An I^2 statistic of less than 40% indicates low heterogeneity usually applied in fixed effect models, a percentage between 40%-85% indicates moderate heterogeneity, which is generally the case in random effects models, and a heterogeneity statistic of over 85% represents substantial heterogeneity and can often mean that studies differ too much for a valid meta-analysis (Higgins et al., 2003; Ioannidis, 2008). However, the I^2 value depends on the size and direction of effects as well as evidence for heterogeneity and thus needs to be interpreted in this context (Higgins et al., 2011). The I^2 value is calculated using the below formula with Q representing the chi-squared statistic and df the degrees of freedom (k-1) (Higgins et al., 2011).

$$I^2 = \left(\frac{Q - df}{Q}\right) \times 100\%$$

Forest and funnel plots were created for meta-analyses results. To examine possible outliers, influential case diagnostics using standardised residuals were assessed for each meta-analysis (i.e. for each outcome, timepoint, and comparison group) with the `influence()` function in the `metafor` package, which amongst others calculates Cook's distances and externally standardised residuals (see Viechtbauer, 2010). Meta-analyses with outliers were repeated with the outlier(s) removed. Additionally, the `leave1out()` function was applied for each meta-analysis, which repeatedly fits the model leaving out one study at a time in models where there are no moderators (Viechtbauer, 2010). Although some have questioned whether outliers need to be removed in meta-analytic investigations (Hunter & Schmidt, 2004), it was deemed important to explore the possible effects of potential outliers using the methods outlined above for reasons of thoroughness.

After fitting the random effects model, the `trimfill()` function of the `metafor` package was employed to complete the non-parametric, rank-based trim-and-fill method to correct for publication bias in meta-analyses (Viechtbauer, 2010) for each of the outcomes at the post-program and follow-up time points compared to inactive and active controls. As part of the `trimfill()` function, the Egger regression is adopted to automatically determine the correct direction of where missing studies need to be filled (Shi & Lin, 2019; Viechtbauer, 2010). The trim-and-fill method can only be used in models without moderators (Viechtbauer, 2010), i.e. not meta-regression analyses. Trim-and-fill funnel plots were created and are presented in Chapter 3.

2.3.5.2 Meta-Regression Analysis

Since the terminology in regression and meta-regression varies throughout the literature, the approach adopted here is that "predictor" refers to dose variables that are hypothesised to predict response, the term "covariate" is used to refer to variables included in some of the dose-response analyses to control for them, for example baseline levels, and "moderators" to refer to variables that are moderators of the dose-response relationship, such as study quality and program type.

To determine dose-response relationships in MBPs, univariate meta-regression analyses were conducted with the primary and composite dose variables, all of which were continuous variables which were added as predictors to the model. Meta-regression analyses can be used to assess and address between-study heterogeneity by examining study-level covariates (predictors) (Thompson & Higgins, 2002). It is worth noting that meta-regression analyses are still considered appropriate even if heterogeneity tests in meta-analyses are not significant, since this could be as a result of low power and so may not be dependably identifying the absence of heterogeneity (Hardy & Thompson, 1998). One of the issues previously associated with meta-regression is that individual, rather than study-level predictors were included as covariates (Thompson & Higgins, 2002). However, it is imperative to note that, different to primary regression analyses, in meta-regression, differences between studies not individuals are assessed, which is addressed here by examining the different doses of MBPs.

As a reminder, the doses included were total number of face-to-face sessions, duration of a face-to-face session (in hours), length in weeks of the program, frequency (number) of recommended mindfulness practices a week, duration of a recommend practice (in minutes), total amount of face-to-face facilitator contact in hours (both in- and excluding zero hours of contact), total recommended use of the program (in hours), total actual use of the program (in hours), program intensity in number of sessions a week (both when excluding and including any all-day retreats), amount of face-to-face facilitator contact in hours a week (both when including and excluding zero hours of contact), recommended use of the program a week (in hours) and actual use of the program a week (in hours). As pre-specified in the PROSPERO statement, separate analyses were run for all dose-outcome relationships, first with all studies with an inactive control group (primary comparison group) and then with all studies with active control groups. This separation was to avoid confounding the dose-response meta-regression analysis through differences between inactive and active control groups since engaging in a different program to mindfulness would compare differently than participants receiving no program (including waitlist) or TAU. As was also pre-specified, meta-regression analyses for each of the dose variables were performed at different follow-up time-points (1-4 months post-program, 5-10 months post-program, and 12-15 months post-program) where data were available, to determine whether dose-response relationships existed with respect to more enduring outcomes. So long as there were sufficient numbers of studies (see Section 2.3.5.3 below), separate dose-response meta-regression analyses were conducted for each combination of outcome (depression, anxiety, stress, and mindfulness), time-point (post-program, 1-4 months follow-up, 5-10 months follow-up, and 12-15 months follow-up), and control type (inactive and active). Dose-response meta-regression analyses were examined separately for each dose variable since different numbers of studies had information available for each dose as is recommended (Viechtbauer, 2010).

As was the case for the meta-analyses, meta-regression analyses were run in R using the ‘`metafor`’, ‘`meta`’ and ‘`ggplot`’ packages (Viechtbauer, 2010; Schwarzer, 2007; Wickham, 2009). A mixed effects model, which is a random effects model that includes study-level predictors to account for part of the heterogeneity and can also be termed a ‘random-effects meta-regression’, was employed to examine whether the dose variables predict effect sizes, using the below formula by Viechtbauer (2010) and the `rma(yi, vi, data = dat)` function.

$$\theta_i = \beta_0 + \beta_1 x_{i1} + \cdots + \beta_{p'} x_{ip'} + u_i \,,$$

with θ_i being the true effect, $\beta_{p'}$ representing the standardised regression coefficient, x_{ij} symbolizing the *j*-th moderator for the *i*-th study and u_i the sampling error ($u_i \sim \mathrm{N}(0, \tau^2)$) with τ^2 denoting the amount of residual heterogeneity not accounted for by predictors in the model (Viechtbauer, 2010).

A random-/mixed-effects, rather than a fixed-effects model, was chosen for the analysis for the same reasons given above for a random effects model for meta-analysis, i.e. that included studies were assumed to be from a random sample representing the entire population of MBP studies with a distribution of effects across studies rather than limiting the analysis only to studies included in the model (Hedges & Vevea, 1998; Viechtbauer, 2010). Effect sizes were computed using Cohen’s *d* statistic along with 95%C.I. Between-study heterogeneity was assessed the I^2-statistic and Q-statistic with I^2 assessing the percentage of between-study heterogeneity and Q examining whether between-study heterogeneity exceeds that expected by chance alone as detailed in the meta-analysis above. Variance of underlying true effect sizes was examined using the tau^2 (τ^2) statistic (Deeks et al., 2008). The restricted maximum likelihood estimator (REML) was chosen to estimate τ^2 and has been found to be efficient and relatively unbiased in random- and mixed-effects models (Viechtbauer, 2005).

For univariate random effects meta-regression with a single covariate, Knapp and Hartung (2003) devised a method to adjust standard errors of the estimated coefficients in meta-regression models to account for the uncertainty of tau^2 (τ^2), which can result in more conservative *p*-values, closer to nominal confidence intervals and reduces the likelihood of Type I errors. Confidence intervals and estimated effect sizes are then based on the *t*-distribution with *k*-*p* degrees of freedom, with *k* being the number of studies in the analysis and the *F*-distribution (*m* and *k*-*p* degrees of freedom) to test the overall model (Viechtbauer, 2010). In simulation studies to test Knapp and Hartung adjustments in various different estimators, the method was found to fit well with the restricted maximum likelihood estimator, which is employed here, as well as the methods of moments estimator, but less well for Bayesian estimators (IntHout et al., 2014; Knapp & Hartung, 2003). Knapp and Hartung adjustments are considered as a helpful method for ensuring more accurate inferences of size and significance of effects in meta-regression (Jackson & Riley, 2014). Furthermore, researchers have previously argued that the Knapp and Hartung method "considerably outperforms" the DerSimonian-Laird adjustment (IntHout et al., 2014, p.1). The Knapp and Hartung adjustment was therefore added to the random effects meta-regression models in the `metafor` package used here, by setting `knha = TRUE` (Viechtbauer, 2010).

Furthermore, visual representation of the meta-regression relationship is seen as essential in reporting meta-regression results (Thompson & Higgins, 2002). Meta-regression plots, which can aid understanding of the relationship between dose of mindfulness and outcome, and visually illustrate the sizes of studies, were therefore created using the `plot()` and `lines()` functions in the `metafor` package (Viechtbauer, 2010).

2.3.5.2.1 Controlling for Type I Errors.

To test the robustness of significant findings and correct for inflation of family-wise alpha due to multiple comparisons, the Holm-Bonferroni sequential rejective multiple test procedure was employed (Holm, 1979) with the multiple dose variables as the number of comparisons (n=15) at every time-point for every outcome separately. For this procedure, significant results are rank-ordered and subjected to the Holm-Bonferroni calculation with the aim of reducing the possibility of a false positive finding (Type I error). The Holm-Bonferroni (HB) formula is as follows, with n being the number of significant hypotheses to test and target α=0.05:

$$HB = \frac{Target\ \alpha}{(n - rank\ +\ 1)}$$

Holm's Sequential Bonferroni Procedure was applied since it is considered a more powerful modification of the Bonferroni correction by using multiple steps to determine robustness as opposed to Bonferroni’s single-step approach (Abdi, 2010). Bonferroni-type corrections are generally the most commonly applied corrections for Type I errors (Vickerstaff et al., 2019). However, Bonferroni-type corrections have previously been criticised for being too stringent particularly in the presence of numerous analyses (Diz et al., 2011), which is the case here due to the large number of dose variables assessed.

Therefore, in addition to the Holm-Bonferroni correction, False Discovery Rate (FDR) control was also employed using the Benjamini-Hochberg procedure (Benjamini & Hochberg, 1995; Glickman et al., 2014). FDR is a method to determine and control for the expected proportion of false positive findings (Type I errors) and adjusts p-values by augmenting them to limit the number of false positives (Benjamini & Hochberg, 1995). FDR corrections have been considered superior for correcting for Type I errors since it is less conservative than Bonferroni-type corrections and has greater power (Glickman et al., 2014; Shaffer, 1995), which is particularly useful in larger datasets such as in this review. Additionally, as opposed to Bonferroni-type corrections, False Discovery Rate controls are not affected by philosophical challenges such as rejecting the composite null-hypothesis rather than individual component hypotheses and confining Type I errors to specific tests (Glickman et al., 2014; Perneger, 1998). The Benjamini-Hochberg FDR control procedure in this review involved five steps: i) ordering *p*-values for all dose-response relationships ($k \leq 15$) from smallest to largest for each outcomes, time-points and control groups separately where one or more of the dose variables significantly predicted the outcome (please note, for some timepoints and outcomes, not enough studies included data on a particular dose and k=15 was therefore adjusted down); ii) rank *p*-values from 1 (smallest) up to 15 (largest); iii) the highest-ranked FDR-adjusted *p*-value is given the same value as the largest *p*-value; iv) the next largest FDR-adjusted *p*-value is the smaller of two options: the previously adjusted *p*-value or the non-adjusted *p*-value multiplied by the total number of *p*-values ($k \leq 15$) divided by the current *p*-value rank (see below formula with q_i=adjusted p-value estimate of FDR, p=uncorrected *p*-value; p_iN=total number of *p*-values; i=*p*-value rank);

$$q_i = p\,\frac{piN}{i}$$

v) repeat step iv) until all *p*-values have been adjusted. Results are considered robust and unlikely Type I errors for FDR-adjusted *p*-values<.05 since less than 5% are believed to be false positives (Benjamini & Hochberg, 1995). Both corrections were performed manually by the PhD researcher. Advantages and disadvantages of the approach for correcting for Type I errors in this review are considered in Chapter 4. Both uncorrected and corrected results with both procedures are reported in the results (Chapters 4 and 5), as is generally recommended (Clark-Carter, 1997).

2.3.5.2.2 Moderator/Subgroup Analyses.

It is worth noting that although any aspects of heterogeneity due to variation in mindfulness dose are addressed through meta-regression analysis, the possibility of additional moderators exists which could account for residual heterogeneity. For this reason, additional moderator and subgroup analyses were performed, as pre-specified. The '`metafor`' and '`ggplot`' packages in the R statistical software were used to perform these moderator and subgroup analyses for significant outcomes from the dose-response meta-regression.[5] This model included the dose, moderator, and moderator x dose interaction as predictors in the meta-regression. Significant interactions were

[5]Moderator analyses were not examined in meta-analyses since exploring moderators relating to the dose-response meta-regression was the aim in this thesis.

planned to be examined with subgroup analyses using the `subset=("")` function. Four moderators were planned to be examined.

2.3.5.2.2.1 Population Group.

First, the categorical moderator population group with the categories “depression”, “other mental health condition”, “long-term physical health condition” and “general population” was planned to be added to the model. The criteria for this review was that any study that adopted a measure of depression as one of their outcomes, both as a primary or a secondary outcome, were eligible to be included in this analysis. It is therefore expected that studies with a wide variety of different populations were included, encompassing clinical populations, such as depression (e.g. Barnhofer et al., 2015) and other mental health difficulties (e.g. Williams et al., 2008) as well as physical health difficulties (Thomas et al., 2017) just as much as studies with general population participants (e.g. Cavanagh et al., 2013). It would therefore be of interest to understand whether a stronger dose-response effect in MBPs is found for some populations but not or less so for others. However, analyses using population group as a moderator were not possible to conduct due to insufficient data available (k<10) and the large variety of populations included, especially for the subgroups “other mental health condition” and “long-term physical health condition” which could not be grouped reasonably and reliably. This limitation is addressed further in Chapters 4 and 5.

To address different populations being included in part, meta-regression analyses were repeated only with studies that had included a depression population (study samples were defined as being from a depression population if participants had been selected by means of a diagnostic interview or scored above a certain threshold on depression measures indicating a level of depression) since depression was the primary outcome as well as with a general population sub-sample (defined as individuals who were not known to have a diagnosis of a mental or physical health condition) compared to inactive controls (the primary control comparison) at post-program (the primary time-point) for the primary outcome depression.

Furthermore, it was planned to conduct clinical significance analyses following steps outlined in previous research (Jacobson et al., 2014; Khoury et al., 2013) on any robust statistically significant dose-response relationships found for psychological distress outcomes. However, there were no such robust significant dose-response relationships found (see Chapter 4 for details). As an exploratory approach, a clinical significance analysis for the group of studies with individuals with a diagnosis of depression and the most commonly employed measure of depression, namely the Beck's Depression Inventory (BDI; Beck et al., 1996), was completed. Studies were allocated to either a low-dose or high-dose group for each of the dose variables separately (low dose and high dose was determined as below or above each median dose value, respectively). Weighted means were then calculated for baseline, post-treatment, and follow-up for low-dose and high-dose groups for each dose variable.

Additionally, to determine whether participating in an MBP made a larger or smaller difference for individuals with varying severities of depression at baseline, for studies with participants with a diagnosis of depression, baseline depression scores from each study were divided into two groups of severe and mild depression calculated as above or below the median level. Dose-response meta-regression analyses for each dose were then completed.

2.3.5.2.2.2 Program Type.

Second, the bivariate moderator 'MBCT/MBSR or close variant' vs. 'other MBPs' was examined since previous research has suggested differences in effectiveness of traditional MBPs (MBCT/MBSR; e.g. Carmody & Baer, 2008; Gu et al., 2015; Parsons et al., 2017) and other MBPs (e.g. Ivtzan et al., 2016; Spijkerman et al., 2016).

2.3.5.2.2.3 Study Quality (RoB).

Third, a continuous moderator of study quality based as judged on the Cochrane RoB tool was added to the model to determine whether quality of studies significantly moderated the strength of the dose-response relationship for each outcome. Where significant moderator effects of risk of bias were found, the median risk of bias score in each analysis was calculated and studies were divided into two groups: high risk of bias and low risk of bias based on whether their score was above or below the median. The reason behind quality of studies having been added as a moderator to the meta-regression model is to determine whether the dose-response relationship differs in more compared to less robust studies. Previous research on risk of bias judgements in 228 meta-analyses found that lower quality (higher risk of bias) studies had on average exaggerated effect size estimates compared to higher quality studies (Savović et al., 2017). Additionally, higher heterogeneity between studies was associated with non-blinding (Savović et al., 2017). Significant effects of lower quality and thus higher risk studies therefore need to be interpreted with caution and the possibility of study quality moderating dose-response relationships exists. If there was an association between study quality and dose, then the former could confound the dose-response relationship. For example, as specified above, if lower quality studies have exaggerated effect sizes in the opposite direction, this could potentially conceal smaller effect sizes of studies that are of higher quality.

2.3.5.2.2.4 Actual Practice Quality Rating.

Fourth, a moderator of studies' actual practice quality rating scores on the Actual Practice Quality Rating Tool for significant results of the actual use of the program doses was also assessed as a continuous moderator. This was completed to determine whether how actual practice was recorded moderated the dose-response relationship particularly for significant dose effects of actual use of MBP doses since incorrect recording of practice quality has previously been identified as an issue and is likely to influence results (see Section 2.3.4.3).

2.3.5.2.3 Controlling for Baseline.

To determine whether baseline scores of outcomes confounded dose-response relationships, mean baseline scores of depression, anxiety, stress, and mindfulness were planned to be controlled for by adding these as covariates to each separate meta-regression model according to outcome where a significant dose-response relationship was found. In each model, the baseline score used was determined by the outcome included as the response in that model, i.e. baseline depression was included in the model where depression was the outcome, baseline anxiety was included in the model where anxiety was the outcome, etc.

Since several different measures for each outcome were used, baseline scores collapsed across a studies' program and control groups were calculated and standardised in line with normative data published for each different measure employed, using the formula

$$z = \frac{blM_n - normM}{normSD}$$

with z being the standardised baseline score, blM_n the baseline mean score for a given study collapsed across program and control groups, and *normM* and *normSD* being the normative means and standard deviations for the measure in question. For each outcome, standardised baseline scores were only calculated for measures where normative data drawn from the general population was available. For instance, standardised baseline depression for studies using the Edinburgh Postnatal Depression Scale (Cox et al., 1987), which was used in O'Leary et al.'s (2015) study, was not calculated since there was no normative data available for this measure as it was devised only for pregnant women.

For depression, normative data to calculate standardised baseline depression scores were taken from the following sources: BDI (Roelofs et al., 2013), CES-D (van Dam & Earleywine, 2011), DASS depression subscale (Crawford & Henry, 2003), HADS depression subscale (Crawford et al., 2001), PHQ-9 (Kocalevent et al., 2013), PHQ-8 (Kroenke et al., 2009a), PHQ-4 (Löwe et al., 2010), SCL depression subscale (Olsen et al., 2004), and BSI depression subscale (Franke et al., 2017).

For anxiety, normative data derived from the following publications: BAI (Creamer et al., 1995), GAD-7 (Löwe et al., 2008), DASS anxiety subscale (Crawford & Henry, 2003), HADS anxiety subscale (Crawford et al., 2001), STAI (Knight et al., 1983), SAS (Olatunji et al., 2006), SCL anxiety subscale (Olsen et al., 2004), and BSI anxiety subscale (Franke et al., 2017).

For stress, normative data sources were as follows: PSS-10 (Nordin & Nordin, 2013), PSS-14 (Remor, 2006), PSS-4 (Warttig et al., 2013), and DASS stress subscale (Crawford & Henry, 2003).

Finally, to calculate standardised baseline levels of mindfulness, normative data from the following articles were employed: FFMQ (de Bruin et al., 2012), MAAS (MacKillop & Andersen, 2007), and FMI (Trousselard et al., 2010). However, since the majority of studies used the FFMQ to measure mindfulness and for over a third of studies, no baseline level of the composite FFMQ scale was available, standardised baseline mindfulness could only be calculated for 60% of studies. Therefore, it was not possible to accurately control for baseline levels of mindfulness; this limitation is discussed in Chapter 5, Section 5.4.3.

After calculation, standardised baseline scores were added as a covariate to the respective meta-regression model(s) that showed a significant dose-response effect for the outcome in question. Controlling for standardised baseline levels for psychological outcomes has been considered as a useful way to estimate the true effect of a program. For instance, previous research detected rumination to predict levels of depression and anxiety only when baseline depression and anxiety were not accounted for but not when baseline scores were controlled for (Nolen-Hoeksema, 2000). Additionally, previous research discovered elevated levels of depression at post-treatment to be correlated with baseline depression (Raison et al., 2005). Baseline levels thus need to be controlled for to better understand the outcome of programs. Particularly for dose-response relationships, confounding could arise if there was an association between dose and baseline scores. Similar to standard regression, an outcome predicted by a trial characteristic (in this case a dose variable) may in fact be predicted by another trial characteristic, which may be known or unknown (Thompson & Higgins, 2002). Particularly in meta-regression, this can be problematic since these different characteristics can be highly correlated (Berlin & Antman, 1994).

2.3.5.3 Number of Studies

Similar to primary research and meta-analyses, researchers argue that it can be problematic to interpret results of a meta-regression accurately since meta-regression models tend to lack robustness if the number of included studies is too small (Borenstein et al., 2009). Although the included study number in meta-regression is considered somewhat arbitrary with no one definite rule to abide by (Borenstein, 2010), several different suggestions exist. For example, the Cochrane handbook (Higgins, 2011) recommends that at least ten studies are required for each study-level variable in the meta-regression model (this includes dose variables as well as baseline levels, moderator and subgroup variables) for the effect to be robust. However, other authors are more lenient in their recommendations and suggest six to ten studies can be sufficient in meta-regression with continuous covariates, and as little as four studies for categorical moderators, but only in cases where sample sizes of included studies are similar and moderate to large (Fu et al., 2010). However, since the sample sizes of included studies in this meta-regression varied considerably (from as little as 16 in both study groups to over 400), the researcher adopted and applied the Cochrane recommended threshold of a minimum of ten studies needing to be included for each study-level variable, i.e. for each of the dose covariates as well as for each of the moderator variable categories. This decision corresponds with recommendations on sample sizes necessary for meta-regression analysis suggesting that the likelihood of finding a clinically meaningful result increases substantially in meta-regression analyses which include a greater number of studies (Fu et al., 2010). For cohesiveness, the restriction of including at least ten studies was applied to both meta-analysis and meta-regression analysis. At 12-15 months follow-up, not enough studies (k<10) for any of the outcomes and dose variables were available and meta-analyses and meta-regression analyses were therefore not completed for this timepoint.

2.3.5.4 Additional Meta-Analyses and Meta-Regression Analyses by Outcome Measures

There is a precedent in the literature for including different outcome measures in meta-analyses (e.g. Blanck et al., 2018; Gu et al., 2015; Khoury et al., 2015; Spijkerman et al., 2016), including for different participant groups and programs. Therefore, meta-analysis and meta-regression analyses pooled across different measures of the same constructs[6], hereby prioritising extraction of the most valid and reliable measure. All included measures had shown good psychometric properties (for details on included measures see Section 3.2.4, Chapter 3). The reason for pooling across different measures was to ensure larger sample sizes and thus power, decreasing the possibility of Type II errors. However, one potential concern from some previous research was that different measures may not necessarily be measuring exactly the same constructs (Fried, 2017; Smarr & Keefer, 2011). Therefore, to determine whether the inclusion of several different measures had any influence on results, in addition to the main analysis, separate meta-analyses and dose-response meta-regression analyses were repeated on a measure-by-measure basis where a sufficient number of studies were available (k≥10).

[6]For the mindfulness outcome, although the majority of studies included trait measures, a small number (k=2) included a state mindfulness measure. When repeating separate analyses with each measure where sufficient data was available (i.e. trait mindfulness measures), findings did not materially differ.

2.4 Chapter 2 Summary

This chapter has delineated the rationale, methods and processes employed for the meta-analysis and dose-response meta-regression analysis. In summary, there is a need for conducting a meta-regression to examine dose-response relationships in MBPs since increasingly different doses of MBPs exist providing mixed results. A common definition of what constitutes an MBP was given (Crane et al., 2017). Study selection followed the Prisma guidelines (Moher et al., 2009) and calculation of dose variables has been outlined. To assess study quality, the Cochrane risk of bias tool (Higgins et al., 2011) has been employed and a tool rating the quality of actual practice recording has been developed by the researcher. The method for data analysis has been described primarily employing the `metafor` R package for meta-analysis and meta-regression in random- and mixed-effects models also detailing moderator and subgroup analyses, controlling for baseline-levels, and completing separate analyses by outcome measures. In the following three chapters, the descriptive statistics and meta-analysis results (Chapter 3) and results of the dose-response meta-regression for psychological distress outcomes (Chapter 4) and mindfulness (Chapter 5) are presented and discussed.

CHAPTER 3

Dose-Response in Mindfulness-Based Programs:

Descriptive Statistics and Meta-Analysis Results and Discussion

3.1 Chapter 3 Overview

This chapter follows on from Chapter 2, which described the rationale and method for the meta-analysis and dose-response meta-regression. This chapter summarizes descriptive statistics of included studies and dose variables as well as study quality. Additionally, results from meta-analyses and trim-and-fill meta-analyses, illustrated numerically and visually, are presented for each of the four outcomes depression, anxiety, stress, and mindfulness, at post-program and follow-up time points compared to inactive and active controls. As a reliability check, meta-analyses are repeated with single-samples and by different measures. Finally, findings are discussed in line with previous literature including limitations and implications. This chapter provided a context as to the general effectiveness of mindfulness-based programs (MBPs) into which the dose-response meta-regression analyses are placed, which are presented in Chapters 4 and 5.

3.2 Descriptive Statistics

As specified in the Prisma diagram in the previous chapter, 203 of the identified studies met the relevant criteria and were included in this review. Due to four studies incorporating two different participant groups which led to two participant groups included in separate analyses for these studies, a total of 207 separate participant groups were analysed, as detailed in Chapter 2.

3.2.1 Details of Study and Participant Characteristics

A summary of study and participant characteristics including country the study was set in, population group, sample size, mean age and gender of participants, program the mindfulness group took part in and program of the control group (unless it was an inactive control) are presented in Appendix Table 3.2.1.[7]

A total of 18,419 participants, were included in this review with the number of participants randomised per study ranging from 16 to 476. Overall, 9,277 participants were randomized to MBP groups and 9,142 to control groups. For analyses comparing MBPs to inactive control groups only, a total of 6,066 participants were randomized to MBPs and 5,888 participants to control groups. For analyses comparing MBPs to active control groups only, a total of 3,211 participants were randomized to MBPs and 3,254 participants to controls. However, sample sizes varied for different analyses since information on every dose was not available for every study. MBPs ranged from traditional MBCT and MBSR or close adaptations to other types of programs. Control groups were either inactive in accordance with the definition specified in the Cochrane Handbook (Higgins et al., 2011: no program, waitlist control or treatment as usual) or various active control groups. Although it was originally planned to group programs with similar active control groups together and analyse these separately, this was not possible since there were not enough studies (k<10) with similar enough control group activities that could reliably be grouped together. Therefore, the analyses remained with the two comparison groups, inactive controls and all active controls.

Studies were set in 25 different countries, including USA (k=58), UK (k=28), Canada (k=15), the Netherlands (k=14), Iran (k=14), China (k=13), Australia (k=12), Germany (k=9), Sweden (k=7), Spain (k=5), Denmark (k=5), Korea (k=3), Belgium (k=2), India (k=2), Ireland (k=2), Italy (k=2), Jordan (k=2), Thailand (k=2), Taiwan (k=2), New Zealand (k=1), Colombia (k=1), Greece (k=1), Israel (k=1), Brazil (k=1), and Norway (k=1). Participants were from different populations, including individuals with (1) depression (k=30), (2) mental health conditions other than depression (k=33), (3) long-term physical health conditions (k=77) and (4) from the general population (k=67). The average age of participants was 43 years (SD=13.44) ranging between 18 and 90 years of age. The majority of participants were female (69.25%).

[7] Although such a table is customarily added within the main text, due to its very large size and to allow for smoother reading, this table was added to Appendices.

3.2.2 Descriptive Statistics of Doses for Included Studies

As specified in Chapter 2, dose relating to MBPs was calculated for 15 dose variables, both primary and secondary dose variables. Primary dose variables were number of face-to-face sessions, duration of a face-to-face session (in hours), length of the program (in weeks), frequency (number) of recommended practices a week and duration of a recommended practice (in minutes). Composite dose variables were total amount of face-to-face facilitator contact (in hours), total amount of face-to-face facilitator contact (in hours) when excluding zero hours of contact, total recommended use of the program (in hours), total actual use of the program (in hours), program intensity (excluding retreats), program intensity (including retreats), amount of face-to-face facilitator contact a week (in hours), amount of face-to-face facilitator contact a week (in hours) when excluding zero hours of contact, recommended use of the program a week (in hours) and actual use of the program a week (in hours). However, not all doses could be calculated for each included study due to either insufficient information being available from published papers and from contacting authors or where authors did not measure this dose. This issue is detailed further in the discussion below. Appendix Tables 3.2.2 and 3.2.3[8] display the primary and composite doses extracted and calculated for each of the 203 studies, respectively.

Table 3.1 presents descriptive statistics for dose variables relating to MBPs taken across studies, including number of studies where this information was available, mean dose, standard deviation, and minimum and maximum dose. For the majority of dose variables, sufficient information was available to calculate doses. Several studies (k=20) did not report on recommended use of the MBP. For the actual use of MBP and actual use of MBP a week doses, this could only be calculated for k=56 studies. In correspondence with authors, the majority confirmed that information for these doses was not collected as part of their study. For amount of facilitator contact dose, k=30 studies had no face-to-face facilitator contact. These studies were delivered online or via other self-help formats, such as bibliotherapy (see Appendix Table 3.2.1 for more information of MBPs employed).

[8]As with Appendix Table 3.2.1, these large tables were added to Appendices to allow for smoother reading.

Table 3.1
Descriptive statistics of dose variables

Dose	*k*	Mean	SD	Min.	Max.
Total no. of face-to-face sessions	203	6.75	3.95	0	30
Duration of a face-to-face session (in hours)	173	2.06	0.89	0.25	8
Program length (in weeks)	203	7.52	2.35	0.36	19
Frequency recommended practice (recommended practices/week)	183	6.54	1.06	1	8
Duration of a recommended home practice (1 practice in minutes)	183	35.92	14.83	6	60
Total amount of face-to-face facilitator contact (in hours)*	203	14.92	10.01	0	60
Total recommended use of the program (in hours)	183	39.2	21.18	1.2	81
Total actual use of the program (in hours)	56	25.68	16.73	1.67	69.96
Program intensity (sessions a week) excl. retreats	173	1.089	0.43	0.25	4
Program intensity (sessions a week) incl. retreats	173	1.12	0.43	0.25	4
Amount of face-to-face facilitator contact a week (in hours)*	203	2.05	1.8	0	18
Recommended use of the program a week (in hours)	183	5.09	2.5	0.23	10.13
Actual use of the program a week (in hours)	56	3.34	2.04	0.7	8.75

k= Number of studies where data was available to calculate this dose; SD=Standard Deviation; Min.=Minimum; Max.=Maximum; *for the contact hours and contact hours/week doses, the analyses were repeated with studies that had zero hours of contact removed.

3.2.2.1 Variability of Included Studies and Doses

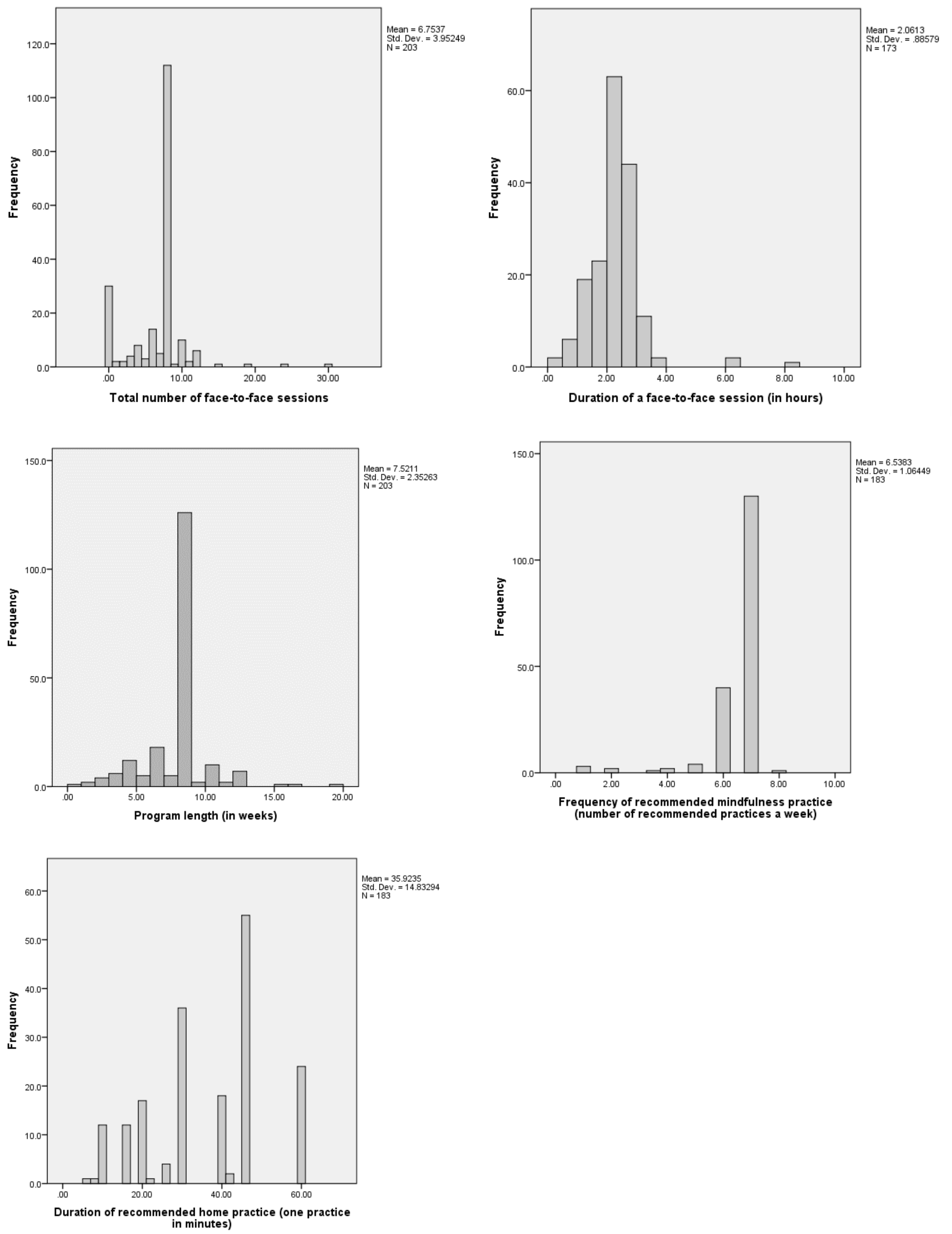
In this review, 57.89% of included studies were traditional MBSR and MBCT programs with 42.11% of studies being other types of MBPs[9]. As can be seen from Table 3.1, the majority of dose variables ranged noticeably with large differences between the minimum and maximum scores. Histograms in Figure 3.1 visually depict variability for each dose. Doses with low variability are program intensity (both when excluding and including retreats). Low variability relating to these doses is discussed in Chapter 5.

[9]When the literature searches were updated in June 2019 as part of the publication process, more online and self-help programs were included, which were generally lower-dose MBPs than traditional MBSR and MBCT.

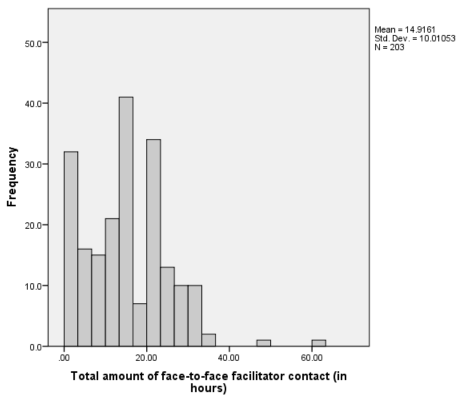
Figure 3.1

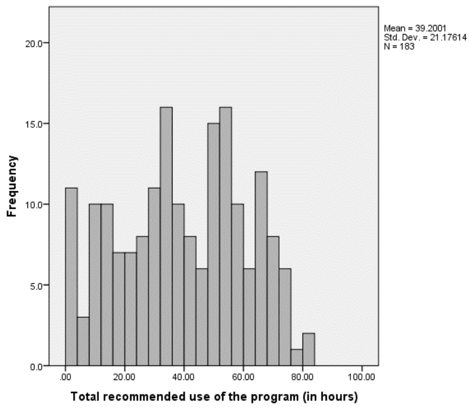
Dose variable histograms showing the frequency of the number of included studies per dose value

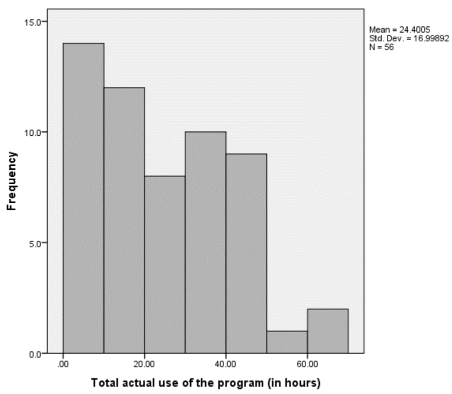
Primary dose variables

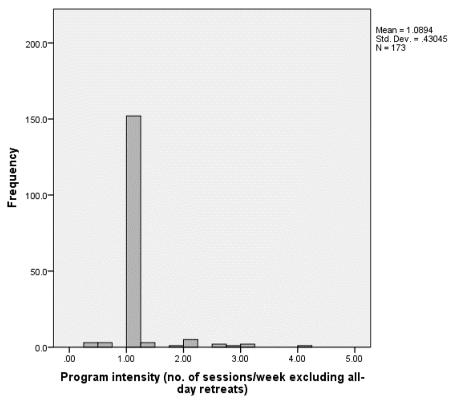


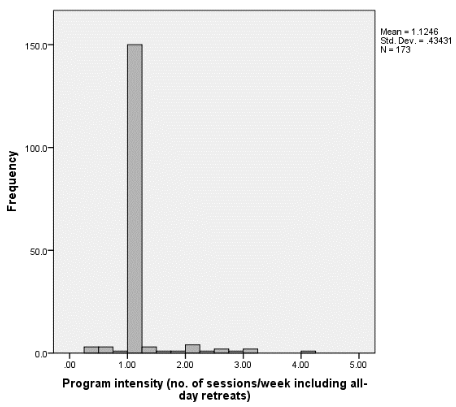
<u>Composite dose variables (facilitator contact doses are not represented separately with no contact studies removed since this can be identified in the facilitator contact dose histograms)</u>

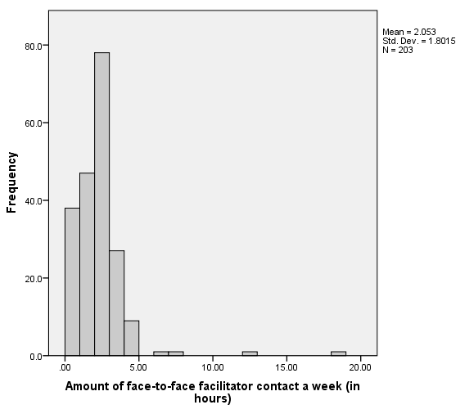


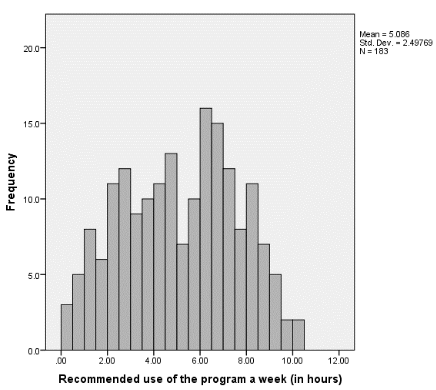


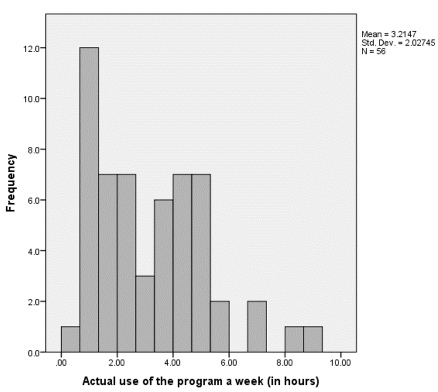












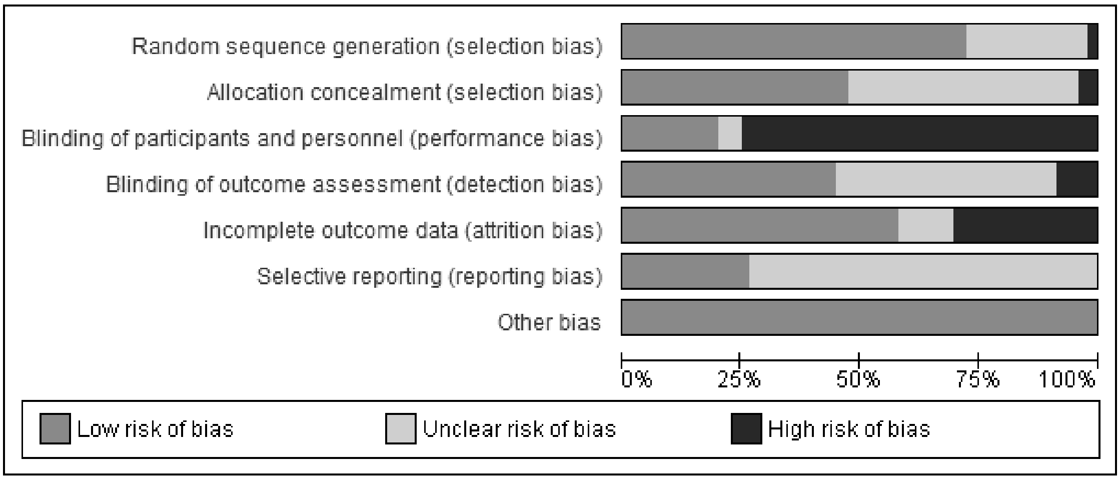
An independent samples t-test which was conducted to determine potential confounding effects between type of program (MBSR/MBCT or close variants vs. other MBPs) and doses (see Appendix Table 3.2.4 for results). Type of program was significantly related to the doses total number of face-to-face sessions, program length, frequency of recommended practice, facilitator contact (both total and a week) and intensity (both when excluding and including any all-day retreats). However, since standardised MBSR and MBCT programs generally involve longer and more intense programs, sessions and practices and usually include face-to-face sessions, it is to be expected that these doses are greater for these types of programs. Program type is added as a moderator for significant dose-response relationships to control this confounding element to an extent (see Chapters 4 and 5).

3.2.3 Quality Assessment

3.2.3.1 Cochrane Risk of Bias (RoB) Tool: Presentation and Interpretation of Results

Figure 3.2 shows the researcher's judgements of each risk of bias domain on the Cochrane RoB tool (Higgins et al., 2011) across all studies, indicating percentages of low, medium, and high risk of bias.

Figure 3.2
Risk of bias graph showing judgements for each domain presented in percentages of k=203



As per the criteria outlined in the previous chapter, all studies included in this review had to have stated that a randomised controlled design was employed. For 72.41% (*k*=147) of studies, an acceptable method has been specified as to how the random sequence used for assigning participants to program and control groups was generated with 26.11% (*k*=53) of studies insufficiently describing the random sequence generation to make a judgement and this was therefore rated as unclear risk of bias. A percentage of 1.48% (*k*=3) of studies described a process of random sequence generation which is considered to be a high risk of bias, such as group allocation based on date (e.g. Cludius et al., 2015) or location (e.g. Kristeller et al., 2014) of participation.

Next, 46.8% (*k*=95) of studies reported an adequate method of concealment of allocation to program and control groups prior to assignment to groups suggesting that there was a low risk of participants or researchers being able to foresee which group individuals were assigned to due to appropriate concealment methods being employed, such as using opaque, sealed envelopes containing allocation information. However, for nearly half of included studies (49.26%; *k*=100) this domain was judged as an unclear risk of bias since it was not detailed how allocation to groups was concealed. For 3.94% (*k*=8) studies, there was a high risk of participants or investigators potentially being able to discern allocation, for instance through allocation by the investigator (e.g. Abholgasemi et al., 2015) or employing an open randomisation schedule (e.g. Pinniger et al., 2012).

Similar to allocation concealment, for the blinding of participants and personnel domain of the RoB tool, the majority of studies (73.89%; *k*=152) were judged as showing a high risk of performance bias since both participants and those involved in delivering the program were aware of the groups participants were randomised to. A percentage of 19.7% (*k*=40) studies were judged as having a low risk of performance bias, all of which had employed active control groups, and for 18.45% (*k*=11) of studies with active controls, blinding was not addressed sufficiently to make a clear judgement. In studies with multiple control groups (i.e. both inactive and active), these were judged as high risk of performance bias since inactive control groups were preferred for the analysis as was specified in the previous chapter (Chapter 2). However, for moderator and subgroup analyses of dose variables and risk of bias scores compared to active controls addressed in later chapters (4 and 5), this was adjusted to scores of low risk for these studies.

When assessing blinding of outcome measures, 44.33% (*k*=90) of studies showed low risk of detection bias due to assessors being blind to participant allocation when analysing data. For 46.8% (*k*=95) of studies, blinding of assessors was not addressed adequately to make a judgement and 8.87%

(*k*=18) of studies were judged as having a high risk of detection bias due to lack of assessor blinding (e.g. Falsafi et al., 2016) which could have influenced results.

Furthermore, there was a low risk of attrition bias for 57.64% (*k*=117) of studies suggesting incomplete outcome data was addressed and handled appropriately by over half of included studies. The adoption of intention-to-treat analysis to address missing data is hereby generally judged as lowering the risk of bias (Cuijpers et al., 2013). However, for 11.82% (*k*=24) of studies, it was unclear how missing data was addressed and 30.54% (*k*=62) of studies were judged as having a high risk of attrition bias, for instance due to intention-to-treat analysis not being employed (e.g. Arif et al., 2017) or participant dropout not being balanced across studies (e.g. Brotto et al., 2012).

Next, for the selective reporting domain, 26.6% (*k*=54) of studies were judged as having a low risk of reporting bias. These studies had registered their trial prospectively on a recognised clinical trial registration site and had indicated depression was to be measured as part of one of the outcomes. According to the criteria for judging risk of bias in the Cochrane RoB tool, studies are to be judged as low risk if outcomes that are of interest to the review have been prespecified on the registered study protocol (Higgins, et al., 2011). For 73.4% (*k*=149) of studies, the risk of selective reporting bias was unclear since these studies had either registered their trial retrospectively (after commencement of data collection) or did not declare study registration rendering it impossible to judge the risk of reporting bias as low or high. No studies were rated as having a high risk of selective reporting.

Finally, all studies (100%; *k*=203) were judged as having low risk of other bias since none of the studies declared questionable conflicts of interest, fraudulent behaviour or had any other potential sources of bias relating to the study design used which were not covered by other domains.

Out of all included studies, 3.94% (*k*=8) were rated low risk of bias in all seven domains. These were Carletto et al. (2017), Duncan et al. (2017), Johns et al. (2016), Kvillemo et al. (2016), Ly et al. (2014), Raja-Khan et al. (2017), Strauss et al. (2018) and Wong et al. (2011). For more detail, Appendix 3.2.5 illustrates the risk of bias judgements made for each of the seven domains separately for each of the 203 studies.

3.2.3.2 Actual Practice Dose Quality Rating Tool

Table 3.2 shows the Actual Practice Dose Quality Rating Tool with scores for memory and social desirability bias as well as total score for each study where this data was available. Memory and social desirability bias were planned to be scored on the tool for 56 studies, the total number of studies where homework was given and practice was recorded. However, for two studies (Jansen et al., 2017 and Nathan et al., 2017) no information on *how* practice was recorded could be identified from either the published paper or from contacting the authors. Therefore, the remaining 54 studies were rated on the Actual Practice Dose Quality Rating Tool. To reiterate, for memory bias, daily practice reporting indicated low risk (score of 1), weekly practice recording indicated medium risk (score of 2) and retrospective reporting of mindfulness practice collected at the end of the program indicated high risk of memory bias (score of 3). For social desirability bias, if practice amount was collected anonymously, this was judged with low risk (score of 1), where practice amount was collected by a member of the research team who was not the instructor of the session, this was judged as medium risk (score of 2) and where the instructors themselves collected practice records, this was judged as high risk of social desirability bias (score of 3). Where information on memory and social desirability was unclear, this was judged as high risk of bias (score of 3). Please see Table 3.3 for the scoring key of the Actual Practice Dose Quality Rating Tool.

Table 3.2
Actual Practice Quality Rating

Study	Memory bias	Social desirability bias	Total
Armstrong & Rimes (2016)	1	3	**4**
Barry et al. (2019)	1	3	**4**
Beattie et al. (2017)	1	3	**4**
Bhayee et al (2016)	1	1	**2**
Boettcher et al (2014)	1	1	**2**
Bostock et al. (2018)	1	1	**2**
Britton et al (2012)	1	3	**4**
Churcher-Clarke et al. (2017)	1	3	**4**
Cludius et al (2015)	1	1	**2**
Cox et al. (2019)	1	1	**2**

Study	Memory bias	Social desirability bias	Total
Falsafi (2016)	1	3	**4**
Fissler et al (2016)	2	3	**5**
Foley et al (2010)	1	3	**4**
Fordham et al (2015)	3	3	**6**
Gallegos et al (2013)	3	3	**6**
Gambrel & Piercy (2015)	3	3	**6**
Glasner et al. (2017)	2	3	**5**
Goldberg et al (2013)	3	3	**6**
Gross et al (2010)	1	1	**2**
Gross et al (2011)	1	1	**2**
Hall et al. (2018)	1	1	**2**
Hazlett-Stevens & Oren (2017)	2	1	**3**
Hearn & Finlay (2018)	1	1	**2**
Hoffman et al (2012)	2	2	**4**
Hou et al (2014)	2	3	**5**
Howells et al (2016)	3	1	**4**
James & Rimes (2018)	2	3	**5**
Jansen et al. (2017)			
Jennings et al. (2017)	3	3	**6**
Johannsen et al (2016)	2	3	**5**
Johns et al (2016)	2	3	**5**
Key et al. (2017)	1	3	**4**
Koszycki et al (2016)	1	2	**3**
Kristeller et al (2014)	1	3	**4**
Kubo et al. (2019)*	1	1	**2**
Kvillemo et al (2016)	1	1	**2**
Ly et al (2014)	1	1	**2**
Ma et al. (2018)	2	2	**4**
Mongrain et al (2016)	1	1	**2**
Moss et al (2015)	1	3	**4**
Nathan et al. (2017)			
Oken et al. (2017)	3	1	**4**
O'Leary et al (2015)	1	1	**2**

Study	Memory bias	Social desirability bias	Total
Rayan & Ahmad (2017)	1	1	**2**
Rimes and Wingrove (2013)	1	3	**4**
Roeser et al (2013)	1	3	**4**
Shearer et al (2015)	2	3	**5**
Snippe et al (2015)	2	3	**5**
Vieten & Astin (2008)	2	3	**5**
Vollestad et al (2011)	1	3	**4**
Wahbeh et al (2016) older adults	1	1	**2**
Warnecke et al (2011)	1	2	**3**
Whitebird et al (2013)	1	3	**4**
Wells et al (2014)	1	3	**4**
Wong et al (2011)	3	3	**6**
Yang et al. (2019)	2	2	**4**

*This study compared two participant groups, which were both included in the analysis and had the same doses for both groups; fields are left blank where no homework was given

Table 3.3
Scoring key: Actual Practice Dose Quality Rating Tool

Bias	Risk level	Details
Memory bias	3	Retrospective reporting (at end of program) / not specified.
	2	Practice is recorded weekly in diary/log.
	1	Practice is recorded daily in diary/log or recorded electronically in online MBPs.
Social desirability	3	Clinician/researcher/instructor collected data; unclear who collected data.
	2	Member of research team collected data, e.g. research assistant (but not main instructor/researcher/clinician).
	1	Anonymous / by mail / online.

3=high risk; 2=medium risk; 1=low risk.

Out of the 54 studies where information on practice recording was available, 61.11% (*k*=33) of studies showed low risk, 24.07% (*k*=13) medium risk and 14.81% (*k*=8) high risk of memory bias. For social desirability, 35.19% (*k*=19) of studies showed low risk, 9.26% (*k*=5) medium risk and 55.56% (*k*=30) were scored as having a high risk of social desirability bias. For the composite Actual

Practice Dose Quality Rating Tool, 29.63% (*k*=16) of studies showed low risk of bias on both scales, 5.56% (*k*=3) showed low risk on one and medium risk on the other scale, 37.04% (*k*=20) showed either low risk on one and high risk on the other or medium risk of bias on both scales, 16.67% (*k*=9) showed at least medium risk of bias on both scales and 11.11% (*k*=6) showed high risk of bias on both scales.

3.2.4 Outcome Measures Employed in Included Studies

For each of the four outcomes (depression, anxiety, stress, and mindfulness), data from several different measures were extracted.

For the depression outcome, the Beck's Depression Inventory (BDI; Beck et al., 1996) was preferred, since the BDI has been developed in both non-clinical and clinical populations, is applicable for different theories of depression and has been translated for international use (Jackson-Koku, 2016; Wang & Gorenstein, 2013). Where the BDI was not used in an included study, other measures were the Centre for Epidemiological Studies Depression Scale (CES-D; Radloff, 1977), depression subscale of the Depression Anxiety and Stress Scale (DASS; Lovibond & Lovibond, 1995), depression subscale of the Hospital Anxiety and Depression Scale (HADS; Snaith & Zigmond, 1983), Hamilton Rating Scale for Depression (HAM-D; Williams, 1988), nine, eight, and four-item versions of the Patient Health Questionnaire (PHQ-9: Kroenke et al., 2001; PHQ-8: Kroenke et al., 2009a; PHQ-4 depression: Kroenke et al., 2009b), depression subscale of the Profile Of Mood States (POMS; McNair et al., 1971), Zung Self-rating Depression Scale (ZSDS; Zung, 1965), depression subscale of the Brief Symptom Inventory (BSI; Derogatis & Melisaratos, 1983), depression subscale of the Symptom-Check-List (SCL-90/SCL-92: Derogatis, 1973; 1992), Edinburgh Postnatal Depression Scale (EPDS; Cox et al., 1987), Inventory of Depressive Symptoms, Self-Rating (IDS-SR; Rush et al., 1996), and depression subscale of the Personality Assessment Inventory (PAI; Morey, 1991).

For anxiety, the preferred measure was the Beck's Anxiety Inventory (BAI; Beck, 1993). It was found the most extensively used instrument for detecting general anxiety in adults and adolescents (Julian, 2011). If the BAI was not used, other measures included the anxiety subscale of the Depression Anxiety and Stress Scale (DASS; Lovibond & Lovibond, 1995), State Trait Anxiety Inventory (STAI; Kendall et al., 1976), Generalised Anxiety Disorder Scale (GAD-7: Spitzer et al., 2006; GAD-2: Skapinakis, 2007), Zung Self-rating Anxiety Scale (ZSAS; Zung, 1971), anxiety subscale of the Hospital Anxiety and Depression Scale (HADS; Snaith & Zigmond, 1983), Hamilton Rating Scale for Anxiety (HAM-A; Williams, 1988), anxiety subscale of the Patient Health Questionnaire (PHQ-4 anxiety: Kroenke et al., 2009), anxiety subscale of the Brief Symptom Inventory (BSI; Derogatis & Melisaratos, 1983), anxiety subscale of the Symptom-Check-List (SCL-90/SCL-92: Derogatis, 1999), anxiety subscale of the Profile Of Mood States (POMS; McNair et al., 1971), Rating Anxiety In Dementia Scale (RAID; Shankar et al., 1999), Liebowitz Social Anxiety Scale-Self-Report (LSAS-SR; Fresco et al., 2001), and anxiety subscale of the Personality Assessment Inventory (PAI; Morey, 1991).

For stress, the preferred measure was the Perceived Stress Scale (PSS; Cohen et al., 1983) since the PSS measures the extent to which specific life events are perceived as stressful in the general population. If the PSS was not used, other measures included the stress subscale of the Depression Anxiety and Stress Scale (DASS; Lovibond & Lovibond, 1995), self-reported job stress from an inventory of teacher stress (Lambert et al., 2001), and Symptoms Of Stress Inventory (SOSI; Leckie & Thompson, 1979).

For mindfulness, the total scale sore of the Five Facet Mindfulness Questionnaire (FFMQ: Baer et al., 2006; FFMQ-sf (short-form): Bohlmeijer et al., 2011) was preferred since this measure was developed from a factor analysis of items derived from existing measures of mindfulness. If the FFMQ was not employed, other measures included the Mindfulness Attention and Awareness Scale (MAAS; Brown & Ryan, 2003), Freiburg Mindfulness Inventory (FMI; Walach et al., 2006), Philadelphia Mindfulness Scale (PHLMS; Cardaciotto et al., 2008), Toronto Mindfulness Scale (TMS; Lau et al., 2006), Cognitive and Affective Mindfulness Scale–Revised (CAMS-R; Feldman et al., 2007), Southampton Mindfulness Questionnaire (SMQ; Chadwick et al., 2008), and the short version of the Kentucky Inventory of Mindfulness Skills (KIMS; Höfling et al., 2011).

As outlined in Chapter 2, separate meta-analyses and meta-regression analyses were conducted with each separate measure of depression, anxiety, stress, and mindfulness, respectively, where enough data was available. Results of these meta-analyses are presented in Section 3.3.2 and measure-by-measure dose-response meta-regression analysis are presented in Chapters 4 and 5.

3.3 Meta-Analysis Results

Using a random effects model with a restricted maximum likelihood estimator (REML) for continuous variables, meta-analyses were completed without containing the dose variables at this stage to test Hypothesis 1 (see Chapter 2, Section 2.2.8). Results of meta-analyses for the outcomes depression, anxiety, stress, and mindfulness compared to inactive and active control groups at the timepoints immediately post-program, 1-4 months follow-up and 5-10 months follow-up, where enough studies ($k \geq 10$) were available, are presented in Table 3.4. As outlined in Chapter 2, at 12-15 months follow-up, $k<10$ studies were available for all outcomes and meta-analyses were therefore not completed.

Compared to inactive controls, participating in an MBP significantly related to decreased depression, anxiety and stress and increased mindfulness with moderate to large effect sizes at post-program and 1-4 months follow-up and small to moderate effect sizes at 5-10 months follow-up, where enough studies were available, thus confirming Hypothesis 1 for this comparison group. Compared to active controls, participating in an MBP was significantly associated with small to moderate effect size changes in the expected directions for all outcomes at post-program, and for depression, anxiety, and mindfulness at 1-4 months follow-up with small effect sizes in expected directions. Participating in an MBP was not significantly related to changes in stress at 1-4 months follow-up compared to active controls, however this is likely a Type II error due to the very small sample size (k=11). At 5-10 months follow-up compared to active controls, a sufficient number of studies ($k \geq 10$) was only available for the meta-analysis with the depression outcome and no significant results were found compared to active controls at this timepoint. However, again, it is likely that the lack of a statistically significant finding is due to low power in this analysis (k=13).

Hypothesis 1 could therefore only partially be confirmed for meta-analyses with the active control comparison group.

Heterogeneity analysis of effect sizes showed a significant Q-statistic for all significant outcomes and a moderate to high I^2-statistic (I^2>40%) for the majority of outcomes (all outcomes compared to inactive controls, majority of outcomes compared to active controls) indicating a high level of heterogeneity with effect sizes varying considerably (Cochran, 1954) and suggesting a percentage of at least 40% of variance in effect sizes was due to differences in true effects rather than differences obtained through sampling error (Higgins et al., 2003). Compared to active controls at follow-up, a high I^2-statistic (I^2>40%) was only found for the depression outcome at 1-4 months follow-up. For anxiety and mindfulness outcomes at 1-4 months follow-up compared to active controls, an I^2-statistic below 40% was found thus indicating low heterogeneity. See Table 3.4 for more detail. Between-study heterogeneity exceeds that expected by chance alone in the majority of meta-analyses therefore warranting moderator analyses which are addressed by dose-response meta-regression in Chapters 4 and 5.

Asymmetric funnel plots suggested evidence of publication bias particularly for depression, anxiety and stress outcomes with trim-and-fill analysis resulting in slightly modified effect size estimates and heterogeneity statistics. However, statistical significance and direction of effect size estimates as well as presence of heterogeneity remained unaltered (see Table 3.4). Nevertheless, results with evidence of publication bias, both for meta-analyses and meta-regression analyses presented in Chapter 4 and 5, need to be interpreted with caution. For the stress outcome at 1-4 months follow-up compared to inactive controls, no evidence of publication bias could be observed. For the mindfulness outcome, no evidence of publication bias could be found for almost all meta-analyses. Slight funnel plot asymmetry for mindfulness could only be observed at 5-10 months follow-up compared to inactive controls; however, results of trim-and-fill analysis again did not alter significance or direction of effect size estimates nor heterogeneity statistics. Therefore, although funnel plots suggest the size of pooled effects may be somewhat biased towards over-estimation of the true effect size, the chance of this bias resulting in the overall significant effect being a Type I error is low.

Table 3.4

Meta-analysis and trim-and-fill meta-analysis results

	Compared to inactive controls		
Outcome	**Post-program**	**1-4 months follow-up**	**5-10 months follow-up**
Depression	k=149; d=-0.6; z=-12.23 [-0.69, -0.5]*** Q(148)=676.29***; I^2=83.74% k_{imp}=23; d_{adj}=-0.73; z_{adj}=-14.39 $_{adj}$[-0.83, -0.63]*** Q_{adj}(171)=1111.43***; I^2_{adj}=86.96%	k=45; d=-0.82; z=-5.97 [-1.1, -0.55]*** Q(44)=348.76***; I^2=92.93% k_{imp}=10; d_{adj}=-1.07; z_{adj}=-7.92 $_{adj}$[-1.33, -0.8]*** Q_{adj}(54)=883.34***; I^2_{adj}=94.68%	k=21; d =-0.34; z=-4.02 [-0.5, -0.17] *** Q(24)=61.69***; I^2=68.28% k_{imp}=4; d_{adj}=-0.45; z_{adj}=-5.09 $_{adj}$[-0.62, -0.28]*** Q_{adj}(24)=117.8***; I^2_{adj}=77.79%
Anxiety	k=100; d=- 0.49; z=-8.93 [-0.59, -0.38]*** Q(99)=393.78***; I^2=79.53% k_{imp}=17; d_{adj}=-0.61; z_{adj}=-10.98 $_{adj}$[-0.72, -0.5]*** Q_{adj}(116)=601.27***; I^2_{adj}=83.11%	k=29; d=- 0.62; z=-3.87 [-0.94, -0.31]*** Q(28)=203.89***; I^2=91.1% k_{imp}=7; d_{adj}=-0.9; z_{adj}=-5.49 $_{adj}$[-1.23, -0.58]*** Q_{adj}(35)=502.1***; I^2_{adj}=93.8%	k=12; d=-0.36; z=-3.54 [-0.55, -0.16] *** Q(11)=25.84**; I^2=56.77% k_{imp}=2; d_{adj}=-0.41; z_{adj}=-4.39 $_{adj}$[-0.59, -0.23]*** Q_{adj}(13)=31.08*; I^2_{adj}=57.74%
Stress	k=51; d=- 0.73; z=-5.37 [-0.99, -0.46]*** Q(50)=499.29***; I^2=92.97% k_{imp}=16; d_{adj}=-1.04; z_{adj}=-8.35 $_{adj}$[-1.29, -0.8]*** Q_{adj}(66)=878.98***; I^2_{adj}=93.74%	k=17; d=-0.98; z=-4.18 [-1.44, -0.52]*** Q(16)=123.7***; I^2=91.58%	k<10
Mindfulness	k=61; d=0.51; z=7.15 [0.37, 0.65] *** Q(60)=224.81***; I^2=77.96%	k=19; d=0.86; z=3.46 [0.37, 1.35] *** Q(18)=138.02***; I^2=94.16%	k 10; d=0.37; z=3.11 [0.14, 0.61] ** Q(9)=18.22*; I^2=51.55% k_{imp}=1; d_{adj}=0.31; z_{adj}=2.5 $_{adj}$[0.07, 0.56]* Q_{adj}(10)=23.72**; I^2_{adj}=58.88%

	Compared to active controls		
Outcome	**Post-program**	**1-4 months follow-up**	**5-10 months follow-up**
Depression	**k=84; d=-0.2; z=-4.17 [-0.29, -0.11]*** Q(83)=258.15***; I^2=69.91% k_{imp}=11; d_{adj}=-0.29; z_{adj}=-5.78 $_{adj}$[-0.39, -0.19]*** Q_{adj}(94)=391.34***; I^2_{adj}=76.55%**	**k=30; d=-0.21; z=-2.5 [-0.37, -0.04]*** Q(29)=141.02***; I^2=67.5% k_{imp}=9; d_{adj}=-0.36; z_{adj}=-4.56 $_{adj}$[-0.52, -0.21]*** Q_{adj}(38)=172.04***; I^2_{adj}=72.29%**	k=13; d=-0.03; z=-.45 [-0.16, 0.1] Q(12)=16.1; I^2=26.96%
Anxiety	**k=50; d=- 0.16; z=-2.96 [-0.26, -0.05]** Q(49)=105.76***; I^2=52.78% k_{imp}=8; d_{adj}=-0.25; z_{adj}=-4.39 $_{adj}$[-0.36, -0.14]*** Q_{adj}(57)=151.23***; I^2_{adj}=64.02%**	**k=22; d=- 0.17; z=-2.66 [-0.29, -0.04]** Q(21)=39.16**; I^2=20.45% k_{imp}=3; d_{adj}=-0.21; z_{adj}=-3.22 $_{adj}$[-0.33, -0.08]** Q_{adj}(24)=44.11**; I^2_{adj}=30.29%**	k<10
Stress	**k=26; d=- 0.33; z=-2.22 [-0.61, -0.04]* Q(25)=135.19***; I^2=86.62% k_{imp}=7; d_{adj}=-0.561; z_{adj}=-3.83 $_{adj}$[-0.85, -0.27]*** Q_{adj}(32)=198.42***; I^2_{adj}=88.65%**	k=11; d=- 0.02; z=-.24 [-0.18, 0.14] Q(10)=5.44; I^2<.01% k_{imp}=3; d_{adj}=-.076; z_{adj}=-1.03 $_{adj}$[-.22, .07] Q_{adj}(13)=10.4; I^2_{adj}<.01%	k<10
Mindfulness	**k=34; d=0.21; z=3.4 [0.09, 0.34] *** Q(33)=60.17**; I^2=46.32%**	**k=13; d=0.23; z=2.91 [0.08, 0.39] ** Q(12)=12.94*; I^2=15.22%**	k<10

k=number of included studies; d=effect size; [] =95% confidence intervals; z=difference in effect size estimates; Q=Cochran's Q-statistic to test heterogeneity; I^2=percentage of between-study heterogeneity; k_{imp}=number of imputed studies based on trim-&-fill; d_{adj}=adjusted effect size estimates based on trim-&-fill; $_{adj}$[]=adjusted 95% confidence intervals based on trim-&-fill; z_{adj}=adjusted difference in effect size estimates based on trim-&-fill; Q_{adj}=adjusted Q-statistic based on trim-&-fill; I^2_{adj}=adjusted I^2 based on trim-&-fill;***=p<.0001; **=p<.01; *=p<.05; significant results in bold; k<10=not enough studies to complete analysis; results of trim-&-fill analysis only added where evidence of publication bias found.

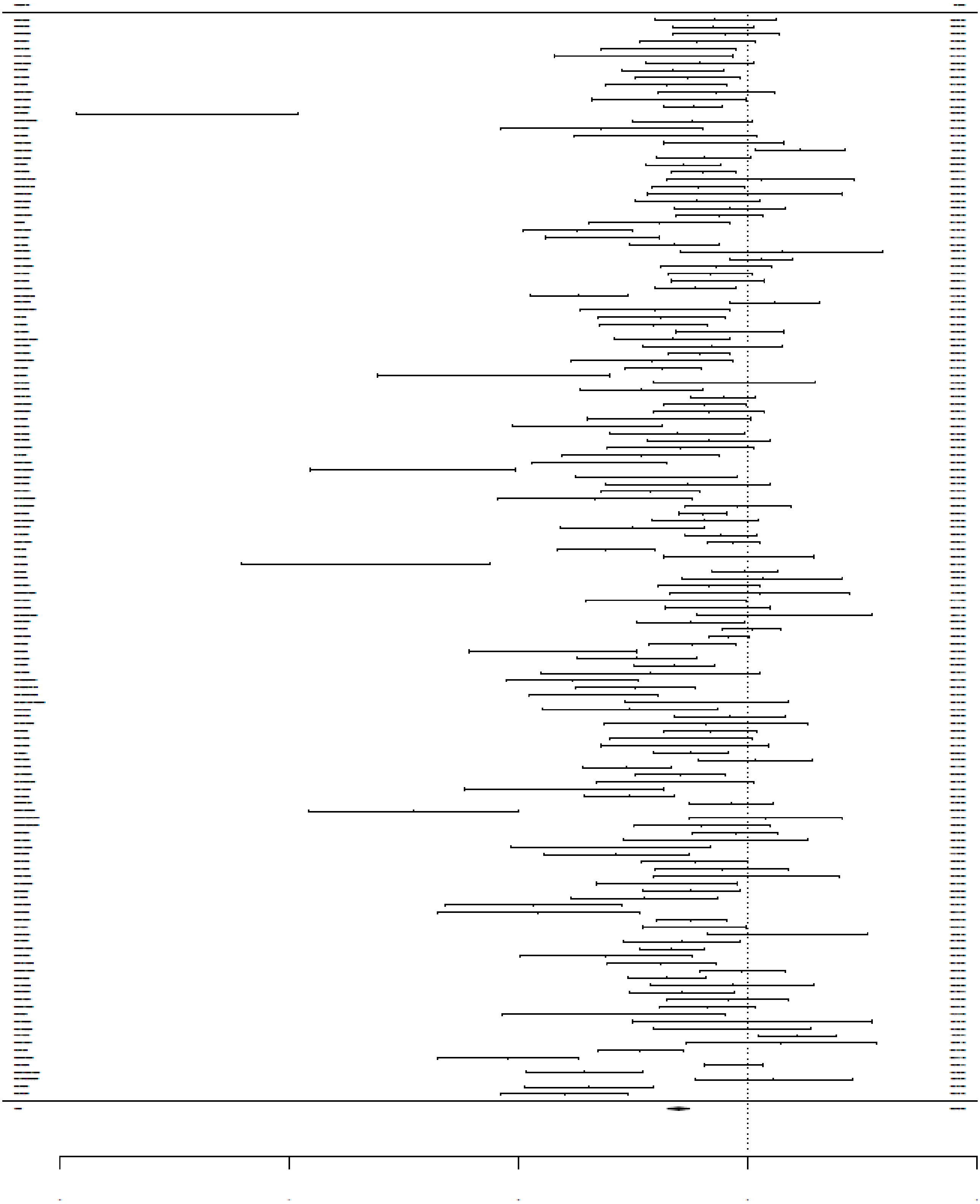
Forest and funnel plots (including corresponding trim-and-fill funnel plots) were created for all outcomes and timepoints. Figure 3.3 shows the forest[10] and funnel plots (including trim-and-fill) for the primary outcome depression at the primary timepoint (post-program) compared to the primary comparison group (inactive controls). One study had a significantly larger than average effect size. After checking, the data from this study had correctly been retrieved from the published paper and when repeating this meta-analysis with this outlier removed, this did not significantly alter results and thus analyses that followed included this study. Furthermore, results for any of the meta-analyses did not show influential cases and results did not substantially differ when repeatedly fitting the model leaving out one study at a time (as an example see Appendix Table 3.3.1 for results for the primary outcome depression at the primary timepoint immediately post-program compared to the primary comparison group inactive controls). This is unsurprising given the large number of studies included. Due to their large size and quantity, remaining forest and funnel plots (including trim-and-fill) for all other meta-analyses (i.e. all depression, anxiety, stress, and mindfulness outcomes, post-program and follow-up timepoints, and both comparison groups), can be found in Appendix Figures 3.3.2 – 3.3.20.

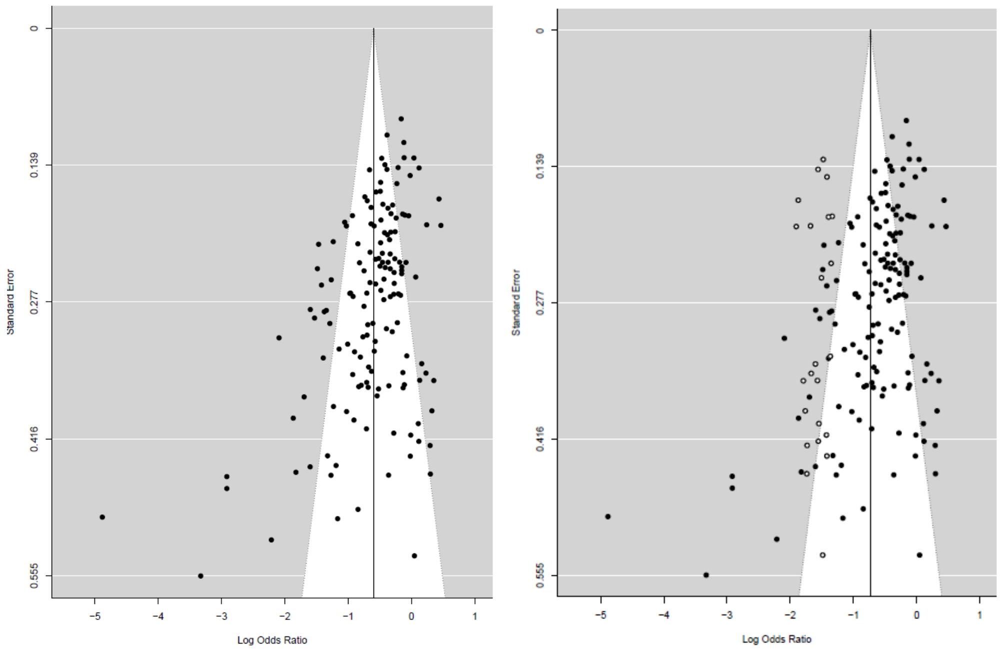
[10]View the PDF of the forest plot in Figure 3.3 here https://cccu-my.sharepoint.com/:b:/g/personal/ss940_canterbury_ac_uk/EQYZ38EUid5IgQ-0k3DQPg4BqV4sciN7p7NpD-yqxZ_iQQ?e=VS4vYe

Figure 3.3

Forest and funnel plots depression compared to inactive controls, post-program

Top: Forest plot; bottom: funnel plot (left), trim-and-fill funnel plot (right)





3.3.1 Meta-Analysis Results with One Sample per Study – Depression at Post-Program

As outlined in the methods (Chapter 2, Section 2.3.3.4), four studies had included two participant groups with different conditions and for the above presented meta-analyses, participants with different conditions were included in the analyses separately. As a reliability check, meta-analyses with the primary outcome depression at post-program were repeated only using one sample per study choosing the larger of the two groups for each of the four studies concerned. Related forest and funnel plots (including trim-and-fill) compared to both inactive and active controls can be found in Appendix Figures 3.3.21 and 3.3.22.

Similar to results of separate-sample meta-analyses for participant groups with different conditions, in the single-sample meta-analysis, MBP participants had significantly lower levels of depression with a small to moderate effect size at post-program compared to inactive controls (z=-12.23; p<.0001; k=146; d=-0.6; 95% C.I.:[-0.7; -0.51], p<.0001) and active controls (z=-4.07; p<.0001; k=83; d=-0.2; 95% C.I.:[-0.29; -0.1], p<.0001). Heterogeneity analysis of effect sizes again showed significant Q-statistics (inactive: Q(145)=601.78; p<.0001; active: Q(82)=256.14; p<.0001) and high I^2-statistics (inactive: I^2=83.89%; active: I^2=70.05%), indicating a high level of heterogeneity with effect sizes varying considerably (Cochran, 1954).

Funnel plots compared to inactive and active controls for the single-sample meta-analyses again show slight asymmetry towards the left of the mean thus indicating publication bias as was observed in the separate-sample meta-analyses presented above. Trim-and-fill analysis resulted in slightly modified effect size estimates compared to both inactive (k=169; z=-14.43; d=-0.74; 95% C.I.:[-0.84; -0.64], p<.0001) and active controls (k=96; z=-5.99; d=-0.31; 95% C.I.:[-0.41; -0.21], p<.0001). Again, trim-and-fill heterogeneity analysis of effect sizes showed a significant Q-statistic and high I^2-statistic compared to inactive controls (Q(145)=668.01; p<.0001; : I^2=87.29%) and active controls (Q(95)=414.3; p<.0001; : I^2=77.54%). Statistical significance and direction of effect size estimates remained unaltered.

3.3.2 Meta-Analysis Results by Outcome Measures

As a reliability check to determine whether inclusion of different measures had influenced results, separate meta-analyses on a measure-by-measure basis were completed, where $k \geq 10$. For depression compared to inactive controls at post-program and follow-up, measure-by-measure meta-analyses remained significant in the expected direction for all measures where enough data were available to run separate meta-analyses, namely BDI, CESD-D, DASS depression and HADS depression. Compared to active controls enough studies were only available for the BDI and CES-D. Meta-analysis results were significant in the expected direction for the BDI but not the CES-D. However, this is likely due to significantly reduced sample size (k=18) for this measure and comparison group. For anxiety, measure-by-measure meta-analyses again remained significant in the expected direction compared to inactive controls for the measures BAI, DASS anxiety and HADS anxiety at post-program (k<10 for follow-up). Compared to active controls, enough studies were only available for the BAI and results were not significant. However, this was marginally significant (95%C.I.=[-0.46, 0.05]) and the sample size was only just above the threshold (k=11). It is therefore likely that this was a Type II error and would have been significant with a larger sample. For stress at post-program and follow-up compared to both inactive and active controls where $k \geq 10$, measure-by-measure meta-analyses with PSS and DASS stress remained significant in the expected direction. Similarly, for mindfulness at post-program where $k \geq 10$, meta-analyses with FFMQ and MAAS remained significant in the expected direction. Congruent to overall meta-analyses, heterogeneity analysis of measure-by-measure meta-analyses showed significant Q-statistics and moderate to high I^2-statistics (I^2>40%) for all outcomes suggesting a high level of heterogeneity with considerably varying effect sizes (Cochran, 1954) and indicating at least 40% of effect size variance being due to differences in true effects (Higgins et al., 2003). Evidence of publication bias was similar across outcomes for the measure-by-measure meta-analyses, except for the BAI measure compared to active controls where participating in an MBP significantly related to decreased anxiety as a result of trim-and-fill analysis. No evidence of publication bias was found for the CES-D measure compared to active controls at post-program, the PSS measure at 1-4 months follow-up compared to inactive controls and FFMQ and MAAS measures compared to inactive and active controls; this was again similar to overall meta-analyses.

Measure-by-measure meta-analyses largely mirrored results of the overall meta-analyses with two exceptions compared to active controls which are likely due to Type II errors because of reduced power and publication bias. Depression and anxiety results compared to active controls therefore still need to be interpreted with caution. Results of measure-by-measure meta-analyses are detailed in Table 3.5. Forest[11] and funnel (including trim-and-fill) plots for the BDI depression at post-program compared to inactive controls are listed in Figure 3.4. The remaining measure-by-measure forest and funnel plots are listed in Appendix Figures 3.3.23 – 3.3.40.

[11]View the PDF of the forest plot in Figure 3.4 here https://cccu-my.sharepoint.com/:b:/g/personal/ss940_canterbury_ac_uk/ETLcIAll_c1LqL0wCOUMNTkBCXpfU9uO6sP-qphmTRMokg?e=xYECf3

Table 3.5

Measure-by-measure meta-analysis results for depression, anxiety, stress, and mindfulness outcomes compared to inactive and active controls

		DEPRESSION		
Measures	**Post-program**		**1-4 months follow-up**	
	Inactive controls	Active controls	Inactive controls	Active controls
BDI	**k=46; d=-0.73; z=-10.32** **[-0.86, -0.59]***** **Q(45)=126.91***; I^2=66.02%** **k_{imp}=3; d_{adj}=-0.78; z_{adj}=-10.56** **$_{adj}$[-0.92, -0.63]***** **Q_{adj}(48)=146.44***; I^2_{adj}=69.82%**	**k=30; d=-0.3; z=-3.68** **[-0.45, -0.14]***** **Q(29)=82.23***; I^2=68.3%** **k_{imp}=2; d_{adj}=-0.34; z_{adj}=-4.12** **$_{adj}$[-0.5, -0.18]***** **Q_{adj}(31)=94.07***; I^2_{adj}=70.85%**	**k=14; d=-0.96; z=-5.5** **[-1.3, -0.62]***** **Q(13)=64.51***; I^2=79.69%** **k_{imp}=2; d_{adj}=-1.09; z_{adj}=-6.18** **$_{adj}$[-1.43, -0.74]***** **Q_{adj}(15)=90.82***; I^2_{adj}=82.78%**	k<10
CES-D	**k=16; d=-0.7; z=-2.14** **[-1.32, -0.06]*** **Q(15)=170.05***; I^2=97.65%** **k_{imp}=4; d_{adj}=-1.02; z_{adj}=-3.41** **$_{adj}$[-1.61, -0.44]***** **Q_{adj}(19)=506.8***; I^2_{adj}=97.81%**	k=18; d=-0.08; z=-0.82 [-0.25, 0.1] Q(17)=54.29***; I^2=66.76%	k<10	k<10
DASS-D	**k=21; d=-0.58; z=-4.43** **[-0.84, -0.33]***** **Q(20)=79.87***; I^2=80.33%** **k_{imp}=5; d_{adj}=-0.78; z_{adj}=-5.79** **$_{adj}$[-1.04, -0.52]***** **Q_{adj}(25)=178.26***; I^2_{adj}=85.98%**	k<10	k<10	k<10
HADS-D	**k=21; d=-0.43; z=-5.48** **[-0.59, -0.28]***** **Q(20)=46.35***; I^2=58.12%** **k_{imp}=2; d_{adj}=-0.48; z_{adj}=-5.86** **$_{adj}$[-0.63, -0.32]***** **Q_{adj}(22)=53.62***; I^2_{adj}=61.09%**	k<10	k<10	k<10

ANXIETY

Measures	**Post-program**		**1-4 months follow-up**	
	Inactive controls	Active controls	Inactive controls	Active controls
BAI	**k=15; d=-0.89; z=-4.13 [-1.32, -0.47]*** Q(14)=90.8***; I^2=86.69% k_{imp}=1; d_{adj}=-0.98; z_{adj}=-4.44 $_{adj}$[-1.41, -0.55]*** Q_{adj}(15)=108.72***; I^2_{adj}=87.9%**	k=11; d=-0.2; z=-1.57 [-0.46, 0.05] Q(10)=26.76**; I^2=66.64% **k_{imp}=4; d_{adj}=-0.38; z_{adj}=-2.94 $_{adj}$[-0.63, -0.13]** Q_{adj}(14)=50***; I^2_{adj}=74.86%**	k<10	k<10
DASS-A	**k=19; d=-0.43; z=-2.94 [-0.72, -0.15]** Q(18)=73.63***; I^2=81.67% k_{imp}=4; d_{adj}=-0.61; z_{adj}=-4.1 $_{adj}$[-0.9, -0.32]*** Q_{adj}(22)=138.77***; I^2_{adj}=85.58%**	k<10	k<10	k<10
HADS-A	**k=22; d=-0.34; z=-3.98 [-0.51, -0.17]*** Q(21)=62.29***; I^2=67.02% k_{imp}=5; d_{adj}=-0.47; z_{adj}=-5.28 $_{adj}$[-.65, -0.3]*** Q_{adj}(26)=104.84***; I^2_{adj}=75.42%**	k<10	k<10	k<10

STRESS

Measures	**Post-program**		**1-4 months follow-up**	
	Inactive controls	Active controls	Inactive controls	Active controls
PSS	**k=35; d=-0.79; z=-4.18 [-1.17, -0.42]*** Q(34)=451.67***; I^2=94.88% k_{imp}=10; d_{adj}=-1.11; z_{adj}=-6.41**	**k=20; d=-0.31; z=-2.62 [-0.54, -0.09]** Q(19)=78.6***; I^2=74.85% k_{imp}=5; d_{adj}=-0.49; z_{adj}=-3.82**	**k=13; d=-1; z=-3.49 [-1.56, -0.44]*** Q(12)=101.58***; I^2=92.35%**	k<10

	$_{adj}$[-1.45, -0.77]* Q_{adj}(44)=725.5***; I^2_{adj}=95.16%**	**$_{adj}$[-0.74, -0.24]*** Q_{adj}(24)=110***; I^2_{adj}=81.2%**		
DASS-S	**k=14; d=-0.57; z=-3.83 [-0.86, -0.28]*** Q(13)=42.53***; I^2=73.96% k_{imp}=3; d_{adj}=-0.73; z_{adj}=-4.86 $_{adj}$[-1.03, -0.44]*** Q_{adj}(16)=77.58***; I^2_{adj}=80.32%**	k<10	k<10	k<10

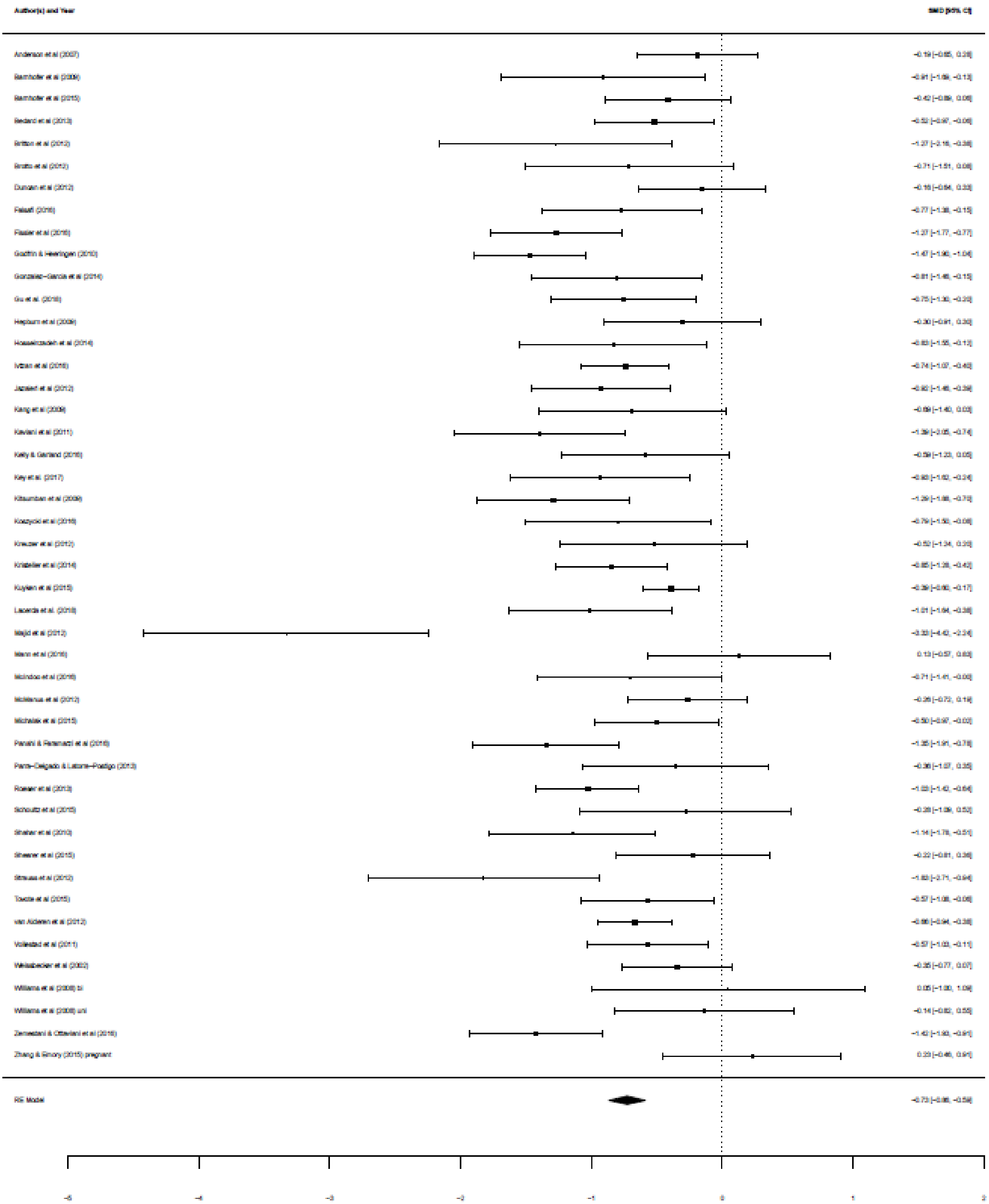
MINDFULNESS

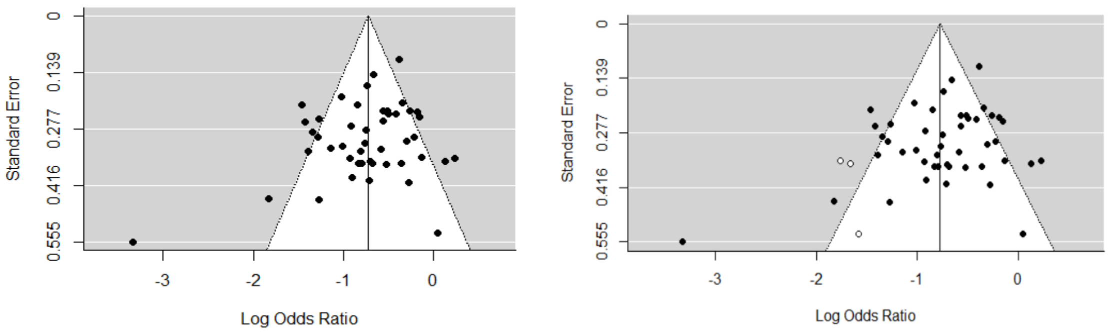
Measures	Post-program		1-4 months follow-up	
	Inactive controls	Active controls	Inactive controls	Active controls
FFMQ	**k=37; d=0.53; z=5.67 [0.34, 0.71]*** Q(36)=137.43***; I^2=78.61%**	**k=21; d=.23; z=3.23 [0.09, 0.37]** Q(20)=32.33*; I^2=38.98%**	k<10	k<10
MAAS	**k=15; d=0.48; z=2.64 [0.12, 0.84]** Q(14)=64.56***; I^2=84.7%**	k<10	k<10	k<10

BDI=Beck's Depression Inventory; CES-D=Centre for Epidemiology Studies Depression Scale; DASS-D=Depression Anxiety, Stress Scale-Depression; HADS-D= Hospital Anxiety and Depression Scale-Depression; BAI=Beck's Anxiety Inventory; DASS-A=Depression Anxiety, Stress Scale-Anxiety; HADS-A=Hospital Anxiety and Depression Scale-Anxiety; PSS=Perceived Stress Scale; DASS-S= Depression Anxiety, Stress Scale-Stress; FFMQ=Five Facet Mindfulness Questionnaire; MAAS= Mindful Attention Awareness Scale; k=number of included studies; d=effect size; [] =95% confidence intervals; z=difference in effect size estimates; Q=Cochran's Q-statistic to test heterogeneity; I^2=percentage of between-study heterogeneity; k_{imp}=number of imputed studies based on trim-&-fill; d_{adj}=adjusted effect size estimates based on trim-&-fill; $_{adj}$[]=adjusted 95% confidence intervals based on trim-&-fill; z_{adj}=adjusted difference in effect size estimates basedon trim-&-fill; Q_{adj}=adjusted Q-statistic based on trim-&-fill; I^2_{adj}=adjusted I^2 based on trim-&-fill;***=p<.0001; **=p<.01; *=p<.05; significant results in bold; k<10=not enough studies to complete analysis; results of trim-&-fill analysis only added where evidence of publication bias found.

Figure 3.4

Forest and funnel plots BDI depression compared to inactive controls, post-program
Top: Forest plot; bottom: funnel plot (left), trim-and-fill funnel plot (right)





3.4 Discussion: Descriptive Statistics and Meta-Analysis

3.4.1 Summary of Findings

This chapter has outlined descriptive statistics followed by presentation of meta-analysis results. Through database searching, 203 randomised controlled trials were included in this review with 18,419 participants randomised either to MBP or control groups. Included MBPs were conducted in several different countries (*k*=25) employing different clinical and general population samples with predominantly female participants varying from 18 to 90 years of age. Fifteen different doses relating to MBPs were extracted and calculated with most included studies having provided relevant information to calculate doses. As expected, most doses varied considerably since many different types of MBPs were included in this review. Study quality was assessed with the Cochrane Risk of Bias tool, which showed a wide range of quality of included studies. Quality was also assessed with the Actual Practice Quality Rating tool, which indicated a low risk of memory bias and a high risk of social desirability bias for most studies who had recorded practice.

Meta-analytic examinations were completed to understand the overall value of participating in MBPs compared to controls in this sample of included studies prior to adding dose variables. Results of meta-analyses where enough data were available showed significant moderate to large between-group differences favouring the MBP group for all outcomes compared to inactive controls at post-program and follow-up timepoints. Compared to active controls, significant between-group differences were observed for all outcomes at post-program and for depression, anxiety, and mindfulness at 1-4 months follow-up with small to moderate effect sizes. For stress, no significant between-group differences were found at 1-4 months follow-up compared to active controls. As outlined in Chapter 1, Section 1.4.1, smaller or no differences in effects found when comparing to active controls may be due to other programs also being effective for outcomes, practice effects of simply taking part in a program, or Type II errors, especially in low-powered samples. Between-study heterogeneity was found as expected. Trim-and-fill analysis to correct publication bias did not substantially alter findings. Results of meta-analyses partially supported Hypothesis 1 since participating in MBPs significantly related to decreased depression, anxiety, and stress and increased mindfulness compared to inactive controls. The program group compared to active controls however only supported Hypothesis 1 at immediately post-program and at 1-4 months follow-up for depression, anxiety, and mindfulness. For the stress outcome at 1-4 months follow-up as well as depression at 5-10 months, Hypothesis 1 was not confirmed compared to active controls. However, this may be due to low power since much smaller numbers of studies were included at follow-up timepoints. Additionally, smaller and null effects can often be associated with active comparison groups where an effect was found compared to inactive controls (Wampold, 2001, see Chapter 1, Section 1.4.1).

As a reliability check, single-sample as well as measure-by-measure meta-analyses were conducted to determine whether having included two different samples for four studies or including different outcome measures influenced results. Results of single-sample meta-analyses reflected findings from overall meta-analyses. Measure-by-measure meta-analysis mirrored overall meta-analysis results for stress and mindfulness outcomes. For depression and anxiety, compared to active controls only, meta-analysis results for one measure (CES-D for depression, BAI for anxiety) were no longer significant. However, this is likely due to publication bias and low power due to substantially reduced sample sizes and thus possible Type II errors. Therefore, results of depression and anxiety compared to active controls still need to be interpreted with caution. Meta-analysis results are discussed next followed by limitations.

3.4.2 Discussion of Meta-Analysis Findings

Results of meta-analyses largely coincide with previous research. The finding that mindfulness programs are generally beneficial, particularly compared to controls receiving no or no different than usual programs at the time, corresponds with previous research professing the positive effects of mindfulness especially with regards to psychological wellbeing (Keng et al., 2011). These findings also relate to theoretical aspects of rumination and worry as mechanisms of depression, anxiety and stress outlined in Chapter 2 (e.g. Borkovec et al., 1983; Nolen-Hoekseman & Morrow, 1993; Segerstrom et al., 200), which can be addressed through mindfulness (Gu et al., 2015). Not only a reduction in maladaptive psychological outcomes, but also increased mindfulness was observed in this meta-analysis for MBP participants, which again remained significant at follow-up. This finding further evidenced that mindfulness practice and participating in an MBP relates to higher levels of mindfulness, which has previously been identified as a beneficial trait facilitating interpersonal and intrapersonal skills and awareness (Davis & Hayes, 2011) resulting in positive outcomes.

Furthermore, results of findings presented above correspond with results from previous meta-analyses. For instance, in a recent review of systematic reviews and meta-analyses of MBSR and MBCT programs for clinical populations, decreased levels of depression, anxiety, stress and increased quality of life were observed (Gotink et al., 2015). Additionally, in a review of 39 MBSR programs for healthcare professionals, participating in MBSR was related to positive outcomes regarding burnout, stress, anxiety, depression, and empathy (Lamothe et al., 2016). Alternatively, participating in MBCT, but not MBSR, was related to reduced clinical levels of depression but not anxiety compared to inactive controls in a previous meta-analysis of 12 studies with participants with a diagnosis of depression or anxiety (Strauss et al., 2014). However, although some of the above-mentioned meta-analyses included a vast number of studies (e.g. Gotink et al., 2015), this meta-analysis had an even larger number of included studies (k=203). Additionally, the meta-analyses conducted here further extended current knowledge by including other types of MBPs aside from standardised MBSR and MBCT programs and thus not limiting results to standardised MBPs only. Results of this meta-analysis therefore also correspond with previous research advocating the value of different types of MBPs such as online MBPs which have been found to relate to beneficial outcomes (Spijkerman et al., 2016). Similarly, in a previous review of MBPs, much shorter, less intense programs without face-to-face contact have been found to be beneficial for wellbeing outcomes

(Creswell, 2017). However, including a wide range of different MBPs in meta-analysis can have its limitations. Therefore, an exploration of methodological and interpretative limitations follows.

3.4.3 Methodological and Interpretative Limitations

Firstly, a limitation relating to inclusion criteria was that studies which were published in a language other than English were excluded since it was impossible to translate research from all languages. The selection of studies may therefore have been biased. However, in academia in general and more specifically in science, publication of research is typically dominated by the English language both for native English research and translated from non-native English research (Ammon, 2012) suggesting that the majority of existing mindfulness research was included in this review. Moreover, most participants in included studies were female which could potentially have biased results. However, literature searches for inclusion of studies were conducted systematically thus reducing bias and previous research has found that in mindfulness research, the majority of participants are generally female with male and other genders underrepresented (Chin et al., 2019). This review is therefore representative of the field.

Included studies also varied considerably in terms of quality across Cochrane Risk of Bias domains. Particularly for risk of selective reporting bias, 73.4% of studies either retrospectively registered their trials or did not register them at all, both of which render judgement of risk of reporting bias impossible. However, in recent years, more stringent rules have been administered in academia with regards to preregistering trials on authorised registration sites such as ClinicalTrial.gov (USA) or ISRCTN registry (international) as well as countless others. Preregistration is now a prerequisite for submitting a manuscript to some high-impact journals such as the BMJ and The Lancet, who only accept trials for review which have been preregistered (British Medical Journal, 2021; van Hateren et al., 2013; Weber et al., 2015). Therefore, there is hope that for future reviews similar to this one, the percentage of studies where risk of reporting bias is high or unknown will diminish. Additionally, for the domain blinding of participants and personnel, high risk of bias was associated with most studies. However, as opposed to placebo-controlled medication trials, in psychotherapy trials such as MBPs, it is often impossible to conceal group assignments to individuals particularly in studies comparing programs to inactive controls which may affect their behaviour during and commitment to the program (Berger, 2016; Karanicolas ***et al.,*** 2010). Furthermore, another issue relating to quality is how accurately actual mindfulness practice was recorded in studies, since generally, high levels of social desirability bias in particular were observed on the Actual Practice Quality Rating tool, which has previously been found problematic in mindfulness research (e.g. Lacaille et al., 2018). Therefore, the reliability of the actual home practice component of the actual use of the MBP dose is questionable, and findings need to be interpreted with caution. To address variability in quality of studies to an extent, scores on the RoB and Actual Practice Quality Rating tools are examined as moderators for significant dose-response relationships (see Chapters 4 and 5). Limitations associated with specific doses are outlined next.

3.4.3.1 Limitations Associated with Calculation of Doses

In this review, the composite dose variables recommended and actual use of the MBP were calculated as a combination of in-session and home practice and learning activities. The strength associated with this way of calculating engagement with MBPs is that these doses represented the entirety of different aspects associated with mindfulness practice. However, having employed an inclusive position regarding mindfulness practice has the disadvantage of not knowing how much of this dose was exclusively recommended or actual formal practice as opposed to engagement in other exercises or discussion. For the doses recommended use of the MBP, this issue was addressed to an extent by having included primary-level doses examining number and duration of recommended home practices, thus assessing practice recommended to be completed outside of sessions only. However again, this still also included exercises other than formal practice. On the other hand, for the dose actual use of the program, from the data that was available either from published papers or through communication with authors, it was not possible to distinguish the different elements of in-session and home practices and other elements. Nevertheless, the aim of the actual use of the MBP dose was not to examine actual engagement in formal mindfulness practice only, but to ascertain the overall actual use of MBPs as a combination of home practice, in-session practice, in-session discussion, and engagement with educational materials both in- and outside of sessions. Arguably, all the different practices and exercises connected with MBPs are of potential importance, as is generally the view of experienced mindfulness teachers (e.g. Kabat-Zinn, 1990; Segal et al., 2002).

Furthermore, it is worth addressing that not all doses and aspects of mindfulness that were planned were able to be examined. Previous research has emphasized the importance of informal mindfulness practice in daily life (e.g. Langer 2014). However, as outlined in Chapter 2, data on informal mindfulness practice was generally not reported in included studies and dose-effects of amount of informal mindfulness practice could therefore not be examined. In future RCTs, it could be helpful to collect separate data on different types of mindfulness practice (formal, informal) and other activities as well as specifying how much of these activities were completed during and outside of sessions. Additionally, as outlined in Chapter 2, it was planned to extract information on MBP teachers' years of mindfulness experience for face-to-face programs to determine whether amount of teacher experience could have influenced results. However, this was not reported in sufficient detail in included studies for this to be assessed. This lack of exploration of mindfulness teachers' experience thus has to be noted as a limitation of this research. Hopefully, in the future, research will report mindfulness teacher experience in a more consistent, standardized manner, which will allow data on this to be used in meta-regression.

Next, mindfulness research has predominantly been completed with MBSR and MBCT programs (Khoury et al., 2013) thus likely limiting variability of MBP types and doses which questions the relevance of conducting a dose-response meta-regression. However, only just over half of included programs were MBSR and MBCT with most dose variables varying considerably in this review. An exception to this are dose variables relating to program intensity; low variability relating to these doses is discussed in Chapter 5. Additionally, several doses relating to program length, amount of contact, intensity and recommended program use were confounded with program type. However, since MBSR and MBCT employ standardised programs, it is to be expected that these doses are higher for these types of programs. Nevertheless, confounding effects relating to MBP type were controlled to an extent by including program type as a moderator for significant dose-response relationships (see Chapters 4 and 5). Limitations relating to meta-analysis findings are explored next.

3.4.3.2 Limitations Relating to Meta-Analysis

Firstly, although trim-and-fill analysis is considered as one of the most sophisticated and popular methods for addressing publication bias (Shi & Lin, 2019), trim-and-fill results need to be interpreted with caution in meta-analyses with substantial heterogeneity, as was the case in this review, since this could influence the power of trim-and-fill and funnel plot asymmetry may be caused by heterogeneity rather than publication bias (Ioannidis et al., 2007; Peters et al., 2007). However, results of trim-and-fill analyses generally neither altered statistical significance and direction of effect sizes nor substantially modified heterogeneity statistics.

Additionally, although it was initially planned to group similar active control groups and analyse these separately, not enough studies (k<10) included sufficiently similar active control activities to reliably analyse and interpret results; this therefore needs to be noted as a limitation. However, grouping different active controls is a common approach in meta-analyses (e.g. Hofmann et al., 2010; Khoury et al., 2013). This approach was also taken for meta-regression analyses presented in Chapters 4 and 5.

Next, for four studies, separate participant groups derived from the same study were included. Participants with different conditions were included in the analyses separately, ensuring that each participant was only ever counted once for each analysis. Therefore, these were separate participant groups engaging in an MBP compared to a control group of participants with the same condition, i.e. an MBP group of cancer patient participants was compared to a control group of cancer patient participants and an MBP group of caregiver participants was compared to a control group of caregiver participants in separate analyses for Kubo et al. (2019), etc. A possible limitation with this approach was that separate groups deriving from the same study still presents an issue for meta-analysis since participants with different conditions still experience a more similar program than would groups in truly independent studies. This therefore violates the assumption for equal independence for participant groups from the same study. However, the number of studies to which this applied was very small (k=4) compared to the overall number of studies, therefore, this did not seem likely to have a material impact on the results. This was confirmed by repeating the meta-analyses with the primary outcome depression with only one sample from each of these four studies included and this made no significant difference to the findings.

Furthermore, different outcome measures were included in the meta-analysis which could have influenced results since different measures may not necessarily measure exactly the same constructs (Fried, 2017; Smarr & Keefer, 2011). However, a precedent in the literature is that different measures are generally included in meta-analysis (e.g. Blanck et al., 2018; Khoury et al., 2015; Spijkerman et al., 2016) and this principle was therefore also followed here. Nevertheless, as a reliability check where enough studies were available, measure-by-measure meta-analyses were conducted which largely found the same results as overall meta-analyses with the exception of two measures for depression and anxiety compared to active controls. However, these non-significant findings are likely due to publication bias and Type II errors since they are marginally significant and a significant finding was found for the inactive control comparison group (cf. Wampold, 2001). Nonetheless, overall meta-analyses particularly for depression and anxiety outcomes still need to be interpreted with caution. Additionally, measure-by-measure analysis was also repeated for the dose-response meta-regression (Chapters 4 and 5).

Finally, a limitation relating to meta-analyses presented was the high between-study heterogeneity observed. However, the aim of this review was to be inclusive of diverse MBPs set in different countries with different facilitators, populations, and delivery methods; a certain degree of heterogeneity is therefore to be expected. To further extend current knowledge on the effectiveness of different aspects of MBPs, a dose-response meta-regression analysis is warranted, which is addressed in the next Chapters (Chapters 4 and 5). Meta-regression analyses are typically employed where high between-study heterogeneity is observed (Higgins & Thompson, 2004).

3.5 Chapter 3 Summary

In this chapter, descriptive statistics of included studies were presented followed by assessment of study quality. Results of meta-analyses were presented thereafter. In summary, included studies varied considerably in quality and doses related to MBPs thus warranting dose-response meta-regression analyses. Based on results of the meta-analysis, participating in MBPs was found to have a positive effect on all outcomes at post-program and follow-up compared to inactive controls. Compared to active controls, MBP participation was significantly associated with change in depression, anxiety and mindfulness at post-program and some follow-up time-points, albeit with a smaller effect. As a reliability check, single-sample and measure-by-measure meta-analyses were conducted with largely similar findings. Results of meta-analyses further extend current research in the field by having included different types of MBPs. This chapter has situated the effectiveness of MBPs for each of the hypothesised outcomes. In the next chapters, the role of dose in MBPs is explored by examining potential dose-response relationships between doses of MBPs and psychological distress outcomes (Chapter 4) and the mindfulness outcome (Chapter 5).

CHAPTER 4

Dose-Response in Mindfulness-Based Programs: Meta-Regression Results and Discussion of Psychological Distress Outcomes

4.1 Chapter 4 Overview

This chapter follows on from Chapter 3, which presented and discussed descriptive statistics and meta-analyses of the dose-response review. This chapter presents the results of the dose-response meta-regression analysis for doses relating to MBPs and the psychological distress outcomes depression (primary outcome), anxiety and stress (secondary outcomes) at post-program and follow-up time points. These are illustrated both numerically and visually. For significant dose-response relationships, baseline levels of the outcome were controlled for, and moderator analyses were completed. Clinical significance and severity analyses as well as measure-by-measure and population-specific meta-regression analyses are completed as reliability checks. Dose-response findings for psychological distress outcomes are discussed in line with previous research followed by limitations and implications.

4.2 Results of the Dose-Response Meta-Regression for Psychological Distress Outcomes

After meta-analyses presented in Chapter 3, dose variables were added as predictors to the model and analysed using meta-regression to test Hypothesis 2 of whether greater doses of MBPs predicted decreased depression, anxiety, and stress. As described in Chapter 2, Restricted Maximum Likelihood (REML) meta-regression using a random effects model with the Knapp and Hartung adjustment (Knapp & Hartung, 2003) was employed to assess whether dose of MBPs predicted between group differences in depression, anxiety, and stress. As a reminder, the continuous dose variables added to this model as predictors were total number of face-to-face sessions, duration of a face-to-face session (in hours), program length (in weeks), frequency of recommended practices a week (number of practices recommended per week), duration of a recommended practice (one practice in minutes), total amount of face-to-face facilitator contact (in hours), total amount of face-to-face facilitator contact (in hours) when excluding zero hours of contact, total recommended use of the program (in hours), total actual use of the program (in hours), program intensity (excluding retreats), program intensity (including retreats), amount of face-to-face facilitator contact a week (in hours), amount of face-to-face facilitator contact a week (in hours) when excluding zero hours of contact, recommended use of the program a week (in hours) and actual use of the program a week (in hours).

Results of meta-regression analyses are presented for psychological distress outcomes depression (primary outcome) and anxiety and stress (secondary outcomes) compared to inactive and active controls, at immediately post-program and 1-4 months follow-up. No significant dose-response relationships were observed for any of the outcomes at 5-10 months follow-up and all results at this timepoint were therefore added to Appendices. At 12-15 months follow-up, not enough studies (k<10) were available for any of the outcomes neither compared to inactive nor active controls to warrant meta-regression.

To control for Type I errors and test robustness of findings, significant dose-response relationships were subject to Holm-Bonferroni and False Discovery Rate (FDR; Benjamini-Hochberg procedure) corrections (see Chapter 2 for details). Additionally, significant dose-response relationships were followed up with moderator analyses to test Hypothesis 4 of dose-response relationships being moderated by MBP type, with MBSR/MBCT programs hypothesised to find a larger effect than other MBPs, and Hypothesis 5 of dose-response relationships being moderated by study quality (risk of bias score on Cochrane Risk of Bias tool) and actual practice recording quality (for actual use of MBP doses only) with lower quality studies hypothesised to show a larger effect. Significant interactions between moderators and doses were further examined with subgroup analysis. Although it was planned to also examine the effects of the categorical moderator participant population group and thus test Hypothesis 3, this could not be completed since there were not enough studies (k<10) available in one or more population categories. However, as a reliability check, separate meta-regression with the depression population group and then again, the general population group were completed for the primary outcome, comparison group and timepoint. Furthermore, to determine whether baseline levels of outcomes confounded significant dose-response relationships, mean baseline scores for outcomes were controlled for by adding these as covariates to each separate meta-regression model according to outcome where a significant dose-response relationship was found. Finally, similar to meta-analysis, measure-by-measure meta-regression analyses were completed.

Meta-regression plots were created for all doses, timepoints, control groups and outcomes (including for all measure-by-measure meta-regression analyses). For purposes of conciseness, all meta-regression plots apart from for the primary outcome (depression) at the primary timepoint (post-program) and for significant dose-response relationships have been added to Appendices.

4.2.1 Dose-Response Meta-Regression Results for Primary Outcome Depression

4.2.1.1 Depression at Post-Program

For depression as the primary outcome at immediately post-program, neither primary nor composite dose variables significantly predicted effect sizes, for inactive or active controls thus not confirming Hypothesis 2 (see Table 4.1). In the table, results for each dose including effect size (Cohen's *d*) of the standardised regression coefficient, standard error of the effect size (SE), confidence intervals (95% CI) as an estimation of probable boundaries within which the true effect is believed to be, test statistic of slope of the model (*t*-value), significance level (*p*-value) to determine whether the dose predictor significantly differed from zero, and number of studies (*k*) included in the model for each dose are displayed. Additionally, heterogeneity statistics are listed for each dose-response meta-regression analysis, including a test for the overall model (*F*-distribution), percentage of heterogeneity accounted for by the meta-regression model (R^2), variance of the underlying true effect sizes (tau^2/τ^2), standard error of tau^2 ($SE\ tau^2$), between-study heterogeneity (Cochran's Q: Q_E) and whether between-study heterogeneity exceeds that expected by chance alone, measured with its significance level ($p(Q_E)$). As illustrated in Table 4.1, confidence intervals (95% CI) crossed zero with significance levels above the threshold of p=.05 for dose-response meta-regression analyses for all dose variables, with significant between study heterogeneity ($p(Q_E)$<.001).[12] As a reliability check, the relevant dose-response meta-regression analyses were repeated without the outlier mentioned in Chapter 3, Section 3.3 excluded and unsurprisingly, this made no significant difference to results.

[12]As outlined in Chapter 2, Section 2.3.5.2, dose-response meta-regression analyses were examined separately for each dose variable. As a reliability check, for the primary outcome, control condition and timepoint, the meta-regression analysis was repeated with all doses included as predictors in the same model producing the same non-significant result.

Table 4.1

Meta-regression analysis results by MBP dose for between-group depression effect sizes at immediately post-program compared to inactive and active controls

Compared to inactive control groups												
Dose	**Meta-regression model**						**Heterogeneity statistics**					
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total no. face-to-face sessions	-0.58	0.01	[-0.03, 0.03]	-0.19	.85	149	0.04	0.00%	0.28	0.04	676.19	<.001
Duration of a face-to-face session	0.07	0.08	[-0.1, 0.23]	0.82	.41	126	0.67	0.00%	0.31	0.05	574.86	<.001
Program length	0.01	0.02	[-0.04, 0.05]	0.19	.84	149	0.04	0.00%	0.28	0.04	669.86	<.001
Frequency of recommended practice	0.02	0.05	[-0.08, 0.11]	0.33	.74	137	0.11	0.00%	0.2	0.03	531.91	<.001
Duration of a recommended practice	-0.01	0.03	[-0.01, 0.01]	-0.21	.84	137	0.04	0.00%	0.2	0.03	525.12	<.001
Composite	***D***	**SE**	**95% CI**	***T***	***P***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total amount of contact	-0.03	0.01	[-0.01, 0.01]	-0.53	.6	149	0.28	0.00%	0.28	0.04	672.62	<.001
Total amount of contact (excl. 0 hours)	0.03	0.01	[-0.01, 0.02]	0.44	.66	126	0.19	0.00%	0.31	0.05	575.41	<.001
Total recommended use of program	<-.001	0.02	[-0.01, 0.04]	-0.09	.93	137	0.01	0.00%	0.2	0.03	530.52	<.001
Total actual use of program	0.02	0.01	[-0.01, 0.01]	0.29	.77	32	0.09	0.00%	0.11	0.05	88.67	<.001
Program intensity excl. retreats	0.07	0.17	[-0.28, 0.41]	0.38	.7	126	0.15	0.00%	0.31	0.05	572.72	<.001
Program intensity incl. retreats	0.11	0.17	[-0.23, 0.44]	0.63	.53	126	0.39	0.00%	0.31	0.05	573.57	<.001
Amount of contact/week	-0.04	0.04	[-0.11, 0.03]	-1.02	.31	149	1.04	0.5%	0.28	0.04	661.99	<.001
Amount of contact (excl. 0 hours)/week	-0.01	0.05	[-0.1, 0.08]	-0.28	.78	126	0.08	0.00%	0.31	0.05	572.68	<.001
Recommended use of program/week	-0.01	0.02	[-0.04, 0.03]	-0.25	.8	137	0.06	0.00%	0.2	0.03	526.61	<.001
Actual use of program/week	0.02	0.05	[-0.08, 0.12]	0.39	.69	32	0.16	0.00%	0.11	0.05	89.73	<.001

Compared to active control groups												
Dose	**Meta-regression model**						**Heterogeneity statistics**					
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total no. face-to-face sessions	-0.01	0.01	[-0.03, 0.02]	-0.58	.56	84	0.34	0.00%	0.13	0.03	249.16	<.001
Duration of a face-to-face session	0.03	0.07	[-0.1, 0.16]	0.4	.69	74	0.16	0.00%	0.13	0.03	214.25	<.001
Program length	0.02	0.02	[-0.02, 0.06]	0.83	.41	84	0.69	0.00%	0.13	0.03	258.09	<.001
Frequency of recommended practice	0.08	0.06	[-0.04, 0.19]	1.33	.19	75	1.76	2.11%	0.09	0.03	193.15	<.001
Duration of a recommended practice	<0.001	0.03	[-0.01, 0.01]	0.09	.93	75	0.07	0.00%	0.09	0.03	195.85	<.001
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total amount of contact	<0.001	0.01	[-0.01, 0.01]	0.09	.93	84	0.01	0.00%	0.13	0.03	255.18	<.001
Total amount of contact (excl. 0 hours)	0.03	0.01	[-0.01, 0.02]	0.55	.58	74	0.3	0.00%	0.13	0.03	213.78	<.001
Total recommended use of program	-0.01	0.02	[-0.01, 0.03]	-0.44	.66	75	0.19	0.00%	0.09	0.03	189.59	<.001
Total actual use of program	-0.02	0.04	[-0.01, 0.01]	-0.42	.68	29	0.18	0.00%	0.09	0.05	74.84	<.001
Program intensity excl. retreats	-0.02	0.12	[-0.25, 0.21]	-0.17	.87	74	0.03	0.00%	0.13	0.03	213.01	<.001
Program intensity incl. retreats	-0.05	0.12	[-0.28, 0.18]	-0.43	.67	74	0.18	0.00%	0.13	0.03	212.2	<.001
Amount of contact/week	-0.03	0.04	[-0.12, 0.06]	-0.74	.46	84	0.55	0.00%	0.13	0.03	249.68	<.001
Amount of contact (excl. 0 hours)/week	-0.02	0.06	[-0.14, 0.09]	-0.38	.71	74	0.14	0.00%	0.13	0.03	214.02	<.001
Recommended use of program/week	-0.01	0.02	[-0.05, 0.02]	-0.73	.47	75	0.53	0.21%	0.09	0.03	186.65	<.001
Actual use of program/week	-0.03	0.03	[-0.09, 0.04]	-0.88	.39	29	0.77	0.00%	0.09	0.04	72.02	<.001

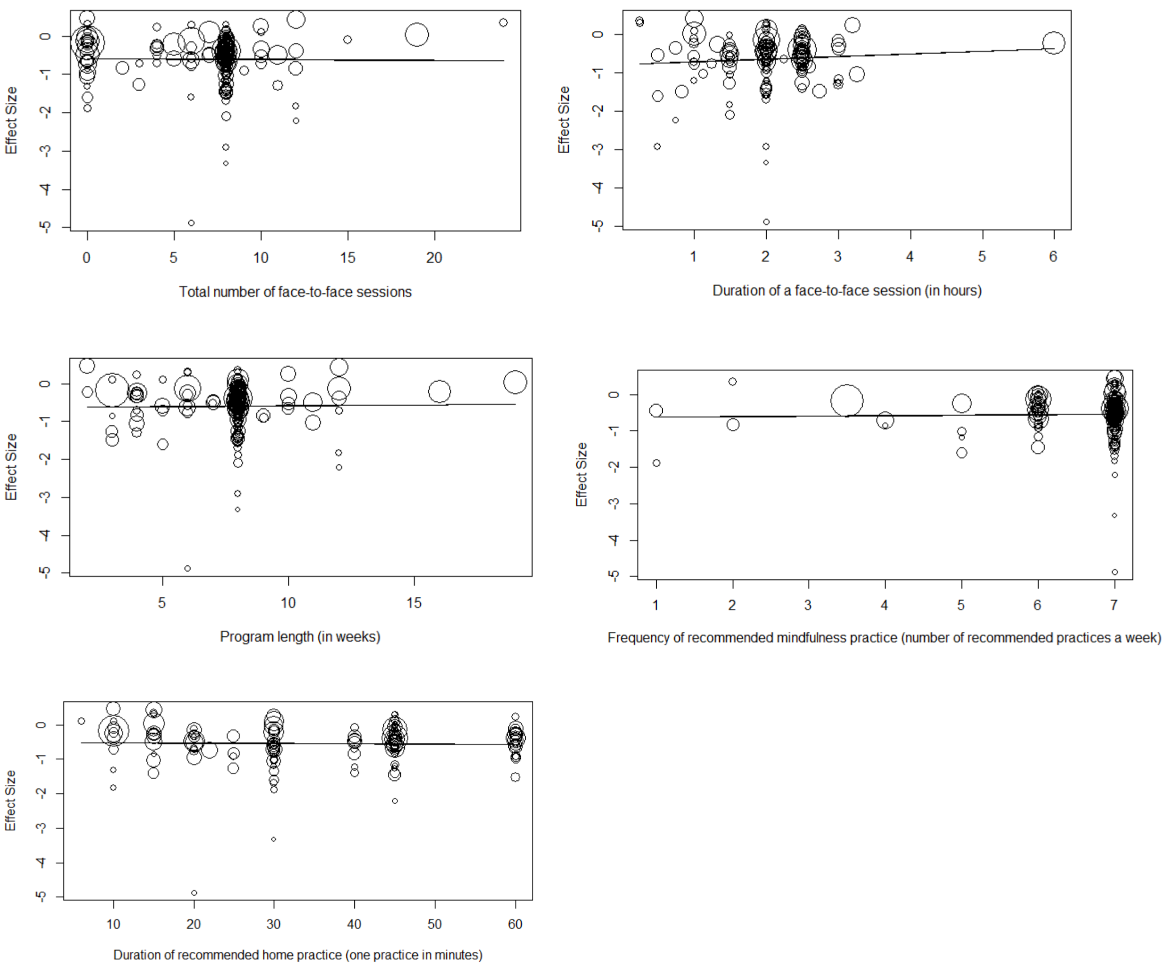
d=effect size of the standardized regression coefficient, SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value= test statistic of slope, *p*-value= significance level; *k*=number of studies; *F*-distribution= test for the overall model; R^2= percentage of heterogeneity accounted for, tau^2/τ^2= variance of the underlying true effect sizes; *SE* tau^2= standard error of tau^2; Q_E= between-study heterogeneity; $p(Q_E)$)=Q_E significance level.

Figure 4.1 shows meta-regression plots for each dose-response meta-regression model listed in Table 4.1 with the sample sizes for each study illustrated by the size of the circle.

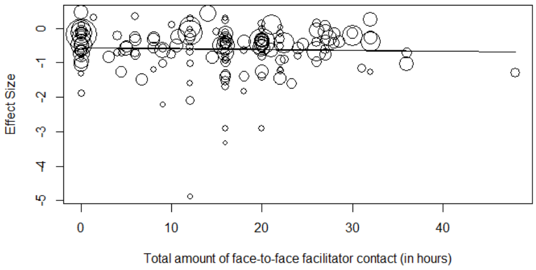
Figure 4.1

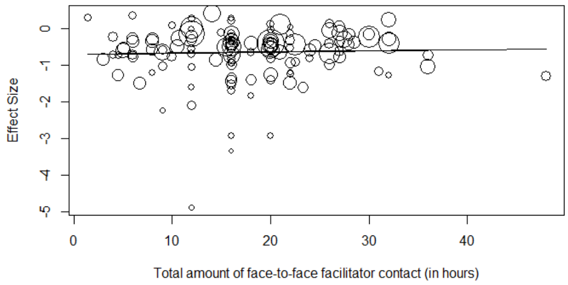
Meta-regression plots for depression at post-program
(compared to inactive and active controls for the doses total number of face-to-face sessions, duration of a face-to-face session (in hours), program length (in weeks), frequency (number) of recommended home practices a week, duration of recommended home practice (one practice in minutes), total amount of face-to-face facilitator contact (in hours), total amount of face-to-face facilitator contact with zero hours of contact excluded (in hours), total recommended use of the program (in hours), total actual use of the program (in hours), program intensity (number of sessions a week excluding all-day retreats), program intensity (number of sessions a week including all-day retreats), amount of face-to-face facilitator contact a week (in hours), amount of face-to-face facilitator contact excluding zero contact a week (in hours), recommended use of the program a week (in hours) and actual use of the program a week (in hours)).

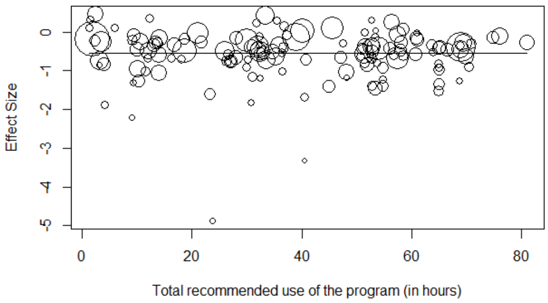
Compared to inactive controls – primary dose variables

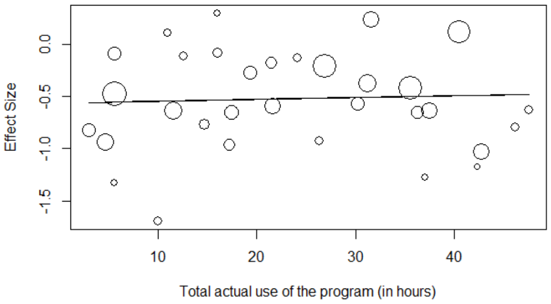


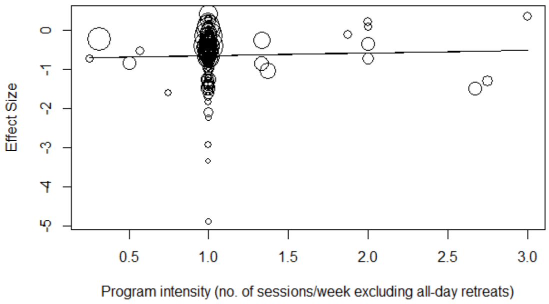
Compared to inactive controls – composite dose variables

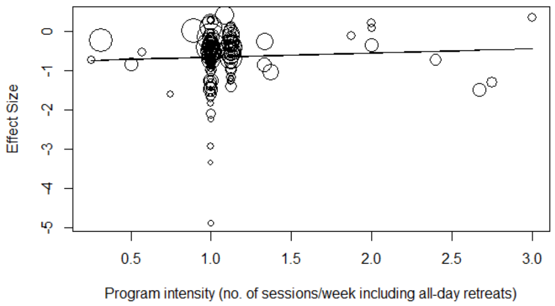


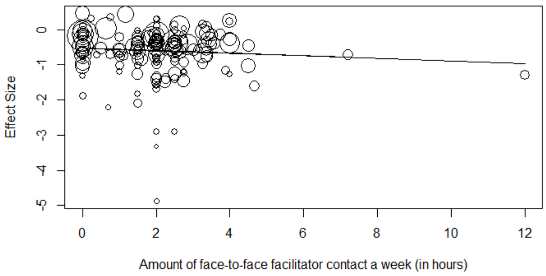


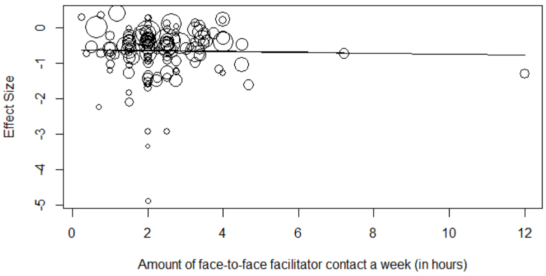


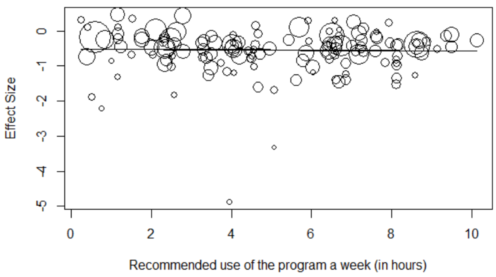


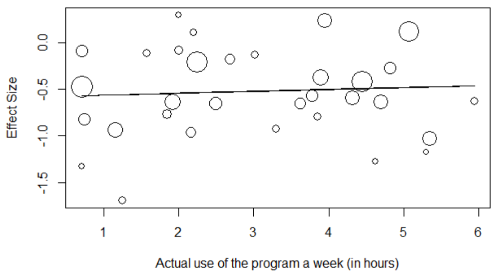




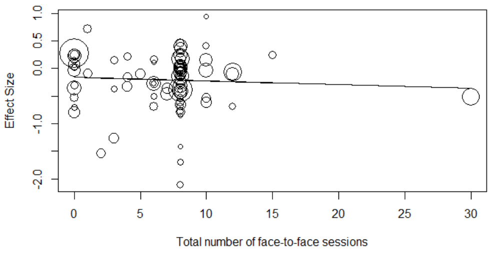


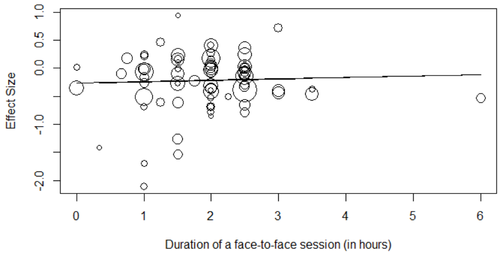


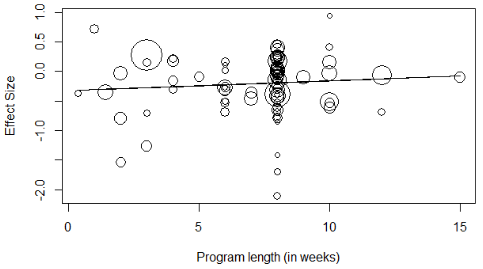


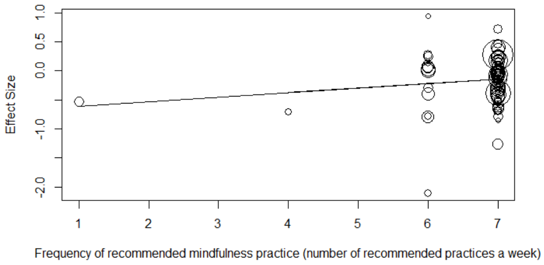


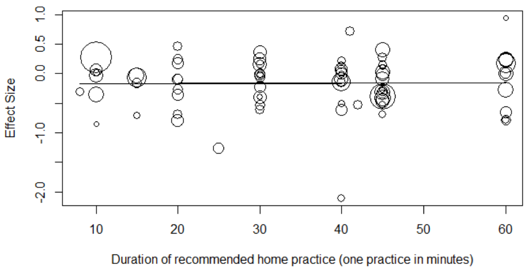
Compared to active controls – primary dose variables



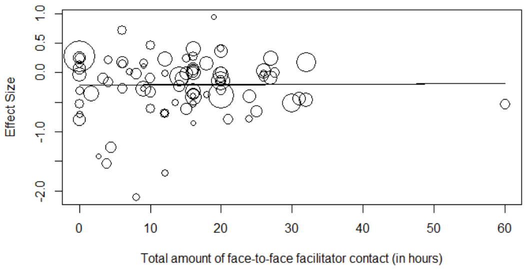


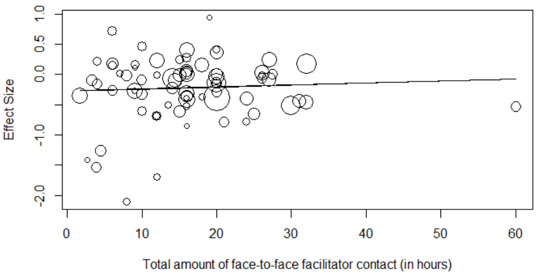


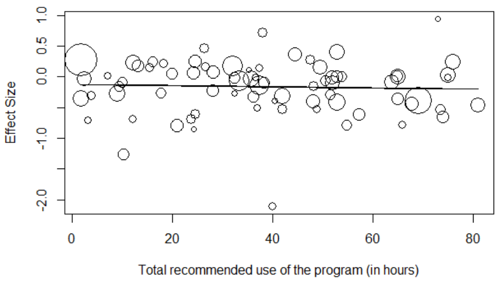


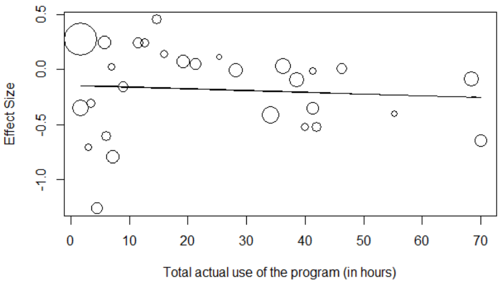


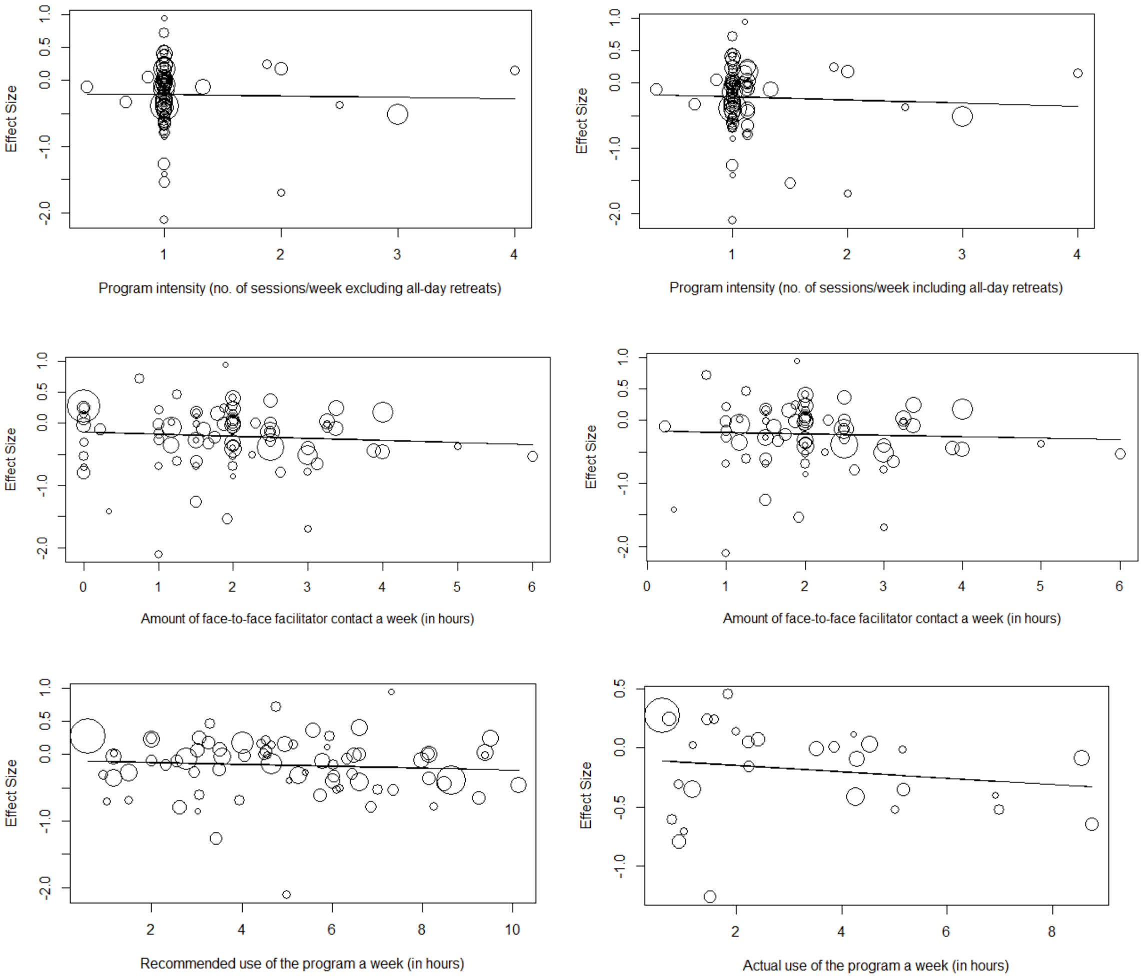
Compared to active controls – composite dose variables











4.2.1.2 Depression at Follow-Up

At 1-4 months follow-up, a significant dose-response relationship between the dose duration of a recommended home practice and depression compared to inactive controls was observed indicating that being asked to practice mindfulness for longer predicted *increased* depression with a small effect size and significant between-study heterogeneity ($p(Q_E)<0.001$). This finding is in the opposite direction to that hypothesized since this dose was associated with increased, as opposed to decreased, depression and Hypothesis 2 is therefore not confirmed. The remaining dose variables including all dose variables compared to active controls did not significantly predict between-group depression effect sizes at 1-4 months follow-up**.** Table 4.2 displays all depression results at 1-4 months follow-up. For the actual use of the program doses predicting outcomes compared to inactive controls, not enough studies (*k*<10) were included to run the analysis. Figure 4.2 shows the meta-regression plot for duration of recommended home practice and depression at 1-4 months follow up.

Meta-regression plots for remaining doses for depression at 1-4 months follow-up are in Appendix Figure 4.2.1.1.

Table 4.2

Meta-regression analysis results by MBP dose for between-group depression effect sizes at 1-4 months follow-up compared to inactive and active controls

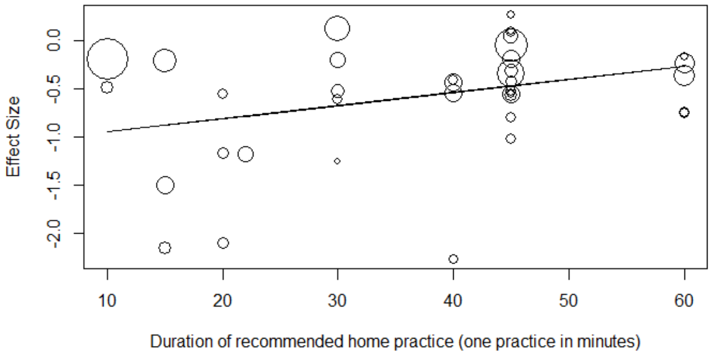
Compared to inactive control groups												
Dose	**Meta-regression model**						**Heterogeneity statistics**					
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total no. face-to-face sessions	-0.06	0.05	[-0.17, 0.05]	-1.09	.28	45	1.18	0.72%	0.75	0.18	320.64	<0.001
Duration of a face-to-face session	0.36	0.29	[-0.23, 0.94]	1.23	.23	40	1.51	0.00%	0.86	0.22	300.23	<0.001
Program length	-0.08	0.09	[-0.25, 0.09]	-0.93	.36	45	0.87	0.00%	0.76	0.19	331.86	<0.001
Frequency of recommended practice	-0.21	0.12	[-0.46, 0.05]	-1.67	.11	38	2.77	7.01%	0.25	0.08	159.79	<0.001
Duration of a recommended practice	**0.01**	**0.01**	**[0.01, 0.03]**	**2.17**	**.04**	**38**	**4.71**	**10.49%**	**0.24**	**0.08**	**175.72**	**<0.001**
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total amount of contact	-0.01	0.02	[-0.04, 0.02]	-0.66	.52	45	0.43	0.00%	0.77	0.19	319.93	<0.001
Total amount of contact (excl. 0 hours)	-0.01	0.02	[-0.04, 0.04]	-0.03	.98	40	0.01	0.00%	0.88	0.23	296.37	<0.001
Total recommended use of program	-0.01	0.01	[-0.01, 0.01]	-0.17	.87	38	0.03	0.00%	0.28	0.09	174.58	<0.001
Total actual use of program*	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity excl. retreats	0.11	0.39	[-0.68, 0.9]	0.28	.78	40	0.08	0.00%	0.88	0.23	299.91	<0.001
Program intensity incl. retreats	0.18	0.39	[-0.63, 0.98]	0.44	.66	40	0.19	0.00%	0.87	0.23	300.93	<0.001
Amount of contact/week	-0.03	0.08	[-0.18, 0.13]	-0.33	.74	45	0.12	0.00%	0.78	0.19	327.62	<0.001
Amount of contact (excl. 0 hours)/week	0.02	0.09	[-0.17, 0.2]	0.16	.87	40	0.03	0.00%	0.88	0.23	298.79	<0.001
Recommended use of program/week	0.01	0.04	[-0.08, 0.09]	0.18	.86	38	0.03	0.00%	0.28	0.09	177.22	<0.001
Actual use of program/week*	-	-	-	-	-	-	-	-	-	-	-	-

Compared to active control groups												
Dose	**Meta-regression model**						**Heterogeneity statistics**					
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total no. face-to-face sessions	0.02	0.02	[-0.03, 0.07]	0.78	.44	30	0.61	3.87%	0.12	0.05	91.01	<0.001
Duration of a face-to-face session	0.04	0.08	[-0.11, 0.19]	0.57	.58	27	0.32	0.00%	0.03	0.03	42.4	0.016
Program length	0.02	0.03	[-0.05, 0.09]	0.52	.61	30	0.27	0.34%	0.12	0.05	98.03	<0.001
Frequency of recommended practice	-0.01	0.06	[-0.13, 0.12]	-0.08	.94	26	0.01	0.00%	0.11	0.05	123.17	<0.001
Duration of a recommended practice	0.01	0.01	[-0.02, 0.02]	1.57	.13	26	2.48	11.67%	0.09	0.05	75.3	<0.001
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total amount of contact	0.01	0.01	[-0.01, 0.02]	0.92	.36	30	0.85	2.95%	0.12	0.05	105.96	<0.001
Total amount of contact (excl. 0 hours)	0.01	0.01	[-0.01, 0.02]	0.14	.89	27	0.02	0.00%	0.03	0.03	42.69	0.015
Total recommended use of program	0.01	0.03	[-0.01, 0.01]	1.65	.11	26	2.71	11.76%	0.09	0.05	79.49	<0.001
Total actual use of program	0.01	0.01	[-0.01, 0.02]	1.32	.22	12	1.74	10.3%	0.15	0.09	48.9	<0.001
Program intensity excl. retreats	-0.16	0.12	[-0.41, 0.09]	-1.27	.22	27	1.61	1.75%	0.02	0.03	40.45	0.026
Program intensity incl. retreats	-0.15	0.13	[-0.4, 0.11]	-1.17	.25	27	1.37	0.00%	0.03	0.03	40.87	0.024
Amount of contact/week	0.01	0.06	[-0.12, 0.14]	0.19	.85	30	0.04	0.00%	0.13	0.06	108.78	<0.001
Amount of contact (excl. 0 hours)/week	-0.06	0.06	[-0.19, 0.07]	-0.89	.38	27	0.79	0.00%	0.03	0.03	41.69	0.019
Recommended use of program/week	0.05	0.03	[-0.01, 0.1]	1.81	.08	26	3.28	14.02%	0.09	0.05	76.53	<0.001
Actual use of program/week	0.05	0.05	[-0.05, 0.15]	1.09	.2	12	1.176	4.49%	0.16	0.1	54.63	<0.001

significant results in bold, **k*<10*; d*=effect size of the standardized regression coefficient, SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value= test statistic of slope, *p*-value= significance level; *k*=number of studies; *F*-distribution= test for the overall model; R^2= percentage of heterogeneity accounted for, tau^2/τ^2= variance of the underlying true effect sizes; *SE* tau^2= standard error of tau^2; Q_E= between-study heterogeneity; $p(Q_E)$=Q_E significance level.

Figure 4.2

Meta-regression plot for duration of a recommended home practice (one practice in minutes) predicting depression at 1-4 months follow-up compared to inactive controls



To test the robustness and possibility of a Type I error of this significant dose-response relationship, Holm-Bonferroni and False Discovery Rate corrections were employed. This finding was no longer significant when applying the Holm-Bonferroni correction since the significant *p*-value (*p*=.037) was greater than the corrected *p*-value when applying the Holm-Bonferroni sequential rejective test procedure (HB: .05/(13-1+1)=.004; .037>.004). Similarly, this finding did not remain significant when applying the FDR correction using the Benjamini-Hochberg procedure since p_{adj}>.05. For the step-by-step Benjamini-Hochberg FDR procedure, see Appendix Table 4.2.1.2.

When controlling for baseline levels of depression to determine whether baseline scores confounded the dose-response relationship between duration of a recommended practice and depression, this result was no longer significant (see Tables 4.3 and 4.4 for results and heterogeneity statistics).

Table 4.3

Meta-regression model for duration of a recommended practice dose controlling for baseline depression at 1-4 months follow-up compared to inactive controls

	d	**SE**	**95% CI**	***t***	***p***
Intercept	-1.02	0.3	[-1.62, -0.41]	-3.42	.002
Duration rec.	0.01	0.01	[-0.001, 0.03]	1.96	.06
Bl depr.	-0.06	0.12	[-0.29, 0.17]	-.53	.6

Duration rec.=duration of recommended practice; bl depr.=baseline depression; *d*=effect size; SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value=test statistic of slope, *p*-value=significance level.

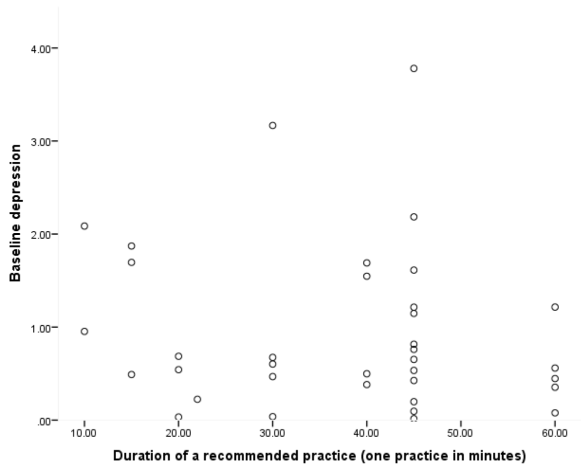
Table 4.4
Meta-regression model fit when controlling for baseline depression for duration of a recommended practice dose at 1-4 months follow-up compared to inactive controls

Test of the interaction:
Simultaneous test that all coefficients (excluding intercept) are zero
F=2.2; df =2, 33; p=.13; R^2=7.34%
Goodness of fit: Test that unexplained variance is zero
Tau^2=0.26; SE=0.09; Q_E=163.47, df=33, p<.0001

F=Test of moderators; df=Degrees of Freedom; R^2=Amount of heterogeneity accounted for; Tau^2=Estimated amount of unexplained/residual heterogeneity; Q_E=Test for residual heterogeneity.

One possible explanation for this non-significant result could be an association between level of depression at baseline and duration of a recommended practice asked for in the program. However, there was no significant correlation between baseline depression and duration of a recommended practice (r=-.13; p=.45). After examination of the scatterplot (Figure 4.3), in most studies, participants were asked to practice 45 minutes a day with varying levels of baseline depression. The lack of a substantial range for this dose could have resulted in the non-significant correlation. Additionally, the decrease in statistical power resulting from a smaller sample and an additional covariate having been added to the model could have led to a possible Type II error.

Figure 4.3
Scatterplot baseline depression and duration of a recommended practice



Therefore, this result does not appear particularly robust and needs to be interpreted with caution.

Next, to assess whether type of MBP or study quality moderated the significant dose-response relationship between the duration of a recommended practice dose and depression, program type and study quality were added as moderators to the meta-regression model compared to inactive controls at 1-4 months follow-up. No significant interaction between duration of a recommended practice and program type (MBSR/MBCT versus other MBPs) was observed (see Table 4.5 for the interaction model and Table 4.6 for test of the model interaction) and Hypothesis 4 therefore needs to be rejected. Similar to controlling for baseline depression, a reason for no interaction effect found for MBP type and duration of a recommended practice could be the lack of substantial variability in scores on this dose particularly for MBSR and MBCT programs.

Table 4.5

Meta-regression interaction model between duration of a recommended practice and type of program (MBSR/MBCT versus other types) for depression at 1-4 months follow-up compared to inactive controls

	d	**SE**	**95% CI**	***t***	***p***
Intercept	-1	.41	[-1.84, -.16]	-2.41	.02
Type	-0.16	.54	[-1.25, 0.93]	-0.29	.77
Duration rec.	0.01	0.01	[-0.01, 0.03]	1.2	.24
Type x duration rec.	0.004	.01	[-0.02. 0.03]	0.28	.78

Duration rec.=duration of recommended practice; *d*=effect size; SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value=test statistic of slope, *p*-value=significance level.

Table 4.6

Meta-regression model fit for the interaction of a duration of recommended practice and type of program (MBSR/MBCT versus other types) for depression at 1-4 months follow-up compared to inactive controls

Test of the interaction:

Simultaneous test that all coefficients (excluding intercept) are zero

F=1.53; df =3, 34; p=.23; R^2=3.4%

Goodness of fit: Test that unexplained variance is zero

Tau^2=0.26; SE=0.08; Q_E=171.61, df=35, p<.0001

F=Test of moderators; df=Degrees of Freedom; R^2=Amount of heterogeneity accounted for; Tau^2=Estimated amount of unexplained/residual heterogeneity; Q_E=Test for residual heterogeneity.

When including study quality as a moderator in the model, again no significant interaction between duration of a recommended practice and study quality (as assessed with scores on the Cochrane Risk of Bias tool) was observed for depression (see Table 4.7 for the interaction model and Table 4.8 for the test of the model interaction). Hypothesis 5 is thus not confirmed.

Table 4.7
Meta-regression interaction model between duration of a recommended practice dose and study quality for depression at 1-4 months follow-up compared to inactive controls

	d	**SE**	**95% CI**	***t***	***p***
Intercept	-0.11	1.8	[-3.77, 3.55]	-0.06	.95
RoB	-0.08	0.15	[-0.38, 0.22]	-0.55	.59
Duration rec.	-0.004	0.045	[-0.09, 0.09]	-0.09	.93
RoB x duration rec.	0.0015	0.004	[-0.01, 0.01]	0.4	.69

Duration rec.=duration of recommended practice; RoB=Risk of Bias score; *d*=effect size; SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value=test statistic of slope, *p*-value=significance level.

Table 4.8
Meta-regression model fit for the interaction of duration of a recommended practice and study quality for depression at 1-4 months follow-up compared to inactive controls

Test of the interaction:

Simultaneous test that all coefficients (excluding intercept) are zero

F=1.66; df = 3, 34; p=.19; R^2=5.26%

Goodness of fit: Test that unexplained variance is zero

Tau^2=0.25; SE=0.08; Q_E=164.72, df=34, p<.0001

F=Test of moderators; df=Degrees of Freedom; R^2=Amount of heterogeneity accounted for; Tau^2=Estimated amount of unexplained/residual heterogeneity; Q_E=Test for residual heterogeneity.

Numerical and visual results of non-significant dose-response relationships for the depression outcome at 5-10 months follow-up compared to inactive and active controls are listed in Appendix Table 4.2.1.3 and Appendix Figure 4.2.1.4, respectively.

4.2.1.3 Results for Depression and General Populations

As a reliability check to ascertain if the general lack of evidence for robust dose-response relationships for the depression outcome was due to different populations being included, the analyses with both primary and composite dose variables were repeated with only the k=27 studies that had included individuals with depression as their participants. Second, the dose-response meta-regression analyses were also repeated with only the k=50 studies with participants from the general population. Both were compared with inactive controls (the primary control comparison) at post-program (the primary time-point) for the primary outcome depression. No significant dose-response relationships were observed for neither the depression population nor the general population with significant between-study heterogeneity observed for all dose variables ($p(Q_E)$<.001). These results mirror the analysis of the overall sample. Tables 4.9 and 4.10 display results of meta-regression results and Appendix Figures 4.2.1.5 and 4.2.1.6 show meta-regression plots for depression and general populations, respectively.

Table 4.9

Meta-regression analysis results by MBP dose for between-group depression effect sizes at immediately post program for the depression population only compared to inactive controls

Dose	Meta-regression model						Heterogeneity statistics					
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	*SE* tau^2	Q_E	$p\ (Q_E)$
Total no. face-to-face sessions	-0.001	0.05	[-0.1, 0.1]	-0.01	.99	27	<.0001	0.00%	0.1	0.04	63.78	<.001
Duration of a face-to-face session	-0.1	0.14	[-0.38, 0.19]	-0.7	.49	26	0.49	0.00%	0.09	0.05	63.38	<.001
Program length	0.05	0.05	[-0.05, 0.14]	1.05	.3	27	1.1	10.14%	0.07	0.04	59.58	<.001
Frequency of recommended practice	0.01	0.1	[-0.2, 0.22]	0.1	.92	25	0.01	0.00%	0.07	0.04	53.95	<.001
Duration of a recommended practice	0.01	0.01	[-0.003, 0.02]	1.58	.13	25	2.5	3.71%	0.06	0.04	50.99	<.001
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	*SE* tau^2	Q_E	$p\ (Q_E)$
Total amount of contact	-0.01	0.01	[-0.03, 0.01]	-1.17	.25	27	1.37	0.23%	0.08	0.04	62.27	<.001
Total amount of contact (excl. 0 hours)	-0.01	0.01	[-0.03, 0.01]	-1	.33	26	1	0.00%	0.09	0.04	62.26	<.001
Total recommended use of program	0.002	0.004	[-0.01, 0.01]	0.51	.61	25	0.26	0.00%	0.07	0.04	53.03	<.001
Total actual use of program*	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity excl. retreats**	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity incl. retreats	-0.32	0.25	[-0.84, 0.21]	-1.25	.22	26	1.57	6.62%	0.08	0.04	59.25	<.001
Amount of contact/week	-0.06	0.04	[--0.14, 0.02]	-1.53	.14	27	2.34	11.62%	0.07	0.04	59.2	<.001
Amount of contact (excl. 0 hours)/week	-0.06	0.04	[-0.14, 0.03]	-1.39	.18	26	1.93	8.64%	0.08	0.04	59.2	<.001
Recommended use of program/week	0.01	0.04	[-0.06, 0.09]	0.37	.71	25	0.14	0.00%	0.07	0.04	53.77	<.001
Actual use of program/week*	-	-	-	-	-	-	-	-	-	-	-	-

*k<10; **all but 2 studies had the same score on this dose; d=effect size of the standardized regression coefficient, SE=standard error of the effect size, 95% CI= confidence intervals; t-value= test statistic of slope, p-value= significance level; k=number of studies; F-distribution= test for the overall model; R^2= percentage of heterogeneity accounted for, tau^2/τ^2= variance of the underlying true effect sizes; *SE* tau^2= standard error of tau^2; Q_E= between-study heterogeneity; $p(Q_E)$)=Q_E significance level.

Table 4.10
Meta-regression analysis results by MBP dose for between-group depression effect sizes at immediately post program for the general population only compared to inactive controls

Dose	Meta-regression model						Heterogeneity statistics					
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total no. face-to-face sessions	0.001	0.02	[-0.3, 0.4]	0.07	.94	50	0.01	0.00%	0.22	0.06	233.4	<.001
Duration of a face-to-face session	-0.004	0.1	[-0.22, 0.21]	-0.04	.97	34	0.001	0.00%	0.29	0.09	175.7	<.001
Program length	0.01	0.02	[-0.04, 0.06]	0.44	.66	50	0.19	0.00%	0.22	0.06	232.82	<.001
Frequency of recommended practice	0.02	0.05	[-0.08, 0.13]	0.45	.65	46	0.2	0.00%	0.16	0.05	181.87	<.001
Duration of a recommended practice	-0.003	0.01	[-0.01, 0.01]	-0.66	.52	46	0.43	0.00%	0.16	0.05	176.93	<.001
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total amount of contact	-0.001	0.01	[-0.02, 0.01]	-0.21	.83	50	0.05	0.00%	0.22	0.06	237.48	<.001
Total amount of contact (excl. 0 hours)	0.03	0.01	[-0.02, 0.03]	0.24	.81	34	0.06	0.00%	0.29	0.09	175.46	<.001
Total recommended use of program	-0.001	0.003	[-0.01, 0.01]	-0.4	.69	46	0.16	0.00%	0.16	0.05	181.89	<.001
Total actual use of program	0.01	0.01	[-0.01, 0.03]	1.41	.18	17	1.99	9.11%	0.13	0.07	54.66	<.001
Program intensity excl. retreats	-0.16	0.23	[-0.63, 0.3]	-0.72	.48	34	0.51	0.00%	0.28	0.09	165.35	<.001
Program intensity incl. retreats	-0.15	0.22	[-0.59, 0.29]	-0.71	.49	34	0.5	0.00%	0.28	0.09	164.83	<.001
Amount of contact/week	-0.03	0.05	[-0.13, 0.07]	-0.57	.57	50	0.33	0.00%	0.22	0.06	235.57	<.001
Amount of contact (excl. 0 hours)/week	-0.02	0.08	[-0.17, 0.14]	-0.2	.84	34	0.04	0.00%	0.28	0.09	172.2	<.001
Recommended use of program/week	-0.02	0.03	[-0.08, 0.04]	-0.75	.46	46	0.56	0.00%	0.16	0.05	179.02	<.001
Actual use of program/week	0.09	0.07	[-0.07, 0.24]	1.23	.24	17	1.52	3.63%	0.14	0.07	58.76	<.001

d=effect size of the standardized regression coefficient, SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value= test statistic of slope, *p*-value= significance level; *k*=number of studies; *F*-distribution= test for the overall model; R^2= percentage of heterogeneity accounted for, tau^2/τ^2= variance of the underlying true effect sizes; *SE* tau^2= standard error of tau^2; Q_E= between-study heterogeneity; $p(Q_E)$)=Q_E significance level.

4.2.1.4 Clinical Significance and Severity Analysis: Depression

As a further reliability check, an exploratory clinical significance analysis was completed for the group with a diagnosis of depression and the most commonly employed measure of depression, namely the Beck's Depression Inventory (BDI) following steps outlined by Jacobson et al. (2014) and Khoury et al. (2013) (see Chapter 2). To recap, included studies were allocated either a low-dose or high-dose group for each of the dose variables separately (low dose and high dose was determined as below or above each median dose value, respectively). Weighted means were then calculated for all timepoints (baseline, post-program, follow-up) for each group (low-dose and high-dose) for each dose variable. At follow-up, only very small samples of studies were included (k<3) and this finding therefore needs to be interpreted with caution. Results showed that although levels of depression generally reduced from baseline to post-treatment (from moderate to mild and mild to minimal) for both, high and low doses, this did not follow a particular pattern across different doses since the degree of change was similar for high and low doses (see Appendix Table 4.2.1.7). This finding is not surprising since no significant robust dose-response relationships were found for the depression outcome. Please note, this clinical significance analysis was exploratory in nature only due to the absence of robust significant findings (see discussion below).

Furthermore, to determine whether participating in an MBP made a larger or smaller difference for individuals with varying severities of depression, baseline depression scores were divided into two groups of severe and mild depression calculated as above or below the median level. Dose-response meta-regression analyses were then completed for each dose (where $k \geq 10$) with depression severity added as a moderator to the model. No significant interaction between depression severity (mild vs. severe) and any of the dose variables was observed (see Appendix Table 4.2.1.8).

4.2.2 Dose-Response Meta-Regression Results for Secondary Outcome Anxiety

4.2.2.1 Anxiety at Post-Program

For the anxiety outcome at immediately post-program, similar to depression, neither primary nor composite dose variables significantly predicted effect sizes for inactive or active controls (see Table 4.11 for results). Hypothesis 2 is again rejected. Significant between-study heterogeneity

($p(Q_E)$<.05) was observed for the majority of meta-regression models with the exception of the actual use of program doses for both comparison groups. Meta-regression plots for anxiety at post-program are listed in Appendix Figure 4.2.2.1.

Table 4.11
Meta-regression analysis results by MBP dose for between-group anxiety effect sizes at immediately post-program compared to inactive and active controls

Compared to inactive control groups												
Dose	**Meta-regression model**						**Heterogeneity statistics**					
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total no. face-to-face sessions	-0.001	0.01	[-0.03, 0.03]	-0.07	.93	100	0.01	0.00%	0.22	0.04	393.28	<.001
Duration of a face-to-face session	0.06	0.08	[-0.11, 0.22]	0.69	.49	83	0.49	0.00%	0.22	0.06	306.44	<.001
Program length	0.004	0.02	[-0.04, 0.05]	0.19	.85	100	0.03	0.00%	0.22	0.04	389.12	<.001
Frequency of recommended practice	0.04	0.05	[-0.056, 0.13]	0.81	.42	91	0.65	0.00%	0.18	0.04	320.22	<.001
Duration of a recommended practice	-0.001	0.004	[-0.01, 0.01]	-0.18	.86	91	0.03	0.00%	0.18	0.03	321.78	<.001
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total amount of contact	-0.01	0.01	[-0.02, 0.01]	-0.93	.36	100	0.86	0.00%	0.22	0.04	388.12	<.001
Total amount of contact (excl. 0 hours)	<0.001	0.01	[-0.02, 0.02]	0.05	.96	83	0.002	0.00%	0.23	0.05	309.9	<.001
Total recommended use of program	-0.001	0.003	[-0.01, 0.01]	-0.38	.71	91	0.14	0.00%	0.18	0.04	320.44	<.001
Total actual use of program	-0.002	0.004	[-0.01, 0.01]	-0.44	.67	26	0.19	0.00%	0.03	0.03	35.79	.057
Program intensity excl. retreats	0.12	0.18	[-0.24, 0.48]	0.67	.51	83	0.44	0.00%	0.23	0.05	309.37	<.001
Program intensity incl. retreats	0.13	0.17	[-0.22, 0.48]	0.75	.46	83	0.56	0.00%	0.23	0.05	309.32	<.001
Amount of contact/week	-0.06	0.05	[-0.14, 0.03]	-1.22	.23	100	1.49	1.65%	0.21	0.04	381.56	<.001
Amount of contact (excl. 0 hours)/week	-0.02	0.06	[-0.14, 0.1]	-0.37	.71	83	0.14	0.00%	0.22	0.05	307.92	<.001
Recommended use of program/week	-0.02	0.02	[-0.07, 0.03]	-0.69	.49	91	0.49	0.00%	0.18	0.04	317.17	<.001
Actual use of program/week	-0.01	0.04	[-0.08, 0.07]	-0.25	.81	26	0.06	0.00%	0.03	0.03	35.81	.057

Compared to active control groups												
Dose	**Meta-regression model**						**Heterogeneity statistics**					
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	*Tau*^{2}	*SE tau*^{2}	Q_E	*p* (Q_E)
Total no. face-to-face sessions	-0.01	0.02	[-0.05, 0.02]	-0.64	.52	50	0.41	0.00%	0.07	0.03	105.71	<.001
Duration of a face-to-face session	0.04	0.07	[-0.11, 0.19]	0.5	.62	44	0.25	0.00%	0.09	0.03	99.34	<.001
Program length	-0.03	0.03	[-0.08, 0.02]	-1.08	.28	50	1.17	0.00%	0.07	0.03	104.63	<.001
Frequency of recommended practice	0.06	0.06	[-0.05, 0.17]	1.08	.29	48	1.17	2.06%	0.04	0.02	79.21	.002
Duration of a recommended practice	<0.001	0.003	[-0.01, 0.01]	-0.06	.95	48	0.004	0.00%	0.04	0.02	81.28	.001
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	*Tau*^{2}	*SE tau*^{2}	Q_E	*p* (Q_E)
Total amount of contact	-0.001	0.01	[-0.01, 0.01]	-0.24	.81	50	0.06	0.00%	0.07	0.03	105.73	<.001
Total amount of contact (excl. 0 hours)	0.001	0.01	[-0.01, 0.01]	0.15	.88	44	0.02	0.00%	0.09	0.03	99.1	<.001
Total recommended use of program	-0.001	0.002	[-0.01, 0.003]	-0.53	.6	48	0.28	0.00%	0.04	0.02	80.87	.001
Total actual use of program	-0.004	0.003	[-0.01, 0.003]	-1.26	.2	18	1.6	0.00%	0.01	0.02	18.55	.293
Program intensity excl. retreats	-0.13	0.25	[-0.64, 0.38]	-0.52	.6	44	0.27	0.00%	0.09	0.03	99.5	<.001
Program intensity incl. retreats	-0.07	0.25	[-0.58, 0.44]	-0.29	.78	44	0.08	0.00%	0.09	0.03	99.28	<.001
Amount of contact/week	-0.02	0.05	[-0.12, 0.07]	-0.5	.62	50	0.25	0.00%	0.07	0.03	105.76	<.001
Amount of contact (excl. 0 hours)/week	-0.01	0.06	[-0.13, 0.12]	-0.1	.92	44	0.01	0.00%	0.09	0.03	99.27	<.001
Recommended use of program/week	-0.01	0.02	[-0.05, 0.04]	-0.25	.8	48	0.06	0.00%	0.04	0.02	81.14	.001
Actual use of program/week	-0.03	0.02	[-0.09, 0.02]	-1.4	.18	18	1.97	11.26%	0.01	0.02	18.18	.314

d=effect size of the standardized regression coefficient, SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value= test statistic of slope, *p*-value= significance level; *k*=number of studies; *F*-distribution= test for the overall model; R^2= percentage of heterogeneity accounted for, tau^2/τ^2= variance of the underlying true effect sizes; *SE* tau^2= standard error of tau^2; Q_E= between-study heterogeneity; $p(Q_E)$)=Q_E significance level.

4.2.2.2 Anxiety at Follow-Up

At 1-4 months follow-up, a significant dose-response relationship between the dose program intensity (when including all-day retreats) and the anxiety outcome compared with active controls was observed in the hypothesised direction with a large effect size and no significant between-study heterogeneity indicating that more intense MBPs predicted reduced levels of anxiety at follow-up compared to active controls. Hypothesis 2 is therefore confirmed. The remaining dose variables including all doses compared to inactive controls did not significantly predict between-group anxiety effect sizes at 1-4 months follow-up. Table 4.12 shows results for all dose-response meta-regression analyses for anxiety at 1-4 months follow up. For the actual use of program doses predicting outcomes compared inactive controls, not enough studies (k<10) were included to run the analysis. For the intensity (when excluding retreats) and frequency of recommended practice doses compared to active controls, all but two studies had the same score on these doses and meta-regression analyses were therefore not completed. Significant between-study heterogeneity ($p(Q_E)$<.001) was observed for all doses compared to inactive controls and for all doses compared to active controls except for duration of a recommended practice and recommended and actual program use (both in total and a week). Figure 4.4 shows the meta-regression plot for program intensity (including retreats) and anxiety at 1-4 months follow-up compared to active controls. Meta-regression plots for remaining doses for anxiety at 1-4 months follow-up are in Appendix Figure 4.2.2.2.

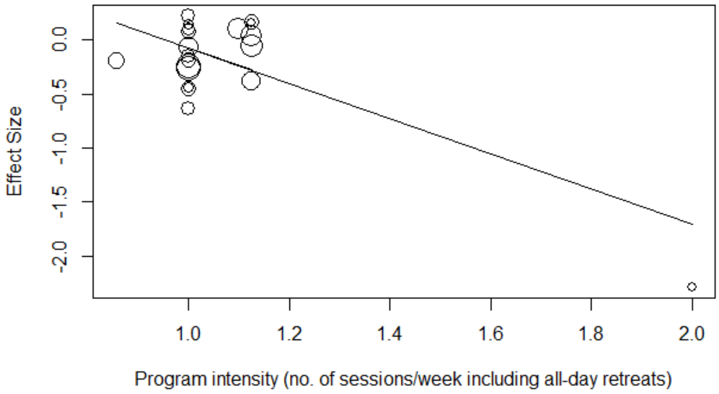
Table 4.12
Meta-regression analysis results by MBP dose for between-group anxiety effect sizes at 1-4 months follow-up compared to inactive and active controls

Compared to inactive control groups												
Dose	**Meta-regression model**						**Heterogeneity statistics**					
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total no. face-to-face sessions	-0.07	0.07	[-021, 0.08]	-0.96	.35	29	0.91	0.00%	0.65	0.2	190.57	<.001
Duration of a face-to-face session	-0.24	0.31	[-0.88, 042]	-0.75	.46	26	0.56	0.00%	0.73	0.24	175.16	<.001
Program length	-0.16	0.1	[-0.36, 0.04]	-1.62	.12	29	2.63	8.27%	0.59	0.19	172.35	<.001
Frequency of recommended practice	-0.27	0.27	[-0.83, 0.28]	-1.03	.32	26	1.05	2.77%	0.34	0.12	98.84	<.001
Duration of a recommended practice	0.02	0.01	[-0.002, 0.04]	1.83	.08	26	3.37	18.05%	0.28	0.11	99.33	<.001
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total amount of contact	-0.01	0.02	[-0.05, 0.03]	-0.64	.53	29	0.41	0.00%	0.66	0.21	183.75	<.001
Total amount of contact (excl. 0 hours)	-0.004	0.02	[-0.05, 0.05]	-0.17	.87	26	0.03	0.00%	0.75	0.25	178.18	<.001
Total recommended use of program	-0.002	0.01	[-0.02, 0.02]	-0.18	.86	26	0.03	0.00%	0.37	0.13	117.18	<.001
Total actual use of program*	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity excl. retreats	0.78	0.46	[-0.17, 1.73]	1.69	.1	26	2.86	9.97%	0.64	0.22	163.82	<.001
Program intensity incl. retreats	0.73	0.42	[-0.13, 1.6]	1.74	.09	26	3.04	10.48%	0.64	0.22	165.2	<.001
Amount of contact/week	-0.02	0.12	[-0.26, 0.23]	-0.13	.9	29	0.02	0.00%	0.68	0.21	197.71	<.001
Amount of contact (excl. 0 hours)/week	0.05	0.15	[-0.254, 0.36]	0.35	.73	26	0.12	0.00%	0.75	0.25	186.99	<.001
Recommended use of program/week	0.001	0.08	[-0.17, 0.17]	0.01	.99	26	<0.001	0.00%	0.37	0.14	119.53	<.001
Actual use of program/week*	-	-	-	-	-	-	-	-	-	-	-	-

Compared to active control groups												
Dose	**Meta-regression model**						**Heterogeneity statistics**					
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total no. face-to-face sessions	-0.02	0.03	[-0.08, 0.04]	-0.68	.51	22	0.46	0.00%	0.03	0.03	38.43	.008
Duration of a face-to-face session	0.07	0.09	[-0.13, 0.27]	0.72	.48	20	0.52	0.00%	0.05	0.04	38.42	.003
Program length	-0.03	0.04	[-0.12, 0.06]	-0.74	.47	22	0.54	0.00%	0.03	0.03	38.32	.008
Freq. of recommended practice*****	-	-	-	-	-	-	-	-	-	-	-	-
Duration of a recommended practice	-0.002	0.003	[-0.01, 0.01]	-0.46	.65	21	0.21	NA%	0	0.02	13.93	.788
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total amount of contact	0.002	0.01	[-0.01, 0.02]	0.24	.81	22	0.06	0.00%	0.03	0.03	39.05	.007
Total amount of contact (excl. 0 hours)	0.002	0.01	[-0.02, 0.02]	0.25	.81	20	0.06	0.00%	0.05	0.04	38.92	.003
Total recommended use of program	<0.001	0.002	[-0.004, 0.01]	0.12	.91	21	0.01	NA%	0	0.02	14.08	.779
Total actual use of program	0.003	0.003	[-0.01, 0.01]	0.83	.43	10	0.69	NA%	0	0.03	6.28	.616
Program intensity excl. retreats****	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity incl. retreats	**-1.63**	**0.43**	**[-2.53, 0.72]**	**-3.77**	**.001**	**20**	**14.2**	**41.84%**	**0.02**	**0.03**	**24.55**	**.138**
Amount of contact/week	-0.02	0.07	[-0.16, 0.12]	-0.3	.77	22	0.09	0.00%	0.03	0.03	39.05	.007
Amount of contact (excl. 0 hours)/week	-0.03	0.09	[-0.21, 0.15]	-0.39	.7	20	0.15	0.00%	0.05	0.04	38.83	.003
Recommended use of program/week	-0.002	0.02	[-0.04, 0.04]	-0.12	.9	21	0.02	NA%	0	0.02	14.08	.779
Actual use of program/week	0.02	0.03	[-0.04, 0.08]	0.82	.44	10	0.68	NA%	0	0.03	6.29	.615

significant results in bold*; *k*<10*;* , **no included studies had zero hours of contact*;* ***all included studies had the same score on this dose*;* ****all but one had the same score on this dose, *****all but 2 studies had the same score on this dose*; d*=effect size of the standardized regression coefficient, SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value= test statistic of slope, *p*-value= significance level; *k*=number of studies; *F*-distribution= test for the overall model; R^2= percentage of heterogeneity accounted for, tau^2/τ^2= variance of the underlying true effect sizes; *SE* tau^2= standard error of tau^2; Q_E= between-study heterogeneity; $p(Q_E)$)=Q_E significance level.

Figure 4.4
Meta-regression plot for program intensity (including all-day retreats) predicting anxiety at 1-4 months follow-up compared to active controls



The result remained significant when applying the Holm-Bonferroni correction since the significant *p*-value (p=.001) was below the corrected *p*-value (HB: .05/(13-1+1)=.004; .001<.004). Similarly, this finding also remained significant when applying the FDR correction using the Benjamini-Hochberg procedure since p_{adj}<.05. For the step-by-step Benjamini-Hochberg FDR procedure, see Appendix Table 4.2.2.3.

When controlling for baseline anxiety, the dose-response relationship between the program intensity (including retreats) dose and anxiety remained significant in the hypothesized direction with a large effect size and no significant between-study heterogeneity (see Tables 4.13 and 4.14 for results and heterogeneity statistics).

Table 4.13
Meta-regression model for program intensity (including retreats) dose controlling for baseline anxiety at 1-4 months follow-up compared to active controls

	d	**SE**	**95% CI**	***t***	***p***
Intercept	1.71	0.49	[0.66, 2.76]	3.5	.004
Intensity (incl. retreats)	**-1.85**	**0.13**	**[-2.9, -0.79]**	**-3.76**	**.002**
Bl anx.	0.07	0.13	[-0.2, 0.34]	0.53	.61

Duration rec.=duration of recommended practice; bl anx.= baseline anxiety; *d*=effect size; SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value=test statistic of slope, *p*-value=significance level; significant dose-response results in bold.

Table 4.14
Meta-regression model fit when controlling for baseline anxiety for program intensity (including retreats) dose at 1-4 months follow-up compared to active controls

Test of the interaction:
Simultaneous test that all coefficients (excluding intercept) are zero
F=7.71; df=2, 14; p=.01.; R^2=68.73%
Goodness of fit: Test that unexplained variance is zero
Tau^2=0.03; SE=0.04; Q_E=19.98, df=14; p=.13

F=Test of moderators; df=Degrees of Freedom; R^2=Amount of heterogeneity accounted for; Tau^2=Estimated amount of unexplained/residual heterogeneity; Q_E=Test for residual heterogeneity.

To assess whether type of MBP or study quality moderated the significant dose-response relationship between program intensity (when including retreats) and anxiety, program type and study quality were added as moderators to the meta-regression model compared to active controls at 1-4 months follow-up. A significant interaction between program intensity (when including retreats) and program type (MBSR/MBCT versus other MBPs) was observed with no significant between-study heterogeneity (see Table 4.15 for the interaction model and Table 4.16 for test of the model interaction). Separate subgroup analyses with the different MBP types to determine for which program type the intensity (including retreats) dose significantly predicted anxiety were not possible since k<10 studies were available for the group of other MBPs at this timepoint. Hypothesis 4 could therefore neither be confirmed nor rejected.

Table 4.15
Meta-regression interaction model between program intensity (including retreats) and type of program (MBSR/MBCT versus other types) for anxiety at 1-4 months follow-up compared to active controls

	d	SE	95% CI	*t*	*p*
Intercept	-1.03	0.76	[-2.65, 0.58]	-1.35	.19
Type	**2.91**	**0.73**	**[1.03, 4.79]**	**3.28**	**.005**
Intensity (incl. retreats)	0.89	0.73	[-0.66, 2.43]	3.28	.24
Type x intensity (incl. retreats)	**-2.97**	**0.83**	**[-4.73, 1.21]**	**-3.58**	**.003**

d=effect size; SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value=test statistic of slope, *p*-value=significance level; significant results in bold.

Table 4.16
Meta-regression model fit for the interaction of program intensity (including retreats) and type of program (MBSR/MBCT versus other types) for anxiety at 1-4 months follow-up compared to active controls

Test of the interaction:
Simultaneous test that all coefficients (excluding intercept) are zero
F=11.54; df =3, 16; p=.0003; R^2=100%
Goodness of fit: Test that unexplained variance is zero
Tau^2=0; SE=.02; Q_E=12.34, df=16, p=.72

F=Test of moderators; df=Degrees of Freedom; R^2=Amount of heterogeneity accounted for; Tau^2=Estimated amount of unexplained/residual heterogeneity; Q_E=Test for residual heterogeneity.

When including study quality as a moderator in the model, no significant interaction between program intensity (when including retreats) and study quality (as assessed with scores on the Cochrane Risk of Bias tool) was observed for anxiety at 1-4 months follow-up compared to active controls thus rejecting Hypothesis 5. No significant between-study heterogeneity was observed in this model (see Table 4.17 for the interaction model and Table 4.18 for the test of the model interaction).

Table 4.17
Meta-regression interaction model between program intensity (when including retreats) and study quality for anxiety at 1-4 months follow-up compared to active controls

	d	**SE**	**95% CI**	***t***	***p***
Intercept	7.07	4.6	[-2.68, 16.82]	1.54	.14
RoB	-0.65	0.55	[-1.81, 0.52]	-1.17	.26
Intensity (incl. retreats)	-6.66	4.43	[-16.06, 2.74]	-1.5	.15
RoB x intensity (incl. retreats)	0.6	0.54	[-0.54, 1.73]	1.11	.28

RoB=Risk of Bias score; *d*=effect size; SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value=test statistic of slope, *p*-value=significance level.

Table 4.18
Meta-regression model fit for program intensity (when including retreats) and study quality for anxiety at 1-4 months follow-up compared to active controls

Test of the interaction:

Simultaneous test that all coefficients (excluding intercept) are zero

F=1.24; df = 3, 16; p=.28; R^2=31.99%

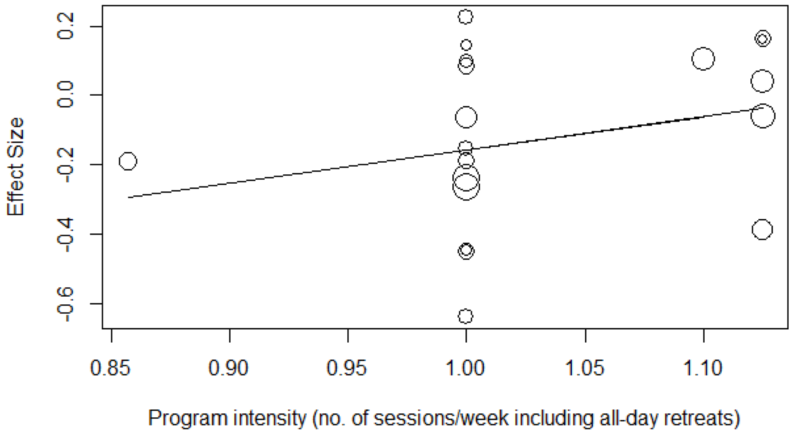
Goodness of fit: Test that unexplained variance is zero

Tau^2=0.02; SE=0.03; Q_E=22.12, df=16, p=.14

F=Test of moderators; df=Degrees of Freedom; R^2=Amount of heterogeneity accounted for; Tau^2=Estimated amount of unexplained/residual heterogeneity; Q_E=Test for residual heterogeneity.

After closer inspection of the meta-regression plot, the significant dose-response relationship between the program intensity (including retreats) dose and anxiety compared to active controls at 1-4 months follow-up appeared due to an extreme score. When repeating the analysis with this outlier removed, this finding was no longer significant (*k*=19; t=1.38; *d*=0.96; 95%C.I.: [-0.5, 2.42]; F(1, 17)=1.91; *p*=.19). Figure 4.5 displays the meta-regression plot with the outlier removed. This result therefore does not appear particularly robust since the significant finding was due to a single study without which the significant effect disappeared.

Figure 4.5
Meta-regression plot for program intensity (incl. all-day retreats) predicting anxiety at 1-4 months follow-up compared to active controls with outlier removed



Numerical and visual results of non-significant dose-response relationships for the anxiety outcome at 5-10 months follow-up compared to inactive controls (*k*<10 compared to active controls) are listed in Appendix Table 4.2.2.4 and Appendix Figure 4.2.2.5, respectively.

4.2.3 Dose-Response Meta-Regression Results for Secondary Outcome Stress

4.2.3.1 Stress at Post-Program

Similar to depression and anxiety outcomes, no significant dose-response relationships were observed for the stress outcome compared to either inactive or active controls at immediately post-program again rejecting Hypothesis 2 for this outcome and timepoint (see Table 4.19 for results). Significant between-study heterogeneity ($p(Q_E)$<.05) was observed again for most meta-regression models except for the actual use of program doses compared to inactive controls. Compared to active controls, not enough studies (*k*<10) were available to complete meta-regression analyses with the actual use of program doses (in total and per week). For the program intensity (excluding retreats) dose, all but one study had the same score and the meta-regression analysis with this dose was therefore not completed. All meta-regression plots for stress at post-program are listed in Appendix Figure 4.2.3.1.

Table 4.19
Meta-regression analysis results by MBP dose for between-group stress effect sizes at immediately post-program compared to inactive and active controls

Compared to inactive control groups												
Dose	**Meta-regression model**						**Heterogeneity statistics**					
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R*^{2}**	***Tau*^{2}**	***SE tau*^{2}**	***Q*$_E$**	***p* (*Q*$_E$)**
Total no. face-to-face sessions	-0.05	0.04	[-0.13, 0.03]	-1.27	.21	51	1.62	1.38%	0.84	0.19	493.07	<.001
Duration of a face-to-face session	0.17	0.2	[-0.23, 0.57]	0.88	.39	38	0.77	0.00%	0.95	0.25	308.7	<.001
Program length	-0.07	0.06	[-0.18, 0.04]	-1.28	.21	51	1.63	1.34%	0.84	0.19	497.16	<.001
Frequency of recommended practice	0.04	0.08	[-0.13, 0.21]	0.48	.64	48	0.29	0.00%	0.88	0.2	480.21	<.001
Duration of a recommended practice	0.01	0.01	[-0.004, 0.03]	1.57	.12	48	2.45	2.71%	0.84	0.2	479.22	<.001
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R*^{2}**	***Tau*^{2}**	***SE tau*^{2}**	***Q*$_E$**	***p* (*Q*$_E$)**
Total amount of contact	0.002	0.01	[-0.03, 0.03]	0.15	.89	51	0.02	0.00%	0.87	0.19	494.61	<.001
Total amount of contact (excl. 0 hours)	0.01	0.02	[-0.03, 0.05]	0.61	.55	38	0.37	0.00%	0.97	0.25	316.52	<.001
Total recommended use of program	0.003	0.01	[-0.01, 0.02]	0.37	.72	48	0.14	0.00%	0.89	0.2	482.42	<.001
Total actual use of program	0.01	0.01	[-0.01, 0.02]	1.23	.24	15	1.52	5.59%	0.01	0.03	14.53	.338
Program intensity excl. retreats	0.41	0.54	[-0.69, 1.51]	0.76	.45	38	0.57	0.00%	0.96	0.25	325.63	<.001
Program intensity incl. retreats	0.33	0.49	[-0.66, 1.32]	0.68	.5	38	0.46	0.00%	0.97	0.25	325.53	<.001
Amount of contact/week	0.05	0.1	[-0.14, 0.24]	0.52	.61	51	0.27	0.00%	0.87	0.19	494.61	<.001
Amount of contact (excl. 0 hours)/week	0.16	0.14	[-0.13, 0.45]	1.13	.27	38	1.28	0.86%	0.94	0.25	316.04	<.001
Recommended use of program/week	0.02	0.06	[-0.1, 0.14]	0.35	.73	48	0.12	0.00%	0.89	0.2	482.55	<.001
Actual use of program/week	0.03	0.05	[-0.08, 0.14]	0.55	.59	15	0.3	0.00%	0.02	0.03	16.05	.247

Compared to active control groups												
Dose	**Meta-regression model**							**Heterogeneity statistics**				
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total no. face-to-face sessions	-0.05	0.05	[-0.15, 0.05]	-1.02	.32	26	1.04	1.84%	0.45	0.16	119.69	<0.001
Duration of a face-to-face session	0.29	0.23	[-0.18, 0.76]	1.27	.22	23	1.62	2.44%	0.53	0.2	118.45	<0.001
Program length	-0.07	0.07	[-0.22, 0.09]	-0.89	.38	26	0.8	0.55%	0.45	0.16	117.5	<0.001
Frequency of recommended practice	-0.02	0.23	[-0.49, 0.46]	-0.07	.94	25	0.01	0.00%	0.41	0.15	125.48	<0.001
Duration of a recommended practice	0.003	0.01	[-0.02, 0.02]	0.3	.76	25	0.092	0.00%	0.41	0.15	120.9	<0.001
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total amount of contact	0.01	0.02	[-0.03, 0.05]	0.49	.63	26	0.24	0.00%	0.48	0.17	134.86	<0.001
Total amount of contact (excl. 0 hours)	0.02	0.02	[-0.03, 0.07]	0.69	.5	23	0.47	0.00%	0.58	0.21	132.81	<0.001
Total recommended use of program	-0.002	0.01	[-0.02, 0.01]	-0.23	.82	25	0.05	0.00%	0.41	0.15	125.13	<0.001
Total actual use of program*	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity excl. retreats**	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity incl. retreats	0.19	0.3	[-0.42, 0.81]	0.65	.52	23	0.43	0.00%	0.57	0.21	131.5	<0.001
Amount of contact/week	0.14	0.15	[-0.17, 0.46]	0.93	.36	26	0.86	0.00%	0.47	0.16	129.54	<0.001
Amount of contact (excl. 0 hours)/week	0.28	0.22	[-0.17, 0.73]	1.31	.21	23	1.73	2.21%	90.54	0.2	121.46	<0.001
Recommended use of program/week	0.03	0.07	[-0.11, 0.16]	0.38	.71	25	0.14	0.00%	0.41	0.15	121.59	<0.001
Actual use of program/week*	-	-	-	-	-	-	-	-	-	-	-	-

*k<10, **all but one had the same score on this dose; *d*=effect size of the standardized regression coefficient, SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value= test statistic of slope, *p*-value= significance level; *k*=number of studies; *F*-distribution= test for the overall model; R^2= percentage of heterogeneity accounted for, tau^2/τ^2= variance of the underlying true effect sizes; *SE* tau^2= standard error of tau^2; Q_E= between-study heterogeneity; $p(Q_E)$)=Q_E significance level.

4.2.3.2 Stress at Follow-Up

At 1-4 months follow-up several significant dose-response relationships between doses relating to MBPs and stress were observed. Table 4.20 displays all dose-response meta-regression results at 1-4 months follow-up where $k \geq 10$. Appendix Figure 4.2.3.2 shows meta-regression plots for all non-significant dose-response relationships for the stress outcome at 1-4 months follow-up where $k \geq 10$. Not enough studies ($k < 10$) were available for the actual use of MBP doses (in total and per week) for both comparison groups; and compared to active controls, all but one study had the same score for program intensity (when excluding retreats). Meta-regression analyses with these doses were therefore not completed. Significant dose-response relationships for stress at 1-4 months follow-up are detailed next.

Table 4.20

Meta-regression analysis results by MBP dose for between-group stress effect sizes at 1-4 months follow-up compared to inactive and active controls

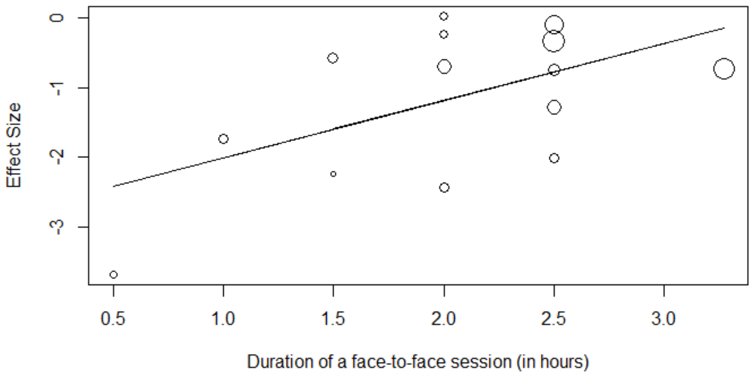
Compared to inactive control groups											
Dose	**Meta-regression model**						**Heterogeneity statistics**				
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	SE tau^2	Q_E	$p\ (Q_E)$
Total no. face-to-face sessions	0.1	0.07	[-0.25, 0.06]	-1.33	.2	17	1.77	5.06%	0.78	0.33	106.42	<.001
Duration of a face-to-face session	**0.82**	**0.35**	**[0.06, 1.59]**	**2.33**	**.038**	**14**	**5.45**	**29.4%**	**0.63**	**0.31**	**71.17**	**<.001**
Program length	-0.03	0.15	[-0.35, 0.29]	-0.2	.85	17	0.04	0.00%	0.89	0.37	123.48	<.001
Frequency of recommended practice	-0.25	0.32	[-0.94, 0.44]	-0.79	.45	14	0.62	0.00%	0.31	0.17	47.69	<.001
Duration of a recommended practice	0.01	0.01	[-0.02, 0.03]	0.58	.58	14	0.33	0.00%	0.33	0.18	57.34	<.001
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	SE tau^2	Q_E	$p\ (Q_E)$
Total amount of contact	-0.01	0.02	[-0.05, 0.04]	-0.26	.79	17	0.07	0.00%	0.89	0.37	117.09	<.001
Total amount of contact (excl. 0 hours)	0.03	0.03	[-0.04, 0.1]	0.85	.41	14	0.72	0.00%	0.92	0.43	85.67	<.001
Total recommended use of program	0.004	0.01	[-0.02, 0.03]	0.32	.76	14	0.1	0.00%	0.35	0.18	60.06	<.001
Total actual use of program*	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity excl. retreats	0.97	0.78	[-0.72, 2.7]	1.25	.23	14	1.57	3.46%	0.86	0.41	87.59	<.001
Program intensity incl. retreats	0.83	0.65	[-0.58, 2.25]	1.28	.22	14	1.65	3.76%	0.86	0.41	87.33	<.001
Amount of contact/week	0.01	0.14	[-0.28, 0.31]	0.1	.92	17	0.01	0.00%	0.89	0.37	118.43	<.001
Amount of contact (excl. 0 hours)/week	0.21	0.18	[-0.18, 0.6]	1.19	.26	14	1.42	2.59%	0.87	0.41	85.47	<.001
Recommended use of program/week	0.02	0.08	[-0.16, 0.2]	0.29	.78	14	0.08	0.00%	0.35	0.19	59.92	<.001
Actual use of program/week*	-	-	-	-	-	-	-	-	-	-	-	-

Compared to active control groups												
Dose	**Meta-regression model**							**Heterogeneity statistics**				
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	*SE* tau^2	Q_E	$p\ (Q_E)$
Total no. face-to-face sessions	-0.04	0.02	[-0.08, 0.003]	-2.09	.07	11	4.36	NA%	0	0.03	3.66	.93
Duration of a face-to-face session	-0.2	0.16	[-0.56, 0.16]	-1.27	.24	10	1.62	NA%	0	0.03	4.26	.83
Program length	-0.04	0.04	[-0.13, 0.06]	-0.9	.39	11	0.81	NA%	0	0.03	4.99	.84
Frequency of recommended practice	0.12	0.17	[-0.26, 0.51]	0.73	.48	11	0.54	NA%	0	0.03	5.14	.82
Duration of a recommended practice	<-.001	0.01	[-0.01, 0.01]	-0.02	.98	11	0.001	NA%	0	0.03	5.44	.79
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	*SE* tau^2	Q_E	$p\ (Q_E)$
Total amount of contact	**-0.02**	**0.01**	**[-0.033, -0.003]**	**-2.74**	**.023**	**11**	**7.56**	**NA%**	**0**	**0.03**	**2.96**	**.97**
Total amount of contact (excl. 0 hours)	**-0.02**	**0.01**	**[-0.04, -0.002]**	**-2.55**	**.034**	**10**	**6.49**	**NA%**	**0**	**0.03**	**2.83**	**.95**
Total recommended use of program	-0.004	0.003	[-0.01, 0.003]	-1.24	.25	11	1.53	NA%	0	0.03	4.65	.86
Total actual use of program*	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity excl. retreats**	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity incl. retreats	**0.21**	**0.05**	**[0.09, 0.33]**	**4.03**	**.004**	**10**	**16.25**	**NA%**	**0**	**0.03**	**1.69**	**.99**
Amount of contact/week	-0.11	0.08	[-0.29, 0.06]	-1.5	.17	11	2.22	NA%	0	0.03	4.36	.89
Amount of contact (excl. 0 hours)/week	-0.14	0.11	[-0.4, 0.12]	-1.23	.25	10	1.51	NA%	0	0.3	4.32	.83
Recommended use of program/week	-0.02	0.03	[-0.09, 0.06]	-0.46	.66	11	0.21	NA%	0	0.03	5.32	.81
Actual use of program/week*	-	-	-	-	-	-	-	-	-	-	-	-

significant results in bold; *k<10, **all but one study had the same score on this dose; *d*=effect size of the standardized regression coefficient, SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value= test statistic of slope, *p*-value= significance level; *k*=number of studies; *F*-distribution= test for the overall model; R^2= percentage of heterogeneity accounted for, tau^2/τ^2= variance of the underlying true effect sizes; *SE* tau^2= standard error of tau^2; Q_E= between-study heterogeneity; $p(Q_E)$)=Q_E significance level.

First, compared to inactive controls at 1-4 months follow-up, a significant dose-response relationship between the dose duration of a face-to-face session and stress was observed (see Table 4.20 and Figure 4.6 for the meta-regression plot). However, this finding was not in the hypothesized direction since a longer face-to-face session predicted *increased* stress with a large effect size and significant between study heterogeneity ($p(Q_E)$<.001).

Figure 4.6
Meta-regression plot for duration of a face-to-face session predicting stress at 1-4 months follow-up compared to inactive controls



This finding was no longer significant when applying the Holm-Bonferroni correction since the significant *p*-value (*p*=.038) was greater than the corrected *p*-value (HB: .05/(13-1+1)=.004; .038>.004). Similarly, this finding did not remain significant when applying the FDR correction using the Benjamini-Hochberg procedure since p_{adj}>.05 (see Appendix Table 4.2.3.3 for the step-by-step Benjamini-Hochberg FDR procedure). This finding is therefore not particularly robust and likely due to a Type I error and needs to be interpreted with caution. Although it was planned to control for baseline stress to determine whether baseline scores confounded the dose-response relationship between duration of a face-to-face session and stress and examine potential moderating effects of program type and study quality, these analyses were not possible since *k*<10 studies were available for each study-level variable and Hypotheses 4 and 5 could therefore not be tested for this result.

Second, compared to active controls at 1-4 months follow-up, three doses significantly predicted effect sizes of stress (see Table 4.20). Specifically, there were significant dose-response relationships with small effect sizes between the doses total amount of face-to-face facilitator contact and total amount of face-to-face facilitator contact (when excluding studies with no contact) and the stress outcome, both in hypothesized directions of greater face-to-face contact predicting significantly lower stress (see Figures 4.7 and 4.8 for the meta-regression plots) thus confirming Hypothesis 2.

Figure 4.7
Meta-regression plot for total amount of face-to-face facilitator contact predicting stress at 1-4 months follow-up compared to active controls

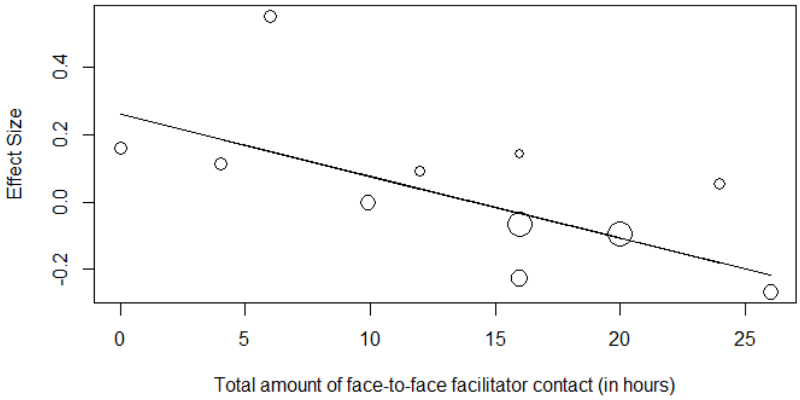
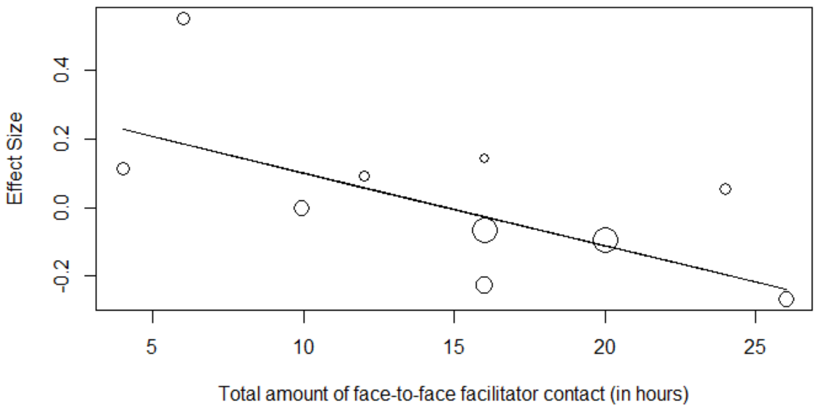
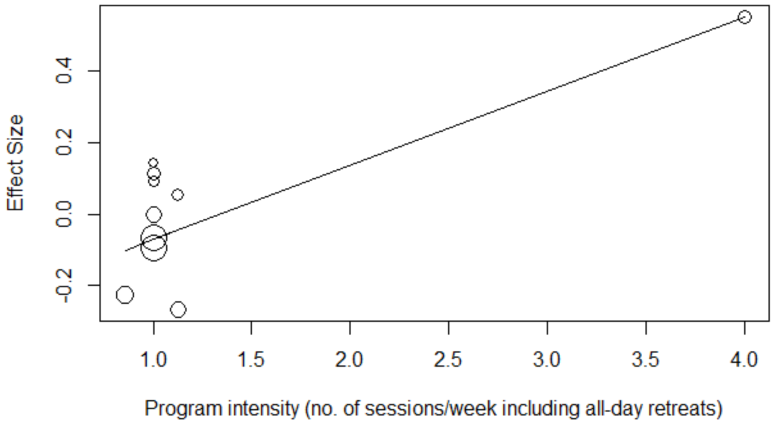


Figure 4.8
Meta-regression plot for total amount of face-to-face facilitator contact (excl. studies with no face-to-face contact) predicting stress at 1-4 months follow-up compared to active controls



Additionally, a significant dose-response relationship between the dose program intensity (when including all-day retreats) and stress was observed in the opposite direction to hypothesized indicating that more intense programs (when including any all-day retreats) predicted *increased* stress at 1-4 months follow-up compared to active controls (see Figure 4.9 for the meta-regression plot).

Figure 4.9
Meta-regression plot for program intensity (incl. retreats) predicting stress at 1-4 months follow-up compared to active controls



No significant between-study heterogeneity ($p(Q_E)$>.05) was observed for either of the three significant dose-response relationships. To test the robustness and possibility of Type I errors of the three significant dose-response relationships found for stress compared to active controls at 1-4 months follow-up, Holm-Bonferroni and False Discovery Rate corrections were employed. When applying the Holm-Bonferroni correction, only the finding of program intensity (when including all-day retreats) predicting stress remained significant since the significant *p*-value (*p*=.004) was below the corrected *p*-value (see Appendix 4.2.3.4 for step-by-step Holm-Bonferroni correction). Similarly, this finding remained significant when applying the FDR correction using the Benjamini-Hochberg procedure since p_{adj}=.048. For the step-by-step Benjamini-Hochberg FDR procedure, see Appendix Table 4.2.3.5. However, after closer inspection of the program intensity (including retreats) meta-regression plot, this finding appears due to one study with considerably higher intensity than other included studies and without this outlier, not enough studies (*k*<10) were available to run a reliable meta-regression analysis. Nevertheless, when the meta-regression analysis was completed with the outlier removed as a check, this finding was no longer significant. This result therefore does not appear particularly robust since the significant finding was due to a single study. The doses total amount of face-to-face facilitator contact (both when including and excluding zero hours of contact) were no longer significant when applying both Holm-Bonferroni and FDR corrections. Therefore, significant dose-response relationships for stress at 1-4 months follow-up compared to active controls do not appear particularly robust and need to be interpreted with caution. Not enough studies (*k*<10) were available to control for baseline and examine potential moderating effects and thus test Hypotheses 4 and 5 for these significant dose-response relationships.

At 5-10 months follow-up, not enough studies (*k*<10) were available for the stress outcome neither compared to inactive nor active controls for any of the dose-variables. Meta-regression analyses for stress at this timepoint were therefore not completed.

4.3 Results of Dose-Response Meta-Regression Analyses by Outcome Measures

As a reliability check, additional meta-regression analyses were conducted on a measure-by-measure basis, results of which are presented below. For purposes of conciseness, overall tables of measure-by-measure meta-regression results are added to Appendices.

4.3.1 Measure-by-Measure Meta-Regression Results: Primary Outcome Depression

For the depression outcome, four measures had enough studies (*k*≥10) to run measure-by-measure meta-regression analyses. These were the Beck’s Depression Inventory (BDI), the Centre for Epidemiological Studies Depression scale (CES-D), the depression subscale of the Depression Anxiety and Stress Scale (DASS-D) and the depression subscale of the Hospital Anxiety and Depression Scale (HADS-D). Results for each measure are presented separately.

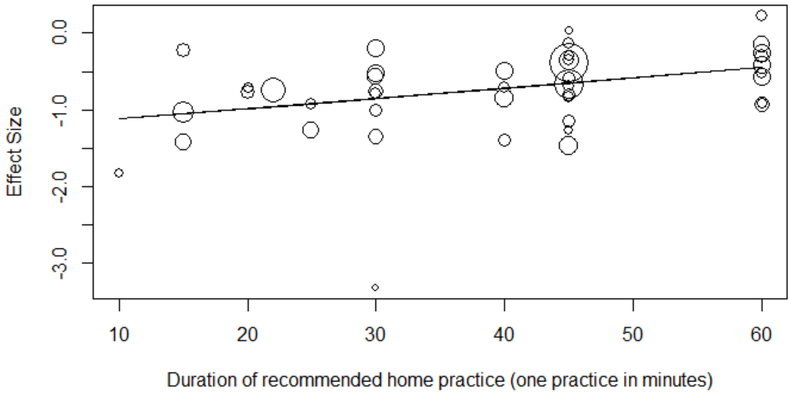
4.3.1.1 Measure-by-Measure Meta-Regression with BDI

4.3.1.1.1 BDI Depression at Post-Program.

At immediately post-program, significant dose-response relationships were found for BDI depression. Appendix Table 4.3.1.1 shows results of all BDI measure meta-regression results at post-program compared to inactive and active controls where *k*≥10. A significant dose-response relationship between the dose duration of a recommended practice and BDI depression (*k*=44; t=2.78; *d*=0.01; C.I.: [0.004, 0.03]; F(1, 42)=7.73; *p*=.008) compared to inactive controls was found with a small effect size and significant between-study heterogeneity ($p(Q_E)$<.001). This finding is in the opposite direction to that hypothesized since this dose was associated with increased, as opposed to decreased, BDI depression. Figure 4.10 shows the meta-regression plot of this significant result and Appendix Figure 4.3.1.2 shows all non-significant BDI meta-regression plots at post-program.

Figure 4.10

BDI meta-regression plot for duration of a recommended practice predicting depression at post-program compared to inactive controls



This finding was no longer significant when applying the Holm-Bonferroni correction since the significant *p*-value (*p*=.008) was greater than the corrected *p*-value (HB: .05/(13-1+1)=.004; .008>.004). Similarly, this finding did not remain significant when applying the FDR correction using the Benjamini-Hochberg procedure since p_{adj}>.05. For the step-by-step Benjamini-Hochberg FDR procedure, see Appendix Table 4.3.1.3. This finding is therefore not particularly robust and likely due to a Type I error and needs to be interpreted with caution. When controlling for baseline BDI depression, this dose-response relationship remained significant with a small effect size in the opposite direction to hypothesised and no significant between-study heterogeneity (see Tables 4.21 and 4.22 for results and heterogeneity statistics).

Table 4.21

Measure-by-measure meta-regression model for duration of a recommended practice dose controlling for baseline BDI depression at post-program compared to inactive controls

	d	**SE**	**95% CI**	***t***	***p***
Intercept	-1.08	0.24	[-1.56, -0.6]	-4.51	<.001
Duration rec.	**0.01**	**0.01**	**[0.001, 0.02]**	**2.3**	**.03**
Bl BDI depr.	-0.13	0.09	[-0.32, 0.06]	-1.38	.18

Duration rec.=duration of recommended practice; Bl BDI depr.=baseline BDI depression; *d*=effect size; SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value=test statistic of slope, *p*-value=significance level; significant dose-response results in bold.

Table 4.22

Measure-by-measure meta-regression model fit when controlling for baseline BDI depression for duration of a recommended practice dose at post-program compared to inactive controls

Test of the interaction:
Simultaneous test that all coefficients (excluding intercept) are zero
F=4.9; df=2, 41; p=.01; R^2=26.37%
Goodness of fit: Test that unexplained variance is zero
Tau^2=0.092; SE=0.04; Q_E=94.76, df=41, p<.0001

F=Test of moderators; df=Degrees of Freedom; R^2=Amount of heterogeneity accounted for; Tau^2=Estimated amount of unexplained/residual heterogeneity; Q_E=Test for residual heterogeneity.

To examine possible moderating effects, the moderators MBP type and study quality were added in separate measure-by-measure meta-regression models. No significant interaction effects between either of the moderators and the dose duration of a recommended practice were observed for BDI depression at post-program compared to inactive controls with significant between-study heterogeneity (see Tables 4.23-4.26 for interaction model and model test results for the moderators MBP type and study quality, respectively).

Table 4.23

Measure-by-measure meta-regression interaction model between duration of a recommended practice and type of program (MBSR/MBCT versus other types) for BDI depression at post-program compared to inactive controls

	d	SE	95% CI	*t*	*p*
Intercept	-1.07	0.29	[-1.66, -0.47]	-3.62	.001
Type	-0.35	0.44	[-1.23, 0.53]	-0.8	.43
Duration rec.	0.01	0.01	[-0.004, 0.02]	1.47	.15
Type x duration rec.	0.01	0.01	[-0.02, 0.03]	0.53	.6

Duration rec.=duration of recommended practice; *d*=effect size; SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value=test statistic of slope, *p*-value=significance level.

Table 4.24

Measure-by-measure meta-regression model fit for the interaction of a duration of recommended practice and type of program (MBSR/MBCT versus other types) for BDI depression post-program compared to inactive controls

Test of the interaction:

Simultaneous test that all coefficients (excluding intercept) are zero

F=2.79; df=3, 40; p=.05; R^2=25.25%

Goodness of fit: Test that unexplained variance is zero

Tau^2=0.09; SE=0.04; Q_E=94.72, df=40, p<.0001

F=Test of moderators; df=Degrees of Freedom; R^2=Amount of heterogeneity accounted for; Tau^2=Estimated amount of unexplained/residual heterogeneity; Q_E=Test for residual heterogeneity.

Table 4.25

Measure-by-measure meta-regression interaction model between duration of a recommended practice and study quality for BDI depression at post-program compared to inactive controls

	d	SE	95% CI	*t*	*p*
Intercept	-2.64	1.49	[-5.66, 0.39]	-1.76	.09
RoB	0.11	0.12	[-0.13, 0.35]	0.93	.36
Duration rec.	0.03	0.04	[-0.04, 0.11]	0.92	.36
RoB x duration rec.	-0.002	0.003	[-0.01, 0.004]	-0.54	.59

Duration rec.=duration of recommended practice; RoB=Risk of Bias score; *d*=effect size; SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value=test statistic of slope, *p*-value=significance level.

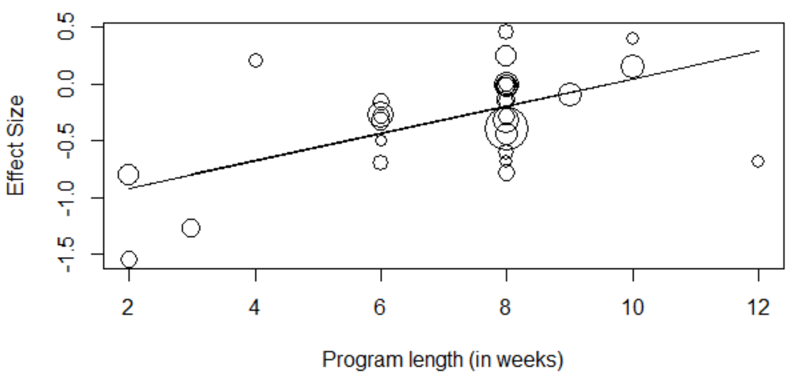
Table 4.26
Measure-by-measure meta-regression model fit for the interaction of duration of a recommended practice and study quality for BDI depression at post-program compared to inactive controls

Test of the interaction:
Simultaneous test that all coefficients (excluding intercept) are zero
F=3.15; df=3, 40; p=.04; R^2=19.87%
Goodness of fit: Test that unexplained variance is zero
Tau^2=0.1; SE=0.04; Q_E=96.97, df=40, p < 0.0001

F=Test of moderators; df=Degrees of Freedom; R^2=Amount of heterogeneity accounted for; Tau^2=Estimated amount of unexplained/residual heterogeneity; Q_E=Test for residual heterogeneity.

Compared to active controls at post-program, a significant dose-response relationship between the dose program length and BDI depression (k=30; t=3.9; d=0.12; C.I.: [0.06, 0.18]; F(1, 28)=15.17; p<.001) was observed with a small effect size and significant between-study heterogeneity ($p(Q_E)$<.001). Again, this finding is in the opposite direction to hypothesized since this dose was associated with increased BDI depression. Figure 4.11 shows the meta-regression plot.

Figure 4.11
BDI meta-regression plot for program length predicting BDI depression at post-program compared to active controls



This finding remained significant when applying the Holm-Bonferroni correction since the significant p-value ($p\leq$.001) was below the corrected p-value (HB: .05/(11-1+1)=.005; .001<.005). Similarly, this finding remained significant when applying the FDR correction using the Benjamini-Hochberg procedure since p_{adj}<.05. For the step-by-step Benjamini-Hochberg FDR procedure, see Appendix Table 4.3.1.4. This result also remained significant when controlling for baseline BDI depression (see Tables 4.27 and 4.28 for results and heterogeneity statistics).

Table 4.27
Measure-by-measure meta-regression model for program length dose controlling for baseline BDI depression at post-program compared to active controls

	d	**SE**	**95% CI**	***t***	***p***
Intercept	-1.14	0.25	[-1.66, -0.62]	-4.51	<.001
Program length	**0.12**	**0.03**	**[0.06, 0.18]**	**3.8**	**.001**
Bl BDI depr.	-0.01	0.11	[-0.23, 0.21]	-0.12	.91

Bl BDI depr.=baseline BDI depression; *d*=effect size; SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value=test statistic of slope, *p*-value=significance level; significant dose-response results in bold.

Table 4.28
Measure-by-measure meta-regression model fit when controlling for baseline BDI depression for program length dose at post-program compared to active controls

Test of the interaction:

Simultaneous test that all coefficients (excluding intercept) are zero

F=7.24; df=2, 27; p=.003; R^2=60.79%

Goodness of fit: Test that unexplained variance is zero

Tau^2=0.05; SE=0.03; Q_E=51.73, df=27, p=.003

F=Test of moderators; df=Degrees of Freedom; R^2=Amount of heterogeneity accounted for; Tau^2=Estimated amount of unexplained/residual heterogeneity; Q_E=Test for residual heterogeneity.

To examine possible moderating effects, the moderators MBP type and study quality were added in separate measure-by-measure meta-regression models. A significant small interaction effect between the dose program length and MBP type was found for BDI depression compared to active controls at post-program. Tables 4.29-4.30 display interaction model and model test results.

Table 4.29

Measure-by-measure meta-regression interaction model between program length and type of program (MBSR/MBCT versus other types) for BDI depression at post-program compared to active controls

	d	**SE**	**95% CI**	***t***	***p***
Intercept	-0.05	0.4	[-0.88, 0.79]	-0.11	.91
Type	-1.51	0.47	[-2.46, -0.55]	-3.23	.003
Program length	-0.02	0.05	[-0.13, 0.08]	-0.42	.68
Type x program length	**0.2**	**0.06**	**[0.08, 0.32]**	**3.3**	**.003**

d=effect size; SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value=test statistic of slope, *p*-value=significance level; significant dose-response results in bold.

Table 4.30

Measure-by-measure meta-regression model fit for the interaction program length and type of program (MBSR/MBCT versus other types) for BDI depression at post-program compared to active controls

Test of the interaction:

Simultaneous test that all coefficients (excluding intercept) are zero

F=10.57; df=3, 26; p=.001; R^2=78.09%

Goodness of fit: Test that unexplained variance is zero

Tau^2=0.03; SE=0.02; Q_E=38.42, df=26, p=.06

F=Test of moderators; df=Degrees of Freedom; R^2=Amount of heterogeneity accounted for; Tau^2=Estimated amount of unexplained/residual heterogeneity; Q_E=Test for residual heterogeneity.

When completing separate subgroup analyses with MBSR/MBCT and other MBP types, this result only remained significant for the subgroup of other MBPs with a small effect size in the unexpected direction of increased BDI depression (k=16; t=5.21; d=0.2; C.I.:[0.11, 0.25]; F(1, 14)=27.11; p<.001) but was not significant for MBSR/MBCT programs. The significant dose-response relationships only having been found for MBPs other than MBSR/MBCT is likely an artefact of the fact that the standardised MBSR/MBCT programs have very little spread in program length, and so is unlikely to show an effect, while other MBP types show a greater range on this dose. Next, a significant small moderating effect was also observed between program length and study quality (scores of Cochrane Risk of Bias tool) for BDI depression at post-program compared to active controls (see Tables 4.31-4.32 for interaction model and model test results).

Table 4.31

Measure-by-measure meta-regression interaction model between program length and study quality (scores on Risk of Bias tool) for BDI depression at post-program compared to active controls

	d	**SE**	**95% CI**	***t***	***p***
Intercept	-4.03	1.28	[-6.66, -1.4]	-3.15	.004
RoB	0.27	0.12	[0.03, 0.51]	2.28	.03
Program length	**0.48**	**0.15**	**[0.16, 0.79]**	**3.13**	**.004**
RoB x program length	**-0.03**	**0.01**	**[-0.06, -0.005]**	**-2.39**	**0.02**

RoB=Risk of Bias score; *d*=effect size; SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value=test statistic of slope, *p*-value=significance level; significant dose-response results in bold.

Table 4.32

Measure-by-measure meta-regression model fit for the interaction program length and study quality (scores on Risk of Bias tool) for BDI depression at post-program compared to active controls

Test of the interaction:

Simultaneous test that all coefficients (excluding intercept) are zero

F=7.76; df=3, 26; p<.001; R^2=71.54%

Goodness of fit: Test that unexplained variance is zero

Tau^2=0.03; SE=0.03; Q_E=43.42, df=26, p=.02

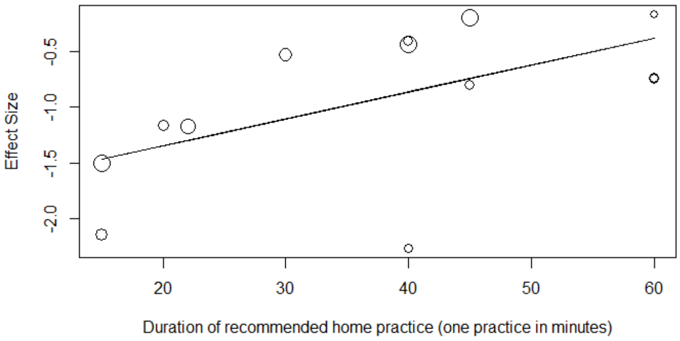
F=Test of moderators; df=Degrees of Freedom; R^2=Amount of heterogeneity accounted for; Tau^2=Estimated amount of unexplained/residual heterogeneity; Q_E=Test for residual heterogeneity.

When running separate subgroup analyses with risk of bias scores below and above the median score, this result only remained significant in the opposite to hypothesised direction of increased BDI for the group of low quality (high risk of bias) studies (*k*=14; t=5.33; *d*=0.21; C.I.:[0.12, 0.3]; F(1, 12)=28.4; *p*<.001). The result of a significant dose-response relationship between the dose program length and BDI depression therefore does not appear particularly robust since the effect size was likely due to low quality studies which have previously been found to have inflated effect sizes; this finding thus needs to be interpreted with caution.

4.3.1.1.2 BDI Depression at Follow-Up.

At 1-4 months follow-up compared to inactive controls, a significant dose-response relationship between the dose duration of a recommended practice and BDI depression (*k*=12; t=2.37; *d*=0.02; C.I.: [0.002, 0.05]; F(1, 11)=5.61; *p*=.037) was observed with a small effect size in the direction opposite to hypothesised (predicting increased BDI depression) and significant between-study heterogeneity (*p(Q_E)*<.001). Appendix Table 4.3.1.5 displays BDI meta-regression results at 1-4 months follow-up. This measure-by-measure finding mirrors the significant dose-response relationship found for this dose and timepoint for the main analysis including all measures. Figure 4.12 shows the meta-regression plot and Appendix Figure 4.3.1.6 shows non-significant meta-regression plots for BDI depression at follow-up.

Figure 4.12
BDI meta-regression plot for duration of a recommended practice predicting BDI depression at 1-4 months follow-up compared to inactive controls



This finding was no longer significant when applying the Holm-Bonferroni correction since the significant *p*-value (*p*=.037) was greater than the corrected *p*-value (HB: .05/(12-1+1)=.004; .037>.004). Similarly, this finding did not remain significant when applying the FDR correction using the Benjamini-Hochberg procedure since p_{adj}>.05. For the step-by-step Benjamini-Hochberg FDR procedure, see Appendix Table 4.3.1.7. This finding is therefore not particularly robust and likely due to a Type I error and needs to be interpreted with caution. Although it was planned to control for baseline BDI depression and examine potential moderating effects of program type and study quality, these analyses were not possible since *k*<10 studies were available for each study-level variable. Compared to active controls and both comparison groups at 5-10 months follow-up, *k*<10 studies were available to complete meta-regression analyses with the BDI measure.

4.3.1.2 Measure-by-Measure Meta-Regression with CES-D

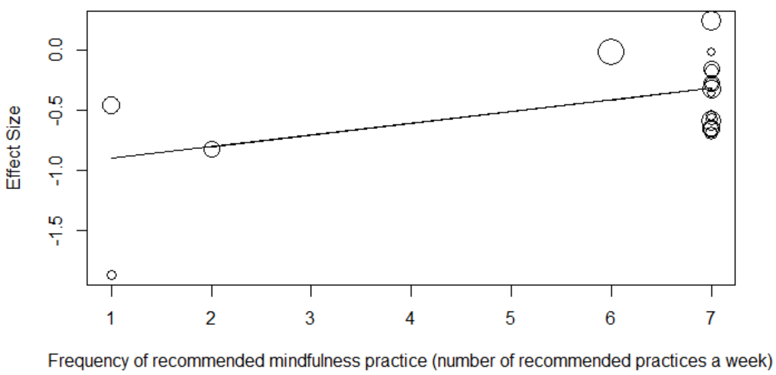
For the CES-D measure, no significant dose-response relationships were found at post-program neither compared to inactive nor active controls. This reflects results found for the overall meta-regression analysis at the post-program timepoint. Appendix Table 4.3.1.8 and Appendix Figure 4.3.1.9 numerically and visually display CES-D meta-regression results at post-program. At follow-up, k<10 studies were available to complete meta-regression analyses with the CES-D measure.

4.3.1.3 Measure-by-Measure Meta-Regression with DASS-D

For the DASS-depression measure, a significant dose-response relationship between the dose frequency of recommended mindfulness practice and DASS-D depression (k=18; t=2.32; d=0.1; C.I.:[0.01, 0.19]; F(1, 16)=5.4; p=.034) compared to inactive controls at post-program was observed with a small effect size in the direction opposite to hypothesised (predicting increased DASS-depression) and significant between-study heterogeneity ($p(Q_E)$=.007). Appendix Table 4.3.1.10 displays DASS-D meta-regression results at post-program. Figure 4.13 shows the meta-regression plot and Appendix Figure 4.3.1.11 shows non-significant meta-regression plots for DASS-D depression at post-program.

Figure 4.13

DASS-D meta-regression plot for frequency of recommended practice predicting DASS-D depression at post-program compared to inactive controls

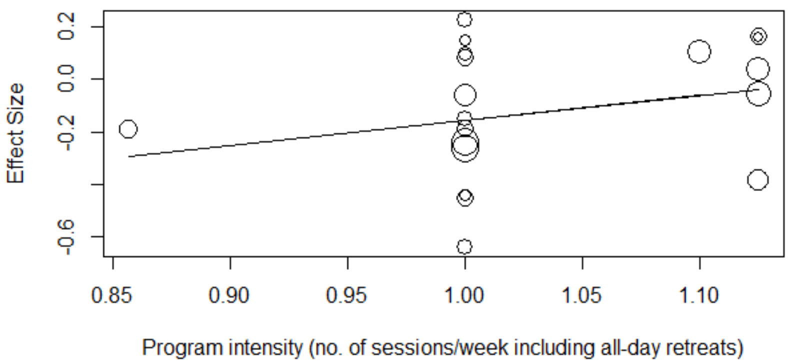


This finding was no longer significant when applying the Holm-Bonferroni correction since the significant p-value (p=.034) was greater than the corrected p-value (HB: .05/(11-1+1)=.005; .034>.005). Similarly, this finding did not remain significant when applying the FDR correction using the Benjamini-Hochberg procedure since p_{adj}>.05 (see Appendix Table 4.3.1.12 for step-by-step Benjamini Hochberg FDR procedure). This finding is therefore not particularly robust and likely due to a Type I error and needs to be interpreted with caution. Although it was planned to control for baseline DASS-D depression to determine whether baseline scores confounded the dose-response relationship and examine potential moderating effects of program type and study quality, these analyses were not possible since *k*<10 studies were available for each study-level variable. Compared to active controls at post-program and both comparison groups at follow-up timepoints, *k*<10 studies were available to complete meta-regression analyses with the DASS-D measure.

4.3.1.4 Measure-by-Measure Meta-Regression with HADS-D

For the HADS-depression measure, a significant dose-response relationship between the dose program intensity (when including all-day retreats) and HADS-D depression (*k*=16; t=-2.23; *d*=0.34; C.I.:[0.78, 0.02]; F(1, 14)=4.97; *p*=.043) compared to inactive controls at post-program was observed with a small to moderate effect size in the opposite to hypothesised direction (predicting increased HADS-D depression) and significant between-study heterogeneity ($p(Q_E)$=.04). Appendix Table 4.3.1.13 displays HADS-D meta-regression results at post-program. Figure 4.14 shows the meta-regression plot for program intensity (including retreats) and Appendix Figure 4.3.1.14 shows non-significant meta-regression plots for HADS-D depression at post-program.

Figure 4.14
HADS-D meta-regression plot for program intensity (including retreats) predicting HADS-D depression at post-program compared to inactive controls



However, this finding was no longer significant when applying the Holm-Bonferroni correction since the significant *p*-value (*p*=.043) was greater than the corrected *p*-value (HB: .05/(11-1+1)=.005; .043>.005). Similarly, this finding did not remain significant when applying the FDR correction since p_{adj}>.05 (see Appendix Table 4.3.1.15 for step-by-step Benjamini-Hochberg FDR procedure). This finding is therefore not particularly robust and likely due to a Type I error and needs to be interpreted with caution. It was not possible to control for baseline HADS-D depression or examine potential moderating effects of program type and study quality since *k*<10 studies were available for each study-level variable. Compared to active controls at post-program and both comparison groups at follow-up timepoints, *k*<10 studies were available to complete meta-regression analyses with the HADS-D.

4.3.2 Measure-by-Measure Meta-Regression Results: Secondary Outcome Anxiety

For the anxiety outcome, three measures had enough studies (*k*≥10) to run measure-by-measure meta-regression analyses at post-program. These were the Beck's Anxiety Inventory (BAI), the anxiety subscale of the Depression Anxiety and Stress Scale (DASS-A) and the anxiety subscale of the Hospital Anxiety and Depression Scale (HADS-A). No significant dose-response relationships were found for either of the three measures at post-program compared to either inactive or active controls where *k*≥10. This reflects results found for the overall meta-regression analysis at the post-program timepoint where no significant dose-response relationships were found for anxiety. Appendix Tables and Figures 4.3.2.1 to 4.3.2.6 display all meta-regression results for the BAI, DASS-A and HADS-A measures at post-program. At follow-up, *k*<10 studies were available for either measure.

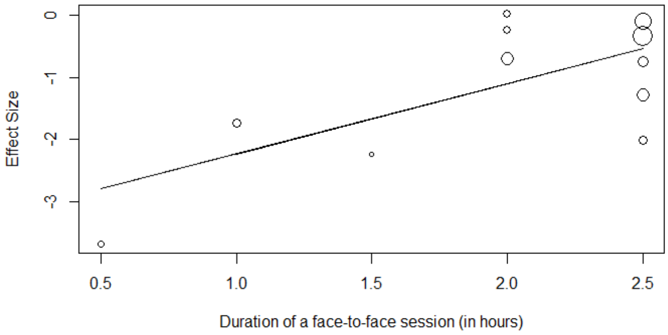
4.3.3 Measure-by-Measure Meta-Regression Results: Secondary Outcome Stress

For the stress outcome, two measures had enough studies (*k*≥10) to run measure-by-measure meta-regression analyses, namely the stress subscale of the Depression Anxiety and Stress Scale (DASS-S) and the Perceived Stress Scale (PSS).

At post-program, no significant dose-response relationships were observed neither for either measure-by-measure meta-regressions neither compared to inactive nor active controls where *k*≥10. Appendix Tables and Figures 4.3.3.1 to 4.3.3.4 display meta-regression results for the DASS-stress and PSS at immediately post-program. The absence of significant dose-response relationships for the stress outcome at post-program mirrored findings of overall meta-regressions at this timepoint.

At 1-4 months follow-up, enough studies (*k*<10) where only available for the PSS measure compared to inactive controls (see Appendix Table 4.3.3.5 and Appendix Figure 4.3.3.6 for PSS at 1-4 months follow-up). A significant dose-response relationship between the dose duration of a face-to-face session and PSS stress (*k*=11; t=2.84; *d*=1.12; C.I.:[0.23, 2.02]; F(1,9)=8.08; *p*=.019) was found at this timepoint with a large effect size in the opposite to hypothesised direction (predicting increased PSS-stress) and significant between-study heterogeneity (*p*(Q_E)<.001). This is the same significant dose-response relationship in the same direction as was found for the overall meta-regression analysis. Figure 4.15 shows the meta-regression plot.

Figure 4.15
PSS meta-regression plot for duration of a face-to-face session predicting PSS stress at 1-4 months follow-up compared to inactive controls



However, this finding was no longer significant when applying the Holm-Bonferroni correction since the significant *p*-value (*p*=.019) was greater than the corrected *p*-value (HB: .05/(12-1+1)=.004; .019>.004). Similarly, this finding did not remain significant when applying the FDR correction since p_{adj}>.05. For the step-by-step Benjamini-Hochberg FDR procedure, see Appendix Table 4.3.3.7. This finding is therefore not particularly robust and similar to the overall meta-regression analysis, is likely due to a Type I error and needs to be interpreted with caution. Although it was planned to control for baseline PSS-stress to determine whether baseline scores confounded this dose-response relationship and examine potential moderating effects of program type and study quality, these analyses were not possible since *k*<10 studies were available for each study-level variable.

Overall, measure-by-measure meta-regression results did not contradict findings form the main meta-regression analyses for anxiety and stress outcomes. For depression, the vast majority of measure-by-measure analyses did not find different results and the small number of significant results in the unexpected direction were either likely Type I errors as established through Holm-Bonferroni and FDR corrections or due to low quality of included studies. Furthermore, while there were occasional significant findings (six out of 120 measure-by-measure meta-regression analyses), these should be treated with caution due to the substantially smaller sample sizes for separate measures (k<1/3 of the overall sample), the inconsistent pattern of findings and the likelihood of Type I errors. Specifically, if the null-hypothesis is correct at a 5% alpha level, we would expect this number of spuriously significant findings (since 5% of 120 is 6), in the absence of any real dose-response relationships.

4.4 Discussion of Dose-Response Meta-Regression Findings for Psychological Distress Outcomes

4.4.1 Summary of Findings

This chapter presented findings of the dose-response meta-regression for psychological distress outcomes for 15 different doses relating to MBPs. At immediately post-program and 5-10 months follow-up, no significant dose-response relationships were found for either of the outcomes depression, anxiety, or stress, therefore not supporting Hypothesis 2 for these timepoints. When repeating the primary analyses only with studies that included participants from a depression and again from the general population, no significant dose-response effects were observed for either. Similarly, results from exploratory clinical significance analysis found that the degree of change was similar for high and low doses and no significant dose-response relationships were observed for different severities of depression. At 1-4 months follow-up, doses associated with recommended practice, program length and intensity, and face-to-face contact significantly predicted effect sizes of depression, anxiety, and stress, albeit at times in unexpected directions. However, none of these findings were particularly robust as established through Holm-Bonferroni and FDR corrections and controlling for baseline levels and thus need to be interpreted with caution. No significant moderating effects for the moderator practice quality were found for significant dose-response relationships thus rejecting Hypothesis 5. For program intensity predicting decreased anxiety, a significant moderating effect of program type was found for MBPs other than MBSR/MBCT, which is contrary to hypothesised thus rejecting Hypothesis 4. However, this significant dose-response relationship was due to an outlier without which it was no longer significant. Measure-by-measure meta-regression analyses did not greatly contradict pooled findings and any further significant results were likely Type

I errors or due to low-quality studies. Dose-response findings are discussed with respect to previous research next.

4.4.2 Discussion of Psychological Distress Dose-Response Findings in line with Previous Research and Theory

The limited evidence of dose-response relationships found coincides to an extent with Cuijpers' et al. (2013) research also not having found a dose effect between doses such as contact time and duration of psychotherapy interventions and the outcome depression. Contrarily, this result does not correspond with findings of the previous meta-analysis by Parsons et al. (2017) on MBPs introduced in Chapter 2 where amount of home practice was significantly associated with positive outcomes. However, Parsons' meta-analysis only included specific types of MBPs, namely MBSR and MBCT, but not others such as self-help MBPs, which generally employ lower doses. Additionally, an association between amount of practice and outcomes may have been found in Parson's review since participant-level variables were examined but not study-level variables (i.e. dose) in different MBPs. Furthermore, the present review included a greater variety of different doses relating to MBPs other than amount of practice. It feels important for reviews to be inclusive of different types and dose variations of MBPs due to the recent increase in self-help MBPs finding positive effects (Spijkerman et al., 2016). Previous research suggesting that purely online MBPs are associated with valuable outcomes for participants could therefore be a reason why no robust significant dose-response relationships were found between amount of face-to-face facilitator contact and psychological outcomes. Additionally, no evidence of a dose-response effect adds to previous literature suggesting that there may not necessarily be a connection between dose relating to MBPs and outcome (Beblo & Schulte, 2017) and that even much shorter, less intense programs without face-to-face contact are beneficial for wellbeing (Creswell, 2017). From a theoretical perspective, the argument could be made that rumination and worry as underlying mechanisms focused on in MBPs (Gu et al., 2015) may be able to be addressed through different, including smaller, doses of mindfulness. Furthermore, a potential explanation for no robust significant dose-response effects found for psychological distress outcomes is that the majority of participants did not experience clinical levels of ill mental health at baseline which may therefore have resulted in a floor effect since there was not much variability for improvement for psychological outcomes for those currently not suffering from severe mental health difficulties. A floor effect for psychological outcomes could therefore have made it more difficult to draw out dose effects since changes in depression, anxiety and stress may not have been significantly different enough to baseline levels to highlight dose effects. However, when repeating the analyses only with studies who included participants from a depression population and again from the general population, no significant dose-response effects were observed for either. Similarly, no difference in dose effects were found when conducting separate analyses for individuals with different severities of depression and when completing exploratory clinical significance analysis. Additionally, participants in included RCTs were mostly novice mindfulness practitioners with the MBP often being the first-time participants had become aware of mindfulness. For novice practitioners even smaller doses could have been beneficial in decreasing depression, anxiety, and stress with larger doses not being significantly more helpful. This corresponds with previous meta-analyses advocating for the benefits of brief MBPs on outcomes (Blanck et al., 2018; Schumer et al., 2018). Another possible reason for a lack of dose-response relationships involving psychological distress outcomes might be the restricted range and variability for some of the dose variables. This does however seem an unlikely explanation since, as could be seen from descriptive statistics of the doses in Chapter 3, there was a reasonable range in the majority of dose variables and lower variability in some doses is discussed further in Chapter 5.

Although no significant dose-response relationships were observed for dose variables at post-program, doses associated with recommended practice, program length and intensity, as well as face-to-face contact significantly predicted effect sizes in depression, anxiety, and stress at follow-up, albeit at times in unexpected directions. Most notably, larger effect sizes in depression were predicted by longer duration of recommended home practices, which was not in the hypothesized direction. However, none of these findings were particularly robust and thus likely due to Type I errors, as was established through Holm-Bonferroni and False Discovery Rate corrections and controlling for baseline levels. If, however future research were to substantiate high durations of recommended practice predicting worse outcomes, this could be explained with the non-monotonic effect of mindfulness practice as outlined by Britton (2019) where negative effects of mindfulness practice, in particular relating to psychological distress, have mostly been found after engaging in larger doses of practice (e.g. Britton et al., 2010; 2014; Sahdra et al., 2017; see Chapter 1, Section 1.4.6.3). Additionally, one possible explanation might be that if homework demands are too high and too overwhelming a challenge, there may be a paradoxical effect of participants practicing less or cease practicing altogether, in particular at follow-up where longer practices are more difficult to sustain. Vettese et al. (2009) found that recommending a certain amount of practice does not necessarily translate to participants engaging in recommended practices, something which is also often not tracked accurately enough. Additionally, difficulties related to the often-large time commitments for practice have previously been associated with the high level of attrition in MBPs (Shapiro et al., 2005) and with compliance with home practice at follow-up (e.g. Dimidjian et al., 2016). This is also supported by previous qualitative research that suggests that lengthy mindfulness practices are perceived as barriers to engagement (Banerjee et al., 2017) and that individuals often prefer shorter practices (Boggs et al., 2014; Klatt et al., 2009). Furthermore, this finding corresponds to an extent with some previous research finding no association between amount of practice and psychological outcomes (e.g. Bondolfi et al., 2010; Jain et al., 2007; Ribeiro et al., 2018). Additionally, for the stress outcome, greater face-to-face facilitator contact was associated with decreased stress at follow-up, however, this finding was not particularly robust and thus likely due to a Type I error. Again, if future research were to substantiate this finding, this coincides with the notion that face-to-face teaching can facilitate the learning of mindfulness practice which in turn may help reduce stress as was argued by Kabat-Zinn (1982) when developing MBSR and corresponds with previous research (e.g. Khoury et al., 2015). Next, greater program intensity (when including any all-day retreats) predicted decreased anxiety and increased stress at follow-up. However, these findings were due to single studies with extreme scores without which the effect disappeared. Finally, greater duration of a face-to-face session predicted increased stress, however, again, this finding was not robust. Therefore, although a small number of significant dose-response effects were found for psychological distress outcomes at follow-up, these were not robust, were frequently based on small sample sizes and often due to single studies without which the effect disappeared. Substantial caution therefore needs to be exercised with regards to these particular findings given that they may well be spurious and until replicated, are arguably not worthy of further consideration. Certain methodological and interpretative limitations prevail on what can be concluded from findings; these are outlined next.

4.4.3 Limitations and Implications

A caution with regards to the interpretation of results is the possibility of false positive findings (Type I error) or false negative findings (Type II error) having occurred for significant and non-significant results, respectively. Type I errors could have occurred for significant effects due to statistical multiple comparisons arising from testing multiple hypotheses for multiple doses at multiple time-points for each outcome, all of which inflate the number of comparisons, thus increasing the possibility of a Type I error (Abdi, 2010). To address this possibility, the Holm-Bonferroni (Holm, 1979) and the False Discovery Rate corrections (Benjamini & Hochberg, 1997; Glickmann et al., 2014) were employed. As outlined in Chapter 2, the Holm-Bonferroni method has been argued to be too stringent (Diz et al., 2011), therefore both corrections were applied and presented here to assess robustness of findings. However, a disadvantage associated with the above family-wise alpha correction procedures is that although the chance of a Type I error reduces, power is reduced further when adopting a correction thus increasing the chance of a Type II error (Nakagawa, 2004). Additionally, the possibility of a Type II error occurring due to failing to reject the null-hypothesis when the alternative hypothesis was in fact true has been found to increase exponentially with a smaller sample size (Hoenig & Heisey, 2001; Schmidt & Hunter, 2015), resulting in reduced power. To reduce the likelihood of a Type II error, analyses undertaken were only completed where $k \geq 10$ studies per study-level variable were available (Fu et al. 2010). However, analyses containing smaller sample sizes ($k>9$) can still be underpowered and thus the possibility of a Type II error still exists; these thus still need to be interpreted with caution (Christley, 2010; Hung et al., 1997). For some analyses with larger sample sizes however, when inspecting meta-regression plots, there do not appear to be trends for a relationship; these therefore appear unlikely due to Type II errors. Furthermore, particularly in meta-analytic investigations, the chance of Type I and II errors can be increased if individual studies already contain errors (Kempton et al., 2008). This is a possibility here due to the large number of different quality studies being included. Additionally, conducting multiple analyses with a large number of variables, including multiple dose variables, controlling for baseline scores and assessing moderating effects, and often small sample sizes has been found to further expand the possibility for a Type I error having occurred for significant effects (Berlin et al., 2002). Therefore, although the chance of Type I and II errors was reduced to an extend by applying corrections for multiple comparisons and instilling a threshold of the number of studies at least needing to be included, the possibility of Type I and II errors still prevails and both significant as well as non-significant results therefore need to be interpreted with caution. Furthermore, Type I errors have been found more probable in meta-regression analyses than in conventional regression analysis due to potential heterogeneity (Higgins & Thompson, 2004).

Although the high between-study heterogeneity observed in meta-analyses (discussed in Chapter 3) was minimised to some extent by adding dose variables of MBPs as well as baseline levels and moderators to the model, moderate to high heterogeneity still existed from confounding variables not controlled for in this review. However, the aim of this review was to be inclusive of a diversity of MBPs; a certain degree of residual heterogeneity is therefore to be expected. To account for residual heterogeneity not addressed by predictors or covariate variables, it is recommended for meta-regressions to be weighted for higher quality and higher power studies to have more weight in the analysis (Thompson & Higgins, 2001). This was addressed here by employing a random effects meta-regression model. Additionally, it is recommended to pre-specify covariates in order to avoid false positive findings (Thompson & Higgins, 2001), which was addressed by pre-registering the review and covariates to be included prior to commencing meta-regression analyses.

Furthermore, although it was originally planned and pre-specified to include population group as a moderator, this could not be completed since there were not enough studies (k<10) available for each category, especially for the groups of mental health conditions other than depression and long-term physical health conditions. Hypothesis 3 could therefore not be tested and this needs to be noted as a limitation. Additionally, due to the nature of meta-regressions, only average baseline scores across participants in a study were available (not individual participant data). Within included studies, it may therefore have been the case that participants with varying levels of mental health difficulties existed. However, participant-level data was not the main focus of this research, but rather study-level data of doses relating to MBPs. These limitations have to an extent been addressed by controlling for average baseline levels and completing separate primary analyses including only participants with depression and only participants from the general population. Moreover, although it was planned to conduct clinical significance analyses on robust significant findings, this was not possible since no such results were found. The approach generally taken in research is for clinical significance analysis to be completed after statistical significance has been established in order to understand the clinical implications behind a statistically significant finding (Jacobson et al., 2014; Khoury et al., 2013). Clinical significance analysis may therefore not be particularly meaningful in the absence of statistically significant effects, but rather only becomes useful in understanding the degree of clinical significance of statistically viable findings (e.g. Deyo et al., 1995; Fethney, 2010, Greenstein, 2003). Nevertheless, as an exploratory approach, clinical significance analysis was completed for the group of studies with individuals with a diagnosis of depression and the most commonly employed measure of depression. Findings did not follow a particular pattern which is unsurprising since no statistically significant robust dose-response relationships were found for depression. Additionally, separate dose-response meta-regression analyses were completed with studies with participants with mild and severe baseline depression to determine whether findings differed for these groups. Again, no significant dose-response relationships were found for different severities of depression. However, it was not possible to complete clinical significance or severity analyses for the group of studies with participants with a diagnosis of an anxiety or stress difficulty since there were not enough studies with participants with clinical levels/a specific diagnosis of anxiety or stress to be grouped reliably for meta-regression analyses. In future, once more RCTs have been completed with participants with diagnoses of depression, anxiety or stress conditions, clinical significance and severity analyses could be repeated with these population groups.

Not only different population groups but also different outcome measures were included in this review. One possible concern is that dose-response effects might be obscured if measures assess slightly different constructs (Fried, 2017). However, as mentioned in Chapter 3, the approach taken here to combine across a range of different measures is typical of meta-analyses in the field (e.g. Blanck et al., 2018; Gu et al., 2015; Khoury et al., 2015; Spijkerman et al., 2016) and the pattern of findings did not materially differ when analyses were repeated on a measure-by-measure basis with the exception of a small number of significant results. However, these results should be treated with caution due to substantially smaller sample sizes and the proliferation of analyses when conducting additional measure-by-measure analyses thus introducing the possible expansion of Type I errors (Aickin & Gensler, 1996). More details relating to limitations associated with smaller sample sizes specifically to the analyses presented in this research are explored in Chapter 5.

Finally, although possible reasons for the lack of dose-response effects found for psychological distress outcomes have been outlined, this is an area that would benefit from further research and possibly theoretical analysis. More detailed recommendations and implications of future research for dose-response in MBPs are outlined in Chapter 5.

4.5 Chapter 4 Summary

In this chapter, results of the meta-regression analyses for psychological distress outcomes (depression, anxiety, stress) were presented including controlling for baseline levels and moderator analyses for significant outcomes. Additionally, reliability checks in the form of clinical significance and severity analyses were completed as well as meta-regressions with different population groups and outcome measures. Generally, no significant robust dose-response relationships were found for either of the outcomes neither for the overall meta-regression nor for measure-by-measure meta-regressions or analyses with different population groups or severities of depression. Results in line with previous research were discussed suggesting ambiguous evidence of a dose-response relationship between MBPs and outcomes also considering potential methodological and interpretative limitations particularly relating to possible Type I and II errors. Next, the question arises whether dose relating to

MBPs predict levels of mindfulness as the key mechanism of mindfulness-based programs. Dose-response meta-regression results with mindfulness as the outcome are presented and discussed in the next chapter (5).

CHAPTER 5

Dose-Response in Mindfulness-Based Programs:

Meta-Regression Results and Discussion

of the Mindfulness Outcome

5.1 Chapter 5 Overview

This chapter follows on from Chapter 4, which presented the dose-response meta-regression for psychological distress outcomes. This chapter presents results of the dose-response meta-regression for the mindfulness outcome as the key mechanism of mindfulness-based programs (MBPs) at immediately post-program and follow-up timepoints. For significant dose-response relationships, moderator and subgroup analyses were completed. Measure-by-measure meta-regression analyses were performed as a reliability check. Results are discussed in line with previous research followed by limitations and implications of dose-response on MBPs for future research and practice.

5.2 Results of the Dose-Response Meta-Regression for Mindfulness

Similar to psychological distress outcomes, results of which were presented in Chapter 4, dose-response relationships were examined with a Restricted Maximum Likelihood (REML) meta-regression using a random effects model with the Knapp and Hartung adjustment (Knapp & Hartung, 2003) to test Hypothesis 2 of whether greater doses of MBPs predicted increased mindfulness. The same continuous dose variables that were added as predictors to meta-regression models for psychological distress outcomes were added as predictors to the meta-regression model for the mindfulness outcome. These doses were total number of face-to-face sessions, duration of a face-to-face session (in hours), program length (in weeks), frequency of recommended practices a week (number of practices recommended per week), duration of a recommended practice (one practice in minutes), total amount of face-to-face facilitator contact (in hours), total amount of face-to-face facilitator contact (in hours) when excluding zero hours of contact, total recommended use of the program (in hours), total actual use of the program (in hours), program intensity (excluding retreats), program intensity (including retreats), amount of face-to-face facilitator contact a week (in hours), amount of face-to-face facilitator contact a week (in hours) when excluding zero hours of contact, recommended use of the program a week (in hours), and actual use of the program a week (in hours). Results are presented compared to inactive and active controls at immediately post-program and 1-4 months follow-up. No significant dose-response relationships were observed at 5-10 months follow-up and results at this timepoint were therefore added to Appendices. At 12-15 months follow-up, not enough studies (k<10) were available neither compared to inactive nor active controls to warrant meta-regression.

For significant dose-response relationships, Holm-Bonferroni and False Discovery Rate (FDR; Benjamini-Hochberg procedure) corrections were employed to control for Type I errors and test robustness of findings. Additionally, significant dose-response relationships were again followed up with moderator analyses to test Hypothesis 4 of dose-response relationships being moderated by MBP type, with MBSR/MBCT programs hypothesised to find a larger effect than other MBPs, and Hypothesis 5 of dose-response relationships being moderated by study quality (risk of bias score on Cochrane Risk of Bias tool) and actual practice recording quality (for actual use of MBP doses only) with lower quality studies hypothesised to show a larger effect. Significant interactions between moderators and doses were further examined with subgroup analysis. Similar to psychological distress outcomes, although it was planned to also examine the effects of the categorical moderator participant population group and thus test Hypothesis 3, this could not be completed since there were not enough studies (k<10) available in some or more population categories. Although planned, baseline levels of mindfulness could not reliably be controlled for since baseline data were not available for over a third of studies (see Chapter 2); this is discussed in the limitations below. Finally, measure-by-measure meta-regression analyses were completed for the mindfulness outcome.

For each meta-regression analysis, the following are presented: effect size (Cohen's *d*) of the standardised regression coefficient, standard error of the effect size (SE), confidence intervals (95% CI) as an estimation of probable boundaries within which the true effect is believed to be, test statistic of slope of the model (*t*-value), significance level (*p*-value) to determine whether the dose predictor significantly differed from zero, and number of studies (*k*) included in the model for each dose. Additionally, heterogeneity statistics for each dose-response meta-regression analysis are listed, including a test for the overall model (*F*-distribution), percentage of heterogeneity accounted for by the meta-regression model (R^2), variance of the underlying true effect sizes (tau^2/τ^2), standard error of tau^2 ($SE\ tau^2$), between-study heterogeneity (Cochran's Q: Q_E) and whether between-study heterogeneity exceeded that expected to occur by chance alone, which is measured with its significance level ($p(Q_E)$). As a reliability check, the relevant meta-regression analyses were repeated with any outlying studies removed. This did not significantly alter results. Meta-regression plots were created for all doses, timepoints and control comparisons (including for measure-by-measure meta-regression analyses). Similar to Chapter 4, all non-significant meta-regression plots are in the Appendices for purposes of conciseness.

5.2.1 Dose-Response Meta-Regression Results for Mindfulness at Post-Program

At immediately post-program, significant dose-response relationships between several different doses associated with actual program use, program intensity and weekly amount of facilitator contact, and the response mindfulness were found in the hypothesized direction of greater doses predicting increased mindfulness thus confirming Hypothesis 2 for these doses (see Table 5.1 for all results at post-program compared to inactive and active controls). Significant between-study heterogeneity ($p(Q_E)>.05$) was observed for all results apart from the actual use of the program (in total and per week) doses compared to both inactive and active controls. Primary dose variables did not significantly predict mindfulness at post-program. Appendix Figure 5.2.1 shows the meta-regression plots for all non-significant results at post-program.

Table 5.1

Meta-regression analysis results by MBP dose for between-group mindfulness effect sizes at immediately post-program compared to inactive and active controls

Compared to inactive controls												
Dose	**Meta-regression model**						**Heterogeneity statistics**					
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total no. face-to-face sessions	0.03	0.02	[-0.02, 0.07]	1.21	.23	61	1.47	0.00%	0.23	0.06	220.26	<.001
Duration of a face-to-face session	-0.05	0.1	[-0.26, 0.15]	-0.52	.61	50	0.27	0.00%	0.28	0.08	187.44	<.001
Program length	0.01	0.03	[-0.05, 0.08]	0.42	.68	61	0.18	0.00%	0.23	0.06	223.04	<.001
Frequency of recommended practice	-0.06	0.08	[-0.21, 0.09]	-0.78	.44	58	0.61	0.00%	0.18	0.05	190.65	<.001
Duration of a recommended practice	0.002	0.004	[-0.01, 0.01]	0.49	.63	58	0.24	0.00%	0.18	0.05	189.08	<.001
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total amount of contact	0.01	0.01	[-0.001, 0.03]	1.8	.08	61	3.23	3.45%	0.22	0.06	217.43	<.001
Total amount of contact (excl. 0 hours)	0.02	0.01	[-0.002, 0.04]	1.84	.07	50	3.38	5.99%	0.25	0.07	179.46	<.001
Total recommended use of program	0.004	0.003	[-0.002, 0.01]	1.36	.18	58	1.86	2.99%	0.17	0.05	184.37	<.001
Total actual use of program	**0.01**	**0.01**	**[0.001, 0.024]**	**2.25**	**.04**	**17**	**5.06**	**60.73%**	**0.01**	**0.03**	**18.57**	**.234**
Program intensity excl. retreats	**1.07**	**0.36**	**[0.35, 1.79]**	**2.97**	**.005**	**50**	**8.83**	**16.68%**	**0.22**	**0.06**	**172.48**	**<.001**
Program intensity incl. retreats	**0.89**	**0.29**	**[0.32, 1.47]**	**3.11**	**.003**	**50**	**9.64**	**20.89%**	**0.21**	**0.06**	**167.51**	**<.001**
Amount of contact/week	**0.13**	**0.05**	**[0.03, 0.24]**	**2.58**	**.013**	**61**	**6.63**	**9.21%**	**0.21**	**0.05**	**211.12**	**<.001**
Amount of contact (excl. 0 hours)/week	**0.21**	**0.07**	**[0.06, 0.36]**	**2.89**	**.006**	**50**	**8.34**	**16.93%**	**0.23**	**0.06**	**173.11**	**<.001**
Recommended use of program/week	0.05	0.03	[-0.01, 0.1]	1.8	.08	58	3.25	5.16%	0.17	0.05	182.67	<.001
Actual use of program/week	0.06	0.05	[-0.05, 0.17]	1.19	.26	17	1.4	17.44%	0.03	0.03	22.18	.103

Compared to active controls												
Dose	**Meta-regression model**							**Heterogeneity statistics**				
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total no. face-to-face sessions	0.004	0.02	[-0.04, 0.05]	0.2	.85	34	0.04	0.00%	0.06	0.03	59.9	.002
Duration of a face-to-face session	0.06	0.11	[-0.16, 0.27]	0.54	.59	31	0.29	0.00%	0.07	0.04	57.79	.001
Program length	-0.01	0.03	[-0.07, 0.04]	-0.49	.64	34	0.24	0.00%	0.06	0.03	58.59	.003
Frequency of recommended practice	-0.04	0.15	[-0.36, 0.27]	-0.28	.78	31	0.08	0.00%	0.04	0.03	47.33	.017
Duration of a recommended practice	0.004	0.01	[-0.01, 0.01]	0.94	.36	31	0.88	0.00%	0.04	0.03	46.19	.022
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total amount of contact	0.01	0.01	[-0.01, 0.02]	0.65	.52	34	0.42	0.00%	0.06	0.03	60.15	.002
Total amount of contact (excl. 0 hours)	0.01	0.01	[-0.02, 0.03]	0.55	.59	31	0.3	0.00%	0.07	0.04	57.81	.001
Total recommended use of program	0.003	0.003	[-0.004, 0.01]	0.87	.39	31	0.76	0.00%	0.04	0.03	46.9	.019
Total actual use of program	**0.02**	**0.01**	**[0.004, 0.03]**	**3.06**	**.014**	**11**	**9.33**	**100%**	**0**	**0.04**	**4.83**	**.849**
Program intensity excl. retreats	-0.1	0.12	[-0.34, 0.15]	-0.83	.41	31	0.69	0.00%	0.07	0.04	56.85	.001
Program intensity incl. retreats	-0.04	0.12	[-0.29, 0.21]	-0.33	.75	31	0.11	0.00%	0.07	0.04	57.77	.001
Amount of contact/week	0.04	0.07	[-0.1, 0.18]	0.62	.54	34	0.39	0.00%	0.06	0.03	59.84	.002
Amount of contact (excl. 0 hours)/week	0.05	0.09	[-0.15, 0.24]	0.5	.62	31	0.25	0.00%	0.07	0.04	57.3	.001
Recommended use of program/week	0.02	0.03	[-0.05, 0.08]	0.5	.62	31	0.25	0.00%	0.05	0.03	47.47	.017
Actual use of program/week	0.11	0.05	[-0.01, 0.23]	2.07	.07	11	4.29	100%	0	0.04	6.66	.673

significant results in bold; *d*=effect size of the standardized regression coefficient, SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value= test statistic of slope, *p*-value= significance level; *k*=number of studies; *F*-distribution= test for the overall model; R^2= percentage of heterogeneity accounted for, tau^2/τ^2= variance of the underlying true effect sizes; *SE* tau^2= standard error of tau^2; Q_E= between-study heterogeneity; $p(Q_E)$=Q_E significance level.

Specifically, significant dose-response relationships in the hypothesized direction between the dose total actual use of the program and mindfulness compared to both inactive and active controls at post-program were found with small effect sizes suggesting that greater actual use of MBPs predicted increased mindfulness. Figures 5.1 and 5.2 show the meta-regression plots.

Figure 5.1
Meta-regression plot for total actual use of the program predicting mindfulness at post-program compared to inactive controls

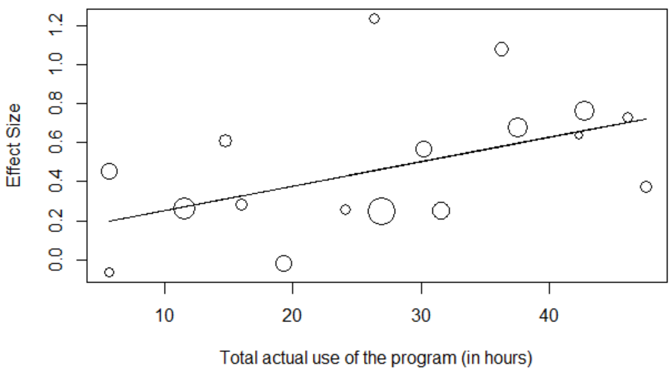
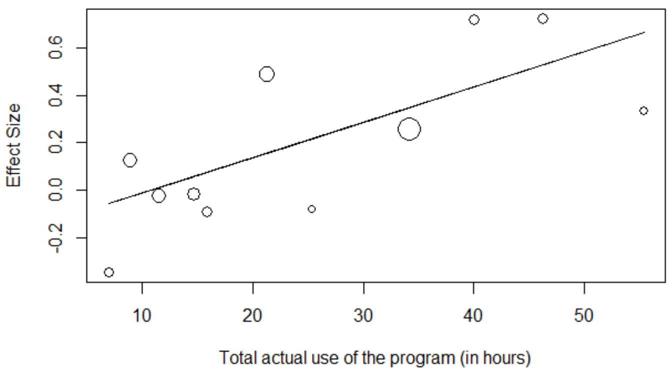


Figure 5.2
Meta-regression plot for total actual use of the program dose predicting mindfulness at post-program compared to active controls



Additionally, program intensity when excluding all day retreats as well as program intensity when including all-day retreats significantly predicted increased mindfulness at post-program with large effect sizes in the MBP compared to the inactive control group suggesting that participating in more intense MBPs predicted greater mindfulness. Figures 5.3 and 5.4 show the meta-regression plots for both.

Figure 5.3
Meta-regression plot for program intensity (excluding retreats) predicting mindfulness at post-program compared to inactive controls

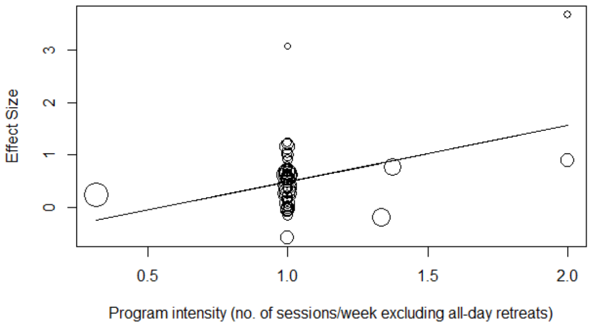
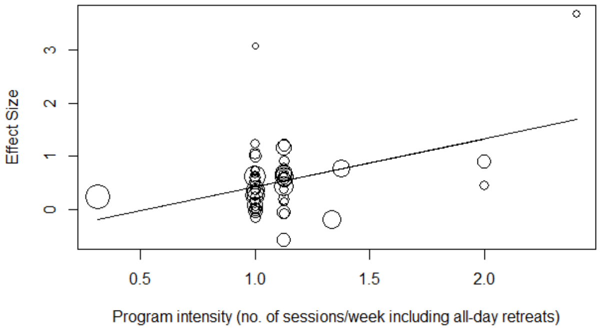


Figure 5.4
Meta-regression plot for program intensity (including retreats) predicting mindfulness at post-program compared to inactive controls



Next, a significant dose-response relationship between amount of face-to-face facilitator contact per week, both when including and excluding zero hours of contact, and the mindfulness outcome were observed in the expected direction at post-program compared to inactive controls, with small to medium effect sizes suggesting that greater weekly contact with a facilitator predicted increased mindfulness. Figures 5.5 and 5.6 show the meta-regression plots of both models.

Figure 5.5

Meta-regression plot for amount of face-to-face facilitator contact a week predicting mindfulness at post-program compared to inactive controls

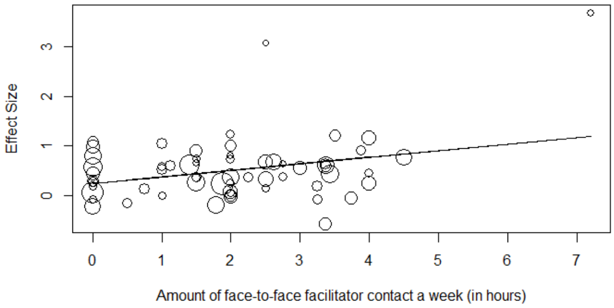
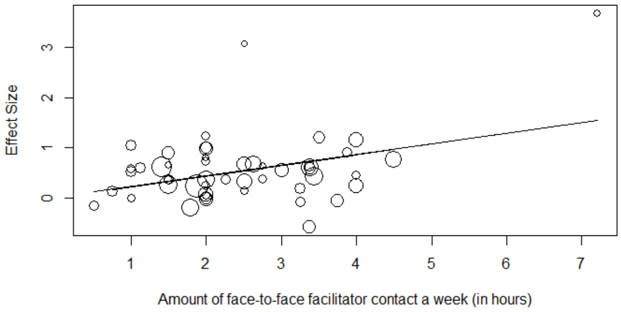


Figure 5.6

Meta-regression plot for amount of face-to-face facilitator contact (excl. no contact) a week predicting mindfulness at post-program compared to inactive controls



As can be seen from Table 5.1, dose-response effects were marginally significant (.1>*p*>.05) for other composite doses (facilitator contact (with and without the inclusion of no contact) and recommended use of the program a week compared to inactive controls; actual use of the program a week compared to active controls) thus increasing the likelihood of genuine effects having occurred.

5.2.1.1 Type I Error Correction

To test the robustness and possibility of a Type I error of significant dose-response relationships at post-program, Holm-Bonferroni and False Discovery Rate (FDR) corrections were employed separately for inactive and active control comparisons.

Compared to inactive controls, when applying the Holm-Bonferroni correction, only the finding of program intensity (when including all-day retreats) predicting mindfulness remained significant since the significant *p*-value (*p*=.003) was below the corrected *p*-value (see Appendix 5.2.2 for step-by-step Holm-Bonferroni correction). However, when applying the FDR correction using the

Benjamini-Hochberg procedure, all dose-response relationships aside from the total actual use of the program dose remained significant since $p_{adj}<.05$ for program intensity and facilitator contact a week doses. For the step-by-step Benjamini-Hochberg FDR procedure for mindfulness compared to inactive controls see Appendix Table 5.2.3.

Compared to active controls, the finding of total actual use of a program significantly predicting increased mindfulness was no longer significant when applying the Holm-Bonferroni correction since the significant *p*-value ($p=.014$) was greater than the corrected *p*-value (HB: .05/(15-1+1)=.003; .014>.003). Similarly, this finding did not remain significant when applying the FDR correction using the Benjamini-Hochberg procedure since $p_{adj}>.05$ (see Appendix Table 5.2.4 for the step-by-step Benjamini-Hochberg FDR procedure). Significant results for the actual use of the program dose for both comparison groups therefore need to be interpreted with caution.

5.2.1.2 Moderator Analysis

Moderator analyses were completed to assess whether significant dose-response relationships between actual program use, program intensity and weekly facilitator contact, and mindfulness were moderated by program type, study quality and actual practice recording quality where the number of studies was sufficient.

5.2.1.2.1 Total Actual Use of the Program Dose.

Although it was planned to examine moderator effects for significant dose-response relationships found for total actual use of the program compared to both control groups, this was not possible since not enough studies ($k<10$) were available for each study-level variable to run these analyses for this dose.

5.2.1.2.2 Program Intensity Doses.

Firstly, for the program intensity (when excluding all-day retreats) dose, the analysis with the moderator program type (MBSR/MBCT vs. other MBPs) was not completed since all MBSR/MBCT-type programs had the same score on this dose (intensity=1). Next, a significant moderate interaction effect was observed between program intensity (when excluding all-day retreats) and study quality (as assessed with scores on the Cochrane Risk of Bias tool) for mindfulness at post-program compared to inactive controls with significant between-study heterogeneity (see Tables 5.2 and 5.3 for interaction model and model test results).

Table 5.2
Meta-regression interaction model between program intensity (when excluding retreats) and study quality for mindfulness at post-program compared to inactive controls

	d	**SE**	**95% CI**	***t***	***p***
Intercept	5.76	3.3	[-0.87, 12.4]	1.75	.09
RoB	-0.49	0.25	[-0.99, 0.03]	-1.89	.07
Program intensity (excl. retreats)	-5.67	3.2	[-12.12, 0.77]	-1.77	.08
RoB x program intensity (excl. retreats)	**0.51**	**0.24**	**[-0.02, 1]**	**2.09**	**.04**

RoB=Risk of Bias score; *d*=effect size; SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value=test statistic of slope, *p*-value=significance level; significant dose-response results in bold.

Table 5.3
Meta-regression model fit for the interaction program intensity (when excluding retreats) and study quality for mindfulness at post-program compared to inactive controls

Test of the interaction:

Simultaneous test that all coefficients (excluding intercept) are zero

F=4.88; df=3, 46; p=.005; R^2=26.42%

Goodness of fit: Test that unexplained variance is zero

Tau^2=0.2; SE=0.06; Q_E=151.45, df=46, p<.001

F=Test of moderators; df=Degrees of Freedom; R^2=Amount of heterogeneity accounted for; Tau^2=Estimated amount of unexplained/residual heterogeneity; Q_E=Test for residual heterogeneity.

When running separate subgroup analyses with risk of bias scores below and above the median score, this result only remained significant for the group of low quality (high risk of bias) studies (k=30; t=3.08; d=1.46; C.I.:[0.49, 2.42]; F(1, 28)=9.5; p=.005). The significant dose-response relationship with the program intensity (when excluding all-day retreats) dose therefore needs to be interpreted with caution since significant results are only found in low quality, high risk of bias studies, which can inflate effect sizes.

Second, for the program intensity (when including all-day retreats) dose, no significant interaction effects between either of the moderators program type or study quality were observed for mindfulness at post-program compared to inactive controls with significant between-study heterogeneity (see Tables 5.4-5.7 for interaction model and model test results for the moderators MBP type and study quality, respectively).

Table 5.4
Meta-regression interaction model between program intensity (including retreats) and program type (MBSR/MBCT vs. other MBPs) for mindfulness at post-program compared to inactive controls

	d	**SE**	**95% CI**	***t***	***p***
Intercept	1.45	1.75	[-2.07, 4.97]	0.83	.41
Type	-2.06	1.79	[-5.67, 1.54]	-1.15	.26
Program intensity (incl. retreats)	-0.88	1.64	[-4.19, 2.43]	-0.54	.59
Type x program intensity (incl. retreats)	1.87	1.67	[-1.5, 5.23]	1.12	.27

d=effect size; SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value=test statistic of slope, *p*-value=significance level.

Table 5.5
Meta-regression model fit for the interaction of intensity (including retreats) and program type (MBSR/MBCT vs. other MBPs) for mindfulness at post-program compared to inactive controls

Test of the interaction:

Simultaneous test that all coefficients (excluding intercept) are zero

F=3.66; df=3, 46; p=.02; R^2=17.11%

Goodness of fit: Test that unexplained variance is zero

Tau^2=0.22; SE=0.06; Q_E=166.24, df=46, p<.001

F=Test of moderators; df=Degrees of Freedom; R^2=Amount of heterogeneity accounted for; Tau^2=Estimated amount of unexplained/residual heterogeneity; Q_E=Test for residual heterogeneity.

Table 5.6
Meta-regression interaction model between program intensity (including retreats) and study quality for mindfulness at post-program compared to inactive controls

	d	SE	95% CI	*t*	*p*
Intercept	2.77	2.92	[-3.12, 8.65]	0.95	.35
RoB	-0.23	0.22	[-0.68, 0.21]	-1.06	.29
Program intensity (incl. retreats)	-2.52	2.67	[-7.9, 0.21]	-0.95	.35
RoB x program intensity (incl. retreats)	0.25	0.2	[-0.15, 0.65]	1.26	.21

RoB=Risk of Bias score; *d*=effect size; SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value=test statistic of slope, *p*-value=significance level.

Table 5.7
Meta-regression model fit for the interaction of program intensity (including retreats) and study quality for mindfulness at post-program compared to inactive controls

Test of the interaction:

Simultaneous test that all coefficients (excluding intercept) are zero

F=4.01; df=3, 46; p=.01; R^2=23.86%

Goodness of fit: Test that unexplained variance is zero

Tau^2=0.2; SE=0.06; Q_E=152.33, df=46, p<.001

F=Test of moderators; df=Degrees of Freedom; R^2=Amount of heterogeneity accounted for; Tau^2=Estimated amount of unexplained/residual heterogeneity; Q_E=Test for residual heterogeneity.

5.2.1.2.3 Amount of Face-to-Face Facilitator Contact a Week Doses.

For the amount of face-to-face facilitator contact a week dose, although approaching significance, no significant interaction effects between either of the moderators program type or study quality were observed for mindfulness at post-program compared to inactive controls with significant between-study heterogeneity (see Tables 5.8-5.11 for interaction model and model test results for the moderators MBP type and study quality, respectively).

Table 5.8
Meta-regression interaction model between amount of contact a week and program type (MBSR/MBCT vs. other MBPs) for mindfulness at post-program compared to inactive controls

	d	**SE**	**95% CI**	***t***	***p***
Intercept	0.55	0.23	[0.09, 1]	2.39	.02
Type	-0.4	0.27	[-0.94, 0.15]	-1.45	.15
Amount of contact/week	-0.01	0.09	[-0.19, 0.17]	-0.08	.94
Type x amount of contact/week	0.21	0.11	[-0.01, 0.43]	1.91	.06

d=effect size; SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value=test statistic of slope, *p*-value=significance level.

Table 5.9
Meta-regression model fit for the interaction of amount of contact a week and program type (MBSR/MBCT vs. other MBPs) for mindfulness at post-program compared to inactive controls

Test of the interaction:

Simultaneous test that all coefficients (excluding intercept) are zero

F=3.5; df=3, 57; p=.02; R^2=11.61%

Goodness of fit: Test that unexplained variance is zero

Tau^2=0.2; SE=0.05; Q_E=203.41, df=57, p<.001

F=Test of moderators; df=Degrees of Freedom; R^2=Amount of heterogeneity accounted for; Tau^2=Estimated amount of unexplained/residual heterogeneity; Q_E=Test for residual heterogeneity.

Table 5.10
Meta-regression interaction model between amount of contact a week and study quality for mindfulness at post-program compared to inactive controls

	d	**SE**	**95% CI**	***t***	***p***
Intercept	0.97	0.8	[-0.63, 2.57]	1.22	.23
RoB	-0.06	0.07	[-0.2, 0.08]	-0.87	.39
Amount of contact/week	-0.52	0.32	[-1.17, 0.13]	-1.61	.11
RoB x amount of contact/week	0.05	0.03	[-0.0004, 0.11]	1.99	.052

RoB=Risk of Bias score; *d*=effect size; SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value=test statistic of slope, *p*-value=significance level.

Table 5.11
Meta-regression model fit for the interaction of amount of contact a week and study quality for mindfulness at post-program compared to inactive controls

Test of the interaction:
Simultaneous test that all coefficients (excluding intercept) are zero
F=4.24; df=3, 57; p=.01; R^2=16.02%
Goodness of fit: Test that unexplained variance is zero
Tau^2=0.19; SE=0.05; Q_E=197.57, df=57, p<.001

F=Test of moderators; df=Degrees of Freedom; R^2=Amount of heterogeneity accounted for; Tau^2=Estimated amount of unexplained/residual heterogeneity; Q_E=Test for residual heterogeneity.

For the amount of face-to-face facilitator contact (when excluding zero hours of contact) a week dose, a significant small interaction effect between this dose and MBP type was found for mindfulness compared to inactive controls at post-program. Tables 5.12 and 5.13 display interaction model and model test results.

Table 5.12
Meta-regression interaction model between amount of contact (when excluding zero hours of contact) a week and program type (MBSR/MBCT vs. other MBPs) for mindfulness at post-program compared to inactive controls

	d	**SE**	**95% CI**	***t***	***p***
Intercept	0.44	0.25	[-0.07, 0.96]	1.75	.09
Type	**-0.89**	**0.39**	**[-1.68, -0.11]**	**-2.29**	**.03**
Amount of contact (excl. 0 hours)/week	0.03	0.1	[-0.17, 0.23]	0.3	.77
Type x amount of contact (excl. 0 hours)/week	**0.36**	**0.14**	**[0.08, 0.64]**	**2.55**	**.01**

d=effect size; SE=standard error of the effect size, 95% CI= confidence intervals; t-value=test statistic of slope, p-value=significance level; significant dose-response results in bold.

Table 5.13
Meta-regression model fit for the interaction of amount of contact (when excluding zero hours of contact) a week and program type (MBSR/MBCT vs. other MBPs) for mindfulness at post-program compared to inactive controls

Test of the interaction:
Simultaneous test that all coefficients (excluding intercept) are zero
F=5.15; df=3, 46; p=.004; R^2=26.21%
Goodness of fit: Test that unexplained variance is zero
Tau^2=0.2; SE=0.06; Q_E=158.14, df=46, p<.001

F=Test of moderators; df=Degrees of Freedom; R^2=Amount of heterogeneity accounted for; Tau^2=Estimated amount of unexplained/residual heterogeneity; Q_E=Test for residual heterogeneity.

When completing separate subgroup analyses with MBSR/MBCT and other MBP types, this result only remained significant for the subgroup of MBPs other than MBSR/MBCT with a small effect size (k=17; t=3.83; d=0.39; C.I.:[0.17, 0.61]; F(1, 15)=14.68; p=.002) but was not significant for MBSR/MBCT programs suggesting that for studies employing non-traditional MBPs, the dose amount of face-to-face contact (excluding zero hours of contact) a week significantly predicted increased mindfulness. The significant dose-response relationship only having been found for MBPs other than MBSR/MBCT is likely an artefact of the fact that the standardised MBSR/MBCT programs have very little spread in weekly amount of face-to-face facilitator contact, and so is unlikely to show an effect, while other MBP types show a greater range on this dose. Next, no significant interaction effect was observed for the amount of face-to-face facilitator contact (when excluding zero hours of contact) a week dose and study quality. Tables 5.14 and 5.15 display interaction model and model test results.

Table 5.14

Meta-regression interaction model between amount of contact (when excluding zero hours of contact) a week and study quality for mindfulness at post-program compared to inactive controls

	d	SE	95% CI	*t*	*p*
Intercept	1.56	1.57	[-1.61, 4.72]	0.99	.33
RoB	-0.11	0.12	[-0.36, 0.13]	-0.94	.35
Amount of contact (excl. 0 hours)/week	-0.69	0.58	[-1.87, 0.48]	-1.19	.24
RoB x Amount of contact (excl. 0 hours)/week	0.07	0.04	[-0.02, 0.16]	1.56	.13

RoB=Risk of Bias score; *d*=effect size; SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value=test statistic of slope, *p*-value=significance level.

Table 5.15

Meta-regression model fit for the interaction of amount of contact (when excluding zero hours of contact) a week and study quality for mindfulness at post-program compared to inactive controls

Test of the interaction:

Simultaneous test that all coefficients (excluding intercept) are zero

F=4.41; df=3, 46; p=.01; R^2=22.26%

Goodness of fit: Test that unexplained variance is zero

Tau^2=0.21; SE=0.01; Q_E=162.56, df=46, p<.001

F=Test of moderators; df=Degrees of Freedom; R^2=Amount of heterogeneity accounted for; Tau^2=Estimated amount of unexplained/residual heterogeneity; Q_E=Test for residual heterogeneity.

5.2.2 Dose-Response Meta-Regression Results for Mindfulness at Follow-Up

At 1-4 months follow-up, a significant dose-response relationship between the dose program intensity (when including all-day retreats) and mindfulness compared with active controls was observed in the opposite to hypothesised direction with a small effect size and no significant between-study heterogeneity indicating that more intense MBPs predicted decreased mindfulness at follow-up compared to active controls. Hypothesis 2 is therefore not confirmed. The remaining dose variables including all doses compared to inactive controls did not significantly predict between-group mindfulness effect sizes at 1-4 months follow-up. Table 5.16 shows results for all dose-response meta-regression analyses for mindfulness at 1-4 months follow up. For the actual use of program doses predicting mindfulness compared to both comparison groups, not enough studies (k<10) were included to run the analysis. For the intensity (when excluding retreats) dose compared to both comparison groups as well as for the total number of face-to-face sessions and frequency of recommended practice doses compared to active controls, all but two studies had the same score on these doses. For program length compared to active controls, all but one study had the same score. Meta-regression analyses with these doses were therefore not completed. Significant between-study heterogeneity ($p(Q_E)$<.001) was observed for all doses compared to inactive controls and none compared to active controls. Figure 5.7 shows the meta-regression plot for program intensity (including retreats) and mindfulness at 1-4 months follow-up compared to active controls. Meta-regression plots for remaining doses for mindfulness at 1-4 months follow-up are in Appendix Figure 5.2.5.

Table 5.16

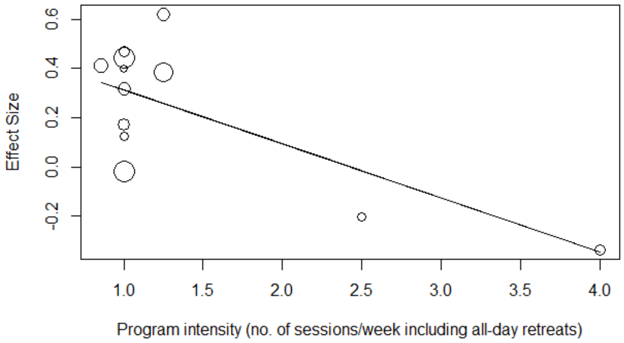
Meta-regression analysis results by MBP dose for between-group mindfulness effect sizes at 1-4 months follow-up compared to inactive and active controls

Compared to inactive control groups												
Dose	**Meta-regression model**						**Heterogeneity statistics**					
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	*SE* tau^2	Q_E	$p\ (Q_E)$
Total no. face-to-face sessions	0.08	0.09	[-0.11, 0.28]	0.88	.39	19	0.78	0.00%	1.11	0.42	134.53	<.001
Duration of a face-to-face session	-0.52	0.41	[-1.38, 0.35]	-1.27	.22	17	1.62	3.4%	1.15	0.46	115.36	<.001
Program length	-0.17	0.15	[-0.48, 0.15]	-1.13	.28	19	1.27	2.72%	1.04	0.4	125.73	<.001
Frequency of recommended practice	0.21	0.38	[-0.59, 1]	0.55	.59	18	0.31	0.00%	0.51	0.21	89.99	<.001
Duration of a recommended practice	-0.02	0.01	[-0.05, 0.01]	-1.61	.13	18	2.6	3.83%	0.46	0.2	95.1	<.001
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	*SE* tau^2	Q_E	$p\ (Q_E)$
Total amount of contact	0.03	0.03	[-0.03, 0.08]	1.02	.32	19	1.04	0.00%	1.09	0.41	126.42	<.001
Total amount of contact (excl. 0 hours)	0.02	0.03	[-0.05, 0.09]	0.72	.48	17	0.52	0.00%	1.26	0.5	113.69	<.001
Total recommended use of program	<-.001	0.01	[-0.02, 0.02]	-0.003	.97	18	<0.001	0.00%	0.54	0.22	92.65	<.001
Total actual use of program*	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity excl. retreats***	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity incl. retreats	1.18	0.77	[-0.46, 2.82]	1.53	.15	17	2.35	11.69%	1.05	0.42	98.57	<.001
Amount of contact/week	0.27	0.15	[-0.05, 0.58]	1.8	.09	19	3.24	14.37%	0.92	0.35	113.92	<.001
Amount of contact (excl. 0 hours)/week	0.29	0.18	[-0.1, 0.69]	1.59	.13	17	2.54	11.13%	1.06	0.43	102.04	<.001
Recommended use of program/week	0.07	0.08	[-0.1, 0.25]	0.91	.38	18	0.83	0.00%	0.5	0.21	86.28	<.001
Actual use of program/week*	-	-	-	-	-	-	-	-	-	-	-	-

Compared to active control groups												
Dose	**Meta-regression model**						**Heterogeneity statistics**					
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	$SE\ tau^2$	Q_E	$p\ (Q_E)$
Total no. face-to-face sessions***	-	-	-	-	-	-	-	-	-	-	-	-
Duration of a face-to-face session	-0.11	0.17	[-0.49, 0.28]	-0.63	.55	12	0.39	0.00%	0.01	0.03	11.19	.343
Program length**	-	-	-	-	-	-	-	-	-	-	-	-
Freq. of recommended practice***	-	-	-	-	-	-	-	-	-	-	-	-
Duration of a recommended practice	0.01	0.01	[-0.01, 0.02]	0.73	.48	12	0.54	0.00%	0.01	0.03	10.98	.359
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	$SE\ tau^2$	Q_E	$p\ (Q_E)$
Total amount of contact	0.02	0.01	[-0.002, 0.04]	1.96	.08	13	3.84	9.12%	0.01	0.03	9.96	.534
Total amount of contact (excl. 0 hours)	0.02	0.01	[-0.09, 0.05]	1.52	.16	12	2.31	0.00%	0.02	0.04	9.96	.444
Total recommended use of program	0.01	0.00	[<-0.001, 0.02]	2.18	.06	12	4.73	85.91%	0.002	0.03	7.92	.636
Total actual use of program*	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity excl. retreats***	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity incl. retreats	**-0.22**	**0.09**	**[-0.4, -0.03]**	**-2.55**	**.029**	**12**	**6.49**	**41.33%**	**0.01**	**0.03**	**7.44**	**.684**
Amount of contact/week	0.06	0.08	[-0.11, 0.22]	0.72	.49	13	0.51	5.21%	0.01	0.03	12.26	.345
Amount of contact (excl. 0 hours)/week	0.01	0.1	[-0.21, 0.22]	0.08	.94	12	0.01	0.00%	0.02	0.04	11.67	.308
Recommended use of program/week	0.04	0.04	[-0.04, 0.12]	1.1	.29	12	1.21	0.00%	0.01	0.03	10.49	.398
Actual use of program/week*	-	-	-	-	-	-	-	-	-	-	-	-

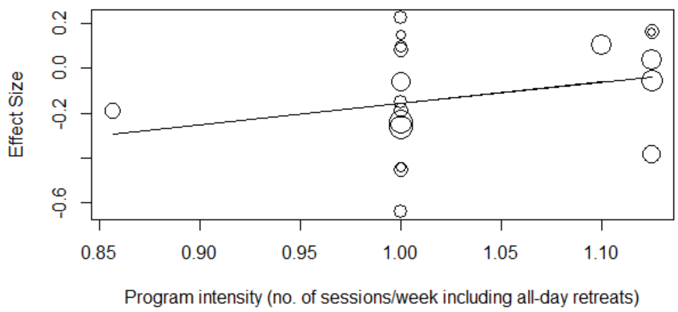
significant results in bold; *k<10, **all but one had the same score on this dose; ***all but 2 studies had the same score on this dose; d=effect size of the standardized regression coefficient, SE=standard error of the effect size, 95% CI= confidence intervals; t-value= test statistic of slope, p-value= significance level; k=number of studies; F-distribution= test for the overall model; R^2= percentage of heterogeneity accounted for, tau^2/τ^2= variance of the underlying true effect sizes; $SE\ tau^2$= standard error of tau^2; Q_E= between-study heterogeneity; $p(Q_E)$)=Q_E significance level.

Figure 5.7
Meta-regression plot for program intensity (including retreats) predicting mindfulness at 1-4 months follow-up compared to active controls



This finding did not remain significant when applying the Holm-Bonferroni correction since the significant *p*-value (*p*=.029) was above the corrected *p*-value (HB: .05/(9-1+1)=.006; .029>.006). Similarly, this finding was no longer significant when applying the FDR correction using the Benjamini-Hochberg procedure since p_{adj}>.05. For the step-by-step Benjamini-Hochberg FDR procedure, see Appendix Table 5.2.6. However, after closer inspection of the program intensity (including retreats) meta-regression plot, this finding appears due to one study with considerably higher intensity than other included studies and when removing this outlier, this finding was no longer significant (*k*=11; t=1.38; *d*=0.96; 95%C.I.: [-0.5, 2.42]; F(1, 17)=1.91; *p*=.19). Figure 5.8 displays the meta-regression plot with the outlier removed. This result therefore does not appear particularly robust since the significant finding was due to a single study without which the significant effect disappeared.

Figure 5.8
Meta-regression plot for program intensity (incl. all-day retreats) predicting mindfulness at 1-4 months follow-up compared to active controls with outlier removed



Not enough studies (*k*<10) were available to examine potential moderating effects of program type and study quality and thus test Hypotheses 4 and 5 for this result.

Numerical and visual results of non-significant dose-response relationships for the mindfulness outcome at 5-10 months follow-up compared to inactive controls (*k*<10 compared to active controls) are listed in Appendix Table 5.2.7 and Appendix Figure 5.2.8, respectively.

5.3 Results of Dose-Response Meta-Regression by Mindfulness Outcome Measures

As a reliability check, additional meta-regression analyses were also completed for mindfulness on a measure-by-measure basis. For purposes of conciseness, overall tables of measure-by-measure meta-regression results are added to Appendices. Two mindfulness measures had enough data available to complete measure-by-measure meta-regression analyses at post-program (*k*<10 at follow-up), namely the Five Facet Mindfulness Questionnaire (FFMQ) and the Mindful Attention Awareness Scale (MAAS). Results for each measure are presented separately.

5.3.1 Measure-by-Measure Meta-Regression with FFMQ

At immediately post-program, significant dose-response relationships were found for FFMQ mindfulness and doses relating to program intensity and weekly amount of facilitator contact in the hypothesized direction of greater doses predicting increased mindfulness compared to inactive controls. These results largely reflect findings from overall meta-regression analysis except for actual use of the program not significantly predicting mindfulness in the FFMQ meta-regression. However, this is likely due to the very small sample size (*k*=12) for this dose. Appendix Table 5.3.1 shows FFMQ meta-regression results compared to inactive and active controls where *k*≥10 and Appendix Figure 5.3.2 shows non-significant FFMQ meta-regression plots, both at post-program. Specifically, significant dose-response relationships in the hypothesized direction between the doses program intensity when excluding all-day retreats (*k*=28; t=3.57; *d*=1.18; C.I.:[0.5, 1.86]; F(1,16)=12.74; *p*=.001) and program intensity when including all-day retreats (*k*=28; t=4.24; *d*=1.17; C.I.:[0.6, 1.73]; F(1,16)=18.01; *p*<.001) were observed for FFMQ mindfulness both with large effect sizes and significant between-study heterogeneity thus reflecting findings of overall meta-regression analysis. Figures 5.9 and 5.10 show the meta-regression plots.

Figure 5.9
FFMQ meta-regression plot for program intensity (excluding retreats) predicting FFMQ mindfulness at post-program compared to inactive controls

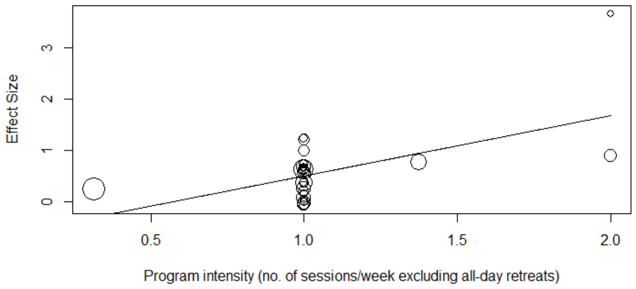
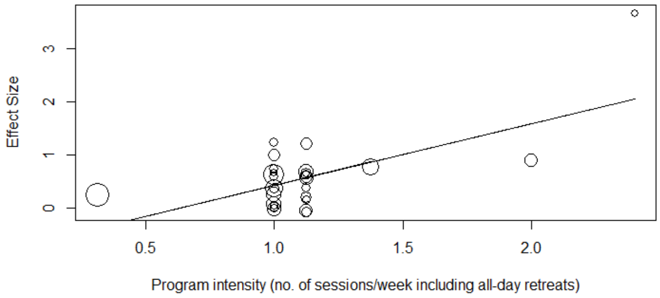


Figure 5.10
FFMQ meta-regression plot for program intensity (including retreats) predicting FFMQ mindfulness at post-program compared to inactive controls



Additionally, a significant dose-response relationship between amount of face-to-face facilitator contact per week, both when including (*k*=37; t=2.56; *d*=0.15; C.I.:[0.03, 0.27]; F(1,35)=6.56; *p*=.015) and excluding (*k*=28; t=3.15; *d*=0.28; C.I.:[0.1, 0.47]; F(1,26)=9.94; *p*=.004) zero hours of contact, and FFMQ mindfulness were observed in the hypothesized direction with small effect sizes, thus again reflecting results of the overall analyses. Figures 5.11 and 5.12 show the meta-regression plots.

Figure 5.11
FFMQ meta-regression plot for amount of face-to-face facilitator contact a week predicting FFMQ mindfulness at post-program compared to inactive controls

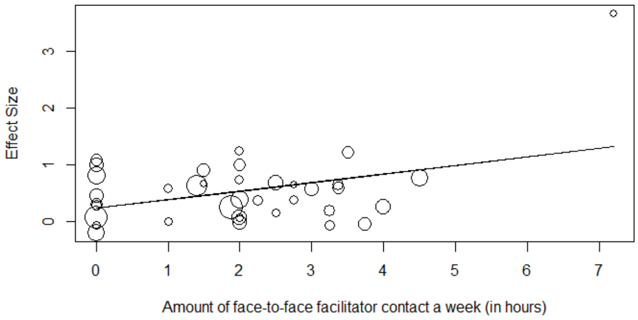
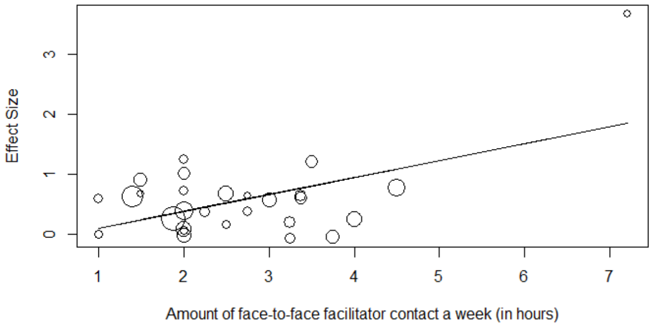


Figure 5.12
FFMQ meta-regression plot for amount of face-to-face facilitator contact (excl. no contact) a week predicting FFMQ mindfulness at post-program compared to inactive controls



When applying the Holm-Bonferroni correction, only the doses relating to program intensity (when excluding as well as including all-day retreats) predicting FFMQ mindfulness remained significant since the significant *p*-values were below the corrected *p*-values (see Appendix 5.3.3 for step-by-step Holm-Bonferroni correction). However, when applying the FDR correction using the Benjamini-Hochberg procedure, the dose-response relationships for the doses program intensity and facilitator contact a week (when excluding no contact) remained significant since p_{adj}<.05. The amount of face-to-face facilitator contact (when including no contact) was marginally significant (*p*=.057) and the lack of significance might be due to smaller sample sizes for the FFMQ measure. For the step-by-step Benjamini-Hochberg FDR procedure for FFMQ mindfulness compared to inactive controls see Appendix Table 5.3.4.

Next, moderator analyses were completed to examine whether significant FFMQ dose-response relationships were moderated by program type (MBSR/MBCT vs. other MBPs) or study quality (scores on RoB tool). Similar to the overall analysis, for the program intensity (when excluding all-day retreats) dose, the analysis with the moderator program type (MBSR/MBCT vs. other MBPs) was not completed since all MBSR/MBCT-type programs had the same score on this dose (intensity=1). For program intensity when including retreats, as with the overall analysis, no significant interaction effects were found with the moderator program type (see Tables 5.17 and 5.18 for interaction model and model test results).

Table 5.17
FFMQ meta-regression interaction model between program intensity (including retreats) and program type (MBSR/MBCT vs. other MBPs) for FFMQ mindfulness at post-program compared to inactive controls

	d	**SE**	**95% CI**	***t***	***p***
Intercept	2.1	2.1	[-2.17, 6.37]	1.02	.32
Type	-2.8	2.1	[-7.13, 1.52]	-1.34	.19
Program intensity (incl. retreats)	-1.53	1.93	[-5.52, 1.52]	-0.79	.44
Type x program intensity (incl. retreats)	2.73	1.95	[-1.3, 6.76]	1.4	.17

d=effect size; SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value=test statistic of slope, *p*-value=significance level.

Table 5.18
FFMQ meta-regression model fit for the interaction of intensity (including retreats) and program type (MBSR/MBCT vs. other MBPs) for FFMQ mindfulness at post-program compared to inactive controls

Test of the interaction:

Simultaneous test that all coefficients (excluding intercept) are zero

F=6.88; df=3, 24; p=.002; R^2=49.07%

Goodness of fit: Test that unexplained variance is zero

Tau^2=0.14; SE=0.07; Q_E=65.81, df=24, p<.001

F=Test of moderators; df=Degrees of Freedom; R^2=Amount of heterogeneity accounted for; Tau^2=Estimated amount of unexplained/residual heterogeneity; Q_E=Test for residual heterogeneity.

Regarding the moderator study quality, no significant moderating effect was found for program intensity when excluding retreats (see Tables 5.19 and 5.20 for interaction model and model test results).

Table 5.19

FFMQ meta-regression interaction model between program intensity (when excluding retreats) and study quality for FFMQ mindfulness at post-program compared to inactive controls

	d	**SE**	**95% CI**	***t***	***p***
Intercept	4.9	3.65	[-2.63, 12.42]	1.34	.19
RoB	-0.43	0.28	[-11.61, 3.11]	-1.19	.24
Program intensity (excl. retreats)	-4.25	3.57	[-11.61, 3.11]	-1.19	.24
RoB x program intensity (excl. retreats)	0.41	0.27	[-0.14, 0.97]	1.54	.14

RoB=Risk of Bias score; *d*=effect size; SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value=test statistic of slope, *p*-value=significance level.

Table 5.20

FFMQ meta-regression model fit for the interaction program intensity (when excluding retreats) and study quality for FFMQ mindfulness at post-program compared to inactive controls

Test of the interaction:

Simultaneous test that all coefficients (excluding intercept) are zero

F=5.1; df=3, 24; p=.01; R^2=38.82%

Goodness of fit: Test that unexplained variance is zero

Tau^2=0.17; SE=0.08; Q_E=69.21, df=24, p<.001

F=Test of moderators; df=Degrees of Freedom; R^2=Amount of heterogeneity accounted for; Tau^2=Estimated amount of unexplained/residual heterogeneity; Q_E=Test for residual heterogeneity.

For the dose program intensity (when including all-day retreats) a significant moderate interaction effect was observed for study quality with significant between-study heterogeneity (see Tables 5.21 and 5.22 for interaction model and model test results).

Table 5.21

FFMQ meta-regression interaction model between program intensity (when including retreats) and study quality for FFMQ mindfulness at post-program compared to inactive controls

	d	SE	95% CI	*t*	*p*
Intercept	5.61	2.91	[-0.39, 11.61]	1.93	.07
RoB	-0.49	0.23	[-0.96, -0.02]	-2.17	.04
Program intensity (excl. retreats)	-4.64	2.68	[-10.17, 0.89]	-1.73	.1
RoB x program intensity (incl. retreats)	**0.44**	**0.2**	**[0.02, 0.86]**	**2.18**	**.04**

RoB=Risk of Bias score; *d*=effect size; SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value=test statistic of slope, *p*-value=significance level; significant dose-response results in bold.

Table 5.22

FFMQ meta-regression model fit for the interaction program intensity (when excluding retreats) and study quality for FFMQ mindfulness at post-program compared to inactive controls

Test of the interaction:

Simultaneous test that all coefficients (excluding intercept) are zero

F=8.04; df=3, 24; p=.001; R^2=57.2%

Goodness of fit: Test that unexplained variance is zero

Tau^2=0.12; SE=0.06; Q_E=57.97, df=24, p<.001

F=Test of moderators; df=Degrees of Freedom; R^2=Amount of heterogeneity accounted for; Tau^2=Estimated amount of unexplained/residual heterogeneity; Q_E=Test for residual heterogeneity.

When running separate subgroup analyses with risk of bias scores below and above the median score, this result only remained significant in the expected direction for the group of low quality (high risk of bias) studies (k=15; t=4.32; d=1.59; C.I.:[0.79, 2.39]; F(1, 13)=18.62; p<.001). The significant dose-response relationship with program intensity (when including all-day retreats) therefore needs to be interpreted with caution since significant results are only found in low quality, high risk of bias studies, which have previously been found to have inflated effect sizes.

For the amount of weekly face-to-face facilitator contact doses (both including and excluding no contact), significant small to moderate interaction effects were found for the moderator MBP type (see Tables 5.23-5.26 for interaction model and model test results).

Table 5.23
FFMQ meta-regression interaction model between amount of contact a week and program type (MBSR/MBCT vs. other MBPs) for FFMQ mindfulness at post-program compared to inactive controls

	d	SE	95% CI	*t*	*p*
Intercept	0.84	0.3	[0.23, 1.46]	2.78	.01
Type	-0.69	0.34	[-1.38, -0.001]	-2.04	.049
Amount of contact/week	-0.15	0.12	[-0.4, 0.1	-1.25	.22
Type x amount of contact/week	**0.39**	**0.14**	**[0.11, 0.66]**	**2.84**	**.01**

d=effect size; SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value=test statistic of slope, *p*-value=significance level; significant dose-response results in bold.

Table 5.24
FFMQ meta-regression model fit for the interaction of amount of contact a week and program type (MBSR/MBCT vs. other MBPs) for FFMQ mindfulness at post-program compared to inactive controls

Test of the interaction:

Simultaneous test that all coefficients (excluding intercept) are zero

F=5.36; df=3, 33; p=.004; R^2=30.91%

Goodness of fit: Test that unexplained variance is zero

Tau^2=0.16; SE=0.06; Q_E=104.38, df=33, p<.001

F=Test of moderators; df=Degrees of Freedom; R^2=Amount of heterogeneity accounted for; Tau^2=Estimated amount of unexplained/residual heterogeneity; Q_E=Test for residual heterogeneity.

Table 5.25
FFMQ meta-regression interaction model between amount of contact (when excluding zero hours of contact) a week and program type (MBSR/MBCT vs. other MBPs) for FFMQ mindfulness at post-program compared to inactive controls

	d	SE	95% CI	*t*	*p*
Intercept	0.74	0.37	[-0.01, 1.5]	2.03	.05
Type	-1.32	0.48	[-2.31, -0.32]	-2.73	.01
Amount of contact (excl. 0 hours)/week	-0.11	0.14	[-0.41, 0.18]	-0.81	.43
Type x amount of contact (excl. 0 hours)/week	**0.54**	**0.17**	**[0.19, 0.9]**	**3.18**	**.004**

d=effect size; SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value=test statistic of slope, *p*-value=significance level; significant dose-response results in bold.

Table 5.26

FFMQ meta-regression model fit for the interaction of amount of contact (when excluding zero hours of contact) a week and program type (MBSR/MBCT vs. other MBPs) for FFMQ mindfulness at post-program compared to inactive controls

Test of the interaction:
Simultaneous test that all coefficients (excluding intercept) are zero
F=7.47; df=3, 24; p=.001; R^2=49.85%
Goodness of fit: Test that unexplained variance is zero
Tau^2=0.14; SE=0.07; Q_E=65.63, df=33, p<.001

F=Test of moderators; df=Degrees of Freedom; R^2=Amount of heterogeneity accounted for; Tau^2=Estimated amount of unexplained/residual heterogeneity; Q_E=Test for residual heterogeneity.

When completing separate subgroup analyses with MBSR/MBCT and other MBP types, these results only remained significant for the subgroup other MBPs with small to moderate effect sizes in the expected direction of increased mindfulness (including no contact studies: k=19; t=3.31; d=0.25; C.I.:[0.09, 0.41]; F(1, 17)=10.92; p=.004; excluding no contact studies: k=11; t=3.73; d=0.46; C.I.:[0.18, 0.74]; F(1, 9)=13.92; p=.005) but was not significant for MBSR/MBCT programs. This is again likely an artefact of standardised MBSR/MBCT programs having very limited spread in weekly amount of face-to-face facilitator contact, while other MBP types show a greater range of contact.

No significant interaction effect was found for neither of the weekly facilitator contact doses and study quality (see Tables 5.27-5.30 for interaction model and model test results).

Table 5.27

FFMQ meta-regression interaction model between amount of contact a week and study quality for FFMQ mindfulness at post-program compared to inactive controls

	d	**SE**	**95% CI**	***t***	***p***
Intercept	1.15	1.03	[-0.94, 3.23]	1.12	.27
RoB	-0.08	0.08	[-0.26, 0.11]	-0.84	.41
Amount of contact/week	-0.62	0.41	[-1.45, 0.21]	-1.53	.14
RoB x amount of contact/week	0.06	0.04	[-0.01, 0.13]	1.86	.07

RoB=Risk of Bias score; d=effect size; SE=standard error of the effect size, 95% CI= confidence intervals; t-value=test statistic of slope, p-value=significance level.

Table 5.28

FFMQ meta-regression model fit for the interaction of amount of contact a week and study quality for FFMQ mindfulness at post-program compared to inactive controls

Test of the interaction:

Simultaneous test that all coefficients (excluding intercept) are zero

F=3.81; df=3, 33; p=.02; R^2=16.5%

Goodness of fit: Test that unexplained variance is zero

Tau^2=0.2; SE=0.08; Q_E=117.16, df=33, p<.001

F=Test of moderators; df=Degrees of Freedom; R^2=Amount of heterogeneity accounted for; Tau^2=Estimated amount of unexplained/residual heterogeneity; Q_E=Test for residual heterogeneity.

Table 5.29

FFMQ meta-regression interaction model between amount of contact (excluding zero hours of contact) a week and study quality for FFMQ mindfulness at post-program compared to inactive controls

	d	**SE**	**95% CI**	***t***	***p***
Intercept	3.82	2.35	[-1.04, 8.68]	1.62	.12
RoB	-0.3	0.18	[-0.68, 0.08]	-1.65	.11
Amount of contact (excl. 0 hours)/week	-1.46	0.86	[-3.24, 0.32]	-1.69	.1
RoB x Amount of contact (excl. 0 hours)/week	0.13	0.07	[-0.01, 0.26]	2	.06

RoB=Risk of Bias score; *d*=effect size; SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value=test statistic of slope, *p*-value=significance level.

Table 5.30

FFMQ meta-regression model fit for the interaction of amount of contact (excluding zero hours of contact) a week and study quality for FFMQ mindfulness at post-program compared to inactive controls

Test of the interaction:

Simultaneous test that all coefficients (excluding intercept) are zero

F=5.08; df=3, 24; p=.01; R^2=36%

Goodness of fit: Test that unexplained variance is zero

Tau^2=0.18; SE=0.08; Q_E=76.05, df=24, p<.001

F=Test of moderators; df=Degrees of Freedom; R^2=Amount of heterogeneity accounted for; Tau^2=Estimated amount of unexplained/residual heterogeneity; Q_E=Test for residual heterogeneity.

Compared to active controls, no significant dose-response relationships were observed, and not enough studies (k<10) were available for the actual use of the program doses to complete this analysis. FFMQ meta-regression analysis compared to active controls at post-program where $k \geq 10$ thus reflects results of overall analyses.

5.3.2 Measure-by-Measure Meta-Regression with MAAS

For the MAAS measure, $k \geq 10$ studies were only available compared to inactive controls at post-program. No significant dose-response relationships were observed for the MAAS measure. However, sample sizes were reduced substantially for this analysis (k<1/4 of the overall sample); the lack of a significant finding is therefore likely a Type II error. Appendix Table 5.3.5 and Appendix Figure 5.3.6 display MAAS meta-regression results at post-program.

5.4 Discussion of Dose-Response Meta-Regression Findings for the Mindfulness Outcome

5.4.1 Summary of Findings

This chapter presented findings of the dose-response meta-regression for the mindfulness outcome. Significant dose-response relationships in hypothesised directions were found. Specifically, larger effect sizes at immediately post-program were predicted by greater actual use of the program (for both comparison groups) as well as by greater weekly face-to-face facilitator contact and MBP intensity doses compared to inactive controls, therefore confirming Hypothesis 2 for these doses at this timepoint. These findings remained largely significant when applying the False Discovery Rate control but were no longer so when Holm-Bonferroni was applied, which may be due to the large number of tests conducted. Given that the Holm-Bonferroni method has been argued by some to be overly conservative (Diz et al., 2011; Glickman et al., 2014), it seems plausible that these findings reflect true underlying dose-response relationships. The dose-response relationship between program intensity (when excluding retreats) at immediately post-program and mindfulness was significantly moderated by study quality with lower quality studies having a larger effect thus confirming Hypothesis 5. This suggests that this finding needs to be interpreted with caution since being due to studies which are at higher risk of bias. The significant dose-response relationship between weekly amount of face-to-face facilitator contact (when excluding no contact studies) was found due to programs other the MBSR/MBCT, which likely vary more greatly in this dose. Hypothesis 4 is therefore not confirmed. At 1-4 months follow-up, a significant dose-response relationship was also observed for intensity when including all-day retreats in the opposite to hypothesised direction. However, this finding was due to an outlier without which it was no longer significant. Measure-by-measure meta-regression analyses generally reflected findings of overall analyses, with any exceptions, particularly relating to the actual use of MBP dose, likely due to significantly reduced sample sizes. Dose-response findings for mindfulness are discussed with respect to previous research and theory next.

5.4.2 Discussion of Mindfulness Dose-Response Findings relating to Previous Research and Theory

As presented in Chapter 4, there were no robust significant dose-response relationships found for psychological distress outcomes. However, this was not the case for the mindfulness outcome. Participants in the included RCTs were mostly novice practitioners with the MBP often being the first time participants had become aware of mindfulness. For instance, participants were often excluded from studies if they had previously practiced mindfulness meditation (e.g. Gambrel & Piercy, 2015; Godfrin & van Heeringen, 2010). A dose-response effect being found for mindfulness, but not psychological distress outcomes, could be due to mindfulness being something that can be increased and learned continuously (Kabat-Zinn, 1990), particularly for those new to it. Additionally, we might expect the relationship with mindfulness to be strongest as it is theoretically the most proximal outcome since mindfulness has previously been identified as the key mechanism in MBPs (Gu et al., 2015).

If that is indeed the case, then there are theoretical reasons as to why we might expect greater face-to-face contact to be more helpful in relation to learning mindfulness. One could be group processes being helpful for learning a skill such as mindfulness (Segal et al., 2002; Yalom, 1983). The group process that is present in many MBPs is thought to be important since it gives a chance for people to ask questions and discuss problems with an experienced mindfulness practitioner and their peers (Kabat-Zinn, 2003). According to Kabat-Zinn (2003), mindfulness should therefore not be something to be learned via books only, it needs to be taught by someone who has experienced it; all other materials used in teaching are considered as supplementary in nature (Bruce et al., 2010).

Regarding significant dose-response relationships found between intensity of MBPs and mindfulness, this finding remained robust when controlling for both multiple comparison methods and therefore does not appear to be due to the inflation of family-wise alpha. However, this finding does need to be interpreted with some caution since it was found to be moderated by study quality within lower quality studies. If, however, this finding would be substantiated in future research, an explanation could be that when first designing MBSR, Kabat-Zinn (1982) argued that the program duration and thus the amount of facilitator contact and the intensity of sessions were some of its key elements to be helpful for individuals with chronic pain. The program was long enough for participants to understand the basic principles of self-regulation and to ascertain vital mindfulness skills, but short enough for participants not to become too dependent on the support of the group, thus aiming for participants to take responsibility for their own practice (Carmody & Baer, 2009). Previous research has advocated for the maximisation of exposure to practice a skill at its learning stage to develop the skill and process information (Breslin et al., 2002). Furthermore, a parallel can be drawn to previous dose-response research by Cuijpers et al. (2013), where greater intensity of psychotherapy interventions significantly predicted outcomes with the explanation that the relationship between facilitator and participants, which is necessary for learning to occur, may develop more quickly when delivery of psychotherapy is more intense (i.e. closer proximity between sessions to aid experiential learning), which could also apply to MBPs. Significant dose-response relationships between doses relating to contact and intensity and the mindfulness outcome were only found when compared to inactive, but not active, controls. This could relate to the possibilities that i) although MBPs may be effective for reasons theoretically given, there may be other interventions/programs which are effective on other grounds and might in fact have a similar level of effectiveness; ii) simply participating in an intervention/program and the expectations this generates can be found helpful. In other words, the question arises to what extent the mindfulness element of MBPs has an effect rather than simply taking part in a program/group. Any effects may thus be due to the common factors experienced when participating in an intervention/program rather than the specific ingredients of the intervention/program itself (=therapy effect) and comparing MBPs against active controls exposes this possibility; iii) effects could be Type II errors, especially where studies are not powered to the level needed to find an effect since when comparing any two or more active interventions/programs, the difference is likely to be smaller due the points raised above and thus greater power is needed (see Chapter 1, Section 1.4.1). The hypothesized dose-response relationships found at post-program did not remain significant at follow-up. On the contrary, greater MBP intensity (including all-day retreats) predicted decreased mindfulness compared to active controls at 1-4 months follow-up, although this effect disappeared when removing a higher-dose study. A possible reason for this finding may be that very intense MBPs may have potentially discouraged individuals, especially those new to mindfulness, to continue practicing after the end of the program.

However, despite significant dose-response relationships for contact and intensity doses, (although these may need to be treated with some caution) this was not found for primary doses such as program and session length and frequency and duration of recommended practice. This could be due to the composite variables of amount of contact and intensity being more nuanced to the learning processes mentioned above and because composite dose variables were assembled from a combination of different primary doses.

Additionally, although there was no significant dose-response relationship found for recommended use of the program, it was found that greater actual engagement with the program (both in-sessions and at home) predicted increased mindfulness compared to controls. However, this finding needs to be interpreted with caution since it was no longer significant when controlling for the inflation of family-wise alpha levels, either by Holm-Bonferroni or False Discovery Rate. However, it is worth noting that the failure to survive these corrections could be a Type II error particularly given that there was a substantially smaller sample size for this dose. Other researchers have found evidence for a relationship between amount of home practice and beneficial outcomes (e.g. Parsons et al., 2017) and if future dose-response meta-regressions with larger sample sizes were to replicate this analysis and finding, this corresponds with previous research and theory advocating for the notion that mindfulness as a skill takes time and commitment to learn (Kabat-Zinn 1990) since greater engagement with the MBP and its practices has been found to strengthen individuals' ability to switch from a doing to a being mode of mind (Williams 2008) and their ability to process attention and thus strengthen individuals' mindfulness skills (Malinowski, 2013).

5.4.3 Limitations and Implications

Small sample sizes were particularly an issue for the actual use of the program dose therefore limiting what can be concluded from analyses with this dose. Additionally, the high risk of social desirability and memory bias were problematic for this dose, limiting what can be concluded from findings. Not enough studies were available to complete moderator analyses with the actual practice quality rating tool to determine whether social desirability and memory bias influenced findings; this therefore needs to be noted as a limitation. Future RCTs should aim to reduce risk of potential memory and social desirability bias by adopting low-risk methods for actual practice reporting, for instance ensuring regular and anonymous (e.g. computerized) practice recording. Small sample sizes were not only an issue for some doses, but also for analyses at follow-up timepoints since some dose-response meta-regression analyses were unable to be completed due to small sample sizes. In particular, it was not possible to examine dose-response relationships at longer-term follow-up timepoints of a year or more to further understand whether dose effects persisted longer-term. This should be examined in future research once more studies with data for longer-term follow-up periods have been completed. An additional confounding factor was the interpretation of results at follow-up timepoints where other influencing aspects not known to the researcher could have altered participants' answers, such as the amount of mindfulness participants practiced since the program end. This information was not possible to extract from included studies and is a limitation of follow-up timepoints.

In addition to small sample sizes, caution needs to be exercised with regards to Type I and II errors as well as high levels of heterogeneity, which has already been discussed in Chapter 4 and also applies here. Similarly, limitations associated with including different outcome measures in the analyses apply for the mindfulness outcome; however, measure-by-measure findings were not substantially different to overall findings with any exceptions likely due to Type II errors.

Regarding further limitations with regards to the explorations of dose-response in MBPs possible in this review, although the potential effects of confounding variables have been controlled for as moderators, some confounding factors could not be examined. For instance, the incidence of adverse events could not be assessed since, as has been acknowledged recently by van Dam et al. (2018), this is currently not measured and reported in studies. It may therefore be prudent for future research to examine the effects of adverse events. Furthermore, some doses showed restricted variability which could have confounded findings (see Chapter 3). This was particularly the case for the program intensity doses, since the majority of MBPs had one session per week. However, although this dose showed lower variability than most other doses, it showed enough variability such that dose-response effects were found for the mindfulness outcome.

Next, the current meta-regressions included participants from a large number of different populations with different histories of psychological and physical health conditions, which were not possible to identify fully for all included participants. For psychological distress outcomes, this was addressed to an extent by controlling for baseline levels of outcomes and performing clinical significance analysis for the primary outcome depression. However, for mindfulness, it was not possible to reliably control for baseline levels and clinical significance analysis was not completed given that mindfulness does not measure a clinical outcome; caution with regards to interpretation thus needs to be exercised. Additionally, due to the heterogeneous sample of studies with participants suffering from various long-term physical health conditions, it was not possible to analyse each homogenous population group separately in a reliable meta-regression because of the very small sample sizes. However, according to Kabat-Zinn (2005), there may be a universality to human suffering regardless of its causes with individuals experiencing similar psychological processes such as worry and rumination. Through mindfulness, attention can therefore be brought away from focusing on adversity and support individuals in being in the present moment with a sense of compassion thus providing a universal aid across individuals with different roots for their suffering

(Feldman & Kuyken, 2011). Nevertheless, it is entirely possible and indeed seems probable that similar processes may result in dissimilar outcomes across different medical and psychological groups and therefore this inability to examine effects across different groups still needs to be noted as a limitation on what can reliably be concluded.

In the future, perhaps a dose-response meta-regression could be repeated once there is more literature, particularly for dose variables where sample sizes were small, and when actual use of the MBP is more reliably reported. Additionally, a dose-response meta-regression analysis for different population groups could be completed once this data is available to support more viable moderator analyses. Exploring common predictors for success in MBPs for individuals of different backgrounds/personalities would also be an interesting area to explore in future meta-analyses to further understand the type and dose of MBP that works best for certain individuals. Future meta-analytic investigations could also explore further moderators of significant dose-response relationships not covered in this review to address residual heterogeneity potentially influencing results, such as group versus individual mindfulness practice and amount of facilitator experience, provided sufficient information can be extracted from papers.

Next, although participants were allocated to program and control groups randomly, the different doses relating to MBPs were not randomly assigned to studies, and thus only predictability but not causation can be inferred from significant effects (Thompson & Higgins, 2001). This needs to be examined in future experimental studies that manipulate dose, which would allow for causal conclusions to be drawn. Finally, despite limitations associated with this dose-response meta-regression and the need to complete experimental RCTs where dose is manipulated, an argument can be made that the value of the dose-response meta-regression is that it can analyse a wider range of doses within a larger sample of different MBPs than would be possible within an individual RCT and is therefore complementary to such RCTs and forms an informative basis of the literature as it currently stands.

5.4 Chapter 5 Summary

This chapter presented results of the dose-response meta-regression for the mindfulness outcome including moderator analyses for significant outcomes. Additionally, meta-regressions by outcome measures were completed as a reliability check. At post-program, significant dose-response relationships between doses relating to actual use of the MBP, amount of weekly face-to-face facilitator contact and MBP intensity and mindfulness were found in expected directions, i.e. greater doses predicting increased mindfulness. Findings were largely mirrored by measure-by-measure analyses. Findings were discussed related to previous research and theory addressing the role of mindfulness as the key mechanism in MBPs. Methodological and interpretative limitations particularly regarding small sample sizes and heterogeneous populations included were discussed followed by implications for future research. The next Chapter (6) presents an experimental examination manipulating amount of mindfulness practice to address ambiguities found in the dose-response meta-regression.

CHAPTER 6

Effect of Length of Mindfulness Practice on Mindfulness, Depression, Anxiety, and Stress: A Randomised Controlled Experiment

6.1 Chapter 6 Overview

In Chapters 2-5, methods, results, and discussion of the dose-response meta-analysis and meta-regression were presented for depression, anxiety, stress, and mindfulness outcomes. Generally, no significant dose-response relationships were found between doses relating to mindfulness-based programs (MBPs) and psychological distress outcomes. Doses relating to actual MBP use, amount of facilitator contact, and program intensity significantly predicted larger positive effect sizes of mindfulness. To experimentally test dose-response meta-regression results in relation to practice amount, an experimental examination of the effects of different lengths of mindfulness practice on mindfulness, depression, anxiety, and stress was conducted, and is presented in this chapter. Firstly, the rationale for this study based on previous theory and research is explored. Methods employed are outlined next followed by the presentation of results showing significant effects particularly of shorter practices on outcomes. Finally, results are discussed in line with previous research and theory followed by study limitations and future research directions.

6.2 Rationale, Research Questions, and Hypotheses

Following on from the dose-response meta-regression (Chapters 2-5; Strohmaier, 2020), an experimental study was conducted to further understand dose-response effects in MBPs. In particular, it is important to explore dose-response effects related to mindfulness practice since formal mindfulness practice forms a central feature of MBPs and they are arguably predicated on an assumption that practicing mindfulness leads to an increase in trait and state mindfulness, which in turn improves wellbeing (Crane et al., 2017). However, there were several limitations to the conclusions which can be drawn from the dose-response meta-regression. First, although the majority of participants in included studies had only limited experience of mindfulness, participants overall differed in their level of prior or current engagement with mindfulness and information on previous or current personal mindfulness practice was not known for all MBPs. Therefore, for cohesiveness, novice practitioners are focused on exclusively in this study. A novice mindfulness practitioner is understood as someone who has limited previous experience of mindfulness and does not currently engage in regular mindfulness practice, in other words, is a “nonmeditator” (Chiesa, 2012, p. 6). Previous research has suggested that change in outcomes is generally more clearly evident in novices compared to long-term practitioners (Beccera et al., 2016). This has been evidenced across several studies, where taking part in MBPs had beneficial effects for novices (e.g. Economides et al., 2018; Khoury et al., 2013; Norris et al., 2018; Ribeiro et al., 2018). Second, studies in the dose-response meta-regression included participants from clinical and general populations; this diversity could have influenced outcomes since it raises important questions about whether there is an optimal length of mindfulness practice and whether effectiveness of different practices may depend on participant group. Third, it was impossible to examine dose-response effects for total length of formal mindfulness practiced since most studies did not log either the actual length of each formal practice or the total amount of formal mindfulness practiced.[13] This was usually cited as a limitation in MBPs. Many included studies also did not distinguish between formal practices and other exercises, which meant that only actual use of the program including all its elements, rather than isolated formal practice, could be examined in the dose-response meta-regression. Fourth, even where robust dose-response effects have been found, causation cannot be inferred due to the nature of meta-regression (Chapter 5). Therefore, this experimental study aimed to control for confounding factors as much as possible by experimentally manipulating and recording practice length isolated from other elements in novice practitioners from the general population to draw stronger conclusions regarding causation.

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[13] In terms of practice length, there are two considerations: i) the overall amount of time spent practicing and ii) the length of a single practice. Research is generally conducted on the overall amount of time spent practicing.

6.2.1 Theory of Mindfulness Practice

While MBPs all emphasize the importance of mindfulness practice, there is considerable variability in their mode of delivery and length of mindfulness practice MBPs teach and recommend for homework (Chapter 2; Strohmaier 2020). According to Kabat-Zinn (1990), the main skill learned in MBPs is mindfulness practice and practices outside of sessions are considered the most fundamental part; group sessions with a facilitator are viewed as secondary in nature with the purpose of being a learning aid to practice. From different theories introduced in Chapter 1, it can be inferred that, in general, greater overall amount of, and also often longer and/or regular practice is believed to be more beneficial (e.g. Malinowski, 2013; Shapiro et al., 2006; Teasdale, 1999; Williams, 2008). Similar to the dose-response meta-regression, Mode of Mind theory in particular applies here again, where individuals need to engage in continued mindfulness practice to be able to switch from a “doing” to a “being” mode of mind to improve mindfulness and disengage from rumination (Williams 2008). Additionally, Kabat-Zinn (1990) has argued that it can be helpful to think of mindfulness as a muscle that needs to be exercised through longer practices. Thus, it seems reasonable to expect greater length and overall amount of mindfulness practice to result in beneficial outcomes.

6.2.2 Previous Research on Longer Mindfulness Practice

In line with theory, some previous research has found beneficial effects of MBPs that recommend and include longer mindfulness practices such as Mindfulness-Based Stress Reduction (MBSR) and Mindfulness-Based Cognitive Therapy (MBCT) where participants complete the majority of practice at home (Kabat-Zinn; 1982; Segal et al., 2002, 2013; Chapters 1-2). For instance, in an MBSR study, greater levels of compliance to formal home practices were related to improved psychological and physical health-related symptoms in chronic pain patients (Rosenzweig et al., 2010). Similarly, in adults with different mental health difficulties, sustained longer mindfulness practice was significantly associated with increased mindfulness and reduced distress symptoms (Carmody & Baer, 2008). Furthermore, in a review, stand-alone mindfulness practices have been found to relate to improvements in mental and physical health (Blanck et al., 2018); this also included longer daily mindfulness practices of 30 minutes or more (e.g. Bell, 2015; Call et al., 2014; Chen et al., 2013). Research has also been completed focusing on how longer home practice can best be supported (Masheder et al., 2020).

However, as has been explored in Chapters 1 (Section 1.4.6.3) and 2 (Section 2.2.2), there are some challenges with more intense MBPs involving longer practices. Especially for novices, longer practices have been described as confusing (Desbordes et al., 2015) and novices, particularly when feeling stressed, have been found more susceptible to mind-wandering during longer practices, which in and of itself may not necessarily be a problem but can be accompanied by significant self-criticism (cf. Crosswell et al., 2020; Frewen et al., 2016). Additionally, qualitative research has suggested that longer mindfulness practices can be perceived as particularly challenging and have been cited as a reason to discontinue practice and thus can present a hindrance to engaging with mindfulness (e.g. participants stated: “I can’t do another body scan, it is way too long for me” (p. 1,658, lines 43-44) and “I realised I was getting bitter because I was not practicing” (p. 1,659, lines 65-66); Banerjee et al., 2017). These difficulties for novices to engage in longer practices may also be reflected in the relatively high attrition[14] from longer MBPs (e.g. Kabat-Zinn & Chapman-Waldrop, 1988; Kuyken et al., 2008), and discontinuation of practice after commencement of a program (Dobkin et al., 2011). This was also found in previous reviews where participants on average only completed around 60-70% of assigned home practices (DiMatteo, 2004; Lacaille et al., 2018) with reasons often being the large time commitment required (Chang et al., 2004; Shapiro et al., 2005).

6.2.3 Previous Research on Shorter Mindfulness Practices

In part to address the challenges of longer programs and practices, and to increase the accessibility of and engagement with MBPs, there has been a recent increase in research with lower-dose MBPs, which usually include shorter practices; these programs have been associated with beneficial outcomes on participants’ mindfulness and psychological distress (Spijkerman et al., 2016). These programs can be delivered face-to-face, but are also increasingly offered through self-help applications, including via apps and online websites (Jones et al., 2016). For instance, in a recent

[14] A relatively high level of attrition has also been found in self-help MBPs, which is reflective of other self-help programs; however, reasons for dropping-out are not generally given (cf. Cavanagh et al., 2014).

randomized controlled trial of 200 university students comparing an active control group with groups engaging in different mindfulness apps (Headspace, Smiling Mind), where participants were asked to practice mindfulness for 10 minutes a day for 10 days, a significant increase in mindfulness and significant positive effects on depression, adjustment to university, and resilience were found (Flett et al., 2019). Additionally, in an MBP study with nurses, brief (5-minute) mindfulness practices were found effective for mindfulness, self-compassion, stress, and burnout (Gauthier et al., 2015) and regularly engaging in brief (10-minute) practices was found to improve cognitive processing and attention (Moore et al., 2012). Correspondingly, a number of online research studies asking general population participants, who were largely novice practitioners, to engage in brief (5-10 minute) daily practices found similarly beneficial effects for trait mindfulness and psychological wellbeing (e.g. Bartlett et al., 2021; Cavanagh et at., 2018; Economides et al., 2018; Howells et al., 2016; Moore et al., 2020). Therefore, beneficial effects including for mindfulness and psychological distress have been identified for MBPs containing longer as well as shorter mindfulness practices.

6.2.4 Previous Endeavours for Measuring Effects of Mindfulness Practice Length

Some endeavours for assessing the effectiveness of different practice lengths have commenced (also see Chapter 2); however, findings are mixed. For example, in a previous meta-analysis of MBSR and MBCT programs, the amount of participants' self-reported formal mindfulness practice significantly correlated with positive outcomes, albeit with a small effect size (Parsons et al., 2017; Chapter 2). Similarly, Greenberg et al. (2018) has found a high-dose of home practice during an MBP to be associated with reduced stress in adults, whereas a low-dose was not. Additionally, higher frequency of formal mindfulness practice has been found to relate to increased wellbeing in participants with varying levels of mindfulness practice experience (Birtwell et al., 2019).

Alternatively, in a review on the utility of home practices in MBSR and MBCT, guidance, length and amount of mindfulness practice varied considerably between studies and only half the studies found positive effects associated with practice length (Lloyd et al., 2018). Additionally, in a recent MBP with novice practitioners examining adherence to mindfulness practices, changes in self-report measures of mindfulness, quality of life, depression and stress were not significantly associated with time spent practicing (Ribeiro et al., 2018). Aside from previous ambiguous results of the relationship between home practice length and outcomes, questions have been raised about their accuracy since amount of practice is often not reported authentically and different methods are used to record practice (Vettese et al., 2009) rendering the interpretation of actual mindfulness practice length difficult. This is particularly the case for practices completed outside of teacher-led sessions. For instance, in the above-mentioned review, only a small number of studies had examined the effects of home practices, and this was not assessed in a controlled way (Lloyd et al., 2018). Inaccuracies in actual mindfulness practice reporting may also arise due to social desirability and memory bias, often due to self-recording of practice, thus proving the extent to which participants were actually practicing mindfulness difficult (Strohmaier, 2020; Vettese et al., 2009; Chapter 3). Additionally, as for the dose-response meta-regression, causation cannot be inferred from reviews such as Parsons et al. (2017) and Lloyd et al. (2018). Another issue with home practices is that simply recommending or asking participants to practice for a certain time does not necessarily translate to participants doing so. This has been demonstrated in previous research by Berghoff et al. (2017), where participants who were asked to practice 20 minutes a day on average did not practice significantly longer than those who were asked to practice 10 minutes a day. Therefore, since beneficial effects have been identified for MBPs containing longer as well as shorter mindfulness practices, there is a need to experimentally explore the effect of different practice lengths isolated from other elements in an MBP. However, in the above-detailed research, practice length was neither experimentally manipulated by comparing longer and shorter practices nor was length controlled tightly and recorded comprehensively. Reliable conclusions on the actual observed effects of longer versus shorter practices can thus arguably not be drawn from research as it currently stands.

In practice, technology-based methods measuring the actual length of time someone engages in mindfulness meditation have commenced. This includes for instance biofeedback meditation cushions measuring the time someone spends sitting on them (e.g. levelsmeditation.com); however, at time of writing, this had not yet been tested empirically. Additionally, online and app-delivered MBPs often include automatic computerized measures on how often and how long a recording is played (e.g.

Bostock et al., 2018). However, even with these methods it is difficult to be confident regarding individuals' actual level of engagement with mindfulness practice. This highlights the need to examine dose-response effects in MBPs using experimental designs so that causal conclusions can be drawn, and in a manner that tightly controls and monitors the amount of mindfulness practice undertaken, i.e. by monitoring practice in-person, so that there can be greater confidence about the "dose" of practice.

6.2.5 Rationale for Choice of Outcomes Measured

For the present study, the same outcomes were included as for the dose-response meta-regression (Chapters 2-5; Strohmaier, 2020), namely depression, anxiety, stress, and mindfulness (for rationale of chosen outcomes, see Chapter 2, Section 2.2.7) to now determine causation in these same outcomes. Since in the dose-response meta-regression, significant, positive dose-response relationships have predominantly been found between MBP doses and mindfulness (Chapter 5), trait mindfulness was chosen as the primary outcome with depression, anxiety, and stress as secondary outcomes.

Furthermore, mindfulness practice has been considered essential for increasing individuals' trait and state mindfulness (Kabat-Zinn, 1990) which in turn act as mechanisms to facilitate changes in rumination and worry (Kiken et al., 2015; Nyklíček & Kuijpers, 2008). Therefore, trait and state mindfulness as mechanisms between practice length and psychological distress were also explored.

Finally, quality of mindfulness practice was considered since it could have altered effects of practice length on outcomes (Ribeiro et al., 2018). Previous research suggested that practice quality is important to control for in mindfulness practice to allow drawing more informed conclusions on practice effects (Goldberg et al., 2014).

6.2.6 Research Questions and Hypotheses

Given the ambiguous findings of effects of formal mindfulness practice in previous research, including in the dose-response meta-regression (Chapters 2-5; Strohmaier, 2020), the uncertainty relating to the accuracy of mindfulness practices completed in MBPs, and the question of causation, this seems important to explore further. Therefore, the impact of shorter and longer mindfulness practices delivered in-person was examined in this randomized controlled experiment to partially test the theory underpinning MBPs, to start to provide more evidence for mindfulness teachers and MBPs with regard to dose of practice (especially for novice practitioners), and to understand the isolated effect, as far as is possible, of different mindfulness practice lengths on outcomes.

Research questions in this study were: (1) Is practicing mindfulness over four sessions beneficial compared to a not-practicing control group? (2) Are longer or shorter mindfulness practices more effective compared to not-practicing controls?

Primary hypotheses to test in this study were that 1) engaging in four longer (20-minute) mindfulness practices results in significant positive changes in trait mindfulness, depression, anxiety, and stress outcomes compared to a not-practicing control group; 2) engaging in four shorter (5-minute) mindfulness practices results in significant positive changes in the same outcomes compared to not-practicing controls; and 3) there is a statistically significant difference between engaging in longer and shorter mindfulness practices with longer practices resulting in larger beneficial effect sizes of outcomes. The direction of the third hypothesis was based on the above-mentioned theoretical grounds for thinking that longer mindfulness practice would lead to higher levels of mindfulness and greater benefit as well as results of the dose-response meta-regression showing increased mindfulness with greater actual engagement in an MBP (including practice).

Additionally, the following secondary hypotheses were tested: 4) the total amount of time of mindfulness practice participants engage in predicts the degree of improvement; 5) change in trait and state mindfulness significantly mediates the relationship between mindfulness practice length and outcomes; 6) average mindfulness practice quality significantly interacts with mindfulness practice length with the combination of higher practice quality and practice length significantly predicting positive outcomes.

6.3 Method

6.3.1 Design

This study was a three-armed, single-blind, four-session randomised controlled experiment employing a mixed between- and within-subjects design. The three arms of the study were longer mindfulness practice (20[15] minutes) in Group 1, shorter mindfulness practice (5 minutes) in Group 2 and no mindfulness practice in Group 3 (active control). To ensure that there was no length of time effect across the three arms, participants also listened to extracts from an audiobook for 5 minutes (Group 1), 20 minutes (Group 2) and 25 minutes (Group 3); therefore, the exercises to be completed in each group lasted the same length of 25 minutes. An active control group was chosen as a further measure of enhancing group allocation concealment and to reduce performance bias. All participants were blinded to group allocation and were not aware of the exercises participants in other groups engaged in; participants were merely informed that other participants may engage in different exercises and were asked not to share the content of the sessions with anyone during the course of the study. Although employing a blinded investigator for a double-blinded experiment would have been preferrable, this was not possible since this research was part of a PhD with the PhD researcher conducting all stages of the research, including recruitment, randomisation, study facilitation, data collection and analysis.

6.3.2 Participants and Recruitment

A priori power analysis using G*Power for finding a small to medium effect (ES=0.25) with α=.05 and power of 0.95 (1–β) suggested a required sample size of 66, with 22 participants in each group. A small to medium effect has previously been found in similar studies and reviews (e.g. Basso et al., 2019; Khoury et al., 2013). Participants were recruited through convenience and snowball sampling. Recruitment was conducted through adverts at Canterbury Christ Church University, for students and staff (academic and non-academic). Adverts included general emails to university staff,

[15] Although 20 minutes is still a relatively short practice, it was thought that while longer than 5 minutes, 20 minutes would not be too challenging for novice practitioners, which was one of the reasons why practices of up to 40-50 minutes length, which can sometimes be the case in MBCT or MBSR programs, were not employed. Secondly, due to practical constraints and to ensure that practices were not too different for the length of time having a confounding effect, the longer practice was kept at 20 minutes.

the monthly staff newsletter, displaying posters around university campuses, advertisements on the undergraduate psychology student virtual learning environment and Research Participation Scheme, advertising the study at a school-wide staff Away Day and visiting postgraduate lectures. Participants were also informed that if they knew of someone who met the inclusion criteria and who might also be interested in taking part, to forward the study information and contact details of the PhD researcher. Advertising did not differ between the three groups since the study was advertised as "examining listening exercises, some of which may include some mindfulness practice".

A total of 71[16] individuals with limited previous meditation experience and no current personal mindfulness practice aged between 21-72 participated. Participants predominately identified as female (71.8%), white British or European (90.1%), and as students, academics, or other university staff. Participants were novice practitioners with limited previous mindfulness practice experience and no current personal mindfulness practice. The majority of participants had never previously engaged in mindfulness practice, with a small number (*N*=5) indicating that they had previously taken part in a mindfulness practice but did not currently practice; however, in all these cases, engagement in mindfulness had taken place between five and ten years prior to taking part in this research. As an incentive for taking part, participants were entered in a prize draw to win £50 online shopping vouchers. Psychology undergraduates could choose course credits for participating instead.

6.3.3 Procedure

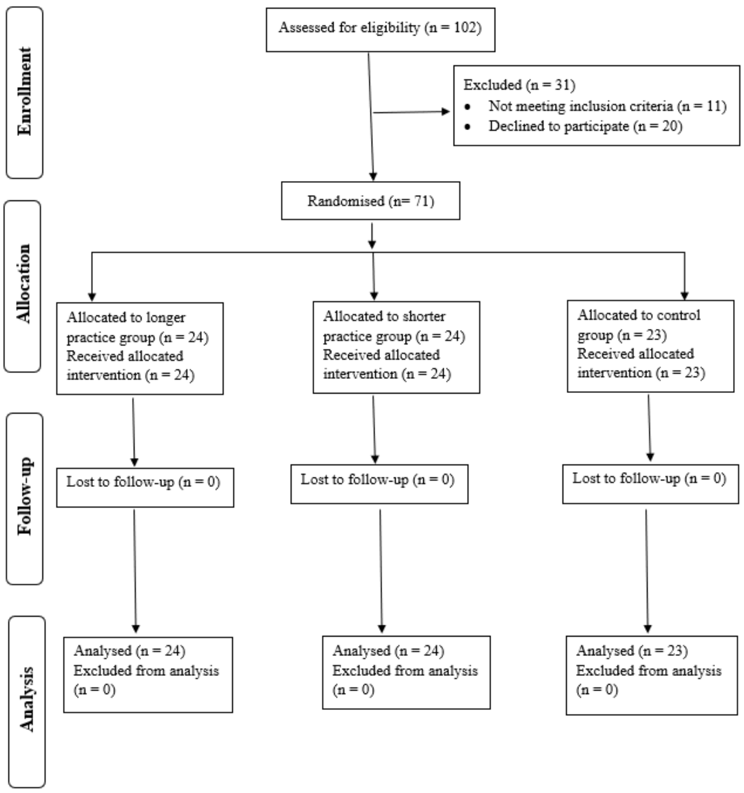
Participants were included in the study if they were i) aged over 18, ii) were novice mindfulness practitioners as defined as having limited mindfulness practice experience. Participants were precluded from participating if they were currently experiencing significant difficulties with their mental wellbeing and/or currently engaged in regular personal formal mindfulness practice or were participating in an MBP at the time of the study.

[16] The PhD researcher continued recruiting past 66 participants due to potential attrition rates given there were multiple sessions and going beyond the needed number would ensure meeting at least 66 across the final sample.

6.3.3.1 Randomization

After expressing an interest in taking part, individuals were sent the study information to provide more details (Appendix 6.3.1). Participants were randomised to groups using block randomisation with block sizes of six by the Microsoft Excel random number function RAND. Block sizes of six were applied since six is a multiple of the number of study groups (three) and is a factor of the overall expected sample size 66. Although randomly selected as opposed to fixed block sizes have been identified as more powerful in assigning participants to groups with equal probability thus reducing selection bias (Efird, 2011), block sizes of six were chosen to maximise statistical power by having balanced sample sizes across groups (Lachin, 1988), since smaller block sizes have been found to result in more balanced groups than larger block sizes (Efird, 2011). No participants dropped out from the study post-randomisation. Figure 6.1 depicts the CONSORT flowchart (Moher et al., 2001).

Figure 6.1
CONSORT flowchart of participant flow through the study



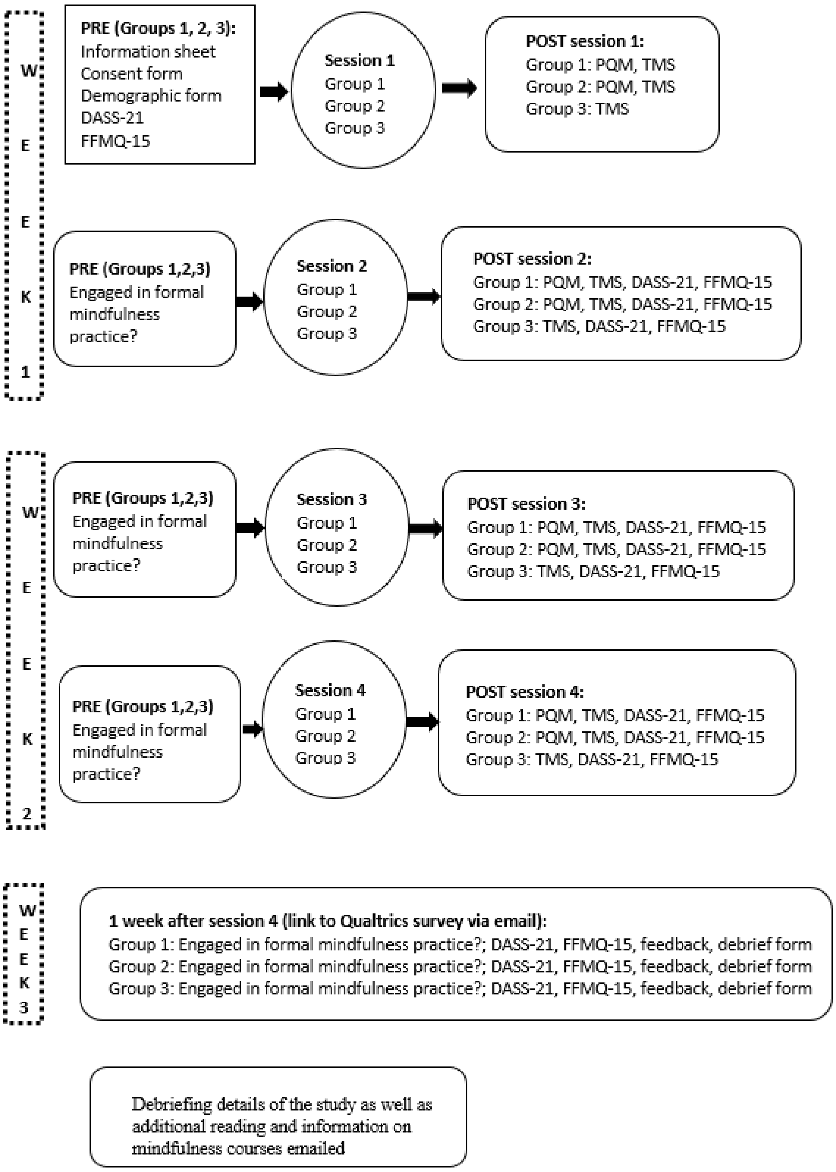
6.3.3.2 Sessions

After randomisation, participants were emailed times and locations of their four sessions, which were administered by the PhD researcher in a quiet seminar room over two weeks, with two sessions on consecutive days each week. All sessions were administered by the same facilitator to avoid therapist effects. To avoid findings being confounded by the time of day the sessions were held, the allocation of the time for each sitting and group affiliation was randomised for each session using the function `sample(1:3, 3, replace=F)` in R (The R Foundation for Statistical Computing, 2019), which gives three random integers between one and three without the same value occurring more than once. The order of groups was randomly assigned anew for each cohort of participants. The Latin Square method, which has previously been found effective (Williams, 1949) was not possible to apply here since there was an unequal number of groups (three) and sessions (four).

Sittings typically comprised four or five participants practicing mindfulness or listening to the audiobook at the same time. Occasionally individual participants had to be rescheduled because they were unable to attend at short notice. This was done since it felt important to ensure that participants attended a total of four sessions if possible, even if a session may not have been in a group. However, this approach was the exception, not the rule. The session content focused solely on listening to the audiobook and mindfulness practice and completing the questionnaires. There was no discussion of participants' experiences during meditation practice or audiobook listening, since the aim of the session was to examine the effect of practice length only. Therefore, a group facilitator rather than a qualified mindfulness teacher was present during sessions to further accommodate isolated practice. The facilitator took qualitative notes on what they observed during the sessions. At the end of the first session, participants from groups 1 (longer practice) and 2 (shorter practice) were given a handout detailing common experiences when practicing mindfulness. This included frequently asked questions (FAQs) that covered potential experiences that can arise during mindfulness practice and was adapted from Cavanagh et al. (2013; Appendix 6.3.2). Participants were asked to read through it in detail at the end of the first session and participants were reminded of the handout in the following sessions.

At the beginning of the study, participants were asked not to engage in any formal mindfulness practices outside of sessions, with this being defined as "listening to audio recordings or taking time to formally sit or lie down to practice mindfulness similar to how it is on the recordings". Participants were reminded of this at the end of every session. However, participants were made aware that if as a result of the mindfulness practices completed in the sessions, they felt more present in everyday life and were relating to present-moment experiences with more gentleness and kindness, that this was absolutely fine. Prior to every in-session practice, participants were informed that they could stop practicing at any point if they felt uncomfortable; when asked about discomfort, none reported feeling uncomfortable. In week three, participants were asked to complete the final measures online, before being debriefed. All participants in all three groups were provided details of ways they could access mindfulness-based programs and additional reading on mindfulness (Appendix 6.3.3 for debrief email) if they so wished after the debrief; participants were asked not to share information on study aims with others while the study was still in progress thus avoiding participants from future cohorts being made aware of the study aims. Figure 6.2 depicts the study flowchart showing process).

Figure 6.2
Flowchart of study process



Note: Order of groups and DASS-21 and FFMQ-15 measures counterbalanced for every session; DASS-21=21-item Depression, Anxiety and Stress Scale; FFMQ-15=15-item Five Facet Mindfulness Questionnaire; PQM=Practice Quality-Mindfulness; TMS=Toronto Mindfulness Scale.

6.3.4 Materials

6.3.4.1 Audio-Recordings

Mindfulness Practice Recordings.

The mindfulness practices were recorded by a qualified mindfulness teacher, Dr Fergal Jones (British Association of Mindfulness-Based Approaches listed teacher). The recording was a mindfulness of the breath meditation practice similar in style to those in MBSR and MBCT programs (Kabat-Zinn, 1990; Segal et al., 2013). Breath meditations are often considered as fundamental in mindfulness practice (Malinowski, 2013). The recordings were edited to create 20-minute and 5-minute practices, which included largely the same content. In the longer version, some instructions on returning attention to the breath were repeated and, in the shorter version, general instructions at the beginning of the practice relating to posture were shortened. Recordings never contained silence for longer than 30 seconds at a time since participants were novice practitioners who have previously been found more susceptible to mind wandering during practices and therefore are thought to require more guidance (Short et al., 2010). In previous research by Berghoff et al. (2017), the exact same recordings for both longer (20-minute) and shorter (10-minute) mindfulness practices were used with the only difference being 10-minutes of silence being added to the end of the longer meditation practice. However, this had been identified as one of the limitations in Berghoff's study since novice practitioner participants' attention was likely compromised without being reminded to return to the breath (Berghoff et al., 2017). See Appendix 6.3.4 for transcripts of the 20-minute and 5-minute recordings.

Audiobook Recordings.

The 5-, 20- and 25-minute audiobook excerpts were from Bryson's (2003) audiobook titled "A Short History of Nearly Everything". Excerpts covered non-fictional information on the history of the earth. Bryson's audiobook was chosen as a control condition since it is of similar nature to non-fictional presentations participants would have heard in their everyday lives such as through university lectures, podcasts, or news reports. Previous research has found non-fictional audiobooks and podcasts to be a helpful control group with participants from the general population (e.g. Basso et al., 2019). For ease of listening, the audio recordings were never more than four seconds shorter or longer than the required lengths of 5-, 20- or 25-minutes but did vary by a few seconds (never more than four) so that the last sentences were finished. Different excerpts were played for each of the four sessions.

6.3.4.2 Measures

Participants were asked to complete the following self-report measures at the timepoints detailed in Figure 6.2 (see Appendices 6.3.5 and 6.3.6 for consent and demographic forms, respectively). Where available and psychometrically robust, shorter versions of measures were selected, in light of research suggesting that participants may be less likely to accurately complete longer self-report questionnaires (Galesic & Bosnjak 2009). Piloting indicated that completing questionnaires should take no longer than 5-10 minutes in every session. To reduce risk of response bias, participants were told that there were no right or wrong answers and were requested to answer questions as honestly as possible. The respective titles of all questionnaires were removed to further minimise response bias (see Appendix 6.3.7 for self-report measures).

Depression, Anxiety and Stress Scale (DASS-21; Henry & Crawford, 2005).

The DASS-21 is divided into three 7-item subscales of depression, anxiety and stress with each subscale ranging in scores from zero to 21 with higher scores indicating greater symptomatology. The scale has shown convergent, discriminant and construct validity and high reliability in a large sample (N = 1,794) of the UK general population (Henry & Crawford, 2005). In the current sample, the total DASS-21 showed good reliability (Cronbach's α=.82). When examining subscales separately, this also showed good internal consistency for depression (α=.78), anxiety (α=.75) and stress (α=.74). Participants were asked to complete the DASS-21 at baseline, after sessions 2, 3 and 4, respectively, as well as online in week three (Time 5).

Five Facet Mindfulness Questionnaire (FFMQ-15; Baer et al., 2012).

At the same timepoints as the DASS-21, participants were asked to complete the FFMQ-15, which measures trait mindfulness, the primary outcome. This questionnaire has shown high levels of convergent validity before and after a mindfulness practice program as well as high reliability in a general population sample (Gu et al., 2016). For calculation of the total scale score, it is recommended to omit the observe subscale items (Baer et al., 2012; Gu et al., 2016) resulting in scores of trait mindfulness ranging between 12 and 60. Each FFMQ-15 subscale ranges between 3 and 15. With the current sample, the total FFMQ-15 showed good internal consistency (Cronbach's α=.84). All but one of its subscales also showed good internal consistency (observe: α=.74; describe: α=.78; acting with awareness: α=.65; non-judging: α=.77; non-reactivity: α=.8). The FFMQ-15 was chosen as the measure of trait mindfulness instead of the trait-Toronto Mindfulness Scale (trait-TMS), since the trait-TMS is seen as being more difficult to complete for novice practitioners and arguably has slightly weaker psychometric properties than the FFMQ (cf. Ireland et al., 2018).

Practice Quality-Mindfulness (PQ-M; Del Re et al., 2013).

Immediately after each of the four mindfulness practices, groups 1 (longer practice) and 2 (shorter practice) were asked to complete the 6-item PQ-M, which is a visual analogue scale where participants are asked to indicate the quality of their practice ranging from 0-100%. At time of writing, to the best of the PhD researcher's knowledge, this was the only tool available to measure practice quality. The PQ-M has shown adequate reliability, and convergent and predictive validity (Goldberg et al., 2014). In the current sample, the PQ-M showed moderately acceptable internal consistency (Cronbach's α=.68). Since this practice quality measure is designed to be completed immediately after a mindfulness practice (Del Re et al., 2013), participants in groups 1 and 2 always listened to the audiobook prior to completing mindfulness practices in every session.

Toronto Mindfulness Scale (TMS; Lau et al., 2006).

All groups completed the TMS, a 13-item questionnaire assessing state mindfulness with the two subscales curiosity (six items) and decentring (seven items). The curiosity subscale ranges from 0-24, the decentering subscale from 0-28, and the total TMS score from 0-52, with higher scores indicating greater curiosity, decentering, and overall state mindfulness, respectively. This scale has shown good reliability and incremental as well as criterion validity in participants with and without previous meditation experience from the general population (Lau et al., 2006; Medvedev et al., 2017). In the current sample, the total scale TMS showed high internal consistency (Cronbach's α=.94) as did subscales (curiosity: α=.89; decentering: α=.88). The TMS was positioned immediately after the PQ-M to assess current state mindfulness as soon as possible after the mindfulness practice (Groups 1 and 2) or audiobook listening exercise (Group 3), followed by the DASS-21 and FFMQ-15 measures.

Formal Practice Check.

At the start of each session and the follow-up survey in week three, participants were given a single question asking whether they had engaged in formal mindfulness practice since the last session and, if so, to provide details (see Appendix 6.3.8). This acted as a precaution to ensure, as much as possible, that the only formal mindfulness practices participants engaged in were during the sessions to tightly control amount of formal mindfulness practiced.

Qualitative Feedback.

At the end of the final online survey (see Figure 6.2), participants were asked three open-ended questions on 1) the effect participating in this study had on them (if any); 2) what participants felt led to this potential effect; and 3) any additional feedback about the study.

6.3.5 Ethical Considerations

Full ethical approval was granted by the Salomons Institute for Applied Psychology ethics panel (Appendix 6.3.9). Observations were addressed and any changes were communicated with the Head of the panel and accepted (see Appendix 6.3.10). All participants gave informed consent. In the information, consent and debrief forms, participants were reminded of their right to withdraw without a reason and had ample opportunity to contact the researcher with any questions before and during the study. Participants were provided with a list of useful contacts should they experience any distress or discomfort as a result of participating in this study, and the details of an independent person, who had no association with the study or the PhD research, should they wish to complain about any part of the study (see information and debrief forms, Appendices 6.3.1 and 6.3.3). Participants were informed that their data would be handled confidentially and that their identifying information would be removed prior to analysis since this was only used to match participants' answers and administer incentives. Prior to data collection and after ethical approval was granted, the study was preregistered on the trial registration site ClinicalTrials.gov (https://clinicaltrials.gov/ct2/show/NCT03797599?term=Study+Examining+the+Effects+of+Mindfulness+and+similar+audio-guided+exercises&rank=1).

6.3.6 Data Analyses

All statistical analyses were completed using the IBM Statistical Package for Social Sciences (SPSS) version 24.0 (IBM Corp., 2016) or the R software for statistical computing versions 3.6.2-4.1.0 (2019-2021). Initially, screening of the dataset, testing assumptions for normality and homoscedasticity as well as descriptive statistics were conducted to gain a better understanding of the sample. Additionally, between group, one-way Analyses of Variance (ANOVA) were conducted to examine whether there were significant differences between scores on outcome variables between groups at baseline which could have influenced subsequent results.

6.3.6.1 Analysis of Variance (ANOVA) to test Primary Hypotheses

As pre-specified in the trial registration as the primary analysis, the statistical analysis focused on timepoints 1 (baseline) and end of study (Time 5). To test the three primary hypotheses, three (group) by two (timepoint: baseline vs. end of study) mixed ANOVAs were performed in SPSS on the outcomes trait mindfulness, depression, anxiety and stress. For some variables, there was some minor deviation from normality evident in residuals as well as slight heteroscedasticity (see Section 6.4.1). Therefore, all analyses were repeated using robust methods to check whether these deviations influenced results. Since at the time, there was not a robust option for conducting mixed ANOVA available in SPSS (Field, 2018), robust mixed ANOVAs were completed in R using the package '`WRS2`', which performs mixed ANOVAs on trimmed means using the function `bwtrim()` (Mair & Wilcox, 2019). Findings from robust methods did not meaningfully differ from the standard method.

Significant interactions were decomposed by running separate one-way ANOVAs for each group and for the two timepoints. Following the former, significant main effects of group were further decomposed by the Tukey, Bonferroni and Games-Howell tests, with all three being included as each method has differing strengths and limitations. The Bonferroni test has previously been considered as conservative but powerful in controlling for Type I errors (Diz et al., 2011) and the Tukey test has shown to be less conservative but more powerful in an analysis with a larger number of comparisons between groups (Lee & Lee, 2018). The Games-Howell test was also run, since it has been shown to be accurate with unequal population variances (Toothaker, 1993; Lee & Lee, 2018). In every case, all three post-hoc tests were in agreement as to whether a finding was significant or not. Cohen's *d* was calculated to determine effect sizes for i) between-group and ii) within-group ANOVAs using the method as described by Lakens (2013).

Robust ANOVAs were completed for both, the between and within group ANOVAs, again using the `WRS2` package and the functions `t1way()` for trimmed means, `lincon()` for post-hoc results on trimmed means (between group ANOVA) and the corresponding bootstrap functions `mcppb20()` for between ANOVAs and `t1waybt()` for repeated measures ANOVAs. ANOVA findings from the robust methods largely did not meaningfully differ from the standard methods. For purposes of transparency, both standard and robust methods are presented in the results.

6.3.6.2 Regression, Mediation and Moderation Analyses to test Secondary Hypotheses

To test Hypothesis 4, bootstrapped linear regression analyses explored whether mindfulness practice length predicted trait mindfulness, depression, anxiety, and stress at post-study, while controlling for baseline levels of respective outcomes. To examine whether any relationship between total practice length and outcomes at Time 5 was significantly mediated by trait or state mindfulness change calculated as baseline scores subtracted from Time 5 scores (Hypothesis 5), bias-corrected bootstrapped mediation analyses were completed with either change in trait or state mindfulness as the mediator. Changes in the two TMS subscales, curiosity and decentering, were added as separate mediators in different mediation models. Mediation analyses were conducted using model 4 of the PROCESS macro version 3.4 by Hayes (2019) with bootstrapping set to 5000 and controlling for scores on the respective outcome measure at baseline. To test Hypothesis 6 that an interaction effect exists between practice length and practice quality when predicting post-study outcomes, moderation analyses were completed using model 1 of PROCESS, again controlling for baseline levels of the respective outcome and with bootstrapping set to 5000. Assumptions for secondary hypotheses were checked (see Section 6.4.1).

Analyses for testing Hypotheses 4, 5 and 6 had been planned on the assumption that the total amount of mindfulness practiced would likely vary substantially within groups, due to participant attrition. However, unexpectedly all participants completed all four sessions. Therefore, moderation analyses included practice length as the dichotomous variable longer vs. shorter practice, and the regression and mediation analyses included the three groups as proxies for different lengths of mindfulness practice with separate regression and mediation analyses for each pair (longer vs. shorter practice; longer practice vs. control; shorter practice vs. control) as dichotomous predictor variables. For the mediation analyses, this resulted in a large number of comparisons (n=36) moving from a continuous predictor to an ordinal predictor with three levels analysed in separate models. To control for possible Type I errors, the Bonferroni correction was applied. Following the correction, significant results needed to have an alpha equal to or below α=.001 (99.9% C.I.). Therefore, the more stringent 99.9% confidence intervals ($p \leq 001$) were entered for mediation analyses. However, Bonferroni-type corrections have previously been criticized as being too strict and for reducing power thus increasing the possibility of Type II errors (Diz et al., 2011; Nakagawa, 2004). Therefore, for mediation analyses, both uncorrected (95% C.I.; p<.05) as well as corrected (99.9% C.I.; $p \leq .001$) results are presented, as recommended (Clark-Carter, 1997).

6.3.6.3 Content Analysis of Qualitative Feedback

Qualitative feedback data was subject to basic content analysis following guidelines by Weber (1990), where each participant's response was coded using emergent coding and then assigned to broad categories/themes. Frequency of responses for each code and category for each of the three groups were then counted and compared between groups (Stemler, 2000). Data were coded by the PhD student and codes were assigned to broad categories. A random third of answers (33.8%; n=24 (n=8 per question)) for each group were independently assigned to codes by Dr Fergal Jones (first supervisor) using the same coding frame and subject to inter-rater reliability analysis using Cohen's kappa (McHugh, 2012).

6.4 Results

6.4.1 Data Screening and Testing Assumptions

Firstly, the dataset was tested for outliers. None of the outcome variables, neither at baseline nor at Time 5, had a z-score above the threshold of $z>3.29$ thus confirming the absence of univariate outliers (Tabachnick & Fidell, 2014). Multivariate outliers were examined with Mahalanobis Distance (MD), which assesses the potential influence of data by their difference to the independent variable (group). MD values were then compared to a chi-square distribution with equal degrees of freedom to determine the probability of a datapoint being a multivariate outlier ($p\leq.001$). Mahalanobis Distance values were low (largest value: MD=6.63), which is not considered to be a concern in a dataset of this size (Barnett & Lewis, 1978). The probability associated with MD values was $p>.001$ for all participants and thus no multivariate outliers were identified in this dataset. Since neither univariate nor multivariate outliers were found, no data were removed. Missing data analyses were not conducted since this dataset did not include any missing data.

Next, tests of assumptions of normality in the data were completed for each analysis and each group using the residuals, which were generated for ANOVA to determine the difference between predicted and observed values. Visual inspection of histograms for standardised residuals showed slight deviation from normality in the distribution of scores especially for depression, anxiety and stress outcomes. For anxiety and stress outcomes at Time 5, standardised residuals showed values above the acceptable threshold of 3.29 (see Appendix Figure 6.4.1 for histograms on standardised residuals for outcomes). Additionally, Kolmogrov-Smirnov and Shapiro-Wilk tests of normality for standardised residuals were completed at baseline and Time 5 (post-study) both for the overall sample as well as for outcomes for each of the three groups separately. However, the Kolmogrov-Smirnov test has previously been identified as quite stringent (Steinskog et al., 2007) and thus needs to be interpreted with caution. Normality tests showed significant deviation from normality again particularly for depression, anxiety and stress outcomes at baseline and Time 5 (post-study) as well as for several FFMQ-15 subscales. Significant deviations from normality were mostly observed for the mindfulness practice groups rather than the control group, especially at Time 5 (post-study).

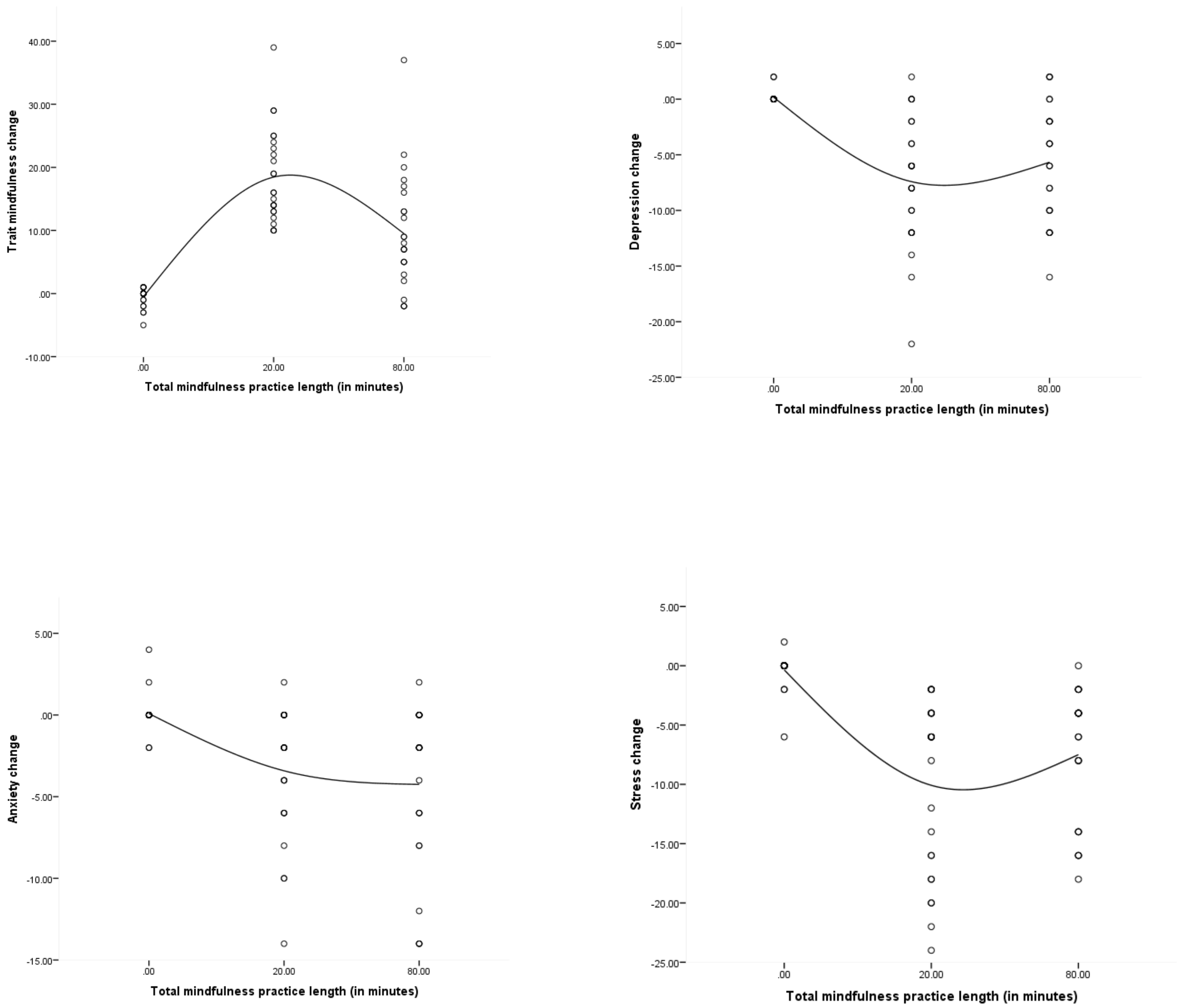
Appendix 6.4.2 shows the SPSS outputs of normality tests on standardised residuals both overall and by participant group. Regarding assumptions for secondary hypotheses, measures of state mindfulness (including subscales) and practice quality were relatively normally distributed (Appendix Figure 6.4.3) although this was inconsequential given that a bootstrapping approach was taken which does not require a normal distribution since it creates its own distribution based on that of the data and is thus considered robust in non-normally distributed data (Hayes, 2009).

Furthermore, assumptions for homogeneity and sphericity needed to be explored since a mixed (between and within subjects) design is employed in this study. The assumption of homogeneity of variance was examined using Levene's test (Appendix 6.4.4). Levene's test has previously been criticised for being inaccurate and therefore needs to be treated with caution (Nordstokke & Zumbo, 2007), however, robust methods were employed in the analyses. At baseline, variances of scores were generally not significantly different for groups for all outcomes except stress. At post-study (Time 5), variances were significantly different for all outcomes. However, it is expected there to be more variance in the intervention groups (longer and shorter mindfulness practice) than in the control group. Sphericity was not an issue in this study since only two timepoints of within participant data were examined, namely baseline (Time 1) and post-study (Time 5). The above introduced robust methods (Section 6.3.6) have been found effective if assumptions for normality and homoscedasticity are not met (Mair & Wilcox, 2019).

Finally, for regression, mediation, and moderation analyses the assumption of linearity needed to be assessed. This assumption was violated since shorter practices showed larger effects on outcomes than longer practices; this resulted in an inverted U-shaped effect of practice length for trait mindfulness and FFMQ-15 subscales. A U-shaped effect of practice length was observed for depression, anxiety and stress. Figure 6.3 displays the change in the primary outcome trait mindfulness and secondary outcomes depression, anxiety and stress for total practice length in minutes (0 minutes for control group, 20 minutes for shorter practice group;80 minutes for longer practice group). Distribution graphs for FFMQ-15 subscales are listed in Appendix Figure 6.4.5.

Figure 6.3

Inverted U-shape of practice length trait mindfulness change (top left), depression change (top right), anxiety change (bottom left) and stress change (bottom right)



For regression, mediation, and moderation analyses, since the assumption for linearity was not met and length of practice was not different other than group assignment (see Section 6.3.6.2), separate dichotomous predictor variables were created, those being longer vs. shorter practice, longer practice vs. control and shorter practice vs. control. As a result, there was only one predictor variable (practice length) for each regression, mediation and moderation model and multicollinearity in predictor variables thus did not need to be considered as an issue.

6.4.2 Descriptive Statistics of Demographics and Group Differences at Baseline and Post-study

Table 6.1 shows demographic details of participants for the overall sample and by groups. At baseline, there were no significant differences in demographics between the three groups, suggesting randomization was successful. As recorded at every session and in the end-of-study online survey, none of the participants in any of the groups engaged in formal mindfulness practices outside of sessions over the course of the study.

Table 6.1
Demographic information by group and group comparison at baseline

	Whole sample	Longer practice	Shorter practice	Control	Group comparison
N	71	24	24	23	
Age M (SD)	39.38 (14.16)	36.58 (12.54)	41.17 (14.31)	40.43 (15.69)	F(2, 68)=0.72 p=.49
Gender (% female)	71.8%	70.8%	83.3%	60.9%	χ^2=2.95 p=.23
Ethnicity N (%)	White: 60 (90.1%) Black: 3 (4.2%) Asian: 2 (2.8%) Other: 2 (2.8%)	White: 19 (87.5%) Black: 1 (4.2%) Asian: 1 (4.2%) Hispanic: 1 (4.2%)	White: 19 (87.5%) Black: 1 (4.2%) Asian: 1 (4.2%) Arab: 1 (4.2%)	White: 22 (95.7%) Black: 1 (4.3%)	χ^2=7.27 p=.7
Occupation *N* (%)	Student: 32 (45.1%) Lecturer: 15 (21.13%) Administrator: 9 (12.67%) Librarian: 5 (7.04%) Teacher: 2 (2.82%) Manager: 2 (2.82%) Other: 6 (8.45%)	Student: 12 (50%) Lecturer: 9 (37.5%) Administrator: 2 (8.3%) Counsellor: 1 (4.2%)	Student: 8 (33.3%) Lecturer: 3 (12.5%) Administrator: 6 (25%) Librarian: 3 (12.5%) Teacher: 1 (4.2%) Writer: 1 (4.2%) Manager: 2 (8.3%)	Student: 12 (52.2%) Lecturer: 3 (13%) Administrator: 1 (4.3%) Librarian: 2 (8.7%) Teacher: 1 (4.3%) Research fellow: 1 (4.3%) Director 1 (4.3%) IT analyst: 1 (4.3%) Retired: 1 (4.3%)	χ^2=5.16 p=.27

N=Number of participants; M=Mean; SD=Standard Deviation

Table 6.2 shows data for each outcome, both at baseline and post-study (Time 5). Baseline scores are representative of normative data from non-practicing general population samples (Gu et al., 2016; Henry & Crawford, 2005).

Table 6.2
Outcome data at baseline(pre) and Time 5 (post) for longer practice, shorter practice, and control

Outcome	**Longer practice**		**Shorter practice**		**Control**	
	Pre M (SD)	Post M (SD)	Pre M (SD)	Post M (SD)	Pre M (SD)	Post M (SD)
Trait mindfulness (FFMQ-15 total)	35.5 (7.98)	45 (4.55)	34.2 (6.74)	52.67 (2.06)	38 (6.73)	37.48 (6.87)
FFMQ-15 Observe	8.21 (2.32)	10.79 (1.93)	8.13 (2.51)	12.79 (1.53)	8.39 (2.84)	8.3 (2.93)
FFMQ-15 Describe	9.63 (2.96)	11.46 (1.93)	8.63 (2.14)	12.92 (1.14)	9.52 (2.23)	9.57 (2.09)
FFMQ-15 Act Aware	8.04 (1.97)	10.71 (1.33)	8.46 (2.48)	13.58 (0.93)	8.57 (1.95)	8.74 (1.98)
FFMQ-15 Non-Judge	10.5 (3.4)	12.71 (2.12)	9.71 (2.48)	13.29 (1.4)	11.22 (2.37)	11.61 (4.9)
FFMQ-15 Non-React	7.33 (2.57)	10.29 (1.55)	7.42 (2.04)	12.29 (0.95)	8.7 (2.58)	8.43 (2.5)
DASS-21 Depression	7.67 (5.86)	2 (2.28)	8.25 (6.05)	0.83 (1.17)	4.87 (4.55)	5.04 (4.74)
DASS-21 Anxiety	6 (6.07)	1.75 (1.98)	4.25 (4.61)	0.83 (1.31)	5.57 (6.06)	5.65 (5.96)
DASS-21 Stress	11.67 (5.71)	4.17 (2.43)	11.83 (7.48)	1.75 (1.59)	10.87 (5.15)	10.52 (5.3)

M=Mean; SD=Standard Deviation; FFMQ-15=Five Facet Mindfulness Questionnaire; DASS-21=Depression Anxiety Stress Scale

6.4.3 Group by Time Comparisons (Testing Primary Hypotheses)

As can be seen from Table 6.3, group by time (baseline vs. end of study) mixed ANOVAs revealed significant group by time interactions for trait mindfulness, depression, anxiety and stress. Subsequent one-way ANOVAs showed that these interactions arose since at baseline the groups did not significantly differ while at post-study, they did. Significant interaction effects between group and time were also observed for FFMQ-15 subscales. Group by time interaction effects did not meaningfully differ for standard and robust methods.

Table 6.3
Group (longer vs. shorter vs. control) by time (baseline vs. end of study) ANOVA results using both standard and robust methods (mixed ANOVA on trimmed means) for all outcomes, including FFMQ-15 sub-scales

Outcomes		**Standard**	**Robust**
Trait mindfulness (FFMQ-15 total)	group	F(2,68)=7.44**; part. η^2=0.18	F(2, 28.02)=5.35*
	time	F(1,69)= 125.37*** part. η^2=0.65	F(1, 30.94)=114.13***
	group*time	F(2,68)=44.7*** part. η^2=0.57	F(2, 21.32)=65.91***
FFMQ-15 Observe	group	F(2,68)=52.4** part. η^2=0.15	F(2, 28.75)=4.09*
	time	F(1,69)=81.16*** part. η^2=0.54	F(1, 31.3)=106.04***
	group*time	F(2,68)=26.71*** part. η^2=0.44	F(2, 21.99)=56.92***
FFMQ-15 Describe	group	F(2,68)=3.01 part. η^2=0.08	F(2, 27.7)= 1.31
	time	F(1,69)=56.8*** part. η^2=0.46	F(1,33.7)= 43.34***
	group*time	F(2,68)=20.28*** part. η^2=0.37	F(2, 24.38)= 22.56***
FFMQ-15 Act Aware	group	F(2,68)=15.89*** part. η^2=0.32	F(2, 27.2)=12.43***
	time	F(1,69)=103.97*** part. η^2=0.61	F(1, 37.48)=78.33***
	group*time	F(2,68)=29.91*** part. η^2=0.47	F(2, 27.14)=31.62***
FFMQ-15 Non-Judge	group	F(2,68)=0.04 part. η^2=0.001	F(2, 26.44)=0.41
	time	F(1,69)=24.96*** part. η^2=0.27	F(1, 43.41)=22.54***
	group*time	F(2,68)=4.98** part. η^2=0.13	F(2, 29.64)=8.14**
FFMQ-15 Non-React	group	F(2,68)=3.63* part. η^2=0.1	F(2, 27.03)=8.08**

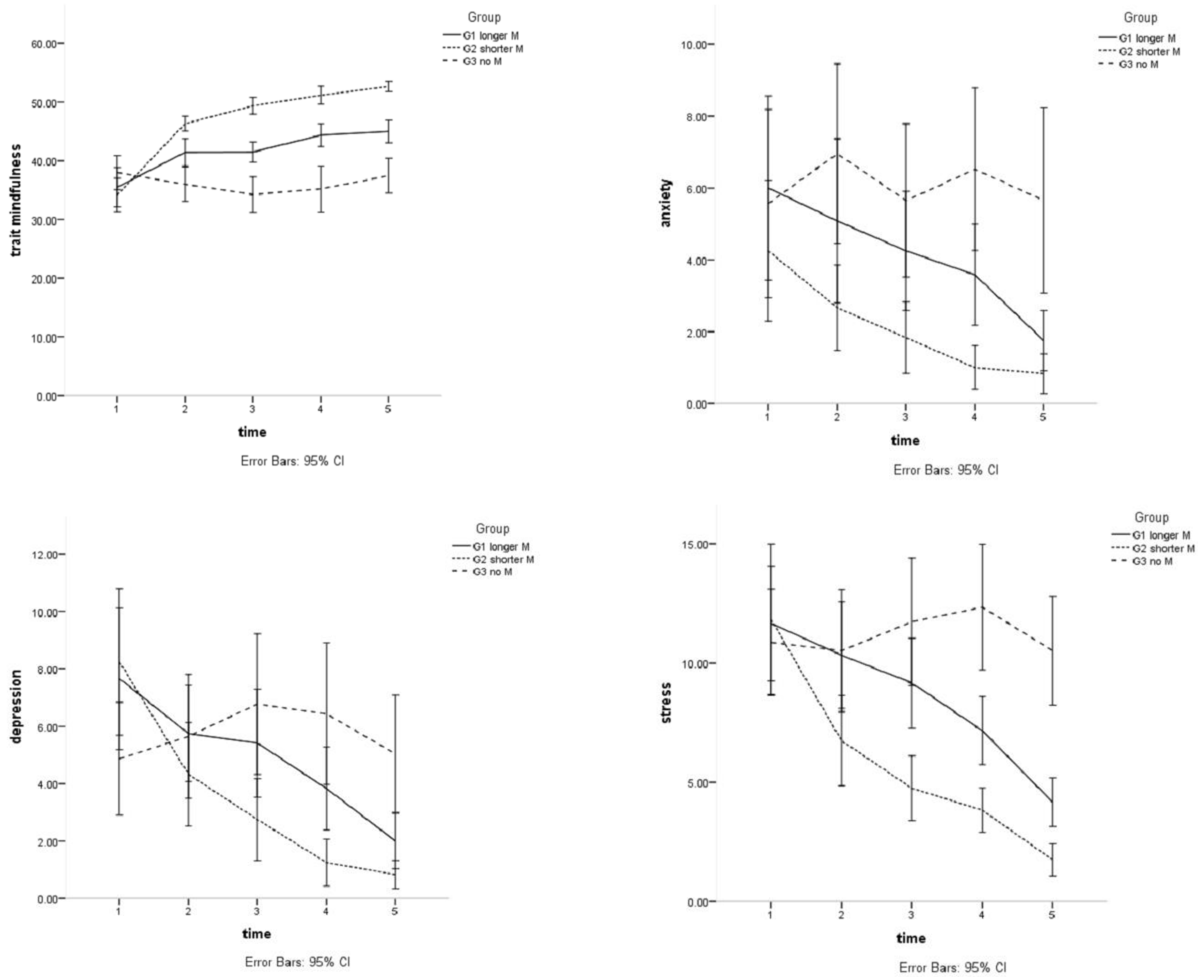
	time	$F(1,69)=27.93$*** part. $\eta^2=0.54$	$F(1, 30.02)=96.63$***
	group*time	$F(2,68)=27.93$*** part. $\eta^2=0.45$	$F(2, 26.73)=63.52$***
DASS-21 Depression	group	$F(2,68)=0.07$ part. $\eta^2=0.002$	$F(2, 29.08)=0.001$
	time	$F(1,69)=63.1$*** part. $\eta^2=0.48$	$F(1, 30.27)=45$***
	group*time	$F(2,68)=17.72$*** part. $\eta^2=0.34$	$F(2, 20.39)=23.1$***
DASS-21 Anxiety	group	$F(2,68)=2.93$ part. $\eta^2=0.08$	$F(2, 28.09)=1.25$
	time	$F(1,69)=31.76$*** part. $\eta^2=0.32$	$F(1, 32.37)=16.64$**
	group*time	$F(2,68)=8.67$*** part. $\eta^2=0.2$	$F(2, 23.07)=8.35$**
DASS-21 Stress	group	$F(2,68)=5.29$** part. $\eta^2=0.14$	$F(2, 29.19)=3.14$*
	time	$F(1,69)=85.73$*** part. $\eta^2=0.56$	$F(1, 27.13)=46.37$***
	group*time	$F(2,68)=20.1$*** part. $\eta^2=0.37$	$F(2, 20.26)=24.81$***

FFMQ-15=Five Facet Mindfulness Questionnaire; DASS-21=Depression Anxiety Stress Scale; ***$p<.001$; **$p<.01$; *$p<.05$; part. η^2= partial eta^2.

Figure 6.4 illustrates how outcomes changed across all five time points, showing trends that are consistent with the baseline–end of study analysis. Equivalent graphs for the FFMQ-15 subscales are available in Appendix Figure 6.4.6. The shorter practice group generally shows higher levels of trait mindfulness and lower levels of depression, anxiety and stress compared to the longer practice group over time.

Figure 6.4

Change in outcomes across all time points
(Baseline (Time 1) to end of study (Time 5), for Group 1 (longer practice), Group 2 (shorter practice) and Group 3 (control). Top left: trait mindfulness (FFMQ-15). Top right: depression (DASS-21). Bottom left: anxiety (DASS-21). Bottom right: stress (DASS-21)).



When examining time and group effects in separate ANOVAs, no significant difference in group was found at baseline; however, at the end of the study there were significant group differences. Specifically, results of the standard and robust between-group ANOVAs (Table 6.4) show that engaging in shorter or longer mindfulness practices resulted in significantly greater trait mindfulness (and subscales observe, describe, act aware and non-react) and significantly lower depression, anxiety and stress compared to controls at post-study with large effect sizes, thus confirming Hypotheses 1 and 2. For the FFMQ-15 subscale non-judge, the between-group effect between shorter practice and controls was not significant for the standard method; and is only just significant (p=.0466) for the robust method. Hypotheses 1 and 2 were therefore not confirmed for FFMQ-15 non-judge for standard methods. Using post hoc testing (see Table 6.4), statistically significant differences between longer and shorter mindfulness practices were observed for trait mindfulness and stress outcomes at post-study with moderate to large effect sizes. This was also found for FFMQ-15 subscales observe, describe, act aware and non-react. However, these findings were in the opposite direction to hypothesized, since shorter as opposed to longer mindfulness practice resulted in larger effect sizes; Hypothesis 3 was therefore not confirmed. For depression, anxiety and FFMQ-15 non-judge, no statistically significant difference was found between the longer and shorter practice groups at post-study.

Table 6.4

One-way between-group ANOVAs, with group as the factor, conducted separately at baseline and end of study, and the subsequent post-hoc tests, where the main effect was significant

BASELINE		
OUTCOMES	**STANDARD**	**ROBUST**
Trait mindfulness	F(2,68)=1.69	F(2, 29.22)=1.35
Observe	F(2,68)=.07	F(2, 28.78)=0.13
Describe	F(2,68)=1.18	F(2, 28.98)=1.56
Act Aware	F(2,68)=.39	F(2, 28.12)=0.83
Non-Judge	F(2,68)=1.72	F(2, 28.55)=1.65
Non-React	F(2,68)=2.35	F(2, 27.65)=2.77
Depression	F(2,68)=2.48	F(2, 29.27)=2.13
Anxiety	F(2,68)=0.63	F(2, 28.69)=0.32
Stress	F(2,68)=0.16	F(2, 28.44)=0.21

POST (TIME 5)						
OUTCOMES	**STANDARD**			**ROBUST**		
Trait mindfulness		F(2,68)=57.13***			F(2, 23.46)=61.92***	
	G1-G2 M(SE)	**G1-G3 M(SE)**	**G2-G3 M(SE)**	**G1-G2 Ψ**	**G1-G3 Ψ**	**G2-G3 Ψ**
	-7.67(1.41)*** *d*=-2.17	7.52(1.42)*** *d*=1.3	15.19(1.42)*** *d*=3.02	-7.63***	6.48**	14.11***
Observe		F(2,68)=24.52***			F(2, 27.7)=19.21***	
	G1-G2 M(SE)	**G1-G3 M(SE)**	**G2-G3 M(SE)**	**G1-G2 Ψ**	**G1&G3 Ψ**	**G2&G3 Ψ**
	B&T: -2(0.63)** GH: -2(0.503)** *d*=-1.15	B&T: 2.49(0.64)** GH: 2.49(0.73)** *d*=1.01	B&T: 4.49(0.64)*** GH: 4.49(0.69)*** *d*=1.93	-1.94**	2.54**	4.48***
Describe		F(2,68)=21.3***			F(2, 26.78)=16***	
	G1-G2 M(SE)	**G1-G3 M(SE)**	**G2-G3 M(SE)**	**G1-G2 Ψ**	**G1-G3 Ψ**	**G2-G3 Ψ**

	B&T:-1.46(0.51)* GH: -1.46(0.46)** *d*=-0.92	B&T: 1.89(0.51)** GH: 1.89(0.59)** *d*=0.94	B&T: 3.35(0.51)*** GH: 3.35(0.49)*** *d*=2.01	-1.19*	1.89**	3.08***
Act Aware		F(2,68)=64.59***			F(2, 24.36)=71.58***	
	G1-G2 M(SE)	**G1-G3 M(SE)**	**G2-G3 M(SE)**	**G1-G2 Ψ**	**G1-G3 Ψ**	**G2-G3 Ψ**
	B&T:-2.88(0.42)*** GH: -2.88(0.33)*** *d*=-2.5	B&T: 1.97(0.43)*** GH: 1.97(0.49)*** *d*=1.17	B&T: 4.84(0.43)*** GH: 4.84(0.45)*** *d*=3.15	-2.94***	1.76**	4.7***
Non-Judge		F(2,68)=1.72			F(2, 25.77)=3.52*	
				G1-G2 Ψ	**G1-G3 Ψ**	**G2-G3 Ψ**
				-0.5	2.07	2.57*
Non-React		F(2,68)=27.85***			F(2, 23.02)=29.11***	
	G1-G2 M(SE)	**G1-G3 M(SE)**	**G2-G3 M(SE)**	**G1-G2 Ψ**	**G1-G3 Ψ**	**G2-G3 Ψ**
	B&T:-2(0.51)** GH:-2(0.37)*** *d*=-1.56	B&T:1.86(0.52)** GH: 1.86(0.61)** *d*=0.9	B&T: 3.86(0.52)*** GH: 3.86(0.56)*** *d*=2.05	-2.38***	1.27*	3.64***
Depression		F(2,68)=11.62***			F(2, 24.55)=4.46*	
	G1-G2 M(SE)	**G1-G3 M(SE)**	**G2-G3 M(SE)**	**G1-G2 Ψ**	**G1-G3 Ψ**	**G2-G3 Ψ**
	B&T: 1.167(0.89) GH: 1.167(0.52) *d*=0.65	-3.04(0.9)** *d*=-0.82	B&T: -4.21(0.9)*** GH: 4.21(1.02)** *d*=-1.23	0.88	-2.9*	-3.78***
Anxiety		F(2,68)=11.41***			F(2, 24.07)=3.76*	
	G1-G2 M(SE)	**G1-G3 M(SE)**	**G2-G3 M(SE)**	**G1-G2 Ψ**	**G1-G3 Ψ**	**G2-G3 Ψ**
	B&T: 0.92(1.06) GH: 0.92(0.48) *d*=0.55	B&T: -3.9(1.07)** GH: -3.9(1.31)** *d*=-0.89	B&T: -4.82(1.07)*** GH: 4.82(1.27)** *d*=-1.13	0.88	-3.16*	-4.03***
Stress		F(2,68)=40.2***			F(2, 26.79)=23.29***	
	G1-G2 M(SE)	**G1-G3 M(SE)**	**G2-G3 M(SE)**	**G1-G2 Ψ**	**G1-G3 Ψ**	**G2-G3 Ψ**
	B&T: 2.42(1)* GH: 2.42(0.59)** *d*=1.18	B&T: -6.36(1.01)*** GH: -6.36(1.21)*** *d*=-1.55	B&T: -8.77(1.01)*** GH: -8.77(1.15)*** *d*=-2.26	2.13**	-6.65***	-8.78***

Trait mindfulness, observe, describe, act aware, non-judge, non-react measured with Five Facet Mindfulness Questionnaire (FFMQ-15); depression, anxiety, stress measured with Depression Anxiety and Stress Scale (DASS-21);****p*<.001; ***p*<.01; **p*<.05; G1=longer practice group; G2=shorter practice group; G3=control group; *M*=mean change; *SE*=Standard Error of mean change; B=Bonferroni post hoc test; T=Tukey post hoc test; GH=Games Howell post hoc test (where B, T and GH are not listed separately, outcomes were the same across all three post hoc tests); *d*=Cohen's *d* effect size; Ψ=pairwise trimmed mean differences.

Results of within group ANOVAs, which examined change over time for each group separately, showed significant changes in expected directions for the longer and the shorter practice group with moderate to large effect sizes and no significant change for the control group. Within-group ANOVA results were equivalent for standard and robust methods (Table 6.5).

Table 6.5
Within group ANOVA results (standard and robust methods) comparing baseline to Time 5 (end of study) for each group separately

Outcomes	Standard			Robust		
	Longer practice	**Shorter practice**	**Control**	**Longer practice**	**Shorter practice**	**Control**
Trait mindfulness	F(1,46)=25.65*** *d*=1.04	F(1,46)=164.5*** *d*=2.4	F(1,44)=0.07	F(1,19.71)=21.32***	F(1,16.6)=108.82***	F(1,27.84)=0.03
Observe	F(1,46)=17.55*** *d*=0.95	F(1,46)=60.54*** *d*=1.83	F(1,44)=0.01	F(1,28.22)=19.48***	F(1,26.99)=59.8***	F(1,28)=0.01
Describe	F(1,46)=6.45* *d*=0.64	F(1,46)=75.06*** *d*=1.64	F(1,44)=0.01	F(1,24.48)=6.12*	F(1,21.55)=55.4***	F(1,26.78)=0
Act Aware	F(1,46)=30.21*** *d*=0.96	F(1,46)=89.61*** *d*=2.18	F(1,44)=0.09	F(1,29.76)=27.04***	F(1,17.22)=71.56***	F(1,27.09)=0.01
Non-Judge	F(1,46)=7.3** *d*=0.82	F(1,46)=38.12*** *d*=1.18	F(1,44)=0.12	F(1,25.32)=5.02*	F(1,24.67)=29.16***	F(1,22)=0.15
Non-React	F(1,46)=23.42*** *d*=0.92	F(1,46)=112.33*** *d*=2.07	F(1,44)=0.12	F(1,27.78)=24.73***	F(1,20.01)=147.04***	F(1,27.99)=0.05
Depression	F(1,46)=19.5*** *d*=-1.08	F(1,46)=34.75*** *d*=-1.28	F(1,44)=0.02	F(1,18.27)=12.3**	F(1,16.31)=25.08***	F(1,27.98)=0.01
Anxiety	F(1,46)=10.62** *d*=-0.85	F(1,46)=12.16** *d*=-0.86	F(1,44)=0.002	F(1,19.96)=6.22*	F(1,16.79)=5.26*	F(1,27.99)=0.003
Stress	F(1,46)=35.1*** *d*=-1.34	F(1,46)=41.7*** *d*=-1.37	F(1,44)=0.05	F(1,18.17)=25.17***	F(1,16.74)=19.78***	F(1,28)=0.01

Trait mindfulness, observe, describe, act aware, non-judge, non-react measured with Five Facet Mindfulness Questionnaire (FFMQ-15); depression, anxiety, stress measured with Depression Anxiety and Stress Scale (DASS-21);****p*<.001; ***p*<.01; **p*<.05; *d*=Cohen’s *d* effect size.

6.4.4 Regression, Mediation and Moderation (Testing Secondary Hypotheses)

As outlined in the methods, it had been planned to explore whether total amount of practice predicted outcomes prior to data collection when it was thought that there would be significant attrition resulting in participants having engaged in largely different amount of mindfulness practiced other than group affiliation alone. However, given that all participants attended all sessions, the relationships between dichotomous predictor variables (longer vs. shorter practice; longer practice vs. control; shorter practice vs. control) and outcomes were examined to test secondary hypotheses.

6.4.4.1 Regression

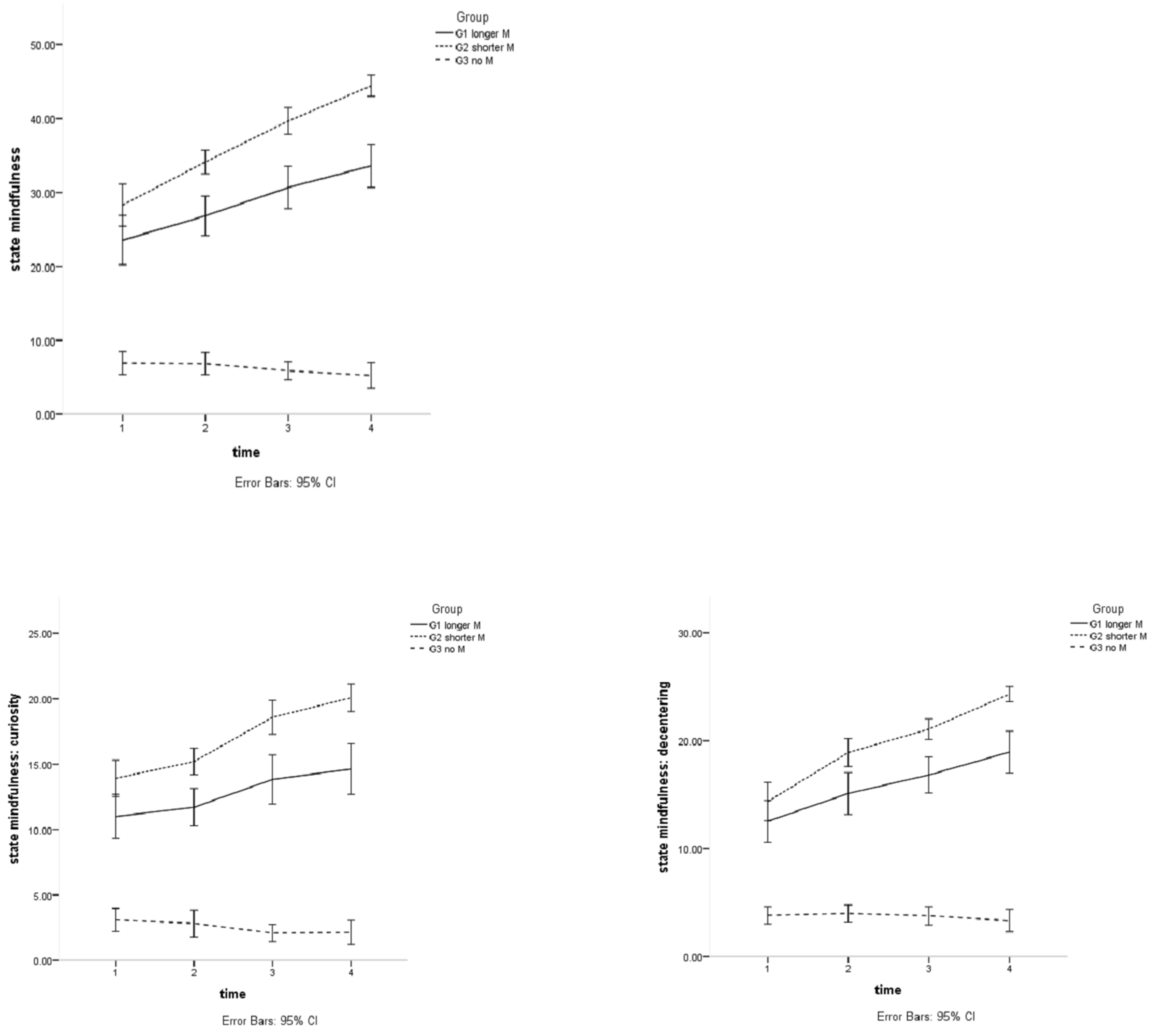
Due to lack of attrition from the study, regression analyses have effectively been completed already and thus added little beyond the ANOVAs presented above. Unsurprisingly therefore, the results from regression analyses did not materially differ (see Appendix Table 6.4.7). The only exception to primary analyses was that depression was significantly predicted by longer vs. shorter practice, however, confidence intervals suggested that this was close to being non-significant (95% C.I. [.004, .04; p=.046]). Since shorter as opposed to longer practices resulted in significantly greater improvements of outcomes, Hypothesis 4 was not confirmed.

6.4.4.2 Effect of Mediators Trait Mindfulness and State Mindfulness

Figures 6.4 (above) and 6.5 show change of mediators trait mindfulness, state mindfulness, curiosity, and decentering over time.

Figure 6.5

State mindfulness change over time (session 1 to 4)
(Group 1 (longer practice), Group 2 (shorter practice) and Group 3 (control); top: total-TMS; bottom: left: curiosity, right: decentering).



Bootstrapped mediation analyses examined whether change in trait and state mindfulness statistically mediated the relationship between group assignment and end-of-study depression, anxiety, and stress. Separate analyses were conducted for each combination of mediator and outcome. In each analysis, baseline levels of the included outcome were controlled for by entering this as a covariate. Due to the non-linearity of the relationship between practice length and outcomes, separate analyses were conducted for each pairing of groups. As outlined in the methods, due to the large number of comparisons in mediation analyses (n=36), a more stringent alpha level (99.9% Confidence Intervals; p=.001) according to the Bonferroni correction was employed to minimize the chance of a false positive finding (Type I error). As recommended, results from both, uncorrected (95%C.I.; p<.05) and corrected (99.9% C.I., $p \leq .001$) mediation analyses are presented (Clark-Carter, 1997).

As can be seen from Table 6.6, results of uncorrected simple mediation analyses showed that neither trait nor state mindfulness change (nor curiosity or decentering change) were significant mediators between shorter vs. longer practice and depression, anxiety or stress outcomes. Trait mindfulness change was a significant statistical mediator in the expected direction between the predictor longer practice vs. control and depression, as well as between shorter practice vs. control and depression. Trait and state mindfulness change, as well as change in curiosity and decentering, significantly statistically mediated the relationship between shorter practice vs. control and anxiety.

Table 6.6

Indirect effects of uncorrected mediation models with mediators trait mindfulness, state mindfulness, curiosity, and decentering and predictors longer vs. shorter practice, longer practice vs. control and shorter practice vs. control

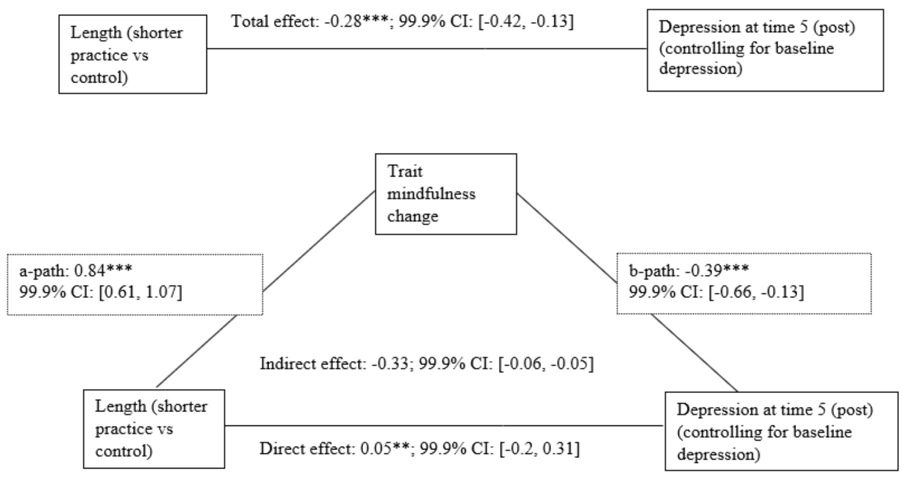
OUTCOME	MEDIATOR TRAIT MINDFULNESS								
	Longer practice vs. shorter practice			**Longer practice vs. control**			**Shorter practice vs. control**		
	b	SE(boot)	95% CI(boot)	*b*	SE(boot)	95% CI(boot)	*b*	SE(boot)	95% CI(boot)
Depression	-0.39	0.3	[-0.89, 0.32]	**0.6**	**0.31**	**[0.02, 1.27]**	**6.62**	**1.72**	**[3.22, 9.99]**
Anxiety	0.27	0.29	[-0.31, 0.87]	-0.07	0.31	[-0.73, 0.52]	**2.58**	**1.39**	**[0.61, 6.13]**
Stress	-0.08	0.38	[-0.87, 0.64]	0.06	0.39	[-0.66, 0.93]	1.42	1.99	[-1.19, 6.67]
	MEDIATOR STATE MINDFULNESS								
	Longer practice vs. shorter practice			**Longer practice vs. control**			**Shorter practice vs. control**		
	b	SE(boot)	95% CI(boot)	*b*	SE(boot)	95% CI(boot)	*b*	SE(boot)	95% CI(boot)
Depression	0.3	0.23	[-0.06, 0.84]	-0.49	0.43	[-1.32, 0.39]	-1.26	1.16	[-3.27, 1.41]
Anxiety	-0.04	0.2	[-0.48, 0.36]	0.14	0.46	[-0.69, 1.12]	**2.75**	**1.15**	**[0.66, 5.14]**
Stress	-0.03	0.21	[-0.17, 0.22]	0.39	0.42	[-0.47, 1.25]	1	0.21	[-0.25, 0.57]
	MEDIATOR CURIOSITY								
	Longer practice vs. shorter practice			**Longer practice vs. control**			**Shorter practice vs. control**		
	b	SE(boot)	95% CI(boot)	*b*	SE(boot)	95% CI(boot)	*b*	SE(boot)	95% CI(boot)
Depression	0.14	0.17	[-0.16, 0.53]	-0.33	0.32	[-0.9, 0.37]	-0.99	0.8	[-2.42, 0.81]
Anxiety	-0.08	0.15	[-0.42, 0.18]	0.19	0.26	[-0.3, 0.76]	**1.51**	**0.74**	**[0.22, 3.1]**
Stress	-0.11	0.12	[-0.44, 0.32]	0.14	0.24	[-0.35, 0.63]	-0.05	0.84	[-1.9, 1.38]
	MEDIATOR DECENTERING								
	Longer practice vs. shorter practice			**Longer practice vs. control**			**Shorter practice vs. control**		
	b	SE(boot)	95% CI(boot)	*b*	SE(boot)	95% CI(boot)	*b*	SE(boot)	95% CI(boot)
Depression	0.31	0.25	[-0.15, 0.88]	-0.26	0.49	[-1.35, 0.6]	-0.76	1.28	[-3.12, 1.96]
Anxiety	0.03	0.2	[-0.34, 0.47]	-0.18	0.41	[-0.94, 0.69]	**2.81**	**1.21**	**[0.42, 5.19]**
Stress	0.11	0.22	[-0.26, 0.63]	0.3	0.43	[-0.56, 1.19]	1.66	1.31	[-0.93, 4.34]

b=effect size indirect effect; SE(boot)=bootstrapped Standard Error; 95% CI(boot)=bootstrapped 95% Confidence Intervals; Trait mindfulness measured with FFMQ-15; depression, anxiety, stress measured with DASS-21; state mindfulness, curiosity, decentering measured with Toronto Mindfulness Scale (TMS); significant results in bold.

However, results of corrected mediation analyses (99.9% C.I.; *p*<.001) showed that only trait mindfulness change remained a significant mediator of the relationship between practice length (shorter practice vs. control) and depression at post-study (Time 5) when controlling for baseline depression with trait mindfulness change significantly predicting small to moderately lower depression at Time 5 (post-study), see Figure 6.6. Corrected mediation models for outcomes with non-significant indirect effects are in Appendix Figure 6.4.8. While mindfulness practice length generally had a significant effect on trait and state mindfulness as indicated by significant a-paths, there was no evidence in the majority of mediation models that this acted as a mediator between practice length and outcomes since the indirect effect was not significant. Since trait and state mindfulness change were significant mediators for only some outcomes in both uncorrected and corrected mediation analyses, Hypothesis 5 could only partially be confirmed.

Figure 6.6

Corrected mediation model for post-study (Time 5) depression
*(length (shorter practice vs. control) as predictor, trait mindfulness change as mediator and baseline depression as a covariate. Top diagram: total effect when excluding mediator, bottom diagram: indirect and direct effects when including mediator (*p<.05; **p<.01; ***p<.001; 99.9% CI=99.9% Confidence Intervals)).*



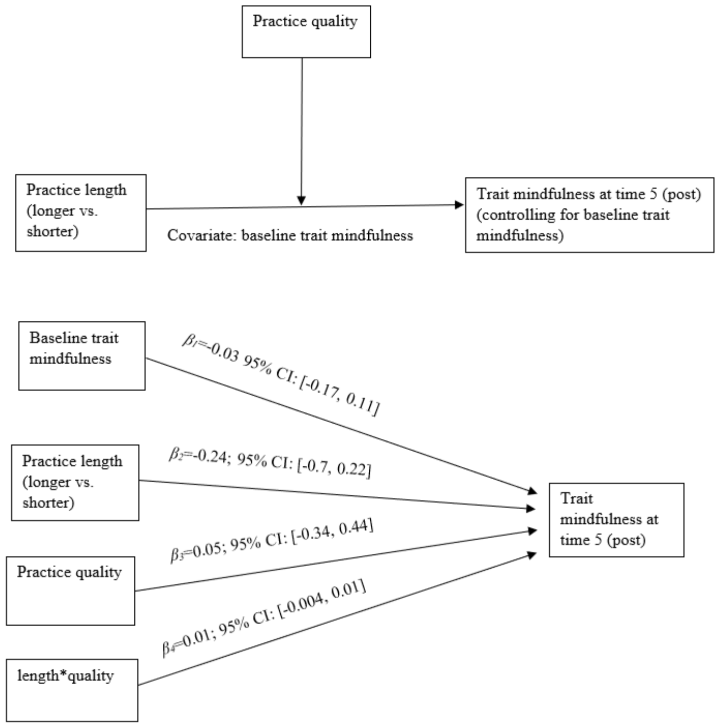
6.4.4.3 Effect of the Moderator Practice Quality

The following results are reported since they were part of the pre-planned analysis. However, these should be interpreted with caution given the relatively low internal consistency mentioned above and other potential limitations of this measure in the context of this study, details of which are discussed below. Moderation analyses revealed that there was no significant interaction between practice length (longer vs. shorter) and practice quality when predicting any of the outcomes, as indicated by the length x quality interaction term failing to increase the amount of variance explained in trait mindfulness (F(1, 43)=0.36, ΔR^2=0.003, *p*=.55), depression (F(1, 43)=0.83, ΔR^2=0.014, *p*=.37), anxiety (F(1, 43)=0.19, ΔR^2=0.002, *p*=.67) or stress (F(1, 43)=0.31, ΔR^2=0.005, *p*=.58). Similarly, no significant interaction effects where found between practice length and average practice quality for the FFMQ-15 subscales observe (F(1, 43)=0.27, ΔR^2=0.004, *p*=.61), describe (F(1, 43) =0.16, ΔR^2=0.003, *p*=.69), act aware (F(1, 43)=0.02, ΔR^2=0.0001, *p*=.9), non-judge (F(1, 43)=3.17, ΔR^2=0.05, *p*=.08) or non-react (F(1, 43)=0.1, ΔR^2=0.001, *p*=.75). Thus, Hypothesis 6 was not supported. See Figure 6.7 for the moderation model for the primary outcome trait mindfulness. Moderation models for depression, anxiety, stress, and FFMQ-15 subscales are listed in Appendix Figure 6.4.9.

Figure 6.7

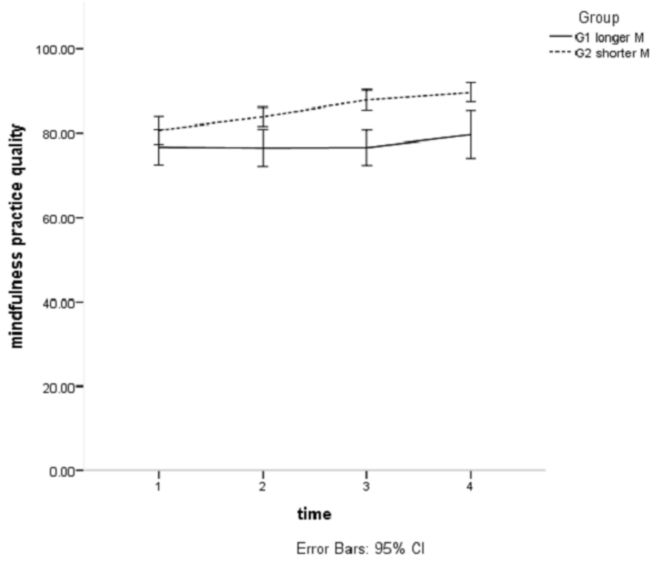
Moderation model diagram for post-study (Time 5) trait mindfulness outcome and moderator practice quality change

*(length*quality=interaction of practice length and practice quality; β_1 = coefficient of baseline trait mindfulness on post-study trait mindfulness; β_2 = coefficient on the effect of practice length on post-study trait mindfulness, β_3 = coefficient on the effect of practice quality on post-study trait mindfulness, and β_4 = coefficient on the effect of the interaction of practice length and practice quality on post-study trait mindfulness*



Graphically, practice quality appears to be increasing more steadily in the group of shorter mindfulness practitioners compared to the longer practice group (see Figure 6.8 for practice quality change over time).

Figure 6.8
Mindfulness Practice Quality change for Group 1 (longer practice) and Group 2 (shorter practice) over time (sessions 1-4)



6.4.5 Qualitative Participant Feedback

The relevant results from the content analysis of participant feedback are briefly considered here. These are not covered in depth since this was not the primary analysis and due to the nature of this being post-study, online participant feedback, the data were relatively sparse. However, the table in Appendix 6.4.10 shows in detail the coding frame including participant quotes separately for each group.

Inter-rater reliability analysis resulted in a Cohen's kappa of κ=.93, which represents almost perfect agreement (McHugh, 2012). Results of the content analysis showed 34 comments on positive experiences of mindfulness practice in the longer practice group compared to 75 comments on positive experiences of practice in the shorter practice group. Particularly compelling are comments from participants in the shorter practice group such as "when I'm feeling negative or scared I now go to my breathing which I never did before" and "doing the mindfulness practice in the study put my mind in a positive mindset the rest of the day." The perceived benefits of brief mindfulness practices were also reflected in 16 comments from participants in the shorter practice group with comments such as "I always thought that to do mindfulness, I'd need a long time to dedicate to this which seemed too challenging to even start. But I've learned that 5 minutes can help!".

Contrarily, there were 18 comments from participants in the longer practice group who expressed some difficulties with practice particularly relating to its length, with comments such as "I think a shorter one would be better for me to learn mindfulness" and "I sometimes found it quite difficult to stop my mind from wandering". Finally, five participants in the longer practice group declared the study to have had no effect compared to 13 in the control group. Participants in the shorter practice group neither expressed difficulties with practice nor stated the study had no effect.

6.4.6 Facilitator Observations

While not part of the formal pre-planned data collection, the facilitator of the sessions (PhD researcher) observed the following, which potentially contributed to the understanding of the findings: 1) after the end of the study, five participants in the shorter practice group were sufficiently interested in the mindfulness practice to spontaneously ask for the mindfulness practice track so they could continue to practice, while no participants in the longer practice group asked for this; 2) in their contemporaneous notes of the session which were taken before data analysis and knowledge of results, the facilitator observed that participants in the longer practice group tended to fidget, shift in their seat, and open their eyes more frequently, whereas this was observed less in the shorter practice group. Limitations of these informal observations are considered in the discussion below.

6.5 Discussion

The purpose of this study was to examine the effect of two different tightly controlled lengths of mindfulness practice for novice practitioners over four face-to-face sessions compared to an active control group.

6.5.1 Effect of Mindfulness Practice Length on Outcomes

Results showed that engaging in both shorter and longer mindfulness practices had significant beneficial effects on trait mindfulness, depression, anxiety, and stress, relative to an active control thus confirming Hypotheses 1 and 2. This corresponds with previous findings that both longer as well as shorter mindfulness practices can improve trait mindfulness and psychological distress (e.g. Brown & Ryan, 2003; Mahmood et al., 2016; Janssen et al., 2018).

Hypothesis 3, that engaging in longer mindfulness practices would result in a larger effect than engaging in shorter practices, was not confirmed. In fact, this effect was reversed, since completing shorter practices had a significantly larger effect on trait mindfulness and stress than longer practices; there was a non-significant trend in the same direction for depression and anxiety. Similarly, in regression analysis, greater improvement of outcomes was found for shorter rather than longer practices thus rejecting Hypothesis 4. This finding was further elucidated by results of the content analysis with more comments about positive experiences made by participants in the shorter than the longer practice group. This is consistent with previous research where five-minute mindfulness practices were found to significantly increase mindfulness and decrease stress in nurses (Gauthier et al., 2015) as well as previous self-help programs finding beneficial effects of brief mindfulness practices (Bartlett et al., 2021; Cavanagh et al., 2018; Economides et al., 2018; Howells et al., 2016; Moore et al., 2020). Additionally, this result coincides to an extent with previous research by Ribeiro et al. (2018) on the effects of home practice in an MBP where positive post-program changes in outcomes, including mindfulness and stress, were found regardless of length of time practiced. Regularly practicing mindfulness, even if practices are brief, is therefore helpful in learning mindfulness and increasing attentional processing, as has been indicated in previous research and theory (cf. Bartlett et al., 2021; Malinowski, 2013; Moore et al., 2012). This finding also corresponds to an extent with the lack of dose-response relationships identified in the previous Chapters 2-5 (Strohmaier, 2020), since both longer and shorter programs appear helpful for depression, anxiety, and stress. It does not however confirm the positive dose-response relationship between actual MBP-use and trait mindfulness found in the dose-response meta-regression, since engaging in shorter rather than longer practices resulted in greater trait mindfulness change in the current study. However, actual MBP-use did not only include practice but also often other learning elements of mindfulness programs and teacher-led enquiry (Chapters 2-5; Strohmaier, 2020), whereas in this study, mindfulness practice isolated from other such elements was examined. The findings in this study also do not correspond with a previous review of MBSR and MBCT by Parsons et al. (2017), who found a small, positive correlation between longer self-reported home practices and outcomes. However, in

Parsons et al.'s review, practice time was not controlled tightly as it is in the present study since self-reported home practices rather than in-person practices were measured. Additionally, it is unclear in both reviews how much prior mindfulness practice experience participants from included studies had. Furthermore, due to the nature of reviews, included data were correlational rather than experimental; it is therefore not possible to infer causation from Strohmaier (2020) and Parsons et al. (2017). When asked about perceived effects of the current study, novice practitioners in the longer but not the shorter practice group mentioned that at times they experienced some difficulties in holding concentration and avoiding mind-wandering during practice and several participants in the longer practice group asked whether there was a shorter alternative to mindfulness practice. This coincides with findings from previous research suggesting novice practitioners can find longer practices challenging and can experience mind-wandering during such practices (Banerjee et al., 2017; Frewen et al., 2016). It may be that when mindfulness is taught to novices with relatively little additional teacher input beyond practice guidance (as was the case in the current study), challenges associated with longer practices lead them to be less effective in general than shorter ones (cf. Desbordes et al. 2015; Frewen et al. 2016). However, in contrast, it is possible that the substantially greater input from an experienced teacher present in some MBPs (e.g. teacher-led enquiry in MBSR and MBCT), may help participants to reframe and remain engaged with such challenges and thus benefit more from longer practices in such MBPs (cf. Segal et al. 2013; Kabat-Zinn, 2003). This may help explain the difference between the dose-response effect observed here and those found by Parsons et al. (2017) and Strohmaier (2020). If this account is correct, an important implication is that optimal amount of mindfulness practice may depend upon amount of teacher-led enquiry included in an MBP, with self-help MBPs that do not include this likely benefitting from substantially shorter practices (cf. Segal et al. 2013). This should be a focus of future research (see below).

Regardless, a noteworthy finding from the current study is that practicing mindfulness for just five minutes on four occasions, over two weeks, can significantly improve individuals' trait mindfulness, and depression, anxiety, and stress levels. This is further elucidated by several participants particularly valuing the shorter practices and commenting on the impact they felt these had on their daily life. The value of shorter practices in reducing stress resembles findings from a previous study with healthy employees, where engaging in brief mindfulness practices significantly reduced work-related stress (Bostock et al., 2018). Additionally, this result aligns with the finding of increased trait mindfulness after brief, app-delivered mindfulness practices in university students (Flett et al., 2019). Shorter practices may thus feel more attainable and sustainable for novice practitioners, especially when there is minimal or no practice experience. A parallel can be drawn here to someone training to run a marathon. One would not be expected to run 42km on day one if they were new to running or had limited previous running experience. On the contrary: it would be expected for this person to slowly work towards their goal of running a marathon by starting with shorter distances. The positive qualitative feedback responses from the shorter practice participants further support this claim. Consistent with this, several participants from the shorter practice group and none from the longer practice group requested the practice recording to continue their practice after the end of this study and the facilitator observed more fidgeting and eyes opening in the longer practice group (though see below for limitations of these data).

For anxiety and depression outcomes, there were no significant differences at post-study between longer and shorter practices although there was a trend towards favouring shorter practices which could have been significant in a larger sample. One possible explanation for this result is that engaging in mindfulness practices regardless of length may be helpful in reducing rumination and worry, since the quality of practices completed rather than simply the time spent practicing may be of importance (Ribeiro et al. 2018). This corresponds with the existing strong evidence base for beneficial effects of mindfulness for psychological distress (e.g. Chiesa & Serretti, 2009; Hofmann et al., 2010; MacKenzie & Kocovski, 2016). Additionally, this result coincides with previous research comparing 20-minute and 10-minute home practices over a two-week MBP where no difference in effectiveness between the longer and shorter practice was found for depression and anxiety (Berghoff et al., 2017).

6.5.2 Mediators Trait and State Mindfulness and Moderator Practice Quality

When assessing underlying processes of practice length on outcomes, results from uncorrected mediation analyses showed significant mediating effects of trait mindfulness change between both practices vs. control and depression. Additionally, significant mediating effects of trait and state mindfulness change between shorter practice vs. control and anxiety were found. However, in corrected mediation analyses, only the model of trait mindfulness change as a mediator between shorter practice vs. control and depression remained significant. This coincides with previous research identifying mindfulness as an underlying mechanism of MBPs for psychological wellbeing (Gu et al., 2015). For both, uncorrected and corrected results, neither trait nor state mindfulness change significantly mediated the relationship between practice length when defined as longer vs. shorter practice and all outcomes. A possible explanation for this could be that mindfulness practice regardless of length has been identified as helpful in reducing levels of psychological distress (e.g. Ribeiro et al., 2018; Strohmaier, 2020) and change in trait and state mindfulness may not have significantly added to this relationship. Another possibility is that the effect may be smaller when comparing the two mindfulness practice groups and failure to find an effect may thus be a Type II error.

Turning to moderation, Hypothesis 6 was not confirmed since mindfulness practice quality did not significantly moderate the effect of different practice lengths on outcomes. Graphically, participants engaging in both practice lengths showed consistently high quality. This coincides with previous research where practice quality was a significant predictor for psychological outcomes at post-program and follow-up even when average practice time was controlled for (Goldberg et al., 2014). One possible explanation for the failure to find a moderating effect of practice quality is that accurate measurement of this has been identified as being difficult (Parsons et al., 2017; Ribeiro et al., 2018). Additionally, it is possible that there was insufficient variation in practice quality between the groups to observe an effect.

6.5.3 Limitations and Implications

Using the PQ-M to measure practice quality was a possible limitation of this study. According to Del Re et al. (2013), the PQ-M is generally used for longer programs such as MBCT and MBSR which include daily home practices and discussion with experienced facilitators during sessions. Additionally, in Del Re et al.’s study, 42% of participants had prior meditation experience, whereas in this study, participants purposely were only included if they had limited meditation experience. Additionally, no discussion with a facilitator took place since the focus was on examining isolated practice length. Participants may therefore have been less familiar with the mindfulness-related terms employed in the PQ-M. Anecdotally, some participants mentioned that they perceived the PQ-M confusing to complete. This was also reflected in the reliability analysis of the PQ-M showing internal consistency below the acceptable threshold in this sample. However, given that this measure has been used in previous similar studies and, to the PhD researcher’s knowledge, was the only relevant published tool to measure mindfulness practice quality at the time, it was deemed appropriate to use. With hindsight, arguably the PQ-M was not a good choice of practice quality measure for this study and related results should be treated with substantial caution. Future research may benefit from employing a questionnaire designed to measure practice quality specifically in novice practitioners once this exists. From qualitative facilitator observations, there was some evidence of difference in practice quality or difficulty in staying with experience between groups. However, these were not formally measured; it would therefore be important for future research to more formally record these and include independent ratings by individuals not aware of practice conditions to reduce potential sources of bias in these data.

Additionally, previous research has found that items on the TMS measure can be more difficult to complete for individuals with little knowledge of mindfulness (Ireland et al., 2018), which could have impacted results. However, the TMS was the most widely used and validated measure of state mindfulness at the time of the study and research, including results of this study, have still found significant increases in state mindfulness in individuals with little mindfulness experience (Lau et al., 2006; Medvedev et al., 2017).

Another limitation of the study was the possibility of common method bias having occurred due to multiple constructs being measured through multiple-item self-report measures (Podsakoff et al., 2003). This seems unlikely to have influenced the differences observed between the three groups as there was no obvious reason for this bias to have systematically varied between groups. However, it could have contributed to the analyses based on correlations between variables measured by self-report scales, most notably the mediation analyses. Some efforts were made to minimize this, such as assuring anonymization and advising participants that there were no right or wrong answers and to respond as honestly as possible (Podsakoff et al. 2003). However, it still may have affected mediation findings.

Due to the relatively large number of comparisons, the possibility exists that significant results were due to Type 1 errors. However, this seems unlikely since there was a consistent pattern of significant results for the primary analysis across outcomes, rather than there being a single incidental positive finding (cf. Abdi, 2010). Non-significant mediation and moderation results were found for practice length (when defined as longer vs. shorter practice) with practice quality and trait and state mindfulness change as moderator and mediators, respectively. This lack of significant mediation and moderation effects could have been due to Type II errors, especially given the high threshold required to attain significance when the control for multiple comparisons was applied (Nakagawa, 2004). However, it is worth noting that mediation and moderation were not the primary analyses in this study.

Moreover, it would be valuable to complete a further experimental examination of mindfulness practice length with experienced, long-term mindfulness practitioners to determine the possible effects of different practice lengths for this group and whether altering practice length would have any effects on trait mindfulness and psychological distress outcomes. The effects of practice quality would again be valuable to explore in a more experienced sample. Additionally, the study sample was limited in only including a mostly female adult, general population sample from largely British and European countries and findings are thus not generalizable across other populations. In future, this research could be duplicated with participants from different backgrounds or clinical populations with various mental or physical health difficulties to further understanding on effects of different practice lengths for these populations.

Furthermore, only two different lengths of practice over a reasonably short time were explored to compare a shorter to a longer practice in a controlled way feasible within the context of a PhD; conclusions about other practice lengths, sessions numbers, and long-term follow-up[17] effects can therefore not be drawn. Additionally, only formal mindfulness practices were examined here without exploring effects of length in informal practices, which arguably would be important to consider (Birtwell et al., 2019; Ribeiro et al., 2018; see Chapter 3, Section 3.4.3.1). In future research, this study could be repeated to explore effects of different practice and program lengths for formal and informal practices both at immediately post-program and at follow-up. This could also include examining the effectiveness of single-session mindfulness programs for novices on outcomes other than psychological distress, such as positive psychological outcomes to advance understanding of the effect of a single mindfulness practice.

Moreover, the size of participant groups that sessions were administered to varied session by session; however, this is unlikely to have influenced results since no group elements such as discussions were part of the study, and because group size did not systematically vary with independent variables. As discussed earlier, future research could also examine the hypothesis that dose-response effects may be moderated by the amount of teacher-led enquiry included in the MBP, with shorter practices being more beneficial than longer ones when there is minimal enquiry and the reverse when there is more substantial teacher involvement.

Finally, it was not possible to state with complete certainty that participants in this study actively engaged in practice rather than simply listened to recordings or let their minds wander. However, the aim of this study was to isolate and control mindfulness practice length in-person as much as possible to ascertain effects of different practice lengths on outcomes, and it is hard to see

[17]Follow-up data was not collected/analysed in the present study for several reasons including i)the study aim was to tightly control mindfulness practice length in-person; ii)it could be deemed unethical to ask participants to engage in a practice length they had found unhelpful (Section 6.4.5) and to ask participants to only practice on certain days a week to align with the study design; iii)difficulty to examine practice effects if shorter practices are completed more regularly than longer practices.

what else could have been done in this regard. Changes in trait and state mindfulness would suggest that participants were engaging, and results of this study could be helpful in designing future mindfulness programs for novices.

6.6 Chapter 6 Summary

This chapter presented findings of a randomised controlled experiment examining effects of a longer and shorter mindfulness practice on outcomes trait mindfulness, depression, anxiety, and stress for novice practitioners from the general population. Results showed beneficial effects of both longer and shorter practices on outcomes compared to controls. Findings also surprisingly suggest shorter practices to be more beneficial for trait mindfulness and stress than longer practices, at least with minimal teacher input in the general population. Corrected mediation analyses showed that trait mindfulness change was a significant mediator between shorter mindfulness practice vs. control and depression. Practice quality did not significantly moderate the relationship between practice length and outcomes, but this may have been due to measurement issues. Results of this study correspond with some previous research finding benefits of shorter mindfulness practices particularly for novice practitioners. Methodological limitations of this study have been discussed and implications for future research and practice have been explored. In the next chapter, a randomised controlled experiment on the effects of a brief, single-session mindfulness practice is presented and its effects on positive psychological outcomes explored.

CHAPTER 7

One-Session Mindfulness of the Breath Meditation Practice: A Randomised Controlled Study of the Effects on State Hope and State Gratitude in the General Population

7.1 Chapter 7 Overview

In the previous chapter, an experimental study was presented which explored the effects of two different lengths of mindfulness practices delivered face-to-face over four sessions, finding beneficial effects of in particular shorter mindfulness practices on mindfulness and psychological distress outcomes. This chapter explores another dose of mindfulness practice, namely a single-dose mindfulness induction delivered online, and its effects on the positive psychological outcomes state hope and state gratitude in the general population. In this chapter, the rationale for this experimental study based on research and theory is outlined followed by a presentation of methods and results. This chapter concludes with a discussion of findings in line with previous research and theory followed by limitations specific to this study as well as directions for future research.

7.2 Rationale, Research Aims, and Hypotheses

So far, this thesis has focused on mindfulness-based programs (MBPs) which incorporate multiple practices and sessions following Crane et al.'s (2017) definition (see Chapter 2, Section 2.3.1 for more detail). In the dose-response meta-regression of such MBPs, no clear dose-response relationships were identified for depression, anxiety, and stress outcomes, while significant dose-response relationships were found for the mindfulness outcome for some doses related to MBPs (see Chapters 4 and 5). Additionally, in the randomised controlled experiment presented in Chapter 6, significant beneficial effects of several, in particular shorter, mindfulness practices over two weeks were found for psychological distress and mindfulness outcomes compared to controls. One dose of mindfulness practice which would be interesting to explore that has so far not been considered in this thesis is the effectiveness of a single-dose mindfulness practice since positive effects of single-dose mindfulness practices have already been identified in the expanding research literature.

7.2.1 Previous Research on Mindfulness Inductions

Although most previous mindfulness research has found positive effects of MBPs delivered over multiple sessions and weeks, using such programs as MBSR and MBCT (Kabat-Zinn, 1990; Segal et al., 2002; 2013; Chapters 1 and 2), more recently, there has been an increase in research with single-session mindfulness practices. These have been utilized due to providing the possibility of tightly controlling length, dose, and type of practice, resulting in researchers being able to draw more specific causal inferences (Tang et al., 2015). Additionally, single-session mindfulness practices are usually more accessible for the general population, therefore providing less of a burden for individuals, while still offering benefits (cf. Heppner & Shirk, 2018). These are often referred to as "mindfulness inductions" (Leyland et al., 2019, p. 108).

Some research utilising mindfulness inductions has focused on its effects on cognitive performance and working memory. For instance, participants showed increased EEG alpha activity while completing a Stroop task after a mindfulness practice indicating mental engagement (Bing-Canar et al., 2016). Similarly, in a recent review of mindfulness inductions (Gill et al., 2020), positive effects on higher-order cognitive functioning when completing complex tasks were identified. Additionally, Imtiaz et al. (2018) found that participants in a mindfulness induction group showed greater engagement in a challenging cognitive task than controls.

Increasingly, research has also focused on the effects of mindfulness inductions on psychological distress outcomes. Perhaps unsurprisingly given their brevity, the effects of mindfulness inductions tend to be found on measures of the participants' psychological state immediately after the induction, rather than on measures of more enduring change in related psychological traits (e.g. Mahmood et al., 2016). Nevertheless, improvements in such state variables, even without the longer-lasting trait changes, can still be valuable, since positive states of mind have been found to improve wellbeing and positive behaviors, and mindfulness inductions have the potential to be repeated multiple times (e.g. Kluemper et al., 2009). This is reflected in findings from research by Johnson et al. (2015) where participants engaging in a single mindfulness practice showed significant beneficial changes in mood states compared to book-listening controls. Similarly, in RCT studies, participants who completed a mindfulness induction showed significantly reduced levels of negatively valanced mind-wandering (Banks et al., 2019) as well as experimentally induced anxiety (Plonka & Moore, 2019). Additionally, a review on the effect of mindfulness inductions found evidence that they resulted in more effective regulation of negative emotions compared to controls (Leyland et al., 2019). It is worth noting that the observed improvements in emotional regulation seem likely to be connected to the improvements in executive functioning, rather than the two being independent effects of mindfulness inductions (cf. Marceau et al. 2018).

Recently, researchers have also begun to assess the effects of brief, single-session mindfulness practices on positive psychological states. Particularly, single-session mindfulness practices were theorized to allow individuals to be more accepting of everyday experiences and reduce negativity bias by increasing state mindfulness (Brown et al., 2007). This was evidenced in research, for instance Mahmood et al. (2016) have shown that a computer-mediated mindfulness practice resulted in increased state mindfulness compared to controls. Moreover, in their review, Heppner and Shirk (2018) have summarised evidence that mindfulness inductions increased mindful states, which in turn were associated with better emotion regulation and more positive social and health behaviours. Other research has suggested that mindfulness inductions also increase subjective optimism (Kiken & Shook, 2011). In a large-scale mindfulness induction study with college students, those who practiced mindfulness showed higher state mindfulness of the body, though post-study state mindfulness was only related to trait mindfulness for those who were experienced meditators (Bravo et al., 2018).

However, although previous research has examined the effectiveness of mindfulness inductions on psychological distress (e.g. Johnson et al., 2015; Leyland et al., 2019) and a start has been made at examining the effectiveness of mindfulness inductions for positive psychological outcomes (e.g. Kiken & Shook, 2011), the effect of a single mindfulness practice on other positive psychological outcomes, such as state hope and gratitude, has yet to be examined.

7.2.2. Research on Higher-Dose MBPs on Positive Psychological Outcomes

We might expect single-dose mindfulness inductions to have an effect on positive psychology variables, such as hope and gratitude, since research has already demonstrated the effects of higher-dose MBPs on such positive psychological outcomes. For instance, a recent meta-analysis on MBPs at work has found these to be effective not only at reducing psychological distress, but also for increasing compassion, empathy, and positive wellbeing (Lomas et al., 2018). Similarly, participating in an app delivered MBP not only decreased stress and irritability, but also improved positive affect compared to active controls (Economides et al., 2018). Additionally, participating in an 8-week online-delivered MBP significantly predicted increased levels of optimism and affect in direct-care employees (Heckenberg et al., 2019), and mindfulness practice has been found to relate to greater hope and gratitude. For instance, Bluth and Eisenlohr-Moul (2017) showed that participating in an MBP has been associated with increased gratitude, which in turn has been found to predict beneficial outcomes, such as job satisfaction (Waters, 2012), and improvements in stress, depression, and happiness (Emmons & McCullough, 2003; O'Leary & Dockray, 2015). Research has also found that through mindfulness practice, individuals show greater awareness of pleasant life events, which in turn increases their wellbeing (Killingsworth & Gilbert, 2010). Additionally, a study examining the effects of mindfulness meditation, delivered face-to-face over 12 weeks in university students, not only found significantly lower anxiety and negative affect, but also increased hope compared to controls (Sears & Kraus, 2009). Similarly, participating in a 6-week mindfulness class predicted significantly greater hope, mediated by lowered levels of stress (Munoz et al., 2018).

Increased levels of positive psychological outcomes, including hope and gratitude, have been found to relate to better wellbeing and reduced psychological distress. For instance, greater hope has been found to relate to factors such as increased self-compassion and life satisfaction (Bailey et al., 2007; Yang et al., 2016). Furthermore, research using a longitudinal design found that individuals' self-reported levels of hope, in particular the agency component of hope, significantly predicted decreased levels of later depression and anxiety in university students (Arnau et al., 2006). Similarly, greater dispositional gratitude was related to lower levels of depression and anxiety in a large general population sample (Petrocchi & Couyoumdjian, 2015) and research with undergraduate students found that gratitude predicted greater life satisfaction mediated by decreased stress (Yildirim & Alanazi, 2018).

7.2.3 Theory of Mindfulness, Hope, and Gratitude

From a theoretical perspective, increasing positive psychological resources has been suggested as a key mechanism of action of mindfulness practice. For example, according to Fredrickson's (2004) Broaden-and-Build theory, increasing positive psychological resources broadens one’s awareness, encourages positive thoughts and actions and builds personal resources; mindfulness practice is thought to aid this process of broadening awareness (Garland et al., 2015). This was also supported by Malinowski and Lim (2015), who found that greater dispositional mindfulness related to positive affect which in turn was associated with increased personal resources and psychological capital components including hope and optimism. According to Snyder’s (2002) theory of hope, which is thought to contain the two components agency and pathways, positive emotions result from an individual’s perceived progress through self-derived pathways towards desired goals and to “motivate oneself via agency thinking to use those pathways” (p. 249). Mindfulness practice has been found to increase awareness of, and support progress towards, goals (Rand & Cheavens, 2009). Although engaging in mindfulness meditation is not itself focused on goal attainment, the acceptance and lucidity resulting from mindfulness practice may in turn awaken the practitioner’s agency and realise the pathways through which tenable goals can be accomplished (Sears & Kraus, 2006).

Furthermore, a grateful disposition and/or state has been theorised to be present when positive emotional valence and a tendency towards mindfully and purposefully experiencing and appreciating positive emotions, and those who have contributed to them, exists, thus increasing and sustaining subjective wellbeing over time (McCullough et al., 2002). Gratitude has therefore been defined as individuals’ mindful awareness of the positive things in life (Emmons & Mishra, 2012). Consistent with this, participating in MBCT has been shown to predict a greater appreciation of daily life events

(Geschwind et al. 2011). Thus, there is both theoretical and empirical support for the beneficial impact of mindfulness and MBPs on positive psychological variables, such as hope and gratitude.

7.2.4 Rationale for Choice of Outcomes Measured

Given the above-mentioned evidence that longer MBPs can increase hope and appreciation for life, and in light of the afore-mentioned theory, it might be expected that brief mindfulness inductions would improve state hope and gratitude. However, this cannot be assumed, since it remains possible that a brief, single mindfulness practice provides an insufficient dose of mindfulness to have an impact (Chapter 2, Section 2.3.1; Strohmaier, 2020). Therefore, this needs to be examined empirically by employing a randomised controlled experimental design. Providing evidence that a brief mindfulness practice can increase state hope and gratitude would be of value, given the considerable benefits associated with hope and gratitude, including decreased psychological distress (Arnau et al., 2006; Emmons & McCullough, 2003; Petrocchi & Couyoumdjian, 2015; Yang et al., 2016) and the potential greater feasibility and acceptability of a single mindfulness practice compared to longer programs (Tang et al., 2015).

Additionally, in above-mentioned previous research, the beneficial effects of a mindfulness induction on state mindfulness (Mahmood et al., 2016) and the association between mindfulness and hope (Malinowski & Lim, 2015) as well as gratitude (Bluth & Eisenlohr-Moul, 2017) have been explored. However, the statistically mediating effect of state mindfulness on the relationship between a single mindfulness practice, compared to controls, and state hope and state gratitude has not yet been explored. Therefore, to further understand the possible mechanisms of a mindfulness induction, the mediating effects of state mindfulness on state hope and state gratitude are important to be examined.

Finally, although a start has been made in above-mentioned research to explore the positive effects of mindfulness practice on state mindfulness and the consequent changes in trait mindfulness (e.g. Bravo et al., 2018; Kiken et al., 2015; Strohmaier et al., 2021 (Chapter 6)), the effect of individuals' baseline trait mindfulness on state mindfulness change after a mindfulness induction, and the associated changes of state hope and gratitude, have not yet been addressed in research. Previous research found trait mindfulness to positively moderate/strengthen the effect between engaging in a brief mindfulness induction and reduced physiological stress responses (Laurent et al., 2014); it would be helpful to know whether further moderating effects of trait mindfulness on positive psychological outcomes via state mindfulness change after a mindfulness induction exist.

7.2.5 Research Aims and Hypotheses

Therefore, given that single-dose mindfulness inductions and positive psychological outcomes were not included in the dose-response meta-regression (Chapters 2-5) and the effect of a single mindfulness practice on positive psychological outcomes was not examined in the experiment presented in Chapter 6, the current study aimed to examine the effects of a brief, single-session mindfulness practice on state hope and state gratitude, as well as the possible mediating effects of state mindfulness. Following Snyder (2002), state hope was taken to be the in the moment sense someone has of their capability and motivation to move towards their goals, while state gratitude was considered to be individuals' in the moment awareness of positive things in their life combined with gratefulness towards those who had contributed to these (Emmons and Mishra 2012; McCullough et al. 2002). Primary hypotheses were that: 1) engaging in a single mindfulness practice would result in improved state hope compared to controls; and 2) engaging in a single practice would improve state gratitude relative to control. The first of these hypotheses was grounded in Snyder (1994) theorizing that meditation can help calm the mind and reduce focus on daily stressors, rumination, and worry, and that this in turn allows greater deployment of attentional resources to focusing on moving towards hoped for goals and so increases the current sense of hope (Munoz et al., 2018). The second hypothesis was grounded in the theory that increased mindful awareness of positive things in life, and others' contribution to these, supports greater gratitude (cf. Emmon and Mishra 2002; McCullough et al. 2002). It is worth noting that a direct relation between state hope and state gratitude was not theorised, but rather that these were seen as distinct constructs that were both hypothesized to be increased by mindfulness practice as well as related greater state mindfulness. Therefore, secondary hypotheses were that: 3) improvement in state mindfulness would statistically meditate the effect of mindfulness practice on state hope; and 4) improvement in state mindfulness would statistically meditate the effect of mindfulness practice on state gratitude. Finally, although a start has been made in exploring the positive effects of mindfulness practice on state mindfulness and the consequent changes in trait mindfulness (e.g. Kiken et al., 2015), the moderating effect of individuals' baseline trait mindfulness on state mindfulness change, after a meditation practice, has not yet been addressed in this way in research. Therefore, an exploratory fifth hypothesis was that baseline trait mindfulness would statistically moderate the effect of mindfulness practice on state mindfulness and hence on state hope and gratitude; in other words, that the mediation models specified in Hypotheses 3 and 4 would be moderated by baseline trait mindfulness in a moderated mediation model.

7.3 Method

7.3.1 Design

This single-session, single-blind[18], online randomised controlled experiment had two arms, namely the experimental group consisting of a 10-minute mindfulness meditation practice and the active control group consisting of a 10-minute non-fictional audio recording. As in the study presented in Chapter 6, an active control group was chosen to reduce performance bias.

7.3.2 Participants and Recruitment

A priori power analysis using G*Power for finding a small to medium effect (ES=0.25), with α=0.05 and power of 0.95 for the primary analyses, suggested a required sample size of 82 or above. With regard to testing the secondary hypotheses, a sample of 462 participants has been recommended for mediation analysis using bias-corrected bootstrapping for small effect sizes in both a- and b-paths (small-small condition) and power of 0.8 (Fritz & McKinnon 2007). A total of 474[19] members of the general public as well as university students and staff (237 each in the mindfulness practice and control groups), aged between 18 and 69, participated. The general population was selected as the participant pool in this study due to the largest evidence-base of effectiveness of mindfulness

[18]This study was considered single-blind since participants could have realised whether they were in the mindfulness arm or not, especially if they had previously engaged in mindfulness practice.
[19]Recruitment continued until after the required number of participants of 462 was met to account for possible attrition. Recruitment culminated at the end of the university's financial year by which date the prize draw had to be administered.

inductions being for the general population (Leyland et al., 2019). Most participants identified as female (69%), indicated their ethnicity as White (75.7%) and their nationality as British (63.9%). Most participants indicated that they did not have a previous (*N*=329) or current (*N*=428) mindfulness practice. Where participants had previously or were currently engaging in mindfulness practice, this included having used, or currently using, mindfulness apps, having previously participated in a mindfulness course (either face-to-face or online), previously having read books on mindfulness, and having previously, or currently, practicing yoga. As an incentive for taking part, participants could choose to be entered in a prize draw to win online shopping vouchers. Psychology undergraduates could choose to receive course credits for participating instead.

The study was advertised online on internationally reaching social media channels, academic research promotion websites (such as Call for Participants) and Canterbury Christ Church University staff and student news outlets (including newsletters, notices, and posters as well as general staff- and student-wide emails), using opportunity and snowball sampling. Participants were informed that they would be asked to listen to something for ten minutes and to make sure they were not disturbed during this time. For study advertising, the briefer title “Study examining a brief online mindfulness and listening exercise” was used which does not disclose the effect being examined; participants only learned this at the end of the study during the debrief.

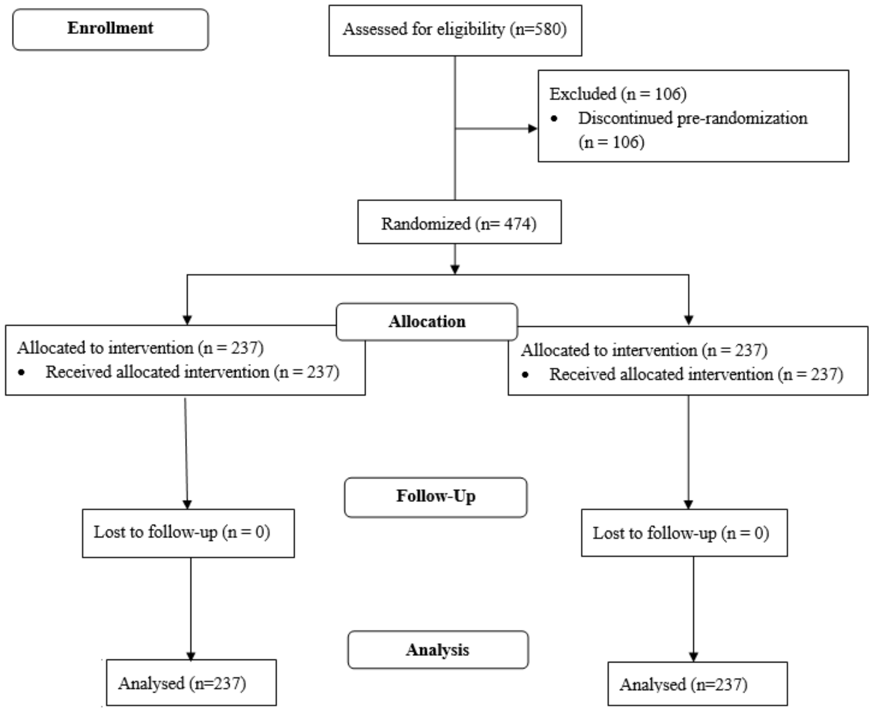
7.3.3 Procedure

Participants were included if they were aged 18 or over. Participants were excluded if they self-identified as currently experiencing severe difficulties with their mental health, in order to minimize the risk of possible harmful effects (cf. Britton 2019; Dobkin et al. 2012, see Chapter 1, Section 1.4.6.3), did not consent to participate, or if they withdrew from the study prior to the randomisation stage. The study was created and conducted using the online survey software Qualtrics (https://www.qualtrics.com). Participants could not continue with the study until the respective 10-minute audio recordings were completed.

7.3.3.1 Randomization

Participants were automatically randomized to the two groups of mindfulness practice or audiobook control with equal probability, using the computerised Qualtrics random block allocation procedure (Qualtrics, 2019). Prior to randomization, 106 participants withdrew from the study. Figure 7.1 shows the CONSORT flowchart of participant flow through the study.

Figure 7.1
CONSORT flow diagram



7.3.4 Materials

7.3.4.1 Recordings

The 10-minute mindfulness practice was recorded by a qualified mindfulness teacher (the PhD researcher's first supervisor FJ). The recording was a mindfulness of the breath meditation practice similar in style to those in MBSR and MBCT (Kabat-Zinn 1990; Segal et al. 2013) and the recordings used for the face-to-face study (Chapter 6); this type of practice also follows Crane et al.'s (2017) definition of what constitutes a mindfulness practice (see Chapter 2, Section 2.3.1 for more detail). The length of the mindfulness practice utilised in this study is similar to other mindfulness induction studies, which generally employ mindfulness practices between five and 15 minutes (e.g.

Heppner & Shirk, 2018; Kiken & Shook, 2011; Mahmood et al., 2016). Additionally, since research conducted online rather than in person tends to generally employ practices lasting at least 10 minutes (e.g. Cavanagh et al., 2018; Flett et al., 2018; Haliwa et al., 2021), this same length was chosen for this online study. The 10-minute practice in this study is thus reflective of online mindfulness induction literature. The transcript of the mindfulness practice is available in Appendix 7.3.1.

Participants in the control group were asked to listen to a 10-minute non-fiction recording on the scientific history of the universe, openly available in the public domain (Rolt-Wheeler, 2015). Reasons for choosing a non-fictional scientific recording are the same as for the study presented in Chapter 6 (Section 6.3.4.1), namely that this was similar to other non-fictional presentations individuals would hear in their daily life and research having found non-fictional recordings a helpful control group for a general population sample in a meditation study (Basso et al., 2019).

7.3.4.2 Measures

After providing informed consent and demographic information, participants were asked to complete the below self-report measures (see Appendix 7.3.2 for full measures). Each of the measures were completed both before and after the mindfulness practice/control listening exercise, with the exception of the Five Facet Mindfulness Questionnaire (FFMQ-15), which was only completed at baseline. Shorter versions of measures were selected, where available and psychometrically robust, since research has suggested that participants are less likely to complete longer measures accurately (Galesic & Bosnjak, 2009). There was no difference in measures administered between the two groups aside from an additional compliance question asked for in the mindfulness practice group. Respective titles of measures were removed for data collection to minimise potential response bias.

State Hope Scale (SHS; Snyder et al., 1996)

The SHS is a six-item measure of state hope with the two subscales of agency and pathways, which have three items each. Total state hope ranges from six to 48 with each subscale ranging from three to 24 with higher scores indicating greater state hope, agency, or pathways, respectively. The SHS has been validated with a large sample of university students showing high reliability and concurrent and discriminant validity (Snyder et al., 1996). Since its development and validation, this scale has been used in multiple studies and with different populations (e.g. Brooks & Hirsch, 2017; Feldman & Snyder, 2000). In the current sample, the total-scale SHS as well as both subscales had high internal consistency (total state hope: Cronbach's α=.91; agency: α=.88; pathways: α=.86).

Gratitude Adjective Checklist (GAC; McCullough et al., 2002)

The GAC is a three-item measure of the affect adjectives grateful, thankful, and appreciative used to assess gratitude. State as opposed to trait gratitude was measured by including "how you feel right now" in the instructions, as per authors' instructions (McCullough et al., 2002). The GAC ranges from three to 15 with higher sores indicating greater state gratitude. This measure has shown high internal consistency and concurrent validity and has been validated across multiple samples (Waters, 2012). In the current sample, the GAC showed high internal consistency (Cronbach's α=.95).

Toronto Mindfulness Scale (TMS; Lau et al., 2006)

The TMS is a 13-item questionnaire assessing state mindfulness with the two subscales curiosity and decentering. The curiosity subscale ranges from zero to 24, the decentering subscale from zero to 28, and the total state mindfulness scale from zero to 52, with higher scores indicating greater curiosity, decentering, and overall state mindfulness, respectively. This scale has shown good reliability and incremental as well as criterion validity in participants with and without previous meditation experience from the general population (Lau et al., 2006; Medvedev et al., 2017). In the current sample, the total scale TMS showed high internal consistency (Cronbach's α=.92) as did the subscales (curiosity: α=.92; decentering: α=.82).

Five Facet Mindfulness Questionnaire (FFMQ-15; Baer et al., 2012)

The FFMQ-15 measures trait mindfulness. This questionnaire has shown high levels of convergent validity before and after mindfulness-based programs as well as high reliability in a general population sample (Gu et al., 2016). For calculation of the total scale score, it is recommended to omit the observe subscale items (Baer et al. 2012; Gu et al., 2016) resulting in scores of trait mindfulness ranging between 12 and 60. Each FFMQ-15 subscale ranges between three and 15.With the current sample, the total FFMQ-15 showed good internal consistency (Cronbach's α = .86), as did all but the observe subscale (observe: α = .69; describe: α = .81; acting with awareness: α=.75; non-judging:

α=.84; non-reactivity: α=.77). However, the observe subscale of the 15-item version of the FFMQ has previously been found problematic and has been considered a poor fit if administered before a mindfulness practice especially in individuals new to mindfulness hence why it is recommended to be omitted when calculating the total-scale FFMQ-15 (Gu et al., 2016). The FFMQ-15 was administered at baseline only.

Compliance Check

As a compliance check, immediately after the recording was played, participants in both groups were asked to indicate from one (not at all) to 10 (completely) how well they paid attention to the recording. Participants in the mindfulness practice group were also asked to indicate how much they felt they were following the guidance during the practice on the same scale (one to 10).

Mindfulness Practice Experience

Along with demographic questions, participants were asked whether they had previously practiced or were currently regularly practicing mindfulness and if so, to provide details. This information was collected to be able to control for participants' previous and current mindfulness practice, since it is possible that the effects of a single mindfulness induction may vary between participants who are new to mindfulness and those who have either some historical or current experience of mindfulness practice.

7.3.5 Ethical Considerations

The study was granted full ethical approval by the Salomons Institute for Applied Psychology Ethics Panel; observations were addressed with and accepted by the head of the panel (Appendix 7.3.3). After reading the study information (Appendix 7.3.4), all participants provided informed consent (Appendix 7.3.5) and were debriefed at the end of the study (Appendix 7.3.6). As part of the above sections, participants were informed of their right to withdraw by creating a unique identifying code, were provided with the contact details of the researcher to ask questions as well as of an independent person to complain to if they wished and were given resources to use in light of potential distress arising from participation in this study. Participants were also informed that their data would be handled confidentially, and that any identifying information would be removed prior to analysis.

After ethical approval had been granted and prior to commencement of data collection (before the study link was live), this study was preregistered on the trial registration site ClinicalTrials.gov (https://clinicaltrials.gov/ct2/show/NCT04099758?term=sarah+strohmaier&draw=2&rank=2).

7.3.6 Data analyses

7.3.6.1 Analysis of Variance (ANOVA) to test Primary Hypotheses

To test the primary Hypotheses 1 and 2, two (group: mindfulness vs. control) by two (timepoint: pre vs. post) mixed Analyses of Variance (ANOVA) were performed in SPSS version 24 (IBM Corp., 2016), on the outcomes state hope, with the two subscales of agency and pathways, and state gratitude. Significant interactions were decomposed by running separate one-way ANOVAs for each group and for the two time points. Cohen's *d* effect sizes for i) between-group and ii) within-group ANOVAs were calculated using the formulae described by Lakens (2013).

Due to minor deviation from normality for some variables (see Section 7.4.1 below), the above analyses were repeated using robust methods of ANOVA on trimmed means with the package 'WRS2' (Mair &Wilcox, 2019) in R versions 4.0.2-4.1.0 (The R Foundation for Statistical Computing, 2020, 2021). These robust methods have been found effective if assumptions for normality and homoscedasticity are not met (Mair & Wilcox, 2019; see Chapter 6, Section 6.3.6.1 for detail). Results of the standard and robust methods did not meaningfully differ; both are presented for purposes of transparency. All above analyses were also completed for state mindfulness, and its two subscales curiosity and decentering, as outcomes. Further subsidiary checks on above outcomes included i) completing standard and robust (on trimmed means using the `ancboot` function of the WRS2 package in R) Analyses of Covariance (ANCOVAs) controlling for the dichotomous variables previous and current practice experience; ii) completing standard and robust ANCOVAs examining effects of the continuous variable of compliance to listening to recordings; iii) repeating all ANOVAs with participants with low compliance of listening to recordings (<5) removed; iv) exploring moderating effects of compliance of listening to recordings between group assignment and outcomes using model 1 in Hayes' (2019) PROCESS macro version 3.4 with bootstrapping set to 5,000, controlling for baseline levels of outcomes, and employing the Bonferroni correction; v) repeating standard and robust ANOVAs with mindfulness practice group participants with low levels of following guidance (<5) removed.

7.3.6.2 Mediation and Moderated Mediation Analyses to test Secondary Hypotheses

To test Hypotheses 3 and 4, mediation analyses were completed using model 4 of Hayes' (2019) PROCESS macro version 3.4, with bootstrapping set to 5,000, as recommended by Hayes (2019), and controlling for baseline levels of the respective outcome. Bias-corrected bootstrapping was employed for mediation analysis to reduce possible skewness in the data (Fritz & McKinnon, 2007). To test the exploratory Hypothesis 5, moderated mediation analyses were completed using model 7 of PROCESS, again controlling for baseline levels of the respective outcome and with bootstrapping set to 5,000. To control for the possible inflation of family-wise alpha levels due to multiple comparisons and thus the possibility of a Type I error having occurred, the Bonferroni correction ($\alpha \leq .001$ (99.9% C.I.)) was applied to significant results in mediation and moderated mediation analyses.

7.4 Results

7.4.1 Data Screening and Testing of Assumptions

No univariate outliers were observed for any of the outcomes including any subscales neither at the pre nor post timepoint since z-scores of outcomes were above the threshold of $z>3.29$ (Tabachnick & Fidell, 2014). Multivariate outliers were assessed with Mahalanobis Distance (MD), which were then compared to a chi-square distribution with equal degrees of freedom. Mahalanobis Distance values were low with the largest value being MD=12.42 which is not considered problematic for a dataset of this size (Barnett & Lewis, 1978). The probability of a datapoint being a multivariate outlier was not significant $p>.05$ for all participants and therefore no multivariate outlier was found or removed from the participant pool. There was no missing data for any of the outcomes neither at pre- nor post-listening exercises for neither group; missing data analyses were therefore not completed.

Normality assumptions for ANOVAs were completed using residuals to examine the potential difference between predicted and observed values. Although no standardised residuals were above the acceptable threshold of 3.29, visual inspection of histograms showed slight deviation from normality in the distribution of scores particularly for the gratitude outcome (see Appendix Figure 7.4.1 for histograms of standardised residuals). Kolmogrov-Smirnov and Shapiro-Wilk tests of normality for standardised residuals were also completed at pre and post timepoints for the overall sample and for the two groups separately, which showed significant deviation from normality for the majority of outcomes (see Appendix 7.4.2 for SPSS output of normality tests on standardised residuals both overall and by group). However, as mentioned previously (Chapter 6, Section 6.4.1), the Kolmogrov-Smirnov test has previously been identified as quite stringent (Steinskog et al., 2007) and thus needs to be interpreted with caution. Histograms for the mediator state mindfulness (including curiosity and decentering) and the moderator trait mindfulness showed roughly normal distributions (see Appendix Figure 7.4.3) although this is again inconsequential since the bootstrapping approach was used which has been considered robust in non-normally distributed data (Hayes, 2009).

Next, the assumption of homogeneity of variance needed to be explored since a between-subjects design was employed. Homogeneity of variance was examined with Levene's test, however, as mentioned in Chapter 6, this test needs to be interpreted with caution due to having been criticised for being inaccurate (Nordstokke & Zumbo, 2007). Variances significantly differed for outcomes at post for the primary outcomes but not pre listening exercises (see Appendix 7.4.4 for results of Levene's test), as expected. The assumption for sphericity did not need to be tested since only two timepoints (pre and post) were examined.

Since mediation, moderation, and moderated mediation analyses were completed, the assumption of linearity needed to be met. Linear relationships were found for all outcomes and their predictors (see Appendix Figure 7.4.5 for scatterplots depicting linearity for all outcomes).

Finally, for moderated mediation analyses, assumptions of multicollinearity needed to be tested due to multiple predictors having been added to the model to determine whether trait and state mindfulness measured the same constructs. Trait mindfulness (including all subscales) and state mindfulness (including subscales curiosity and decentering) only correlated to a small to moderate amount (r<.5) and thus did not measure exactly the same (Dancey & Reidy, 2011; Appendix 7.4.6).

Additionally, as VIF was above one and well below 10 and tolerance was low (tolerance<0.2) no collinearity in the data was observed (Field, 2018).

7.4.2 Demographic Characteristics and Outcome Data at Baseline and Post Timepoints

Table 7.1 shows the demographic characteristics of the study sample as a whole and per group. At baseline, there was no significant group difference in demographic variables and participants did not significantly differ in amount of previous and current mindfulness practice experience. Table 7.2 shows outcomes for each group at pre and post timepoints. Scores were within the range of what would be expected for a general population sample based on normative data means and standard deviations across state hope (Snyder et al., 1996), gratitude (Waters, 2012), state mindfulness (Lau et al., 2006), and trait mindfulness (Gu et al., 2016) measures suggesting that a similar degree of variability was captured in this sample as in other general population samples. There were no significant differences between participants who withdrew and those who completed the study, neither in demographics nor for any of the outcome variables at baseline (Appendix 7.4.7).

Table 7.1
Demographic information by group and group comparison at baseline

	Whole sample	Mindfulness practice group	Control group	Group comparison
N	474	237	237	
Age M (SD)	29.31 (10.79)	30.05 (11.43)	28.58 (10.07)	t=1.48; p=.14
Gender N (%)	327 (69%) female	167 (70.5%) female	160 (67.5%) female	χ^2=1.39; p=.71
	141 (29.7%) male	66 (27.8%) male	75 (31.6%) male	
	3 (0.6%) non-binary	2 (0.8%) non-binary	1 (0.4%) non-binary	
	3 (0.6%) prefer not to say	2 (0.8%) prefer not to say	1 (0.4%) prefer not to say	
Ethnicity N (%)	33 (7%) Asian	21 (8.9%) Asian	12 (5.1%) Asian	χ^2=19.23; p=.2
	40 (8.4%) Black	15 (6.3%) Black	25 (10.5%) Black	
	359 (75.7%) White	186 (78.5) White	173 (73%) White	
	30 (6.4%) Mixed Background	6 (2.5%) Mixed Background	24 (10.2%) Mixed Background	
	8 (1.7%) Other Ethnic Background	6 (2.5%) Other Ethnic Background	2 (0.8%) Other Ethnic Background	
	4 (0.9%) Prefer not to say	3 (1.3%) Prefer not to say	1 (0.4%) Prefer not to say	
Nationality N (%)	303 (63.9%) British	139 (58.6%) British	164 (69.2%) British	χ^2=112.86;
	97 (20.5%) European	57 (24%) European	40 (16.9%) European	p=.36
	13 (2.7%) African	6 (2.5%) African	7 (3%) African	
	21 (4.4%) North American	12 (5.1%) North American	9 (3.7%) North American	
	4 (0.8%) South American	3 (1.3%) South American	1 (0.4%) South American	
	6 (1.4%) Caribbean	3 (1.3%) Caribbean	3 (1.3%) Caribbean	
	22 (4.6%) North and South Asian	12 (5.1%) North and South Asian	10 (4.2%) North and South Asian	
	3 (0.6%) New Zealander	3 (1.3%) New Zealander	3 (1.3%) More than 1 nationality	
	5 (1.1%) More than 1 nationality	2 (0.8%) More than 1 nationality		
Occupation *N* (%)	16 (3.4%) Arts	7 (3%) Arts	9 (3.7%) Arts	χ^2=21.22;
	18 (3.8%) Construction and Production	8 (3.4%) Construction and Production	10 (4.2%) Construction and Production	p=.57
	52 (11%) Education	26 (11%) Education	26 (11%) Education	
	19 (4%) Hospitality	8 (3.4%) Hospitality	11 (4.6%) Hospitality	
	114 (24.1%) Office and Sales	55 (23.2%) Office and Sales	59 (24.9%) Office and Sales	
	3 (0.6%) Retired	1 (0.4%) Retired	2 (0.8%) Retired	
	48 (10.1%) Social and Health Care	29 (12.2%) Social and Health Care	19 (8%) Social and Health Care	
	12 (2.5%) Unemployed	7 (3%) Unemployed	5 (2.1%) Unemployed	
	185 (39%) University Student	91 (38.4%) University Student	94 (39.8%) University Student	

	7 (1.5%) Prefer not to say	5 (2.1%) Prefer not to say	2 (0.8%) Prefer not to say	
Previous mindfulness practice *N* (%)	329 (69.4%) No 145 (30.6%) Yes	164 (69.2%) No 73 (30.8%) Yes	165 (69.6%) No 72 (30.4%) Yes	χ^2=130.71; *p*=.54
Current mindfulness practice *N* (%)	428 (90.3%) No 46 (9.7%) Yes	218 (92%) No 19 (8%) Yes	211 (89%) No 26 (11%) Yes	χ^2=39.11; *p*=.51

N=Number of participants; *M*=Mean; *SD*=Standard Deviation

Table 7.2
Outcome data at pre and post timepoints for mindfulness and control groups

Outcome	Mindfulness group (N=237)		Control group (N=237)	
	Pre M (SD)	Post M (SD)	Pre M (SD)	Post M (SD)
State Hope (SHS total)	29.48 (8.27)	34.62 (7.37)	28.31 (9.06)	28.91 (9.17)
SHS Agency	14.09 (4.6)	16.83 (4.28)	13.56 (5.03)	14.51 (5.08)
SHS Pathways	15.38 (4.33)	17.79 (3.71)	14.75 (4.68)	12.63 (4.73)
State Gratitude (GAC)	10.14 (3.25)	12.04 (2.29)	9.74 (3.68)	8.62 (3.67)
State mindfulness (TMS total)	22.33 (9.74)	30.55 (10.1)	22.65 (10.96)	21.95 (11.19)
TMS Curiosity	11.96 (5.84)	14.76 (5.28)	12.02 (6.28)	11.62 (6.33)
TMS Decentering	10.33 (4.93)	15.79 (5.59)	10.63 (5.52)	10.33 (5.65)
Trait mindfulness (FFMQ-15 total)	35.09 (4.11)	-	35.7 (3.96)	-
FFMQ-15 Observe	9.23 (2.47)	-	8.91 (2.44)	-
FFMQ-15 Describe	8.59 (2.57)	-	9.35 (1.96)	-
FFMQ-15 Act Aware	9.08 (1.85)	-	8.91 (1.74)	-
FFMQ-15 Non-Judge	9.85 (2.3)	-	9.68 (2.1)	-
FFMQ-15 Non-react	8.99 (2.47)	-	8.62 (2.3)	-

M=Mean; *SD*=Standard Deviation; SHS=State Hope Scale; GAC=Gratitude Adjective Checklist; TMS=Toronto Mindfulness Scale; FFMQ-15=Five Facet Mindfulness Questionnaire (pre only).

7.4.3 Group by Time Comparison to test Primary Hypotheses

Group (mindfulness practice vs. control) by time (pre vs. post) mixed ANOVAs showed significant interactions for total state hope, and both hope subscales, and state gratitude (see Table 7.3 for both standard and robust results). Thus, participating in a mindfulness practice resulted in significantly higher levels of state hope and state gratitude compared to controls, confirming Hypotheses 1 and 2.

Table 7.3
Group by time ANOVAs using standard and robust (mixed ANOVA on trimmed means) methods testing primary Hypotheses (1 and 2)

Outcomes		**Standard**	**Robust**
State Hope (SHS total)	group	$F(1, 472) = 25.92$*** part. $\eta^2 = .05$	$F(1, 273.11) = 19.66$***
	time	$F(1, 472) = 135.43$*** part. $\eta^2 = .22$	$F(1, 147.32) = 104.41$***
	group*time	$F(1, 472)=154.94$*** part. $\eta^2 = .25$	$F(1, 147.32) = 122.09$***
SHS Agency	group	$F(1, 472) = 21$*** part. $\eta^2 = .04$	$F(1, 262.25) = 18.03$***
	time	$F(1, 472) = 125.46$*** part. $\eta^2 = .21$	$F(1, 153.66) = 96.54$***
	group*time	$F(1, 472)=135.94$*** part. $\eta^2 = .21$	$F(1, 153.66) = 112.99$***
SHS Pathways	group	$F(1, 472) = 24.06$*** part. $\eta^2 = .05$	$F(1, 275.26) = 17.38$***
	time	$F(1, 472) = 106.53$*** part. $\eta^2 = .18$	$F(1, 152,98) = 80.38$***
	group*time	$F(1, 472)=129.64$*** part. $\eta^2 = .23$	$F(1, 152.98) = 95.49$***
State Gratitude (GAC)	group	$F(1, 472) = 23.99$*** part. $\eta^2 = .05$	$F(1, 249.31) = 16.4$***
	time	$F(1, 472) = 105.14$*** part. $\eta^2 = .18$	$F(1, 152.61) = 57.07$***
	group*time	$F(1, 472)=133.63$*** part. $\eta^2 = .22$	$F(1, 152.61) = 72$***

SHS=State Hope Scale; GAC=Gratitude Adjective Checklist. ****p*<.001; part. η^2=partial eta squared.

Subsequent one-way ANOVAs revealed that groups significantly differed at the post mindfulness/control exercise timepoint whereas they did not at baseline (see Table 7.4 for standard and robust between-group ANOVA results for pre and post timepoints).

Table 7.4
The main effect of group in between-group, one-way, standard, and robust ANOVAs conducted separately for pre and post timepoints

	PRE	
Outcomes	**Standard**	**Robust**
State Hope (SHS total)	F(1, 472)=2.14	F(1, 283.89) = 0.3
SHS Agency	F(1, 472)=1.44	F(1, 281.1) = 0.36
SHS Pathways	F(1, 472)=2.34	F(1, 281.65) = 0.51
State Gratitude (GAC)	F(1, 472)=1.58	F(1, 283.87) = 0.78
	POST	
Outcomes	**Standard**	**Robust**
State Hope (SHS total)	F(1, 472)=71.92*** *d*=0.68	F(1, 272.47) = 71.33***
SHS Agency	F(1, 472)=59.24*** *d*=0.49	F(1, 269.3) = 58.64***
SHS Pathways	F(1, 472)=65.54*** *d*=1.21	F(1, 263.39) = 61.31***
State Gratitude (GAC)	F(1, 472)=74.05*** *d*=1.12	F(1, 219.27) = 56.38***

SHS=State Hope Scale; GAC=Gratitude Adjective Checklist. ***p<.001; **p < .01; *p < .05; *d*=Cohen's *d*.

One-way ANOVAs on each group separately showed significant pre to post increases for the mindfulness group but not for controls (see Table 7.5 for standard and robust results).

Table 7.5
Main effect of time from within group, standard and robust ANOVAs conducted separately on each group

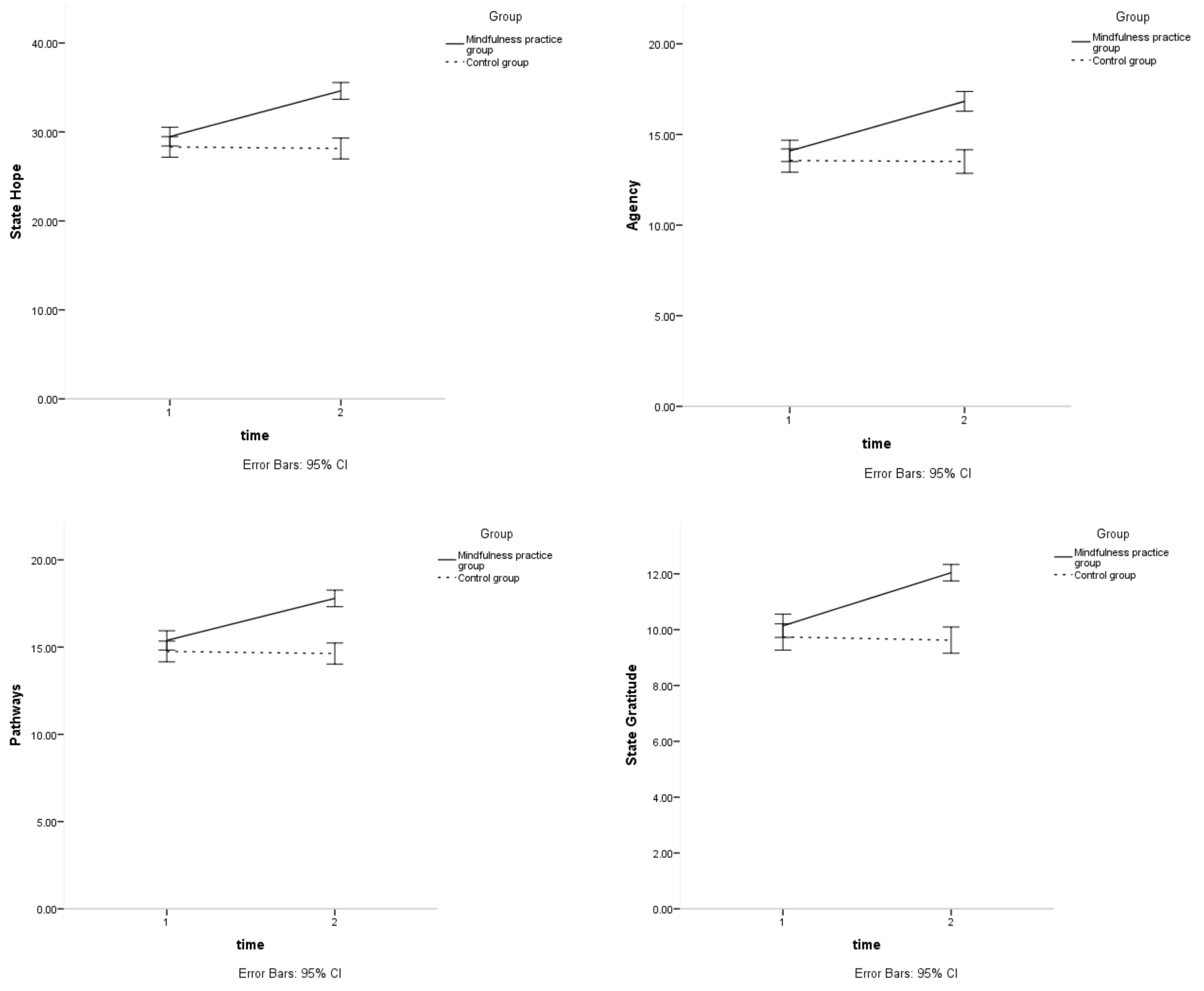
Outcome	**Standard**		**Robust**	
	Mindfulness group	Control group	Mindfulness group	Control group
State Hope (SHS total)	$F(1, 236)=147.78$*** $d=0.79$	$F(1, 236)=8.82$	$F(1, 264.95) = 51.99$***	$F(1, 283.39) = 0.07$
SHS Agency	$F(1, 236)=135.57$** $d=0.75$	$F(1, 236)=1.45$	$F(1, 278.74) = 51.4$**	$F(1, 284) = 0.06$
SHS Pathways	$F(1, 236)=122.36$*** $d=0.72$	$F(1, 236)=7.6$	$F(1, 250.48) = 39.46$**	$F(1, 283.99) = 0.06$
State Gratitude (GAC)	$F(1, 236)=124.05$*** $d=0.72$	$F(1, 236)=10.38$	$F(1, 220.74) = 37.38$***	$F(1, 283.97) = 0.08$

SHS=State Hope Scale; GAC=Gratitude Adjective Checklist; *d*=Cohen's *d*; ***$p<.001$; **$p < .01$; *$p < .05$.

Figure 7.2 shows the pre- to post change in state hope and state gratitude by group.

Figure 7.2

Mean outcome measures at pre (time 1) and post (time 2), for the mindfulness and control groups. Top left: state hope (total-SHS); top right: agency (SHS); bottom left: pathways (SHS); bottom right: state gratitude (GAC).



Similar to state hope and gratitude, when repeating mixed, between- and within-group ANOVAs with the outcomes state mindfulness, and the two TMS subscales curiosity and decentering, significant interactions were observed with mindfulness practice participants showing significantly greater state mindfulness, curiosity, and decentering at the post-timepoint compared to controls (see Appendix Tables 7.4.8-7.4.10 for standard and robust results of mixed, between-group and within-group ANOVAs for state mindfulness, curiosity, and decentering).

In subsidiary analyses, the main effect of mindfulness practice compared to control remained significant when controlling for both, previous and current mindfulness practice experience (see Appendix 7.4.11 for SPSS outputs). Compliance[20] to listening to the recording was significantly different between groups (t=3.25; p=.001; 95% C.I.[0.24, 0.98]) with mindfulness practice group participants indicating that they were on average paying more attention to the recording (M=7.42; SD=1.99) than the control group (M=6.81; SD=2.08). However, compliance was generally high (only 32 participants out of 474 scored <5 on the compliance scale) indicating that most participants (N=442) paid what was considered a sufficient amount of attention. When examining compliance, the effect of mindfulness practice remained significant for all outcomes compared to controls for standard and robust methods[21] (see Appendix 7.4.12 for SPSS outputs). When repeating analyses with participants with low compliance (compliance<5) removed, findings did not differ from the overall analyses neither for standard nor robust methods (Appendix 7.4.13). Furthermore, an exploratory moderator analysis with bias-corrected bootstrapping set to 5,000 was completed to determine whether compliance to listening to recordings moderated the relationship between group allocation and outcomes at post-study while controlling for baseline levels of respective outcomes. Moderator analyses showed that amount of compliance significantly strengthened the relationship between group assignment and increased state hope and gratitude. This remained significant for all outcomes when employing the Bonferroni correction to control for Type I errors. Table 7.6 shows corrected interaction effects for hope and gratitude outcomes (results of corrected moderation effects for state mindfulness and TMS subscales are in Appendix Table 7.4.14).

[20]As outlined in the methods (Section 7.3.4.2) there were two ratings of compliance, namely amount of attention paid and ability to follow mindfulness practice guidance. The rating that was used for the subsidiary ANCOVA and moderator analyses was amount of attention paid since this was the compliance measure that both groups completed.

[21]Although the assumption of homogeneity of regression slopes for ANCOVA was met, the assumption of independence of the covariate and experimental effect needed for ANCOVA was violated (groups significantly differed in amount of attention paid). Therefore, robust ANCOVAs were completed on trimmed means using the ancboot function of the WRS2 package (Mair & Wilcox, 2019); results of robust ANCOVAs showed that all findings remained significant when controlling for compliance.

Table 7.6
Corrected (99.9% C.I.) interaction effects between group (mindfulness vs. control) and compliance to listening to recordings for the outcomes state hope (including subscales agency and pathways) and state gratitude

	Group x Compliance						
	F(1,469)	**ΔR^2**	***p***	***b***	**SE(boot)**	**t**	**99.9%C.I.**
State Hope (SHS total)	47.57	0.02	<.001	2.38	0.3	8.03	[1.4, 3.36]
SHS Agency	38.26	0.02	<.001	1.26	0.17	7.42	[0.7, 1.82]
SHS Pathways	43.61	0.02	<.001	1.14	0.15	7.28	[0.63, 1.65]
State Gratitude (GAC)	90.12	0.04	<.001	1.13	0.11	10.31	[0.77, 1.49]

ΔR^2=adjusted R^2 change; *b*= moderation effect; SE(boot)=bootstrapped Standard Error; 95% C.I.= 95% Confidence Intervals.

Additionally, most mindfulness practice participants indicated that they were able to follow the mindfulness practice guidance reasonably well (*M*=7.42; *SD*=2.08) with only 17 of 237 participants indicating a low level (<5) of following the guidance. When repeating analyses with participants of low level of following guidance removed, this did not alter results (see Appendix 7.4.15).

7.4.4 Effect of the Mediator State Mindfulness Change to test Secondary Hypotheses

Bias-corrected bootstrapped mediation analyses with resampling set to 5,000 examined whether pre to post change in state mindfulness statistically mediated the relationship between group assignment and state hope at the post timepoint, while controlling for baseline state hope. The same analysis was repeated for the SHS subscales agency and pathways, and state gratitude, and all analyses were repeated with change in TMS subscales curiosity and decentering taking their respective turns as mediator.

Change in state mindfulness, as well as change in curiosity and decentering, significantly mediated the relationship between group allocation and each of the four outcomes (i.e. state hope, the two hope subscales, and state gratitude), thus confirming Hypotheses 3 and 4. These results remained significant when controlling for the inflation of alpha-levels due to multiple comparisons by applying the Bonferroni correction (i.e. 99.9% C.I.), see Table 7.7.

Table 7.7
Corrected indirect effects in bootstrapped mediation models
(Group allocation as independent variable, post mindfulness/control exercise outcome as the dependent variable, and baseline outcome as the covariate (each row represents a separate mediation model))

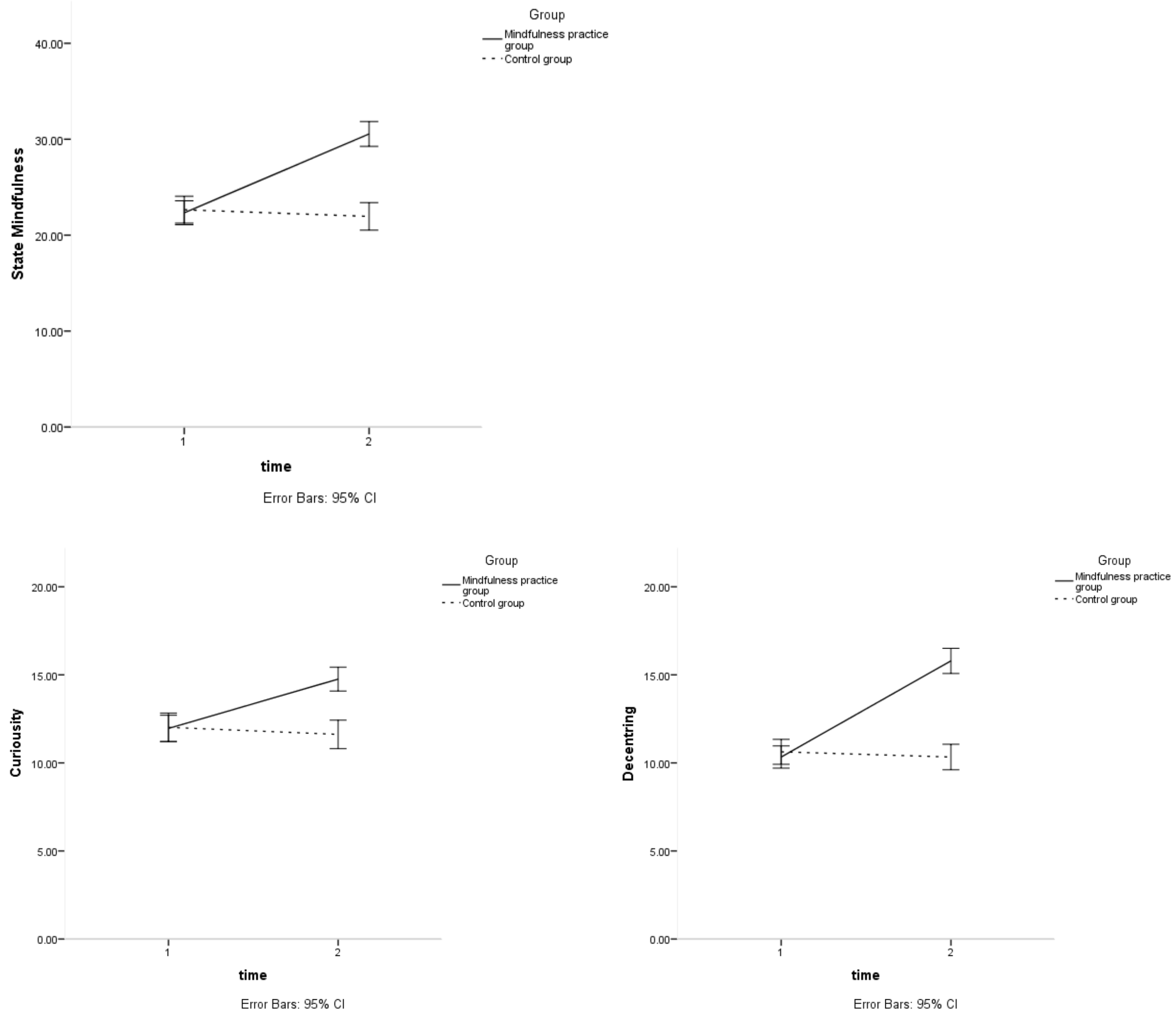
Outcome	**Mediator: Change in State Mindfulness**		
	b	SE(boot)	99.9% CI (boot)
Hope (total)	3.02	0.42	[1.62, 4.55]*
Agency	1.46	0.22	[0.7, 2.14]*
Pathways	1.53	0.21	[0.84, 2.32]*
Gratitude	1.12	0.16	[0.67, 1.72]*
	Mediator: Change in Curiosity		
	b	SE(boot)	99.9% CI (boot)
Hope (total)	1.81	0.33	[0.87, 3.01]*
Agency	0.88	0.17	[1.31, 2.69]*
Pathways	0.92	0.17	[0.43, 1.47]*
Gratitude	0.69	0.13	[0.27, 1.16]*
	Mediator: Change in Decentering		
	b	SE(boot)	99.9% CI (boot)
Hope (total)	3.26	0.44	[0.97, 3.58]*
Agency	1.58	0.22	[0.52, 2.08]*
Pathways	1.65	0.22	[0.96, 2.47]*
Gratitude	1.14	1.16	[0.67, 1.72]*

b=estimate of indirect effect; SE boot=bootstrapped Standard Error; 99.9% C.I.= 99.9% Confidence Interval; *significant after Bonferroni correction.

Figure 7.3 shows change of state mindfulness, curiosity and decentering from pre to post for both groups.

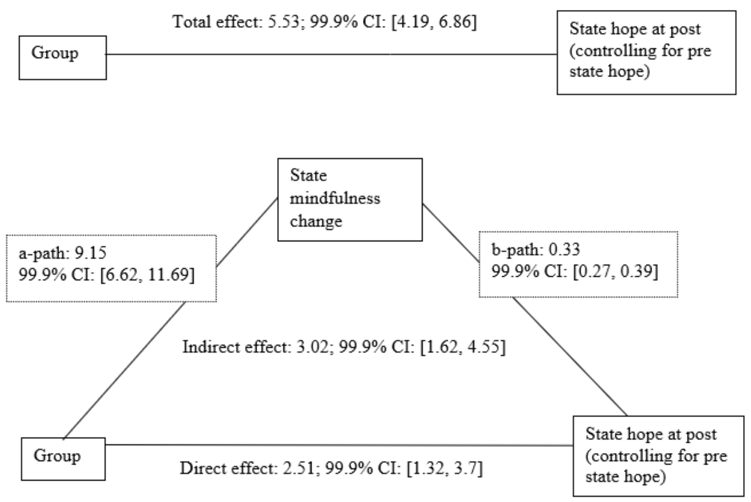
Figure 7.3

Change in state mindfulness from pre (time 1) to post (time 2)
Group 1 (mindfulness practice) and group 2 (control). Top left: state mindfulness (total-TMS), bottom left: curiosity (TMS), bottom right: decentering (TMS))



As an example, Figure 7.4 shows the mediation model for the outcome state hope (total-SHS) and mediator change in state mindfulness (total-TMS). Remaining mediation models are in Appendix Figure 7.4.16.

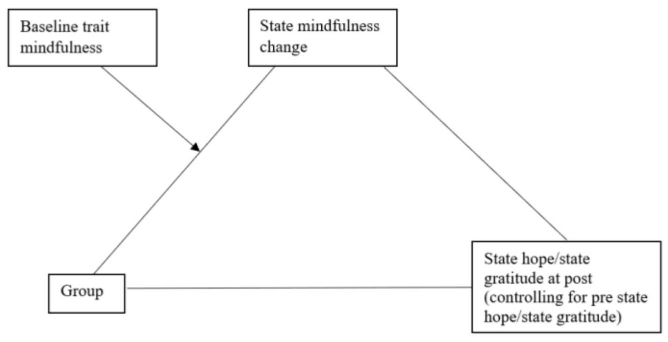
Figure 7.4
Corrected mediation model for dependent variable post state hope, independent variable group allocation, mediator state mindfulness change, and covariate baseline state hope.
Top diagram: total effect when excluding mediator. Bottom diagram: indirect and direct effects when including mediator.



7.4.5 Effect of Moderated Mediation with Moderator Trait Mindfulness and Mediator State Mindfulness Change

More speculatively, bootstrapped moderated mediation analyses examined whether the above mediation effects were moderated by baseline trait mindfulness as shown in Figure 7.5.

Figure 7.5
Hypothesised moderated mediation model.



No significant moderated mediation effects were found for the moderator trait mindfulness (total-FFMQ-15) for either of the mediators or outcomes (see Table 7.8 for uncorrected moderated mediation effects). As an example, Figure 7.6 shows the moderated mediation model for the outcome state hope (total-SHS), mediator change in state mindfulness (total-TMS) and moderator trait mindfulness (total-FFMQ-15). Remaining moderated mediation models are in Appendix Figure 7.4.17.

Table 7.8

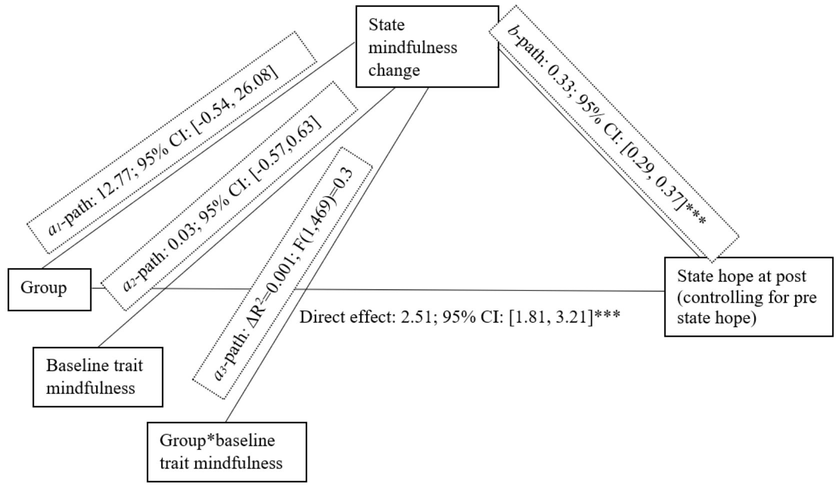
Moderated mediation effects with moderator trait mindfulness (total-FFMQ-15) and mediators change in state mindfulness (total-TMS), curiosity (TMS) and decentering (TMS)

Outcome	**Mediator: Change in State Mindfulness**		
	b	SE(boot)	95% CI (boot)
Hope (total)	-0.03	0.06	[-0.15, 0.09]
Agency	-0.02	0.03	[-0.07, 0.04]
Pathways	-0.02	0.03	[-0.07, 0.05]
Gratitude	-0.01	0.02	[-0.05, 0.04]
	Mediator: Change in Curiosity		
	b	SE(boot)	95% CI (boot)
Hope (total)	-0.05	0.05	[-0.15, 0.06]
Agency	-0.03	0.03	[-0.08, 0.02]
Pathways	-0.02	0.03	[-0.08, 0.04]
Gratitude	-0.01	0.02	[-0.05, 0.03]
	Mediator: Change in Decentering		
	b	SE(boot)	95% CI (boot)
Hope (total)	0.002	0.05	[-0.1, 0.12]
Agency	-0.004	0.03	[-0.05, 0.05]
Pathways	0.003	0.03	[-0.05, 0.06]
Gratitude	0.01	0.02	[-0.03, 0.05]

b=estimate of moderated mediation effect; SE boot=bootstrapped Standard Error; 95% C.I.= 95% Confidence Intervals.

Figure 7.6

Moderated mediation model for post post-study state hope (SHS-total) with group allocation as predictor, state mindfulness change (total-scale TMS) as mediator, baseline trait mindfulness (FFMQ-15 total) as moderator and baseline state hope as covariate.
*(95% CI=95% Confidence Intervals; *p<.05; ***p<.001; ΔR^2= adjusted R^2 change)).*



As an exploratory additional analysis, the moderated mediation analyses were repeated with the individual subscales of the FFMQ-15 taking their turn as moderator in the model. Only one of these were significant: acting with awareness significantly moderated (strengthened) the effect between mindfulness practice and the mediators change in total-scale state mindfulness and curiosity (but not decentering). In other words, for individuals with higher levels of trait acting with awareness, a greater effect of mindfulness practice on state mindfulness was found. However, these findings need to be treated with caution as this was not a pre-planned analysis and was no longer significant when applying the Bonferroni-correction; it may thus be a Type I error. Table 7.9 shows moderated mediation effects with FFMQ-15 subscale moderators. As an example, Figure 7.7 shows the significant moderated mediation model for state hope (total-SHS), mediator change in state mindfulness (total-TMS), and moderator acting with awareness (FFMQ-15). Appendix Figure 7.4.18 shows remaining significant uncorrected moderated mediation models with the moderator acting with awareness.

Table 7.9

Moderated mediation effects with FFMQ-15 subscales
(Moderators observe, describe, acting with awareness, non-judge and non-react, and mediators state mindfulness (total-TMS), curiosity (TMS) and decentering (TMS)
(Each row reports a different analysis, since each analysis only contained one moderator and one mediator))

MODERATOR: OBSERVE			
Outcome	**Mediator: Change in State Mindfulness**		
	b	SE(boot)	95% CI (boot)
Hope (total)	0.004	0.11	[-0.2, 0.23]
Agency	0.004	0.05	[-0.1, 0.11]
Pathways	0.01	0.05	[-0.09, 0.12]
Gratitude	-0.02	0.04	[-0.1, 0.05]
	Mediator: Change in Curiosity		
	b	SE(boot)	95% CI (boot)
Hope (total)	0.02	0.1	[-0.16, 0.22]
Agency	0.01	0.05	[-0.07, 0.11]
Pathways	0.02	0.05	[-0.07, 0.12]
Gratitude	-0.01	0.04	[-0.08, 0.06]
	Mediator: Change in Decentering		
	b	SE(boot)	95% CI (boot)
Hope (total)	-0.01	0.1	[-0.19, 0.19]
Agency	-0.003	0.05	[-0.09, 0.09]
Pathways	0.002	0.05	[-0.09, 0.1]
Gratitude	-0.02	0.03	[-0.09, 0.04]
MODERATOR: DESCRIBE			
Outcome	**Mediator: Change in State Mindfulness**		
	b	SE(boot)	95% CI (boot)
Hope (total)	0.12	0.1	[-0.08, 0.33]

Agency	0.06	0.05	[-0.04, 0.16]
Pathways	0.06	0.05	[-0.05, 0.16]
Gratitude	0.03	0.04	[-0.05, 0.11]
	Mediator: Change in Curiosity		
	b	SE(boot)	95% CI (boot)
Hope (total)	0.11	0.1	[-0.07, 0.3]
Agency	0.05	0.05	[-0.04, 0.15]
Pathways	0.05	0.05	[-0.04, 0.15]
Gratitude	0.03	0.04	[-0.04, 0.1]
	Mediator: Change in Decentering		
	b	SE(boot)	95% CI (boot)
Hope (total)	0.1	0.1	[-0.09, 0.29]
Agency	0.05	0.05	[-0.04, 0.14]
Pathways	0.05	0.05	[-0.05, 0.14]
Gratitude	0.02	0.03	[-0.04, 0.09]
MODERATOR: ACTING WITH AWARENESS			
Outcome	**Mediator: Change in State Mindfulness**		
	b	SE(boot)	95% CI (boot)
Hope (total)	**0.32**	**0.14**	**[0.07, 0.59]**
Agency	**0.17**	**0.07**	**[0.04, 0.3]**
Pathways	**0.16**	**0.07**	**[0.03, 0.3]**
Gratitude	**0.1**	**0.05**	**[0.01, 0.2]**
	Mediator: Change in Curiosity		
	b	SE(boot)	95% CI (boot)
Hope (total)	**0.35**	**0.12**	**[0.12, 0.6]**
Agency	**0.18**	**0.06**	**[0.07, 0.31]**

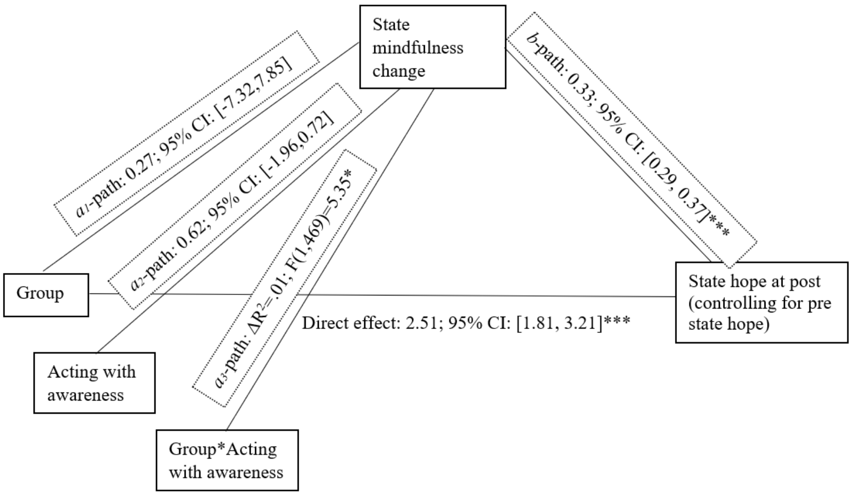
Pathways	**0.18**	**0.06**	**[0.06, 0.3]**
Gratitude	**0.12**	**0.05**	**[0.03, 0.22]**
	Mediator: Change in Decentering		
	b	SE(boot)	95% CI (boot)
Hope (total)	0.18	0.12	[-0.06, 0.44]
Agency	0.1	0.06	[-0.02, 0.22]
Pathways	0.09	0.06	[-0.03, 0.22]
Gratitude	0.05	0.04	[-0.04, 0.14]
MODERATOR: NON-JUDGE			
Outcome	**Mediator: Change in State Mindfulness**		
	b	SE(boot)	95% CI (boot)
Hope (total)	0.01	0.12	[-0.22, 0.24]
Agency	0.01	0.06	[-0.11, 0.12]
Pathways	-0.003	0.06	[-0.12, 0.11]
Gratitude	-0.02	0.04	[-0.1, 0.07]
	Mediator: Change in Curiosity		
	b	SE(boot)	95% CI (boot)
Hope (total)	0.13	0.11	[-0.09, 0.34]
Agency	0.07	0.05	[-0.04, 0.17]
Pathways	0.06	0.06	[-0.06, 0.17]
Gratitude	0.03	0.04	[-0.05, 0.11]
	Mediator: Change in Decentering		
	b	SE(boot)	95% CI (boot)
Hope (total)	-0.12	0.1	[-0.32, 0.09]
Agency	-0.05	0.05	[-0.16, 0.05]
Pathways	-0.07	0.05	[-0.17, 0.04]

Gratitude	-0.06	0.04	[-0.13, 0.01]
MODERATOR: NON-REACT			
Outcome	**Mediator: Change in State Mindfulness**		
	b	SE(boot)	95% CI (boot)
Hope (total)	0.003	0.11	[-0.22, 0.22]
Agency	0.02	0.06	[-0.08, 0.14]
Pathways	-0.003	0.06	[-0.11, 0.11]
Gratitude	-0.02	0.04	[-0.1, 0.06]
	Mediator: Change in Curiosity		
	b	SE(boot)	95% CI (boot)
Hope (total)	0.11	0.11	[-0.1, 0.32]
Agency	0.07	0.05	[-0.03, 0.18]
Pathways	0.05	0.05	[-0.06, 0.15]
Gratitude	0.02	0.04	[-0.06, 0.11]
	Mediator: Change in Decentering		
	b	SE(boot)	95% CI (boot)
Hope (total)	-0.1	0.1	[-0.29, 0.1]
Agency	-0.03	0.05	[-0.13, 0.07]
Pathways	-0.05	0.05	[-0.15, 0.05]
Gratitude	-0.05	0.03	[-0.12, 0.02]

b=estimate of moderated mediation effect; SE boot=bootstrapped Standard Error; 95% C.I.= 95% Confidence Intervals; significant results in bold.

Figure 7.7

Moderated mediation model for post post-study state hope (SHS-total) with group allocation as predictor, state mindfulness change (total-scale TMS) as mediator, baseline acting with awareness (FFMQ-15) as moderator and baseline state hope as covariate.
*(95% CI=95% Confidence Intervals; *p<.05; ***p<.001; ΔR^2= adjusted R^2 change)*



7.5 Discussion

The purpose of this study was to examine the effect of a brief, online, mindfulness of the breath induction on state hope and state gratitude compared to an active control group.

7.5.1 Effects of Mindfulness Induction on State Hope and State Gratitude Outcomes

Results showed that, relative to audiobook-listening control, a single-dose, brief mindfulness practice increased state hope and state gratitude, with medium to large between-group effect sizes. Thus, primary Hypotheses 1 and 2 were confirmed. These findings correspond with previous research on the effectiveness of mindfulness on hope (Munoz et al., 2018; Sears & Kraus, 2009) and gratitude (Bluth & Eisenlohr-Moul, 2017; Geschwind et al., 2011), but extend this to a brief, single-session mindfulness practice. Practicing mindfulness has therefore not only been found to be helpful for reducing psychological distress and increasing mindfulness, as found in research for example as detailed in Chapter 1 (Section 1.4), and as identified in the dose-response meta-regression (Chapters 2-5; Strohmaier, 2020) and the experimental study on the effectiveness of several, face-to-face practices (Chapter 6; Strohmaier et al., 2021), but a single-session practice has also been found helpful for building positive psychological resources such as state hope and state gratitude. This aligns with the theoretical stance that through mindfulness practice, participants are able to increase awareness of pathways towards their individual goals and awaken agency thinking (Rand & Cheavens, 2009; Sears & Kraus, 2006). Additionally, findings of this study were that state gratitude increased after a brief mindfulness practice, suggesting that individuals may be better able to mindfully experience and appreciate positive emotions after a brief practice (McCullough et al., 2002).

Furthermore, the findings provide additional evidence for the effectiveness of single-dose mindfulness inductions, which previously included greater cognitive functioning and working memory performance (Bing-Canar et al., 2016; Gill et al., 2020), reduced psychological distress (Johnson et al., 2015; Leyland et al., 2019), and increased levels of other positive psychological outcomes such as optimism (Kiken & Shook, 2011). Through conducting this research study, more specific causal inferences can also be drawn on the effectiveness of a single-dose 10-minute mindfulness practice (Tang et al., 2015), namely that this has been found as useful in increasing state hope and state gratitude. While changes to state hope and gratitude do not have the same long-term effects as changes to trait variables (cf. Kiken et al., 2015), enhancement of positive psychological states, such as hope and gratitude, have been found to be beneficial due to improving individuals' positive states of mind, which has been found to increase wellbeing and incite positive behaviours (Kluemper et al., 2009). In particular, research has found that greater levels of state hope are associated with shielding individuals from momentary negative emotions and accelerating recovery from and resilience towards such emotions (Ong et al., 2006). Additionally, a higher level of state gratitude has been discovered to be related to a more positive emotional reaction to present events and experiences (Sansone & Sansone, 2010).

The positive benefits of a single-session mindfulness practice are arguably noteworthy, not least considering the brevity of the isolated practice. The current finding on effects on state hope and gratitude add weight to the idea of including single, brief mindfulness practices in wellbeing and positive psychology programs, given the considerable benefits associated with hope and gratitude (e.g. Emmons & McCullough, 2003; Yang et al., 2016; Yildirim & Alanazi, 2018). Additionally, the fact that these findings were observed despite the remote (online) mode of delivery suggests that a brief mindfulness induction might usefully be included in self-help programs designed to promote hope and/or gratitude, which is likely more accessible to the general population due to its online delivery.

7.5.2 Mediation, Moderated Mediation, and Compliance Findings

Mediation analyses showed that improvement in state mindfulness, and in curiosity and decentering, statistically mediated the relationship between mindfulness practice (vs. control) and improvements in state hope and state gratitude. This result remained significant when correcting for multiple statistical comparisons and thus appears to be a reliable finding. It corresponds with research finding similar positive effects of mindfulness practice on state mindfulness, curiosity and decentering (e.g. Joseffson et al., 2014; Shapiro et al., 2011). Additionally, this finding coincides with previous research observing a positive effect of a computer-delivered brief mindfulness meditation practice on state mindfulness compared to controls (Mahmood et al., 2016). This result also corresponds with findings from a recent review of mindfulness inductions relating to enhanced mindful states, which in turn were associated with positive health-related outcomes (Heppner & Shirk, 2018). Furthermore, the statistical mediating role of state mindfulness fits with the theoretical stance that engaging in a mindfulness practice increases individuals' state mindfulness, thus reducing negativity bias (Brown et al., 2007). Findings from this study thus further expand previous research by including the positive psychology variables state hope and state gratitude as outcomes in this mediation model.

Turning to moderated mediation, no significant moderating effects were found for baseline trait mindfulness. Therefore, the exploratory hypothesis that higher trait mindfulness would strengthen the relationship between mindfulness practice and improvement in state mindfulness, and hence hope and gratitude, was not supported. At first sight, this finding does not coincide with research showing that, in individuals with greater trait mindfulness, mindfulness practice was associated with greater improvement in state mindfulness (Bravo et al., 2018). However, in Bravo et al.'s study, this was only found for the observe subscale of the trait mindfulness measure (FFMQ) and only for those with meditation experience, whereas the majority of participants in the current study had no prior meditation experience. Additionally, as outlined in Section 7.4.3.2 above, the observe subscale of the FFMQ-15 has been found to perform differently in novice and experienced meditators, and so is not a comparable measure across these two populations (Gu et al., 2016); in fact, it showed low internal consistency for the current sample.

Nevertheless, in the current study, when examining separate trait mindfulness (FFMQ-15) subscales, a significant moderated mediation effect was found for the subscale acting with awareness. Particularly, higher baseline acting with awareness was associated with greater increases in state mindfulness and curiosity after the mindfulness induction. However, this analysis was not pre-planned, and the finding was no longer significant when controlling for inflated risk of Type I errors, and thus needs to be interpreted with caution. If, however, this result was to be substantiated in further research, a possible explanation could be that individuals who habitually are more aware of their present moment experience may be more susceptible to a single, brief mindfulness practice and thus respond with increased curiosity and in-the-moment state mindfulness. This account corresponds to an extent with previous research examining the diverse contributions of the five trait mindfulness facets measured with the FFMQ, namely that the facet acting with awareness was the strongest to contribute to reducing distress (Medvedev et al., 2021; Roemer et al., 2021). If the findings of the current study were in fact to be manifested in future research, dispositional acting with awareness may be the strongest predictor of reduced distress through the mechanism of increased state mindfulness and in turn greater state hope and gratitude. Additionally, previous research has also found that trait acting with awareness is related to greater self-regulation (Short et al., 2016) as well as an increased awareness of accessible resources (Cash & Whittingham, 2010), which in this case arguably state mindfulness, hope, and gratitude are. In particular, individuals with habitually higher levels of acting with awareness might find it easier to reconnect with this through a brief mindfulness practice which in turn may then improve state mindfulness and thus outcomes state hope and gratitude, whereas for individuals with lower dispositional acting with awareness, the effect is perhaps less since this disposition is less familiar and so a shorter practice may not be as helpful.

Furthermore, when examining the amount of compliance to listening to recordings, the findings indicated that mindfulness practice participants paid significantly more attention to their recording than controls to theirs, and amount of compliance was found to significantly strengthen the relationship between group assignment and increased state hope and gratitude in corrected moderation models. Significant differences in compliance to listening to recordings in mindfulness practice and active audiobook-listening control groups have also been found in previous mindfulness induction studies (e.g. Kiken & Shook, 2011; Yusainy & Lawrence, 2015). It is perhaps unsurprising that participants in the mindfulness practice group indicated that they paid more attention to their recording since this involved active listening and following instructions of purposefully turning awareness towards the breath, whereas listening to a non-fictional audiobook involved more passive listening without any invitations to follow instructions; this form of passive listening has previously been associated with mind-wandering (Varao Sousa et al., 2013). Nevertheless, when controlling for compliance, the above-mentioned findings remained significant.

7.5.3 Limitations and Implications

Any study examining effects of mindful practice is open to the question of to what extent participants actually engaged with the practice (also see Chapter 6, Section 6.5.3); this may be particularly pertinent when the study is conducted online, in the absence of any direct researcher monitoring of participants' engagement. However, there is reason to think that this was not a substantial concern in this case, as if anything, disengagement would reduce the impact of a program, but significant medium to large effects were nevertheless observed. Furthermore, processes were included to support and monitor engagement. In particular, i) participants were informed at the beginning that they would be asked to listen to something for 10 minutes and not be disturbed, ii) a timer was added to ensure participants could not complete post-measures until after the mindfulness/control audio recording had finished, and iii) participants were asked to rate their level of engagement post-practice. Nevertheless, future research could examine whether the effects of a mindfulness induction would be different if other aspects found in longer MBPs were present, such as a group discussion of experiences with peers or the presence of an experienced teacher (also see Chapter 6, Section 6.5.3).

A second potential limitation is that the mediator and outcome variables were all measured by self-report questionnaires, which could have introduced common method bias due to social desirability effects and/or demand characteristics. This could have inflated the evidence of mediating effects in the mediation analysis (Podsakoff et al., 2003). Efforts were made to minimise possible bias by having employed an active control group and by not disclosing in advance the effects being examined nor which was the intervention and which the control group. However, since double-blinding is generally not possible in evaluations of psychosocial interventions (Berger, 2016; Karanicolas ***et al.,*** 2010; also see Chapter 3 Section 3.4.3), the possibility of such bias could not be eliminated. In particular, the insufficient control of demand characteristics is still an issue, since participants may have realized from the combination of the study title and the audio they listened to which group they were allocated to. Furthermore, due to the measures assessing state mindfulness, state hope, and state gratitude being administered at pre and post timepoints with only a 10-minute practice in between, there is a possibility that participants remembered questionnaire items and their responses, thus increasing the risk of response bias. Some caution in the interpretation of the findings is therefore warranted, and if it were possible to replicate this study using measures that did not rely on self-report, that would be helpful. However, it is currently unclear how state hope and gratitude could be measured other than through self-report, since arguably, a person can best tell themselves how they feel. One possibility might be to explore implicit attitude measures of hope and gratitude. However, to the best of the PhD researcher’s knowledge, there are not yet such tests available for these constructs. If/when such measures exist, these might also help address issues related to demand characteristics in the current study.

Thirdly, due to absence of additional measurement timepoints after the post-mindfulness/control timepoint (since this was not the focus of the current study), it is not possible to determine how long-lasting the effects of a single-dose mindfulness practice on state hope and state gratitude are. Future research would benefit from examining the longevity of the observed outcomes.

Fourthly, while the use of the Bonferroni correction controlled the risk of Type I errors, Bonferroni-type corrections have been criticised as being too stringent and for inflating the probability of Type II errors (Nakagawa et al., 2004; also see Chapters 4 and 6). In the current study, this is particularly relevant to the post-hoc moderated mediation finding that the moderator acting with awareness was significant prior to the correction, but not afterwards. Therefore, it would be helpful for future research to attempt to replicate this finding.

Fifthly, although the findings provide evidence that improvement in state mindfulness statistically mediated the effect of the mindfulness induction on state hope and gratitude, they fall short of meeting Kazdin's (2007) criteria for providing good evidence of a causal mediation pathway. For example, the mediator and outcome variables were measured at the same time points, and the mediator was not experimentally manipulated. That said, convincing evidence that meets Kadzin's criteria is likely to be accumulated across a range of studies, rather than by one study alone (cf. Gu et al., 2015).

Finally, most participants in this study identified as female and white British. The findings should therefore be generalised with caution to the wider population. In future, it will be important to repeat this research with a more representative sample. Replication of this study for individuals with physical or mental health difficulties would also be valuable as a platform to the potential incorporation of mindfulness inductions into therapeutic programs. Additionally, in this study, a mindfulness of the breath meditation practice was utilised, which is commonly used in MBPs (Crane et al., 2017). It is unclear whether different mindfulness practices, for instance a single-dose body scan, would have similar effects on state hope and gratitude. Future research would therefore benefit from further investigations into the effectiveness of different types of single-dose mindfulness inductions on positive psychology outcomes. Nevertheless, the finding that engaging in a single, brief, mindfulness practice can improve state mindfulness, state hope and state gratitude is encouraging and offers the prospect of including such brief practices in programs aimed at nurturing mindfulness, hope, and gratitude.

7.6 Chapter 7 Summary

This chapter presented findings from a single-dose, online-delivered, brief, mindfulness induction on the outcomes state hope and state gratitude, in the general population, compared to an audiobook-listening control group. This study has demonstrated that a 10-minute, remotely delivered, mindfulness induction had a medium to large positive effect on state hope and state gratitude for individuals from the general population. These effects are arguably impressive, given the brevity and online delivery of the practice, and offer the prospect of promoting such brief practices for individuals to nurture hope and gratitude. It was found that state mindfulness statistically mediated the relationship between the mindfulness induction practice and state hope and gratitude outcomes, whereas firm conclusions relating to results from moderated mediation analyses cannot be drawn prior to replication in future research. Future research could helpfully also explore the longevity of the observed outcomes and benefit from employing more diverse samples and examining whether similar effects can be observed with other types of single-dose mindfulness practices. In the next chapter, the findings from all research conducted in this thesis are discussed including overall limitations and implications for future research and practice.

CHAPTER 8

General Thesis Discussion:

Findings, Limitations, and Implications,

Thesis Contribution and Conclusion

8.1 Chapter 8 Overview

Over the course of the previous chapters, the research findings of a dose-response meta-regression and two experimental studies to examine different doses in mindfulness-based programs (MBPs) and mindfulness practice have been presented. In this chapter, the contribution of this research to the field and study of mindfulness is discussed. Findings from each part of the research conducted in this thesis are synthesised and situated within wider mindfulness research and relevant theory. Next, overall methodological and interpretative limitations of research from this thesis are given. Implications of thesis findings for future research and practice are outlined followed by the thesis conclusion.

8.2 Summary of Research Findings

Research in the field of mindfulness has previously been conducted with different doses relating to MBPs and mindfulness practice. However, to the best of the PhD researcher's knowledge, a purposeful examination of dose in different MBPs and mindfulness practice through a comprehensive dose-response meta-regression and randomised controlled experiments had not been conducted in this way prior to this thesis. Therefore, the aim of this thesis was to examine the effectiveness of different doses related to MBPs to further understanding of this within the field of mindfulness literature due to the vast amount of ever-growing research available with different MBPs and mindfulness practice doses (Goldberg et al., 2020; Chapter 1). Additionally, dose was important to examine for reasons of accessibility to different MBPs, and effectiveness for different population groups and outcomes.

There were three broad parts to this thesis. Firstly, to examine dose-response relationships of different doses related to MBPs, a large-scale comprehensive review of randomised controlled trials (RCTs) was undertaken and subject to meta-analysis and meta-regression analysis. Findings from the initial meta-analysis showed that, as expected, participating in an MBP was associated with positive outcomes for psychological distress (depression, anxiety, stress) and mindfulness, relative to both inactive (i.e. no intervention/program, waitlist control, or treatment as usual) and active controls (i.e. any exercises/programs or activities other than mindfulness practice, see Chapter 3, Section 3.2.1), though greater effect sizes were observed compared to inactive controls. Turning to meta-regression, no robust significant dose-response relationships were found between doses related to MBPs and depression, anxiety, and stress outcomes, meaning there was no evidence of a difference in effectiveness between different doses of MBPs in relation to psychological distress. There were however significant dose-response relationships for the mindfulness outcome for doses related to face-to-face facilitator contact, program intensity, and actual use of the MBP (i.e. actual session attendance plus actual completion of home practices) where greater doses were associated with greater mindfulness post-program, most notably when compared to inactive controls.

There were several limitations to the dose-response review relating to the actual amount of mindfulness practice. This included actual practice data often not being recorded at all or not recorded accurately or comprehensively, in particular when practices were completed outside of sessions, and the fact that participants' previous experience with mindfulness was not always known, and that different clinical and general population samples were included in the review. Additionally, it was not possible to infer causation from the meta-regression with regards to the significant dose-response effects concerning the mindfulness outcome. Therefore, the second part of research in this thesis was to experimentally examine actual mindfulness practice length isolated from other elements found in MBPs in novice practitioners from the general population, to draw stronger conclusions regarding causation. This was completed using a randomised experiment comparing two, tightly controlled, mindfulness practice lengths over several sessions, to active audiobook-listening controls. Findings indicated that in addition to the two different practice lengths resulting in decreased depression, anxiety, and stress and increased trait mindfulness compared to controls, when assessing the two different practice lengths, briefer practices resulted in significantly increased trait mindfulness and decreased stress outcomes than longer practices, in these general population novices, when practice was isolated from other elements of MBPs.

Following on from the findings of the dose-response review and the study examining effects of different practice lengths, the effectiveness of a single-dose online-delivered mindfulness induction RCT was examined since a single dose practice had not yet been assessed so far in this PhD. Additionally, although the effectiveness of mindfulness programs and practices had been explored for psychological distress outcomes, the effects of a single mindfulness practice for positive psychological outcomes had not yet been examined. As outlined in Chapter 7, participating in MBPs and mindfulness practice has already been found to increase positive psychological outcomes which in turn were found to be related to decreased psychological distress and wellbeing (e.g. Emmons & McCullough, 2003; Munoz et al., 2018). Therefore, an online-delivered mindfulness induction RCT was conducted examining effects for positive psychological state outcomes compared to active audiobook-listening controls. Findings suggested that participating in a 10-minute mindfulness practice resulted in increased state hope, state gratitude, and state mindfulness, relative to audiobook listening controls. Therefore, positive effects of mindfulness were not only found after several mindfulness practices for psychological distress, but also after a single practice for positive psychological state outcomes, although no assumptions can be made with regards to the longevity of this effect after a single-dose practice.

The findings from this thesis have built on previous research in the field; in the following section the overall findings are discussed in relation to this.

8.3 Synthesised Discussion of Findings

Research findings as presented in previous chapters and above correspond to some extent with mindfulness research in the field. Since the specific findings from each part of this thesis have already been discussed in detail in relevant chapters (Chapters 3-7), a synthesised discussion of how overall findings from this thesis relate to previous and current research in the wider field of mindfulness is now provided, firstly with regards to MBP doses other than practice and then with mindfulness practice doses.

8.3.1 Discussion of Findings of MBP-related Doses (other than practice)

Research conducted for this thesis, including from the dose-response meta-analysis and meta-regression (Chapters 3-5) as well as the face-to-face (Chapter 6) and online (Chapter 7) experimental studies, showed significant benefits of participating in different doses related to MBPs for psychological distress, trait and state mindfulness, and positive psychological state outcomes (see Chapters 3-7). The benefits of taking part in these MBPs correspond with other research having found positive effects of participating in MBPs (e.g. Goldberg et al., 2021; Keng et al., 2011; Khoury et al., 2013; Malpass et al., 2012; see Chapter 1). When examining the different doses related to MBPs more closely, the following was found in this thesis in correspondence with research in the field.

MBPs of varying doses, i.e. related to program length, intensity, type of delivery (i.e. face-to-face, group, self-help, online), etc., can be considered helpful since, in the dose-response meta-regression as presented in Chapter 4, no significant dose-response relationships between doses related to MBPs and psychological distress outcomes were found. Therefore, more intense and longer MBPs, which are often delivered face-to-face, such as mindfulness-based stress reduction (MBSR; Kabat-Zinn, 1990) and mindfulness-based cognitive therapy (MBCT; Segal et al., 2002), were associated with positive changes in depression, anxiety, and stress, but so were abbreviated versions of these as well as other programs, including those delivered via self-help and/or online methods, over fewer weeks, with briefer sessions and briefer/less frequent recommended home practices (see Chapter 4). Less intense MBPs with fewer sessions were also found beneficial in the two RCTs presented in this thesis, both when delivered in face-to-face sessions (Chapter 6) as well as when delivered online as a mindfulness induction (Chapter 7). This corresponds with previous research where briefer and less intense versions of MBPs, including mindfulness inductions, as well as MBPs of different modes of delivery, have been found beneficial for psychological outcomes (e.g. Blanck et al., 2018; Bostock et al., 2019; Carmody & Baer, 2009; Creswell, 2017; Champion et al., 2018; Demarzo et al., 2017; Economides et al., 2018; Haliwa et al., 2021; Klatt et al., 2009; Leyland et al., 2019; McConville et al., 2016; Schumer et al., 2018; Spijkermann et al., 2016; Chapter 1). Different to previous reviews however, the dose-response review presented in Chapters 2 to 5 has further expanded findings of previous reviews by including various MBP types within the same review (see Chapter 3 for details of MBPs included).

Turning to mindfulness as an outcome, doses of more intense MBPs and greater face-to-face contact with an experienced facilitator were associated with increased mindfulness at post-program in the dose-response review (Chapter 5), which relates to Kabat-Zinn (1982) proposing that for individuals to learn mindfulness, more intense MBPs are needed. One possible explanation of this could be that higher-intensity MBPs with greater contact with an experienced facilitator appeared more helpful in nourishing mindfulness since this may have allowed participants to more deeply discuss any uncertainties with a facilitator and peers. This coincides with previous research of higher-dose MBPs suggesting that group processes are important and that contact with an experienced facilitator is vital (Kabat-Zinn, 2003; Segal et al., 2002; Yalom, 1983; Chapter 5). When no such contact with an experienced facilitator to discuss processes with was available, lower-dose and less intense MBPs were found to be more beneficial for trait and state mindfulness outcomes. This was demonstrated in the experimental studies, where a less intense, lower-dose MBP was found to significantly increase trait mindfulness (Chapter 6) and a single mindfulness practice improved state mindfulness (Chapter 7). Participants in the experimental studies, which did not include facilitator discussions, were mostly novice practitioners with limited mindfulness practice experience. It might therefore be understandable that a change in trait and state mindfulness can be observed from different MBP doses, even lower doses, though more intense programs might be best aided with more face-to-face teacher contact to develop an understanding of practice and discuss any difficulties (see

Chapters 5 and 6). This coincides with previous research, where higher-intensity MBPs with an experienced facilitator present were found to increase mindfulness (e.g. Cladder-Micus et al., 2018), but so did lower-intensity MBPs delivered via self-help methods (e.g. Cavanagh et al., 2018; Mahmood et al., 2016).

A possible explanation as to why no significant dose-response relationships were found for psychological distress outcomes when they were found for the mindfulness outcome (see Chapters 4 and 5) could be that more intense and higher-dose MBPs such as MBCT and MBSR were originally designed for clinical populations with physical and mental health difficulties and thus higher-dose programs may perhaps be more fitting for clinical population groups (Kabat-Zinn, 1990; Segal et al., 2002; 2013). However, research in this thesis largely included general population participants without severe mental or physical health difficulties; less intense and briefer MBPs may therefore have been sufficient in decreasing psychological distress in these participants resulting in a floor effect (see Chapters 4 and 6 for detail). Additionally, as opposed to psychological distress, mindfulness is something that can be built on and increased continuously (see Chapter 5, Section 5.4.2), and a greater difference in mindfulness is likely particularly observed for those new to mindfulness, which the majority of participants in this thesis were (Chapters 3, 6, and 7). A more detailed exploration of doses related to mindfulness practice is provided next.

8.3.2 Discussion of Findings of Mindfulness Practice Doses

Mindfulness practice has been argued to be the most important element in MBPs, with others, such as educational materials, considered as supplementary (Crane et al., 2017; Kabat-Zinn, 1990; Chapters 2 and 6). Findings throughout this thesis have suggested that different doses of mindfulness practice can be beneficial for improving psychological distress, mindfulness, and state positive psychological outcomes. This corresponds with research having found positive effects for various doses of mindfulness practice within different MBPs (e.g. Khoury et al., 2013; Leyland et al., 2019; Lomas et al., 2019; Parsons et al., 2017; Spijkerman et al., 2016). Additionally, Ribeiro et al. (2018) found no difference in effectiveness of the amount of time spent practicing, suggesting different practice lengths were helpful for outcomes. However, the study reported in Chapter 6 showed that when isolating mindfulness practice from other elements of MBPs, such as discussions with an experienced facilitator or peers, and comparing tightly controlled longer to shorter practices, shorter practices had a significantly greater effect on trait mindfulness and stress outcomes than longer practices in novice practitioners. This finding of briefer practices improving trait mindfulness and decreasing stress coincides with previous research suggesting beneficial effects of regular, brief mindfulness practices (Bartlett et al., 2021; Economides et al., 2018; Howells et al., 2016; Moore et al., 2020), particularly when such practices are isolated from other elements of MBPs, as is also often the case in self-help programs (Cavanagh et al., 2018). Additionally, in the online mindfulness induction reported in Chapter 7, an isolated, brief practice was found to have a significant positive effect on state mindfulness, state hope, and state gratitude. The positive effectiveness of a mindfulness induction for positive psychological outcomes corresponds with previous research finding similar positive effects of mindfulness inductions for state mindfulness (Mahmood et al., 2016) and psychological outcomes (Johnson et al., 2015; Leyland et al., 2019).

These briefer, and lower-dose mindfulness practices and programs might at first glance appear as though they do not fit within the Buddhist foundations of mindfulness (see Chapter 1, Section 1.2.1). However, it seems conducive to begin a practice in a way that feels accessible for individuals who are just starting out and who have limited or no prior knowledge of mindfulness. There may be a historical precedent for the use of briefer mindfulness practices within some Buddhist traditions, where novices are often encouraged to begin with briefer practices (sometimes as brief as five minutes twice a day) before expanding to longer practices (Dorjee, 2021). Therefore, though there is good evidence for MBPs that start with longer practices (i.e. MBSR, MBCT, see Chapters 1 and 2), findings from the thesis point to something that may actually align with the spiritual background of mindfulness. There is thus arguably a value in researching the different doses of mindfulness practice since they fit with some of the traditionally intended lengths of practices (though of course, such conclusions need to be drawn tentatively since the intention of practice differs in spiritual and secular perspectives (Dorjee, 2010; 2016; Purser & Loy, 2013; Purser 2015b; Chapter

1)). Therefore, perhaps in particular for novice practitioners, starting with briefer practices may be best.

Nevertheless, despite significantly greater increases in mindfulness having been found after briefer practices relative to longer practices in the in-person RCT (Chapter 6), in the dose-response review, greater actual use of an MBP, which included practice as well as other elements such as discussions with a facilitator and peers, was associated with increased mindfulness post-program (Chapter 5). However, this was not a statistically robust finding and needs to be interpreted with caution (see Chapter 5, Section 5.4.2). If this finding was substantiated in future research, a possible explanation of greater actual practice and MBP engagement relating to increased mindfulness corresponds with previous reviews where greater practice was found to be associated with improved outcomes in MBSR and MBCT programs (Carmody & Baer, 2009; Parsons et al., 2017), and also relates to theory (see Section 8.4 below). However, in the dose-response review, MBPs other than the higher-dose MBSR and MBCT programs were included (Chapter 3). Similar to the finding that the benefits of more intense MBPs were likely aided by greater face-to-face contact with an experienced mindfulness teacher (see Section 8.3.1 above), a greater dose of actual engagement with MBPs may have been associated with increased mindfulness at post-program since any difficulties especially associated with longer practices could be discussed with an experienced teacher. Longer mindfulness practices may therefore allow individuals to engage more deeply with practice and perhaps identify a greater range of experiences, such as a certain level of judgement and criticism in themselves and how to relate to these, which can then be explored further with a mindfulness teacher and peers, thus resulting in greater self-development (cf. Segal et al., 2013). In contrast, in the context of self-help MBPs where there is usually minimal to no teacher involvement, longer practices may present a greater barrier as participants are not able to discuss uncertainties or difficulties due to the lack of support available; shorter practices may thus be more accessible in such MBPs, especially for novices (Chapter 6; Strohmaier et al., 2021).

Furthermore, longer recommended home practice was related to increased depression at follow-up, though this was not statistically robust (see Chapter 4). If this finding was to be substantiated in future research, engaging in longer practices after the end of the MBP might have been a barrier since at this stage, the experienced facilitator to discuss difficulties of practice with was no longer available. Longer practices may therefore have seemed too overwhelming a challenge resulting in participants likely ceasing practice altogether. This corresponds with previous research by Dobkin et al. (2012) arguing that asking participants to engage in longer practices can have adverse effects for some, especially when difficulties are not addressed. Additionally, longer mindfulness practices can also have a non-monotonic effect, where an initial beneficial relationship between practice and outcomes turns negative with increasing practice (Britton, 2019; Chapter 1, Section 1.4.6.3). A parallel can be drawn between novices beginning a mindfulness practice and someone starting to exercise (also see Chapter 6). In both, an experienced teacher/coach being present might be key. Previous research has found that the presence of an experienced coach can increase positive affectivity and maintenance of exercise in individuals (Strauch et al., 2019). Additionally, smaller and regular doses may be more helpful than practicing too much at first and then not return to it. Global public health research has found that starting with smaller and briefer sport exercises followed by gradually increasing the amount showed the most favourable results (World Health Organisation, 2010) with health benefits of exercise already apparent after lower doses (Haseler et al., 2019); the same may be true for mindfulness practice as evident from research in this thesis.

8.4 Theoretical Implications

The research findings presented in this thesis have not only shown evidence to support and built on findings from previous studies but have also shown evidence to [partially] support the tenets of theory on mindfulness (introduced in Chapter 1), rumination and worry (introduced in Chapter 2), and positive psychology (introduced in Chapter 7). Although theories on the benefits of practicing mindfulness have been supported by research in this thesis, how research particularly pertaining to the dose and amount of practice and engagement with mindfulness and MBPs required for a helpful outcome relates to other theories is less obvious, in particular relating to psychological distress outcomes. Research findings are discussed with relevant theory, firstly for psychological distress outcomes, then with the mindfulness outcome, and finally with positive psychological outcomes.

8.4.1 Theoretical Implications of Psychological Distress Outcomes

Theories, such as Metacognition (Teasdale, 1999) and Mode of Mind (Segal et al., 2002; Williams, 2008), as well as Kabat-Zinn (1990) drawing a parallel between mindfulness practice and a muscle that needs to be exercised, generally suggest that higher doses of MBPs and a greater amount of mindfulness practice are related to decreased psychological distress. However, this implication of theories was largely not supported by the dose-response review and RCT examining different practice lengths since lower-dose MBPs and practice were found similarly, if not more, helpful than higher-dose MBPs and practices, especially for novices.

According to Teasdale et al.'s (1999) theory on metacognition, metacognitive insight, as achieved through engagement in MBPs and mindfulness practice, is essential for implicational level understanding thus resulting in a change of an individual's relationship to their thoughts in the sense that thoughts are understood for what they are (thoughts) as opposed to assigning value (Segal et al., 2002). According to Teasdale's model, continued engagement with MBPs and longer mindfulness practices are needed for this change to occur (Segal et al., 2002; Teasdale, 1999). However, findings from the dose-response meta-regression suggest that there was no evidence of a difference in the degree of helpfulness between higher and lower-dose MBPs and practices relating to a positive change in depression, anxiety, and stress (Chapter 4). Additionally, in the RCT examining effectiveness of different practice lengths (Chapter 6), both longer and shorter practices resulted in decreased depression and anxiety, whereas for stress, briefer practices were found more effective than longer practices in novices. Nevertheless, it is important to note that not all participants in the dose-response review and none in the experimental study (see Strohmaier, 2020 and Strohmaier et al., 2021) had reported clinical levels of depression, whereas Teasdale's model was developed with a particular focus on treating relapse and recurrence of depression and when studies of this particular context (i.e. MBCT and MBSR for clinical populations) were reviewed in previous research, there did seem to be a dose-response effect (Parsons et al., 2017).

Furthermore, according to Modes of Mind theory, more engagement with mindfulness and its practices has been associated with disengaging from the maladaptive processing mode, termed the “doing” mode of mind, and switch to a “being” mode, where present moments are experienced without judgement thus disrupting negative thought patterns and habitual rumination (Williams, 2008). Rumination and worry have been identified as underlying mechanisms addressed through participating in MBPs and mindfulness practice (Gu et al., 2015; Segerstrom et al., 2000) and findings from research in this thesis support the notion that mindfulness is helpful for addressing rumination and worry. However, no difference in effectiveness was observed for smaller doses relating to MBPs nor for briefer practices for depression, anxiety, and stress. In the RCT examining the effectiveness of different practice lengths in novices (Chapter 6), shorter practices were found more helpful for stress and mindfulness outcomes. Therefore, as is evident from research in this thesis presented in previous chapters, shorter, regular practices may be sufficient in achieving the switch from a doing to a being Mode of Mind since arguably, disengagement from the internal processes of rumination and worry may already be addressed through shorter practices and lower-dose engagement with MBPs, especially in a novice, non-clinical population when practice is isolated from other MBP elements. It is possible that dose may not be an essential consideration in the efficacy of MBPs for psychological distress outcomes and that the theories regarding their mechanisms of action may therefore be incomplete and could benefit from further development in this respect.

8.4.2 Theoretical Implications of the Mindfulness Outcome

In contrast to psychological distress outcomes, the findings in relation to the mindfulness outcome in this thesis support theory to an extent. According to Shapiro et al.’s (2006) theory on the mechanisms of mindfulness, the axioms intention, attention, and attitude (IAA) are seen as the fundamental building blocks of mindfulness, which resemble Kabat-Zinn’s definition of mindfulness stating attention needs to be paid “on purpose, in the present moment, and non-judgementally” during mindfulness practice (Kabat-Zinn, 1994, p. 4). According to Shapiro et al., practicing with the right intention, attention and attitude is something which requires continued practice and engagement with mindfulness for the meta-mechanism reperceiving to occur (see Chapter 1 for more detail).

Additionally, Kabat-Zinn (1990; 2003) advised that MBPs should be delivered via relatively intense face-to-face programs over several weeks by an experienced teacher for individuals to best be able to learn mindfulness. Findings from the dose-response review are supportive of these theories, since greater face-to-face contact, program intensity and actual MBP use were associated with greater mindfulness post-program (Chapter 5). Therefore, it appears as though the techniques of paying attention in the correct way, with the right purpose and quality as outlined by Shapiro et al. (2006) may be particularly aided by increased face-to-face contact with teachers, a closer proximity of sessions, and actual MBP use. Additionally, greater doses related to MBPs being associated with improved levels of mindfulness is theoretically coherent since, arguably, mindfulness is the mechanism proposed to be increased through practice and engagement in MBPs (Gu et al., 2015).

However, when examining the effects of different mindfulness practice lengths experimentally and when practices were isolated from group elements of MBPs, such as discussions with an experienced teacher, shorter mindfulness practices resulted in greater changes in trait mindfulness post-program than longer practices in novices (Chapter 6). Therefore, it is not obvious how Shapiro et al.'s (2006) theory relates to results of the RCT examining practice lengths. One possible explanation could be that it may be more difficult to cultivate intention, attention, and/or attitude in a longer practice than a shorter one, without the aid of a teacher. It appears that when learning something through self-help methods, briefer practice may be better to start with whereas undertaking longer practices may require more support from an experienced facilitator (see Section 8.3.2 above; Chapter 6).

Furthermore, according to the Liverpool Mindfulness Model (LMM; Malinowski, 2013), practicing mindfulness with the motivational factors of motivation, intention, expectations, and attitudes enhances attention in correspondence with cognitive and emotional flexibility. This in turn is thought to enhance mental and physical wellbeing through non-judging awareness. As evidenced in the study presented in Chapter 6, especially in novices, it seems that for positive outcomes to occur as a result of these increased motivational factors and attention, longer practices are not necessarily needed, and that shorter mindfulness practices can be just as beneficial, if not more so, to achieve greater trait mindfulness and reduced stress. In fact, previous research found that brief, but regular mindfulness practices of 10 minutes, after initial training, were beneficial for electrophysiological markers of attentional control (Moore et al., 2012). Research in this thesis has found that not only several mindfulness practices, but also a single mindfulness practice, can already result in increased state mindfulness and other positive psychological states. These are discussed in relation to theory next.

8.4.3 Theoretical Implications of Positive Psychological Outcomes

Findings presented in Chapter 7 confirmed the theoretical stance that practicing mindfulness is not only beneficial for reducing rumination and worry and increasing mindfulness, but also as a mechanism of action building positive psychological resources. Positive psychology theories, such as the Broaden-and-Build theory, have suggested mindfulness practice elicits positive changes in psychological resources through the mediator of increased positive affect and broadening of awareness, thus building personal resources and adaptive responses to stressors (Fredrickson, 2004; Garland et al., 2015; Johnson et al., 2021). Research conducted in this thesis is consistent with the above theory since increases in the positive psychological outcomes state hope and gratitude were found after a mindfulness practice (Chapter 7).

Furthermore, engaging in a brief mindfulness practice resulting in greater state hope as found from research presented in this thesis corresponds with hope theory (Snyder, 2002; Rand & Cheavens, 2009), where mindfulness practice is thought to increase awareness of one’s personal goals and awakens the pathways towards realising these goals, thus supporting agency thinking. Additionally, the findings that a brief mindfulness practice has improved state gratitude supports the theory and definition of gratitude as a mindful awareness of present positive emotions, events, and experiences (Emmons & Mishra, 2012; McCullough et al., 2002).

However, it is not possible to determine the long-term effectiveness of a single mindfulness practice on positive psychological outcomes from research in this thesis. Despite having aimed to minimise any potential biases and control for possible methodological issues when conducting research, the above and other limitations with what can be concluded from findings of this thesis still prevail; these are discussed next.

8.5 Methodological and Interpretative Limitations

Despite the contribution research presented in this thesis has made in bringing new knowledge to the field of mindfulness, particularly relating to furthering understanding of dose in MBPs and mindfulness practice, there are limitations as to what can reliably be concluded from the research completed. Limitations specific to each part of the thesis have been discussed in detail in relevant chapters (Chapters 3-7). General limitations relating to the methodology employed as well as interpretative limitations in this thesis are discussed below.

8.5.1 Methodological Limitations

Firstly, there are some limitations relating to the philosophical stance of post-positivism within the critical realist ontology based on which the methods adopted in this thesis were chosen. In particular, in post-positivism, the fundamental, all-encompassing truth cannot be understood completely since although some knowledge can be captured, it can never fully be comprehended due to confounding factors such as contextual differences as well as researcher and participant biases influencing the research process (Guba, 1990; see Chapter 1). In this thesis, through meta-analytical investigation and experimental studies, knowledge of dose-response relationships and effects of different doses of mindfulness programs and practice was thought to be advanced, while at the same time recognising that it is impossible to comprehend dose-response relationships absolutely. This is due to unknown contextual differences within studies included in the review and individual differences of participants not known to the researcher, and which were impossible to ascertain fully. Confounding factors which limit what can be concluded from results include differences in participants' previous knowledge and experience of mindfulness or different motivations for taking part in research studies. Psychological research, such as that completed in this PhD, is conducted with human participants who by nature are very much individually different and exist within different contexts and thus their responses to self-report questionnaires, despite reliable and valid measures having been employed in studies, will need to be interpreted with a degree of uncertainty due to confounding factors, some of which even the participants themselves may not be aware of (c.f. Lucas, 2018). Therefore, limitations of the philosophical approach taken in thesis are that we cannot draw absolute conclusions about dose relating to MBPs. However, arguably, the post-positivist approach is a more realistic stance to be taking when completing research within the social sciences with human participants who are individually and contextually different than the more stringent paradigm of positivism thus understanding that research does not result in a complete knowledge of reality (de Souza, 2014). Findings from the dose-response review as well as the two empirical studies are thus generalisable only to an extent (Miller, 2000) and dose effects relating to MBPs and mindfulness practice might be different with a different population. Nevertheless, despite its limitations, the post-positivist paradigm within the critical realist ontology was employed in this thesis since this was considered the best philosophical position to answer the research questions by the PhD researcher.

Other methodological limitations which could have affected findings relate to the calculation of the mindfulness practice dose variables in the dose-response review since formal mindfulness practices outside of engaging in other exercises or discussions were not possible to be extracted from included studies (Chapter 3). Additionally, possible Type I and II errors limit what can be concluded from results of thesis findings due to multiple statistical comparisons and low power (Chapters 4-7). Due to the nature of meta-analytical investigations where different MBP doses were not randomly assigned to studies, causality could not be inferred from the dose-response review (Chapters 2-5). Limitations also relate to the measures employed pertaining to assessing constructs since different measures may not necessarily measure exactly the same construct (Chapter 2).

Much was done to minimise bias, thus addressing methodological limitations as much as possible throughout the different research elements within this thesis. This included RCTs being employed throughout, both in the review and in empirical studies, to reduce participant and researcher bias (see Chapter 1, Section 1.6.1). Due to proposed differences in effectiveness between inactive and active controls (Karlsson & Bergmark, 2014), these were analysed separately in the review (Chapter 2, Section 2.3.4), and active audiobook-listening controls were employed in empirical studies

(Chapters 6 and 7) to minimise bias (Wampold, 2001). Included studies in the review were also subject to evaluation using the Cochrane Risk of Bias and Actual Practice Quality Rating tools. Moderating effects of bias were examined, baseline levels of outcomes were controlled for, and separate analyses were run with different population groups and measures employed, where possible. Clinical significance analysis was also run (Chapter 4). To support the robustness of significant findings, corrections for multiple comparisons (Type I errors) were employed in the review and empirical studies and recommended guidance and power analysis were followed and applied to ensure that the sample size was sufficient for each analysis to minimise Type II errors occurring (see Chapters 2, 4, 6, and 7 for detail). Additionally, throughout research in this thesis, valid and reliable measures were employed, both for studies completed by the PhD researcher, and in studies included in the dose-response review. However, caution still needs to be exercised since self-report measures have been argued to be limited due to social desirability bias, which can only be controlled to an extent (Lucas, 2018). Furthermore, expectancy bias based on what a researcher might expect to be found may be a limitation in this thesis (cf. Krägeloh et al., 2019). However, this seems unlikely to have occurred, since based on most previous theory and research, the PhD researcher expected, and in fact hypothesised in the dose-response review and in-person RCT examining practice length, that greater doses related to MBPs and mindfulness practice would be associated with more beneficial changes to outcomes than lower doses, which was largely not confirmed.

Therefore, although there are methodological limitations in research conducted in this thesis, considerable methods were applied to minimise bias as much as possible including preregistration of the review and both studies on recognised research registration sites, which prespecified outcomes. Moreover, it was intended to support replicability by including detailed plans of the methods employed. In addition to methodological limitations, there are also limits regarding what can be interpreted from findings; this is discussed next.

8.5.2 Interpretative Limitations

Interpretations based on results from research within this thesis have been made (see Chapters 3-7 and Section 8.3 above), however, there are some limitations with regards to what can reliably be concluded from research. Firstly, although much can be interpreted from findings in this primarily quantitative thesis, a different understanding as accomplished with qualitative methods was not achieved, although arguably, this was not the research aim of this thesis. Limitations of mindfulness research need to be communicated precisely outside of academia to minimise the assumption of participating in an MBP and engaging in mindfulness practice being understood as a panacea (van Dam et al., 2018; see Chapter 1).

Next, it was not possible to interpret the longer-term effectiveness of doses relating to mindfulness practice since doses at follow-up were generally neither recorded in detail in studies included in the dose-response review (see Chapters 3-5) nor collected in the two RCTs presented in Chapters 6 and 7. This inability to determine how much participants actually practiced after the commencement of a program is also something which is often criticised in self-help, and digitally delivered MBPs (cf. Cavanagh et al., 2014; Mrazek et al., 2019). Furthermore, although sufficiently powered samples were included in the research completed in this thesis (see Chapters 2, 6, and 7 for details), there are limitations relating to the extent to which findings can be generalised. Since all participants in the review and the two experimental studies needed to volunteer and consent to take part in a mindfulness study, there are likely individual differences in participants wanting to take part in a study, and to have an interest and availability to practice mindfulness, than those who did not (Pepping et al., 2016). Similarly, long-term mindfulness practitioners were generally not included as participants in research completed in this thesis. This is a limitation since this minimizes the conclusions that can be drawn on the effectiveness of MBPs for this population (Britton, 2019). Additionally, individuals with severe mental health difficulties were excluded from participating in empirical studies for ethical reasons (Chapters 6 and 7) which renders it impossible to conclude whether mindfulness practice, as assessed in these studies, was found helpful or harmful for those with severe mental health difficulties. Equally, including experienced practitioner participants could have resulted in ambiguity regarding whether outcomes were based on prior practice experience or from practices completed in the study. Therefore, only novices were included in the RCT examining practice lengths and previous practice experience was controlled for in the online mindfulness induction study. Nevertheless, excluding long-term practitioners and those with severe mental health conditions from samples limits research on whether/how long-term meditation is related to mental health difficulties and/or possible discontinuation of practice, which has been identified as an issue within the mindfulness literature (Britton, 2019; Chapter 1).

Finally, although several different doses were examined in the dose-response review as well as in empirical studies, not all possible doses related to MBPs and mindfulness practice could be assessed within the scope of this thesis. For instance, doses related to informal practice and teacher experience (see Chapter 3), or how different types of mindfulness practice other than mindfulness of the breath meditation affect results (see Chapters 6 and 7), were not explored. Therefore, conclusions associated with these doses as well as for MBPs and samples not included in this research cannot be drawn from this thesis. However, this gap provides an opportunity for future research to examine dose in MBPs for the same, and other outcomes. Additionally, in the cases of non-significant dose-response relationships, the proposed future research would also need to go some way to address possible limitations of the meta-regression that may have contributed to non-significant findings, such as the accuracy of measurement of actual practice since actual amount of mindfulness practiced at home was often either not collected at all or not accurately measured (Chapters 2 and 3).

In general, although there were several limitations in research conducted in this thesis, arguably, the approach taken was thought to be the most appropriate to answer the research questions. Any limitations were addressed as best as possible. Implications of the research conducted in this thesis for future research and practice are discussed next.

8.6 Implications of Thesis Research for Future Research and Practice

The implications from the research conducted in this PhD for future research directions and practice in relation to the specific research conducted within each part of this thesis have been given in Chapters 3 to 7. These, as well as wider implications of findings relating to dose in MBPs and mindfulness practice for future research and practice, are explored here while appreciating that the below is by no means exhaustive.

8.6.1 Implications for Future Research

Through peer-reviewed publications resulting from research in this thesis, other research has already referred to and built on findings and have taken the work further. Some of this research is presented next. For example, a recent RCT cited and expanded the finding in the dose-response review that there were no significant dose-response relationships between doses related to length and intensity of MBPs and psychological distress outcomes by designing and examining an abbreviated MBP with reduced program and session length (Karing & Beelmann, 2021). As was found in the dose-response review as well as the experimental studies in this thesis, this lower-dose MBP was found effective for psychological outcomes (Karing & Beelmann, 2021). Similarly, a recent study on mindfulness for archery performance built on the findings from the dose-response review (Strohmaier, 2020) and the RCT examining effects of practice lengths (Strohmaier et al., 2021) where briefer mindfulness practice sessions were found more effective than longer alternatives (Wu et al., 2021). Additionally, a recent study building on the finding that there were no significant dose-response relationships between MBP length and psychological distress outcomes, as published in the dose-response review (Strohmaier, 2020), examined the effectiveness of MBPs of different lengths during the COVID-19 pandemic finding that participating in these MBPs significantly decreased stress (Lim et al., 2021). In a recent RCT examining practice effects of daily life mindfulness practice, researchers not only drew on the finding of no dose-response relationships found in Strohmaier (2020), but also on the finding by Strohmaier et al. (2021) that novices may particularly benefit from briefer practices (Manigault et al., 2021). Findings from the dose-response review have also informed other recent reviews (e.g. Goldberg et al., 2021; Hutchinson et al., 2021) and the review has been included in a meta-meta-analysis (Fischer et al., 2020).

Other work following on from work and publications resulting from this PhD could also focus on further experimentally examining different doses related to MBPs to better understand causation. This could for instance include further experimental studies into the effectiveness of longer mindfulness practices and doses related to amount of teacher-led enquiry or peer discussions, as alluded to in Chapter 6. Additionally, the examination of practice effects in the experimental studies

(Chapters 6 and 7) has specifically focused on mindfulness of the breath meditation practices. In future, the effectiveness of mindfulness practice length and mindfulness inductions could also be examined for different practices, such as the body scan, which may or may not benefit from a longer or shorter practice duration. Similarly, future research could also assess dose-response effects related to informal mindfulness practices, which has started to be explored further recently (cf. Birtwell et al., 2019; Chapter 3).

Secondly, as well as exploring the effects of additional doses in future research, the dose-response review could be repeated with different types of programs which incorporate mindfulness; perhaps following a wider definition of MBPs than that provided and followed in this thesis, by Crane et al. (2017). This could for instance include programs such as Acceptance and Commitment Therapy (ACT), Compassion-Focused Therapy (CFT), Dialectical Behaviour Therapy (DBT), Loving-Kindness Meditation (LKM), etc., which include some mindfulness practices, but where the main focus is not on mindfulness according to Crane et al. (2017; see Chapter 2, Section 2.3.1). However, caution needs to be exercised with regards to the extent of conclusions being able to be drawn if there are a wide variety of programs included and thus perhaps a narrower focus of MBPs, or separate reviews incorporating different types of MBPs, may be more justified, to reduce heterogeneity. Additionally, the dose-response review could be repeated, this time separating studies based on whether they were individual-based programs, such as most online or self-help programs, or group-based programs and how this relates to outcomes, in particular mindfulness. Yalom (1983) advocated for the beneficial effects of groups in therapy in his theory. Keeping Yalom's theory in mind, whether dose-response relationships differ between group-based and non-group based MBPs may therefore be interesting to examine. A start of this has already been made, for instance a recent study examined one-to-one in-person mindfulness programs for stroke survivors (Wrapson, et al., 2021).

Thirdly, once more research with different clinical populations has been completed, the dose-response review could be repeated, this time further separating studies with participants with different mental or physical health conditions to determine whether different doses related to MBPs have different effects depending on the population group (see Chapter 5). Experimental studies with different doses related to mindfulness practices could then be completed with these clinical populations to determine the effectiveness of different doses. Similarly, dose-response research could be conducted with different general population participants in the future, for instance with individuals with different occupations (see Chapter 1, Section 1.4.3). Furthermore, participants included in research completed in this thesis were largely novice practitioners (although previous mindfulness practice experience was not reported on in several included studies in the dose-response review and was thus missing, see Chapter 3, Section 3.2.2 for detail). Future research could examine doses related to MBPs and mindfulness practice for individuals with longer-term mindfulness practice experience. This could for example also include asking long-term practitioners to vary the amount of mindfulness practice they engage in to examine which dose of practice best works for this population and whether this differs from novices. Some previous and recent research has already examined the effectiveness of mindfulness in longer-term compared to novice practitioners (e.g. Birtwell et al. 2019; Droit-Volet et al., 2015; Desbordes et al., 2015; Fennel et al., 2016; Yordanova et al., 2021); how different doses may be helpful for individuals with varying experiences would therefore be interesting to explore as well.

Fourthly, as well as different programs and populations, the effects of doses related to MBPs and practices for outcomes other than those explored in this thesis would be interesting to examine further. For example, this could include neuropsychological changes resulting from different doses related to mindfulness practice. Previous research has already made a start at examining the neuroanatomical facets of the brain in long-term meditators (Lüders & Kurth, 2019; Chapter 1) and has compared neural and cognitive processes in expert and novice meditators (e.g. Wang et al. 2021; Yordanova et al., 2021); however, the effect of different mindfulness practice doses on neuropsychological outcomes for meditators who differ in practice experience has yet to be examined. Another possibility would be for future research to employ neurological or cognitive measures to examine moment-by-moment physiological changes resulting from different doses of mindfulness practice. Additionally, future research into dose-response in MBPs could further explore the effectiveness of different MBP doses on adverse effects of practice (Britton, 2019; Chapter 5).

Fifthly, further research into dose effects of mindfulness could also include additional data collection dates or applying other research methods. This could, for instance, comprise a program where outcomes are measured before and after each session to understand when a dose effect commences, in line with sudden gains (Tang & DeRubeis, 1999) and learning theories (e.g. Ajzen, 1991; Köhler, 1925). A start of this has already been made recently by Levi et al. (2021), who have investigated the daily dose-response hypothesis of mindfulness practice by applying an intensive experience sampling method where participants were asked to complete measures of mindfulness, affect, decentering, and practice twice-daily over the course of an MBP. Levi et al. (2021) found that although the daily dose of practice significantly predicted same-day state mindfulness, decentering and emotional valence, this did not carry over to post-program effects. However, a limitation of their research was that there was no comparison group and no experimental assignment to practice dose conditions, limiting the conclusion that can be drawn thus requiring further research. Furthermore, qualitative research, perhaps alongside an experimental examination of dose in MBPs, could be helpful in trying to understand participants' perceptions of why dose effects may arise (or not) in the way they do. Data collection during MBPs in the form of observation of, for instance, group discussions for different dose MBPs could also be interesting to explore in future, which would also improve ecological validity.

Sixthly, since some theories of mindfulness programs were not confirmed by the meta-regressions as discussed above, future research could also further test existing mindfulness theories and further examine underlying processes (Rosenkranz et al., 2019). Additionally, the dose-response meta-regression could be repeated in the future once doses related to actual mindfulness practice are collected more comprehensively in included studies. Future experiments could also incorporate a longitudinal design to examine dose-response over several years and whether the dose-response effect increases or decreases with more experience.

Finally, research in this thesis, in particular the dose-response review, which included a large number of studies and doses and where causation could not be concluded, could also inform grant funding applications for future experimental RCTs to make the case as to why further research into dose in MBPs and mindfulness practice is necessary, thus further enhancing knowledge in the field. In addition to implications for research, findings presented in previous chapters have also had several implications for practice, these are outlined next.

8.6.2 Implications for Practice

Research findings in this thesis have provided evidence on the benefits of briefer practices, particularly when there is limited to no facilitator guidance available, as well as of self-help and online MBPs, including mindfulness inductions. Possible implications for practice are the increased accessibility, flexibility, and wider reach of lower-dose MBPs and practices, especially at times when attending face-to-face classes is impossible such as during the current global pandemic or for busy individuals with responsibilities, to engage in mindfulness at a time that best works for them. A start on research of such low-dose MBPs adapted for the pandemic has already been made (e.g. González-García et al., 2021; Zhang et al., 2021). Findings concerning the benefits of online and self-help MBPs also correspond with recent increases in lower-dose self-help and online-delivered MBPs (Birtwell et al., 2021; Lim et al., 2021; Mrazek et al., 2019; O'Connor et al., 2018). Self-help and online MBPs may also be more cost-effective than higher dose and/or in-person MBPs, though this needs to be examined further (cf. Duarte et al., 2018).

Furthermore, findings from this thesis could also be helpful in designing future MBPs, which may include different doses depending on whether a program is delivered via self-help methods or in-person. This could, for instance, include the design of online studies to include brief practices, or for longer practices, incorporating virtual facilitator contact or group discussions online. A start on this has already been made (e.g. Bartlett et al., 2021; Moore et al., 2020). Additionally, future programs could include personalised MBPs where participants choose the doses relating to MBP and practice which best suit them, which could again ensure greater accessibility, though further research would need to be conducted on this first. As mentioned in Chapter 7, future therapeutic programs could also incorporate mindfulness inductions as part of another program, both when delivered online and in person. Therefore, research from this thesis provided several potential opportunities for future research and practice.

8.7 Chapter Summary and Thesis Conclusion

In summary, findings from research presented in this thesis coincide with previous research in the mindfulness literature as well as some psychology theory. However, it is important to be aware of limitations of what can reliably be concluded from findings. Nevertheless, findings of research as presented in the previous chapters has already had and is likely to have further implications for future research and practice. Although much previous mindfulness research has been conducted on a variety of MBPs (as outlined in Chapter 1), an examination of different types of doses related to MBPs and mindfulness practice to ascertain which doses are most effective had not yet been completed prior to this thesis. Therefore, a comprehensive dose-response meta-regression was conducted to further understanding on whether a dose-response relationship existed between MBP and practice doses and outcomes. Additionally, since causation could not be inferred from the dose-response review (Chapters 2-5), two experimental studies were completed to assess the effectiveness of different mindfulness practice doses on outcomes (Chapters 6 and 7). Findings from thesis research provided answers to the research aims and questions outlined in Chapter 1, Section 1.5. Specifically, although there were dose-response relationships found for the mindfulness outcome in the dose-response review, no significant dose-response relationships were discovered for psychological distress outcomes, meaning that different doses related to MBPs, and mindfulness practices were found similarly helpful for depression, anxiety, and stress with larger doses relating to MBPs and practices not being significantly more helpful than smaller doses. Mindfulness appears to be better learnt through a greater actual engagement with an MBP and related practices and contact and discussions with a mindfulness teacher and peers. Nevertheless, if no teacher or discussion elements are present in an MBP, shorter mindfulness practices seem to be more helpful than longer practices, in particular for novices. When examining the effectiveness of different practice lengths in novice practitioners, when such practices were isolated from other elements of MBPs, briefer practices resulted in greater improvements in stress and mindfulness than longer practices, whereas both practice lengths were found to be more helpful for all outcomes compared to controls in the first randomised experiment (Chapter 6). In a second randomised experiment (Chapter 7), a brief mindfulness induction practice resulted in improvements in state positive psychological outcomes. Therefore, in response to the research aims and questions addressed in this thesis, findings were that different types of doses related to MBPs and mindfulness practice can be helpful, and that in particular for novices, briefer practices may be best to start with, especially in self-help programs where no experienced facilitator is present. However, it feels important to note that this thesis has not provided a definitive conclusion on dose in MBPs, but rather has explored some aspects of dose. Much more future research needs to be completed looking at other elements of dose (see Section 8.6). Overall, research in this thesis has contributed to the field of mindfulness literature and furthered understanding of the role of dose in MBPs and mindfulness practice, thus creating new knowledge on which further research can, and has already been, built.

Finally, it feels important to note that in this thesis, it has not been the intention of the researcher to present lower-dose mindfulness practices and MBPs as superior or "better than" higher-dose practices or programs or to regard longer programs as unnecessary when there are briefer programs available. Rather, the researcher's position is that due to the brevity and often online/self-help delivery of lower-dose MBPs, these can be more accessible to novices from the general population who have limited time available due to other commitments. Thus, if lower-dose, less intense, self-help delivered mindfulness programs were not available, these individuals may not have the opportunity to engage with mindfulness at all. Nevertheless, this does not mean that longer practices and more intense programs are not helpful, in fact, there is much research evidence stating that these programs are beneficial (e.g. see Chapters 1 and 2). However, such higher-level doses may be less accessible. Arguably, if individuals have the option to already reap some benefits from lower-dose MBPs, even if this does not lead to a state of enlightenment at first, then this is a positive result from the perspective of the researcher. It is the researcher's view resulting from findings of research in this thesis, that different MBP and mindfulness practice doses can coexist harmoniously for mindfulness to be accessible and available for everyone who finds it helpful.

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APPENDICES

Appendices Chapter 2

Appendix 2.3: Methods
Appendix 2.3.1 PROSPERO Pre-Registration: Dose-Response Meta-Regression

PROSPERO
International prospective register of systematic reviews

NHS
National Institute for Health Research

UNIVERSITY of York
Centre for Reviews and Dissemination

Systematic review

1. * Review title.

Give the working title of the review, for example the one used for obtaining funding. Ideally the title should state succinctly the interventions or exposures being reviewed and the associated health or social problems. Where appropriate, the title should use the PI(E)COS structure to contain information on the Participants, Intervention (or Exposure) and Comparison groups, the Outcomes to be measured and Study designs to be included.

The relationship between intensity of mindfulness-based interventions and depression outcomes: a dose-response meta-analysis of randomised controlled trials of mindfulness-based interventions on depressive symptom severity

2. Original language title.

For reviews in languages other than English, this field should be used to enter the title in the language of the review. This will be displayed together with the English language title.

3. * Anticipated or actual start date.

Give the date when the systematic review commenced, or is expected to commence.

31/01/2017

4. * Anticipated completion date.

Give the date by which the review is expected to be completed.

01/10/2018

5. * Stage of review at time of this submission.

Indicate the stage of progress of the review by ticking the relevant Started and Completed boxes. Additional information may be added in the free text box provided.
Please note: Reviews that have progressed beyond the point of completing data extraction at the time of initial registration are not eligible for inclusion in PROSPERO. Should evidence of incorrect status and/or completion date being supplied at the time of submission come to light, the content of the PROSPERO record will be removed leaving only the title and named contact details and a statement that inaccuracies in the stage of the review date had been identified.
This field should be updated when any amendments are made to a published record and on completion and publication of the review. If this field was pre-populated from the initial screening questions then you are not able to edit it until the record is published.

The review has not yet started: No

Review stage	Started	Completed
Preliminary searches	Yes	Yes
Piloting of the study selection process	Yes	Yes
Formal screening of search results against eligibility criteria	Yes	Yes
Data extraction	Yes	Yes
Risk of bias (quality) assessment	Yes	Yes
Data analysis	Yes	Yes

Provide any other relevant information about the stage of the review here (e.g. Funded proposal, protocol not yet finalised).

6. * Named contact.

The named contact acts as the guarantor for the accuracy of the information presented in the register record.

Sarah Strohmaier

Email salutation (e.g. "Dr Smith" or "Joanne") for correspondence:

sarah.strohmaier@canterbury.ac.uk

7. * Named contact email.

Give the electronic mail address of the named contact.

sarah.strohmaier@canterbury.ac.uk

8. Named contact address

Give the full postal address for the named contact.

Salomons Centre for Applied Psychology

Canterbury Christ Church University

1 Meadow Road

Tunbridge Wells, Kent TN1 2YG

9. Named contact phone number.

Give the telephone number for the named contact, including international dialling code.

01227 92 7092

10. * Organisational affiliation of the review.

Full title of the organisational affiliations for this review and website address if available. This field may be completed as 'None' if the review is not affiliated to any organisation.

Canterbury Christ Church University, University of Sussex, Sussex Partnership NHS Foundation Trust

Organisation web address:

www.canterbury.ac.uk, www.sussex.co.uk, www.sussexpartnership.nhs.uk

11. * Review team members and their organisational affiliations.

Give the personal details and the organisational affiliations of each member of the review team. Affiliation refers to groups or organisations to which review team members belong. **NOTE: email and country are now mandatory fields for each person.**

Ms Sarah Strohmaier. Canterbury Christ Church University
Dr Fergal Jones. Canterbury Christ Church University
Dr Clara Strauss. University of Sussex
Dr Kate Cavanagh. University of Sussex

12. * Funding sources/sponsors.

Give details of the individuals, organizations, groups or other legal entities who take responsibility for initiating, managing, sponsoring and/or financing the review. Include any unique identification numbers assigned to the review by the individuals or bodies listed.

Canterbury Christ Church University is sponsoring this research

Grant number(s)

13. * Conflicts of interest.

List any conditions that could lead to actual or perceived undue influence on judgements concerning the main topic investigated in the review.

None

14. Collaborators.

Give the name and affiliation of any individuals or organisations who are working on the review but who are not listed as review team members. **NOTE: email and country are now mandatory fields for each person.**

15. * Review question.

State the question(s) to be addressed by the review, clearly and precisely. Review questions may be specific or broad. It may be appropriate to break very broad questions down into a series of related more specific questions. Questions may be framed or refined using PI(E)COS where relevant.

This review aims: 1) to evaluate if there is a relationship between the intensity of mindfulness-based interventions and depressive symptom severity outcomes in adults ('dose-response').

2) if a dose-response relationship is found, to identify moderators of this relationship.

16. * Searches.

State the sources that will be searched. Give the search dates, and any restrictions (e.g. language or publication period). Do NOT enter the full search strategy (it may be provided as a link or attachment.)

The search terms [mindful* OR MBCT OR MBSR AND random* OR RCT] will be searched for in the title, abstract and keywords since the inception of the database up to the end of the year 2017. The following sources will be searched: 1. Electronic databases containing peer-reviewed journals, from PsycInfo, Web of

Science, MEDLINE, CINAHL. 2.Clinical trials registration sites, including clinicaltrials.gov & ISRCTN Registry. 3. Reference lists from included papers.

Inclusion criteria: Studies need to (1) be randomised controlled trials; (2) be in the English language; (3) be published in a peer-reviewed journal or registered on a clinical trials registration site; (4) contain a mindfulness-based intervention for adults with a majority of intervention content focused on mindfulness principles and practice; (5) be an intervention which involves more than one session; (6) include a quantitative measure of depression as an outcome measure.

Exclusion criteria: Papers will be excluded if (1) they are a laboratory experiment examining the effects of mindfulness rather than testing an intervention; (2) they include the same data as another included paper, (3) all control interventions include a mindfulness component.

17. URL to search strategy.

Give a link to a published pdf/word document detailing either the search strategy or an example of a search strategy for a specific database if available (including the keywords that will be used in the search strategies), or upload your search strategy.Do NOT provide links to your search results.

Alternatively, upload your search strategy to CRD in pdf format. Please note that by doing so you are consenting to the file being made publicly accessible.

Yes I give permission for this file to be made publicly available

18. * Condition or domain being studied.

Give a short description of the disease, condition or healthcare domain being studied. This could include health and wellbeing outcomes.

Depression and low mood in adults.

19. * Participants/population.

Give summary criteria for the participants or populations being studied by the review. The preferred format includes details of both inclusion and exclusion criteria.

Participants are adults over the age of 18 from either clinical or non-clinical populations.

20. * Intervention(s), exposure(s).

Give full and clear descriptions or definitions of the nature of the interventions or the exposures to be reviewed.

Interventions will be grounded in mindfulness principles and practices following the definition of mindfulness-based interventions outlined by Crane et al (2016). In line with this definition, interventions can include a range of different mindfulness practices or consist of a single practice only. The mindfulness teacher is required to have specific competencies and qualities to be able to deliver mindfulness practice(s) effectively, has engaged in appropriate training and commits to ongoing good practice, and participates in learning with his/her students (see Crane et al, 2016 for more details). However, increasingly, solely digital and other self-help delivery methods of mindfulness practices are of interest and are included in Crane's (2016) definition.

Program structure, length and frequency of sessions and home practices can be adapted to best suit the target population and context. However, to allow for comparisons between programs, mindfulness intervention programs need to surpass one session to be included in this study. Additionally, although some programs such as ACT, DBT and CFT may include some practices which are informed by mindfulness, these programs would not be included here since mindfulness meditation is not considered as central and what the program is mainly based on (Crane et al, 2016). These programs therefore do not meet Crane's criteria and are thus excluded for this study. Interventions will vary in duration (number of sessions, number of weeks, length of recommended mindfulness practices) and frequency (frequency of session and frequency of recommended mindfulness practice).

Please see above for a complete list of inclusion and exclusion criteria.

21. * Comparator(s)/control.

Where relevant, give details of the alternatives against which the main subject/topic of the review will be compared (e.g. another intervention or a non-exposed control group). The preferred format includes details of both inclusion and exclusion criteria.

Studies with any type of control condition will be included (active or inactive). Sub-group analyses will be conducted for different types of control condition (active and inactive). Studies with any type of control condition will be included so long as the control condition does not include mindfulness practice.

22. * Types of study to be included.

Give details of the types of study (study designs) eligible for inclusion in the review. If there are no restrictions on the types of study design eligible for inclusion, or certain study types are excluded, this should be stated. The preferred format includes details of both inclusion and exclusion criteria.

Randomised controlled trials.

23. Context.

Give summary details of the setting and other relevant characteristics which help define the inclusion or exclusion criteria.

Randomised controlled trials can be in any setting, both clinical and non-clinical, provided inclusion and exclusion criteria are met.

24. * Main outcome(s).

Give the pre-specified main (most important) outcomes of the review, including details of how the outcome is defined and measured and when these measurement are made, if these are part of the review inclusion criteria.

The primary outcome for the meta-analysis is post-intervention depressive symptom severity (irrespective of the primary outcome of the included trial).

* Measures of effect

Please specify the effect measure(s) for you main outcome(s) e.g. relative risks, odds ratios, risk difference, and/or 'number needed to treat.

Immediately post-intervention is the primary time-point. Measures taken at follow-up time points will be included for analysis as well where this data is available.

25. * Additional outcome(s).

List the pre-specified additional outcomes of the review, with a similar level of detail to that required for main outcomes. Where there are no additional outcomes please state 'None' or 'Not applicable' as appropriate to the review

Given that previous studies have suggested that mindfulness-based interventions are also helpful with anxiety symptom severity, stress and mindfulness (e.g. Kabat-Zinn, 1990), secondary outcomes that will be measured include anxiety symptom severity, stress and mindfulness .

* Measures of effect

Please specify the effect measure(s) for you additional outcome(s) e.g. relative risks, odds ratios, risk difference, and/or 'number needed to treat.

Immediately between-group post-intervention effects on anxiety symptom severity, stress and mindfulness is the primary time-point. Measures taken at follow-up time points will be included for analysis as well where this data is available.

26. * Data extraction (selection and coding).

Describe how studies will be selected for inclusion. State what data will be extracted or obtained. State how this will be done and recorded.

Titles and abstracts of all identified studies will be screened by one author (SS) with ineligible studies being excluded. As a reliability check, a sample of the full-texts of 100 eligible papers will be screened by all authors against inclusion/exclusion criteria. The level of agreement between authors will be reported and if sufficiently high (r.8); the remaining full-text papers will be screened by SS with any areas of uncertainty discussed between all authors. Post-intervention and follow-up (if available) means and standard deviations will be extracted for each outcome (depressive symptom severity, anxiety symptom severity, stress, mindfulness) for each condition (mindfulness-based intervention and control group(s)). If a study includes more than one control group, inactive control groups will be preferred for the main analysis given that the majority of studies are likely to have inactive control groups. This therefore reduces heterogeneity. Where a study includes an active control group (both, in addition to an inactive control group or as the only control group of the study), all active control groups of studies where the nature of the intervention was similar will be grouped carefully and analysed separately . Active control groups are included in this study, since according to the FDA (2016), higher quality studies include active controls. Information about the dose of the intervention will also be extracted, including number of face-to-face sessions, duration of face-to-face sessions, length in weeks of the intervention, frequency of recommended mindfulness practice (number of practice sessions recommended per week), duration of recommend practice sessions in minutes, number of types of mindfulness practice included and actual/self-reported use of the intervention (where reported). In addition, participant characteristics, intervention type, setting and attrition rates will also be extracted.

27. * Risk of bias (quality) assessment.

Describe the method of assessing risk of bias or quality assessment. State which characteristics of the studies will be assessed and any formal risk of bias tools that will be used.

Publication bias will be assessed by checking funnel plots and Rosenthal's Failsafe N for each outcome. All studies will be rated against the Cochrane Collaboration risk of bias tool (2010).

28. * Strategy for data synthesis.

Provide details of the planned synthesis including a rationale for the methods selected. This **must not be generic text** but should be **specific to your review** and describe how the proposed analysis will be applied to your data.

Between-group effect sizes on all outcome measures at post-intervention and follow-up (where available) will be calculated using either Review Manager (RevMan) version 5.2 (Cochrane Collaboration, 2012) or Comprehensive Meta-Analysis (CMA version 3.3; Biostat, 2014). Meta-regressions will be conducted to examine whether the dose variables described above predict these effect sizes. Random effects models will be employed.

29. * Analysis of subgroups or subsets.

State any planned investigation of 'subgroups'. Be clear and specific about which type of study or participant will be included in each group or covariate investigated. State the planned analytic approach.

If a dose-response relationship is found, moderators of this will be examined, including type of intervention (MBCT/MBSR or close variant versus other MBI), population (mental health versus physical health versus non-clinical) and format of the intervention.

30. * Type and method of review.

Select the type of review and the review method from the lists below. Select the health area(s) of interest for your review.

Type of review

Cost effectiveness
No

Diagnostic
No

Epidemiologic
No

Individual patient data (IPD) meta-analysis
No

Intervention
No

Meta-analysis
Yes

Methodology
No

Narrative synthesis

No

Network meta-analysis
No

Pre-clinical
No

Prevention
No

Prognostic
No

Prospective meta-analysis (PMA)
No

Review of reviews
No

Service delivery
No

Synthesis of qualitative studies
No

Systematic review
Yes

Other
No

Health area of the review

Alcohol/substance misuse/abuse
No

Blood and immune system
No

Cancer
No

Cardiovascular
No

Care of the elderly
No

Child health
No

Complementary therapies
No

COVID-19
No

Crime and justice
No

Dental
No

Digestive system
No

Ear, nose and throat
No

Education
No

Endocrine and metabolic disorders

No

Eye disorders
No

General interest
No

Genetics
No

Health inequalities/health equity
No

Infections and infestations
No

International development
No

Mental health and behavioural conditions
No

Musculoskeletal
No

Neurological
No

Nursing
No

Obstetrics and gynaecology
No

Oral health
No

Palliative care
No

Perioperative care
No

Physiotherapy
No

Pregnancy and childbirth
No

Public health (including social determinants of health)
No

Rehabilitation
No

Respiratory disorders
No

Service delivery
No

Skin disorders
No

Social care
No

Surgery
No

Tropical Medicine
No

Urological
No

Wounds, injuries and accidents
No

Violence and abuse
No

31. Language.

Select each language individually to add it to the list below, use the bin icon to remove any added in error.
English

There is an English language summary.

32. * Country.

Select the country in which the review is being carried out from the drop down list. For multi-national collaborations select all the countries involved.

England

33. Other registration details.

Give the name of any organisation where the systematic review title or protocol is registered (such as with The Campbell Collaboration, or The Joanna Briggs Institute) together with any unique identification number assigned. (N.B. Registration details for Cochrane protocols will be automatically entered). If extracted data will be stored and made available through a repository such as the Systematic Review Data Repository (SRDR), details and a link should be included here. If none, leave blank.

34. Reference and/or URL for published protocol.

Give the citation and link for the published protocol, if there is one

Give the link to the published protocol.

Alternatively, upload your published protocol to CRD in pdf format. Please note that by doing so you are consenting to the file being made publicly accessible.

Yes I give permission for this file to be made publicly available

Please note that the information required in the PROSPERO registration form must be completed in full even if access to a protocol is given.

35. Dissemination plans.

Give brief details of plans for communicating essential messages from the review to the appropriate audiences.

The dose-response meta-analysis will be submitted for publication in a peer-reviewed journal.

Do you intend to publish the review on completion?

Yes

36. Keywords.

Give words or phrases that best describe the review. Separate keywords with a semicolon or new line. Keywords will help users find the review in the Register (the words do not appear in the public record but are included in searches). Be as specific and precise as possible. Avoid acronyms and abbreviations unless these are in wide use.

mindfulness

mindfulness-based cognitive therapy

mindfulness-based stress reduction

dose-response relationship

meta-analysis

systematic review

depression

37. Details of any existing review of the same topic by the same authors.

Give details of earlier versions of the systematic review if an update of an existing review is being registered, including full bibliographic reference if possible.

38. * Current review status.

Review status should be updated when the review is completed and when it is published. For newregistrations the review must be Ongoing.
Please provide anticipated publication date

Review_Completed_published

39. Any additional information.

Provide any other information the review team feel is relevant to the registration of the review.

40. Details of final report/publication(s) or preprints if available.

This field should be left empty until details of the completed review are available OR you have a link to a preprint.

Strohmaier, S. (2020) The Relationship Between Doses of Mindfulness-Based Programs and Depression, Anxiety, Stress, and Mindfulness: A Dose-Response Meta-Regression of Randomized Controlled Trials. Mindfulness. https://doi.org/10.1007/s12671-020-01319-4

Give the link to the published review.

https://doi.org/10.1007/s12671-020-01319-4

https://link.springer.com/article/10.1007/s12671-020-01319-4

Appendix 2.3.2: Email Templates to Corresponding Authors

Template of First Email to Authors:

Paper attached

Subject: Query regarding your study on …. Please reply by ***date*** …

Dear Prof/Dr *study author(s) name*,

My name is Sarah Strohmaier, PhD student and Research Assistant in Psychology at Canterbury Christ Church University in the UK.

Together with colleagues at the University of Sussex we are currently conducting a dose-response meta-analysis of mindfulness-based interventions (PROSPERO Register for Systematic Reviews registration number: CRD42017056864) and read with interest your study titled ... (attached), which has met our inclusion criteria.

However, we could not find all of the information we require for the analysis in the published article.

For the meta-analysis we would need post-intervention and follow-up (where applicable) means and standard deviations for both the intervention group as well as all control groups (active as well as passive control groups, where applicable) for *outcome measures on depression, anxiety, mindfulness and stress, where applicable* along with the number of participants in each group.

Additionally, it would be helpful to have any information you have in relation to the “dose” of your study, including aspects such as total length of the intervention (e.g. 26 hours over 8 weeks), attendance criteria (i.e. minimum number of sessions attended to be included in the analysis and final total number of sessions attended by participants), as well as recommended time of home practices and compliance to home practices, (i.e. number of days/ hours of home practice in total during the whole intervention).

Would it be possible to send us this information by ***date***?

Thank you very much.

Kind regards,
Sarah Strohmaier

AFHEA, MBPsS
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PhD researcher - Psychology
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Template of Follow-Up Emails to Authors

Paper attached

Subject: Query regarding your study on …. Please reply by ***date***

Dear Prof/Dr *study author(s) name*,

Apologies for emailing you again, we appreciate you are busy.

For the dose-response meta-analysis I had contacted you about before (PROSPERO Register for Systematic Reviews registration number: CRD42017056864, also see below email), we were still wondering whether it would be possible for you to send us *insert data and information needed*

If you would like your study to be included in our meta-analysis, please email us your data and study characteristics by ***date***.

We will commence with the analysis of data after this date.

Thank you very much.

Kind regards,
Sarah Strohmaier

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Appendices Chapter 3

Appendix 3.2: Descriptive Statistics Results

Appendix Table 3.2.1

Details of included studies

Study	Country	Population	N	Mean age in years, (SD), range	% Female	Mindfulness group	Control group
Abholgaseni et al. (2015)	Iran	Patients with depression	30	29; 20-40	60%	MBCT	Beck's Cognitive Therapy
Advocat et al. (2016)	Australia	Patients with Parkinson's Disease	57	63.3 (8.1); 39-75	57.90%	Mindfulness-based lifestyle program	WL
Aemla-Or (in press)	Thailand	Nursing students	126	19.17 (0.86); 19-20	91%	MBSR	WL TAU
Ahmadpanah et al. (2017)	Iran	Older adults with Major Depressive Disorder	36	69.23	100%	Detached Mindfulness	Leisure activities
Alsaraireh & Aloush (2017)	Jordan	Nursing students	181	22 (2.1)	62%	Mindfulness meditation	Exercise
Anderson et al. (2007)	Canada	Healthy adults	72	Not specified	Not specified	MBSR	WL
Arch et al. (2013)	USA	Veterans with anxiety disorders	105	45.91 (13.68); 22-78	17%	Adapted MBSR	CBT
Arif et al. (2017)	UK	Patients with tinnitus	61	56.05	53.50%	Mindfulness meditation	Relaxation therapy
Armstrong & Rimes (2016)	UK	Individuals with high levels of neuroticism	34	25.5	91.18%	MBCT	Online self-help
Astin et al. (2003)	USA	Patients with fibromyalgia	64	45	99.20%	Mind-body intervention including MBSR	Educational support group
Asuero et al. (2014)	Spain	Primary health care professionals	68	47	92%	Mindfulness education programmed	WL

Study	Country	Population	N	Mean age in years, (SD), range	% Female	Mindfulness group	Control group
Bahrani et al. (2017)	Iran	Women with Multiple Sclerosis	47	36.39	100%	Mindfulness-integrated CBT	WL TAU
Barnhofer et al. (2009)	UK	Patients with chronic/recurring depression	28	41.93	67.85%	MBCT	WL
Barnhofer et al. (2015)	UK	Patients with history of suicidal depression	78	43.7 (12.1).	48.33%	MBCT	I: TAU; A: cognitive psychoeducation
Barry et al. (2019)	Australia	Doctoral candidates	82	38	81.5	Daily guided mindfulness practice with audio CD	No program
Beattie et al. (2017)	Australia	Pregnant women	20	28.7	100%	Mindfulness in Pregnancy Program (MIPP)	Pregnancy support group
Bedard et al. (2013)	Canada	Individuals with traumatic brain injuries and history of depression	76	46.77 (13.37)	45%	MBCT	WL
Benn et al. (2012)	USA	Parents & educators of children with special needs	59	46.3; 26-60	91.67%	Stress Management & Relaxation Techniques program	WL
Bergen-Cico et al. (2014)	USA	Veterans in primary care with PTSD	62	48 (16)	10%	Primary Care brief Mindfulness Program	TAU
Bhayee et al. (2016)	Canada	Healthy adults	26	32.65 (4.8)	46.15%	Technology supported mindfulness training	Online Math’s training
Black et al. (2015)	USA	Older adults with sleep disturbances	49	66.3 (7.4)	67%	Mindful Awareness Practices (MAP)	Sleep Hygiene Education (SHE)
Boettcher et al. (2014)	Sweden	Individuals with a form of anxiety	84	38 (10.3)	71.40%	Internet-based Mindfulness Training	Online discussion forum
Bogosian et al. (2015)	UK	Patients with Multiple Sclerosis	36	52.16	54.65%	Adapted MBCT over Skype	TAU

Study	Country	Population	N	Mean age in years, (SD), range	% Female	Mindfulness group	Control group
Bostock et al. (2018)	UK	Healthy employees	238	35.5 (7.7); 23-61	59.2%	Mindfulness meditation app	WL
Bower et al. (2015)	USA	Women survivors of breast cancer	65	46.9; 28.4-60	100%	Mindful Awareness Practices (MAP)	WL
Braenstroem et al. (2010)	Sweden	Cancer patients	60	51.8 (9.86)	98.59%	Adapted MBSR	WL
Britton et al. (2012)	USA	Individuals with a recurrent form of unipolar depression	24	46.4	84.25%	MBCT	WL
Brotto et al. (2012)	Canada	Women with cervical or endometrial cancer	31	54 (8.23); 31–64	100%	MBCT	WL
Brown et al. (2016)	USA	Caregivers of family members with dementia	34	61.14 (10.41); 39-88	84.20%	Adjusted MBSR	Social support group
Carletto et al. (2017)	Italy	Patients with Multiple Sclerosis with depressive symptoms	90	44.6	71.10%	Body-affective mindfulness intervention	Psychoeducation
Cavanagh et al. (2013)	UK	University students	58	24.70 (6.44); 19-51	88.46%	Online mindfulness intervention	WL
Cavanagh et al. (2018)	UK	University staff & students	155	31.02	78.67%	Online mindfulness intervention	1) Mindfulness psychoeducation 2) WL
Chacko et al. (2016)	USA	Individuals who had undergone bariatric surgery	18	53.95	84%	Adapted MBSR	Counselling session with dietician
Chadwick et al. (2016)	UK	Induvial diagnosed with schizophrenia	93	42; 18-65	50%	Person-Based Cognitive Therapy (PBCT)	TAU
Chen et al. (2017)	China	Patients with intrauterine adhesion	151	30.9	100%	MBSR	WL
Cherkin et al. (2016)	USA	Individuals with chronic low back pain	200	49.3 (12.3); 20-70	65.70%	MBSR	I: TAU; A: CBT

Study	Country	Population	N	Mean age in years, (SD), range	% Female	Mindfulness group	Control group
Chiesa et al. (2012;2015)	Italy	Patients with major depression	43	47.61	74.50%	MBSR	Psychoeducation
Chu et al. (2010)	Taiwan	Graduate students	19	24.42	47.37%	Mindfulness intervention	Relaxation group
Churcher Clarke et al. (2017)	UK	Individuals with dementia	28	80.61 (9.40); 61–95	48.38%	Adapted from MBCT & MBSR	TAU
Cladder-Micus et al. (2018)	Netherlands	Patients with chronic, treatment-resistant depression	106	47.1 (10.25)	62.23%	MBCT	TAU
Cludius et al. (2015)	Germany	Individuals with a diagnosis of OCD	49	40.59	66.67%	Self-help mindfulness training	Self-help Manual of Progressive Muscle Relaxation (PMR)
Cox et al. (2019)	USA	ICU patients with cardiorespiratory failure	49	49.5 (15.1)	44%	Self-directed mindfulness app	Critical illness education
Danilewitz et al. (2016)	Canada	Medical students	22	Not specified	73.30%	Adapted MBSR	WL
Dimidjian et al. (2016)	USA	Pregnant women with MDD	55	29.85	100%	MBCT	TAU
Duncan et al. (2012)	USA	HIV positive adults	65	48.05	16%	MBSR	WL
Duncan et al. (2017)	USA	Pregnant women	38	Not specified	100%	Mindfulness-based pain avoidance in childbirth	Standard childbirth education
Dvorakova et al. (2017)	USA	University students	109	18.2 (0.4); 18–19	66%	Learning to BREATHE program	No program
Dykens et al. (2014)	USA	Mothers of children with autism	138	19.85 (7.53); 2-54	27.70%	MBSR	Positive Adult Development (PAD)

Study	Country	Population	N	Mean age in years, (SD), range	% Female	Mindfulness group	Control group
Falsafi (2016)	USA	University students	44	22.1; 18-50	86.40%	Mindfulness intervention	I: No program; A: Yoga
Faramarzi et al. (2015)	Iran	Pregnant women	86	24.19	100%	MBCT	TAU
Farver-Vestergaard et al. (2018)	Denmark	Patients with chronic obstructive pulmonary disease	84	67.2 (7.74)	57.1%	MBCT	Pulmonary rehabilitation
Fissler et al. (2016)	Germany	Individuals with depression	74	42.5	60%	Brief mindfulness training	Regular rest periods
Foley et al. (2010)	Australia	Individuals with a cancer diagnosis	107	55.18 (10.60); 24–78	77%	Adapted MBCT	WL
Fordham et al. (2015)	UK	Patients with psoriasis	21	41.17 (13.09); 22-70	55.17%	MBCT	TAU
Franco et al. (2010)	Spain	Public secondary school teachers	68	40.2 (18.3); 24-58	50%	Flow meditation program	Psychomotor therapy program
Gallegos et al. (2013)	USA	Older adults	200	72.77	62%	MBSR	WL
Gambrel & Piercy (2015)	USA	Couples expecting their first child	66	31.67; 22-46	50%	Mindful Transition to Parenthood	WL
Garland et al. (2016)	USA	Men with substance misuse	116	37.63	0%	Mindfulness-Oriented Recovery Enhancement (MORE)	I: TAU; A: CBT
Gayner et al. (2012)	Canada	Gay men with HIV	103	44; 25-64	0%	Adapted MBSR	WL
Geschwind et al. (2011)	Netherlands	Adults vulnerable to depression	129	43.99	76%	MBCT	WL
Glasner et al. (2017)	USA	Stimulant-dependent adults	43	45.3 (8.9)	28.60%	Mindfulness-based relapse prevention	Health education
Godfrin & van Heeringen (2010)	Belgium	Individuals with a history of depression	106	45.65	81.15%	MBCT	WL

Study	Country	Population	N	Mean age in years, (SD), range	% Female	Mindfulness group	Control group
Goldberg et al. (2013)	USA	Individuals who smoke	104	42.1 (12.3)	54.10%	Mindfulness-based smoking cessation	No program
Gonzalez-Garcia et al. (2014)	Spain	Long-term diagnosed and treated HIV-infected patients	39	49.4 (5.1)	48.70%	MBCT	TAU
Gross et al. (2010)	USA	Recipients of solid organ transplants	115	51	45%	MBSR	Health education
Gross et al. (2011)	USA	Individuals with chronic insomnia	27	50.25; 21-65	72.50%	MBSR	Pharmacotherapy
Gross et al. (2017)	USA	Patients awaiting kidney transplantation	51	54 (12)	56%	Telephone-adapted MBSR	Telephone-based support group
Gu et al. (2018)	China	Undergraduate students with ADHD	54	20.26; 19-24	44.55%	MBCT	WL
Hall et al. (2018)	China	University students	101	22.3 (2.63)	69.31%	Online MBI	No program
Hauge et al. (2015)	Denmark	Patients with Multiple Chemical Sensitivity	69	53	82.55%	MBCT	TAU
Hazlett-Stevens & Oren (2017)	USA	University students	68	22.1 (4.7); 18-41	75%	MBSR bibliotherapy	No program
Hearn & Finlay (2018)	UK	Individuals with depression & chronic pain following spinal cord injury	67	44.4	54%	Online mindfulness intervention	Online psychoeducation
Henderson et al. (2012; 2013)	USA	Women with breast cancer	105	49.8 (8.4)	100%	MBSR	Nutrition education
Hepburn et al. (2009)	UK	Individuals in remission from depression	43	45.01	79.07%	MBCT	TAU
Hoffman et al. (2012)	UK	Women with breast cancer	214	49.55	100%	MBSR	WL
Hosseinzadeh & Barahmand (2014)	Iran	Drug-dependent men	33	29.5; 17-43	0%	MBCT	WL

Study	Country	Population	N	Mean age in years, (SD), range	% Female	Mindfulness group	Control group
Hou et al. (2013)	UK	Caregivers for relatives with a chronic condition	141	57.49 (8.83).	83%	MBSR	Self-help booklet
Howells et al. (2016)	UK	Healthy adults	121	40.3	85.50%	Smartphone-based mindfulness intervention	List making app Catch Notes
Ivtzan et al. (2016)	UK	Educators & office workers	168	40.82	78.85%	Positive Mindfulness Program (PMP)	WL
James & Rimes (2018)	UK	University students with perfectionism	60	21; 18-39	81.65	MBCT	Self-help CBT
Jang et al. (2016)	Korea	Women with breast cancer	24	51.5	100%	Mindfulness-based Art Therapy	WL
Jansen et al. (2017)	Germany	Healthy adults	31	63.5 (5.7); 52–81	60%	MBSR	I: No program; A: Karate
Jasbi et al. (2018)	Iran	Veterans with PTSD	48	52.97	0%	MBCT	TAU
Jazaieri et al. (2012)	USA	Individuals with social anxiety disorder	60	32.8 (8.4)	52%	MBSR	I: No program; A: Aerobic exercise
Jedel et al. (2014)	USA	Patients with moderately severe ulcerative colitis	51	42.86	56.15%	MBSR	Mind-body lectures
Jennings et al. (2017)	USA	Teachers	202	41.5; 22–73	93%	Cultivating Awareness and Resilience in Education (CARE)	WL
Johannsen et al. (2016)	Denmark	Women with breast cancer	137	56.75	100%	Adapted MBCT	WL
Johns et al. (2016)	USA	Persistently fatigued breast and colorectal cancer survivors	70	56.65	90.10%	Adapted MBSR	Psychoeducation and support

Study	Country	Population	N	Mean age in years, (SD), range	% Female	Mindfulness group	Control group
Joseffson et al. (2014)	Sweden	Healthy adults	69	49.65	91%	Mindfulness programmed	I: WL; A: Relaxation training
Kang et al. (2009)	South Korea	Nursing students	32	22.47	100%	Stress coping program based on mindfulness	No program
Kaviani et al. (2011)	Iran	University students	45	20.6	100%	MBCT	WL
Kearney et al. (2013)	USA	Veterans with PTSD	47	52	21.30%	MBSR	TAU
Kearney et al. (2016)	USA	Veterans with gulf war illness	47	49.95	9.35%	MBSR	TAU
Kelly & Garland (2016)	USA	Survivors of interpersonal violence	39	41.5 (14.6)	100%	MBSR with trauma-specific psychoeducation	WL
Key et al. (2017)	Canada	Individuals with OCD	39	43.37 (14.03)	47.20%	MBCT	WL
Kitsumban et al. (2009)	Thailand	Older adults with mild to moderate depression	54	69.26; 60-80	100%	Cognitive Mindfulness Practice Program (CMPP)	TAU
Kolahkaj & Zargar (2015)	Iran	Women with Multiple Sclerosis	40	24.8	100%	MBSR	TAU
Koszycki et al. (2016)	Canada	Patients with Seasonal Affective Disorder	33	39.67	79%	Mindfulness intervention	WL
Kreuzer et al. (2012)	Germany	Patients with chronic tinnitus	31	50.65	47.23%	Mindfulness and body therapy	WL
Kristeller et al. (2014)	USA	Overweight individuals with Binge Eating Disorder	92	46.55; 20–74	88%	Mindfulness-Based Eating Awareness Training (MBEAT)	I: WL; A: Psychoeducation
Kubo et al. (2019)*	USA	Cancer patients and caregivers	97	59	69%	Headspace App	WL
Kulz et al. (2018)	Germany	Patients with residual OCD	125	38.62 (12.0); 19-70	77%	MBCT	Psychoeducation

Study	Country	Population	N	Mean age in years, (SD), range	% Female	Mindfulness group	Control group
Kuyken et al. (2015)	UK	Individuals with depression	348	49.5	76.50%	MBCT	Antidepressant medication
Kvillemo et al. (2016)	Sweden	University students	76	29; 19-37	79.59%	Internet-based MBSR	Internet-based expressive writing
la Cour & Petersen (2015)	Denmark	Individuals with chronic pain	73	47.68; 19–76	85%	MBSR	WL
Lacerda et al. (2018)	Brazil	Individuals with stress complaints	44	36.61	54.5%	Adapted MBSR	WL
Lee & Jung (2018)	Canada	University students	206	20.6	51.5	Mhealth App	WL
Lengacher et al. (2016)	USA	Breast cancer survivors	299	56.6 (9.7)	100%	MBSR	WL
Liu et al. (2019)	China	patients with thyroid cancer receiving radioactive iodine therapy	102	42.35	36%	MBSR	TAU
Ly et al. (2014)	Sweden	Individuals with depression	81	36 (10.8)	70%	Mindfulness via smartphone app	Behavioral activation via smartphone app
Ma et al. (2018)	China	University students	56	27.84 (7.94); 18-47	57.89%	Mindfulness-based coping with university life	I: Discussion group A: No program
Majid et al. (2012)	Iran	Individuals with Generalized Anxiety Disorder	31	32.19 (2.21); 25-39	0%	MBSR	No program
Mak et al. (2015)	China	University students and staff	183	22.8 (6.504)	66.30%	Mindfulness intervention	No program
Mallya & Fiocco (2015)	Canada	Older adults	97	69.26	73.50%	MBSR	Reading and relaxation

Study	Country	Population	N	Mean age in years, (SD), range	% Female	Mindfulness group	Control group
Manicavasagar et al. (2012)	Australia	Patients with major depression	45	46	64%	MBCT	CBT
Mann et al. (2016)	UK	Individuals with depression	32	44.75; 40-51	92%	MBCT for parents	TAU
Manotas et al. (2014)	Colombia	Health care professionals	78	39.05; 25-59	90.36%	Adapted MBSR	No program
Matvienko-Sikar & Dockray (2017)	Ireland	Pregnant women	25	33.87 (3.04) 27-40	100%	Online body scan and gratitude intervention	TAU
McIndoo et al. (2016)	USA	University students with depression	34	19.2 (1.67)	62%	Individual MBSR	I: WL; A: Behavioral Activation
McManus et al. (2012)	UK	Individuals with symptoms of hypochondriasis	74	42.6	29%	MBCT	WL TAU
Meize-Grochowski et al. (2015)	USA	Patients with postherpetic neuralgia	27	72 (9.6); 55-90	55.60%	Home mindfulness practice	WL TAU
Michalak et al. (2015)	Germany	Patients with major depression	71	50.87	62.26%	MBCT	I: TAU; A: Cognitive Behavioral Analysis System of Psychotherapy (CBASP)
Moir et al. (2016)	New Zealand	Medical students	232	21 (3)	53%	Mindfulness intervention	WL
Mongrain et al. (2016)	Canada	Healthy adults	476	32.64 (11.39); 18-72	64.50%	Mindfulness meditation	I: No program; A: Positivity
Moritz et al. (2006)	Canada	Individuals with a mood disturbance	109	44.03	83.64%	Mindfulness meditation	I: WL; A: Spirituality

Study	Country	Population	N	Mean age in years, (SD), range	% Female	Mindfulness group	Control group
Moss et al. (2015)	USA	Older adults living in a retirement community	39	82 (7.2); 63-94	82.05%	Adapted MBSR	WL
Nakamura et al. (2013)	USA	Cancer survivors with sleep disturbance	39	52.6	75.44%	Adapted MBSR	Sleep Hygiene Education (SHE)
Nathan et al. (2017)	Canada	Patients with painful diabetic peripheral neuropathy	62	59.7 (8.8)	56%	MBSR	WL TAU
Oken et al. (2010)	USA	Dementia caregivers	21	64.46; 45-85	80.65%	Mindfulness based on MBCT & MBSR	Education group developing self-care tools
Oken et al. (2017)	USA	Stressed older adults	128	59.8; 50–85	79.85%	Individual MBCT	WL
O'Leary & Dockray (2015)	Ireland	Healthy adults	20	28.35 (6.65)	100%	Mindfulness online intervention	I: WL; A: online gratitude intervention
Omidi et al. (2013) MDD	Iran	Patients with major depression	60	28, (8); 18-45	66.67%	MBCT	I: TAU; A: CBT
Omidi et al. (2013) PTSD	Iran	Patient with PTSD	62	39-49	0%	MBSR	TAU
Pan et al. (2019)	Taiwan	Pregnant women	96	32.83 (3.83)	100%	Mindfulness-Based Childbirth and Parenting programme	Traditional education classes
Panahi & Faramarzi (2016)	Iran	University students with PMS	60	20	100%	MBCT	WL
Parra-Delgado & Latorre-Postigo (2013)	Spain	Patients with fibromyalgia	31	52.67 (10.08); 30-77	100%	MBCT	TAU
Parswani et al. (2013)	India	Patients with coronary heart disease	30	48.94; 30-65	0%	MBSR	TAU

Study	Country	Population	N	Mean age in years, (SD), range	% Female	Mindfulness group	Control group
Pearson et al. (2018)	Australia	Individuals with diabetes	67	59.4 (12.4)	48.26%	Self-directed audio-based mindfulness practice	Education about diabetes classes
Perez-Blasco et al. (2013)	Spain	Breast-feeding mothers	21	34.33 (4.72)	100%	Mindfulness intervention	WL
Perich et al. (2013)	Australia	Individuals with bipolar disorder	95	not specified	65.50%	Adapted MBCT	TAU
Philippot et al. (2012)	Belgium	Individuals with a tinnitus	25	60.34	40%	MBCT	Relaxation group
Pinniger et al. (2012)	Australia	Adults with self-reported feelings of stress/ anxiety/ depression	45	44.39 (14.27); 18-80	90.90%	Mindfulness meditation based on MBSR	I: WL; A: Tango Dance
Pinniger et al. (2013)	Australia	Adults with self-reported feelings of stress/ anxiety/ depression	34	39.5; 18-68	89.10%	Mindfulness meditation based on MBSR	I: WL; A: Tango Dance
Pots et al. (2014)	Netherlands	Individuals with depression	151	48, (11.29); 20–81	78.10%	MBCT	WL
Pradhan et al. (2007)	USA	Patients with rheumatoid arthritis	63	54.5	87.30%	MBSR	WL
Querstet et al. (2018)	UK	Healthy adults	118	40.68 (10.45); 21-62	80.5%	Online MBI	WL
Raja-Khan et al. (2017)	USA	Individuals with overweight or obesity	57	44.5 (12.5)	100%	MBSR	Health education
Rayan & Ahmad (2017)	Jordan	Parents of children with Autism Spectrum Disorder	104	36.24 (8.5)	70.20%	Mindfulness-based intervention	No program
Rimes & Wingrove (2013)	UK	Individuals with Chronic Fatigue Syndrome	35	43.3	82.86%	MBCT	WL

Study	Country	Population	N	Mean age in years, (SD), range	% Female	Mindfulness group	Control group
Rodgers et al. (2019)	Australia	Individuals with Parkinson's Disease	36	63.7 (8.76); 40-77	45%	MBCT	WL
Roeser et al. (2013)	USA	Primary and secondary school teachers	113	46.9 (9.2); 27-64	89%	Mindfulness training	WL
Sarenmalm et al. (2017)	Sweden	Patients with breast cancer	114	not specified	100%	MBSR	No program
Sasikumar & Latheef (2017)	India	Individuals with diabetes	38	47	60.56%	MBSR	No program
Schellekens et al. (2017)*	Netherlands	Lung cancer patients and their partners	79	58.8	52.50%	MBSR	TAU
Schmidt et al. (2011)	Germany	Patients with fibromyalgia	112	52.5 (9.6)	100%	MBSR	I: WL; A: Education, social support, relaxation
Schoultz et al. (2015)	UK	Patients with Inflammatory Bowel Disease	24	49.14	77.27%	MBCT	WL
Schroevers et al. (2015)	Netherlands	Patients with diabetes	24	55.4	42%	Individual MBCT	WL
Shahar et al. (2010)	Israel	Healthy adults	45	46.66	85.83%	MBCT	WL
Shapiro et al. (1998)	USA	Medical students	73	not specified	56%	MBSR	WL
Shearer et al. (2015)	USA	Psychology undergraduate students	46	not specified	57%	Adapted MBSR	I: No program; A: Destress with dogs
Skovbjerg et al. (2012)	Denmark	Adults with Multiple Chemical Sensitivity	26	51.6	94.60%	MBCT	TAU
Snippe et al. (2015)	Netherlands	Patients with diabetes and comorbid depression	91	not specified	not specified	Individual MBCT	CBT

Study	Country	Population	N	Mean age in years, (SD), range	% Female	Mindfulness group	Control group
Song & Lindquist (2015)	Korea	Nursing students	44	19.5	81.81%	MBSR	WL
Spahn et al. (2013)	Germany	Breast cancer survivors with chronic tumor-associated fatigue	55	56.7	100%	Multimodal mind-body program	Walking intervention
Speca et al. (2000)	Canada	Cancer outpatients	90	51	90.20%	Adapted MBSR	WL
Spek et al. (2013)	Netherlands	Individuals with Autism Spectrum Disorder	41	42.25	65.85%	MBCT for ASD	WL
Stefanaki et al. (2015)	Greece	Women with Polycystic Ovary Syndrome	38	25.85	100%	Mindfulness stress management program	No program
Strauss et al. (2012)	UK	Individuals with chronic depression	28	43 (10.6)	71.43%	Group Person-Based Cognitive Therapy (PBCT)	TAU
Strauss et al. (2018)	UK	Individuals with OCD	37	18-51	64.5%	Mindfulness-based approach to ERP	Group ERP
Sundquist et al. (2015)	Sweden	Patients with depression, anxiety, stress & adjustment disorders	169	41.5	79.53%	Mindfulness intervention based on MBCT & MBSR	TAU
Tang et al. (2015)	China	Patients with epilepsy	60	35.12	46.70%	Mindfulness training	Social support group
Taylor et al. (2014)	UK	University students	79	28.61 (9.12)	81%	MBCT self-help	WL
Thomas et al. (2017)	Canada	Patients undergoing hemodialysis	32	65 (13)	34%	individual chairside meditation practices	TAU
Tovote et al. (2014)	Netherlands	Individuals with diabetes and comorbid depression	62	53.1 (11.8)	49%	MBCT	I: WL; A: CBT

Study	Country	Population	N	Mean age in years, (SD), range	% Female	Mindfulness group	Control group
van Aalderen et al. (2012)	Netherlands	Patients with depression	205	47.5	71%	MBCT	TAU
van Dam et al. (2014)	USA	Individuals experiencing symptoms of anxiety/ depression/ stress	34	39.6	64.10%	Mindfulness Meditation Training (MMT)	WL
van den Hurk et al. (2012)	Netherlands	Recurrently depressed patients currently in remission	71	49.8 (12.2); 24–84	67.65%	MBCT	WL
van der Zwan et al. (2015)	Netherlands	Individuals who suffer from stress	50	26.2	73.33%	Mindfulness meditation	Heart rate variability biofeedback
van Ravensteijn et al. (2013)	Netherlands	10% of the most frequently attending patients in primary care	111	47.05	74.35%	MBCT	Enhanced TAU
van Son et al. (2013)	Netherlands	Patients with diabetes with low levels of emotional wellbeing'	139	56.5	49.65%	Adapted MBCT	WL
Vieten & Astin (2008)	USA	Pregnant women	31	33.9 (3.8)	100%	Mindful Motherhood Intervention	WL
Vollestad et al. (2011)	Norway	Individuals with symptoms of anxiety	76	42.5	67.10%	MBSR	WL
Wahbeh et al. (2016) OA	USA	Older adults	16	76.2	50%	Internet Mindfulness Meditation Intervention (IMMI)	Education
Wahbeh et al. (2016) PTSD	USA	Veterans with PTSD	50	52.13	5.75%	Mindful body scan and breathing intervention	Sitting quietly
Warnecke et al. (2011)	Australia	Medical students	56	23.92; 20-29	65%	audio-based mindfulness intervention	WL

Study	Country	Population	N	Mean age in years, (SD), range	% Female	Mindfulness group	Control group
Weissbecker et al. (2002)	USA	Patients with fibromyalgia	90	48.03 (10.09); 23-74	100%	MBSR	WL
Wells et al. (2014)	USA	Individuals with episodic migraines	19	45.55	89.50%	MBSR	TAU
Whitebird et al. (2013)	USA	Family caregivers	78	56.8 (9.9)	88.50%	MBSR	Community caregiver education and support
Williams et al. (2008)*	UK	Individuals with bipolar disorder	48	Not specified	Not specified	MBCT	WL
Winnebeck et al. (2017)	Germany	Patients with depression	69	41.5	60%	Brief mindfulness training	Psychoeducation
Wolever et al. (2012)	USA	Healthy adults	149	42.9	76.60%	Mindfulness at work stress management	I: no treatment; A: Viniyoga stress reduction
Wong et al. (2011)	China	Individuals with chronic pain	99	47.9 (7.84)	"majority female"	MBSR	education on pain management
Wong et al. (2016)	China	Individuals with Generalized Anxiety Disorder	109	50 (10.02)	79.10%	MBCT	group psychoeducation
Wong et al. (2018)	China	Post-menopausal women	197	52.0 (3.09)	100%	MBSR	Menopause education control
Woolhouse et al. (2014)	Australia	Pregnant women	23	32.89 (0.63); 19-45	100%	Mind Baby Body Program	TAU
Yazdanimehr et al. (2016)	Iran	Pregnant women	63	26.35	100%	Mindfulness-integrated CBT	No program
Yang et al. (2019)	China	Pregnant women	123	30.85	100%	Online MBI	TAU
Younge et al. (2015)	Netherlands	Patients with heart disease	257	43.2	46.30%	Online MBI	TAU

Study	Country	Population	N	Mean age in years, (SD), range	% Female	Mindfulness group	Control group
Zautra et al. (2008)*	USA	Patients with rheumatoid arthritis with and without a history of depression	99	52.41	67.83%	Mindfulness meditation	CBT
Zemestani & Ottaviani (2016)	Iran	Individuals with substance misuse and comorbid depression	74	30.1 (9.7); 18-52	20.30%	Mindfulness-based relapse prevention	TAU
Zhang et al. (2015a)	China	Older adults with chronic insomnia	60	78.1	41.67%	MBSR	WL
Zhang & Emory (2015b)	USA	Pregnant women	33	25.3 (4.6)	100%	Mindful Motherhood Intervention	TAU
Zhang et al. (2017)	China	Leukemia patients in chemotherapy	65	38.35 (8.93); 18-70	47.37%	Mindfulness-based psychological care	TAU

*These studies compared two participant groups, which were both included in the analyses; MBSR = Mindfulness-Based Stress Reduction; MBCT = Mindfulness-Based Cognitive Therapy; CBT = Cognitive-Behavioral Therapy; WL = Waitlist; TAU = Treatment As Usual; MBI=Mindfulness-Based Intervention; I: Inactive control group; A: Active control group; standard deviation (SD) and range for age were not always available.

Appendix Table 3.2.2

Primary doses of included studies

Study	No. face-to-face sessions	Duration of a face-to-face session (in hours)	Program length (in weeks)	Frequency recommended practice (a week)	Duration of a recommended practice (in minutes)
Abholgaseni et al. (2015)	12	1	12		
Advocat et al. (2016)	6	2	6	7	20
Aemla-Or (in press)	8	2.5	8	6	60
Ahmadpanah et al. (2017)	8	1	8		
Alsaraireh & Aloush (2017)	30	1	10		
Anderson et al. (2007)	8	2	8	7	30
Arch et al. (2013)	10	1.5	10	7	30
Arif et al. (2017)	5	0.667	15	7	20
Armstrong & Rimes (2016)	8	2	8	7	45
Astin et al. (2003)	8	2.5	8		
Asuero et al. (2014)	8	2.5	8	7	45
Bahrani et al. (2017)	8	2	8	7	45
Barnhofer et al. (2009)	8	2	8	6	60
Barnhofer et al. (2015)	8	2	8	6	60
Barry et al. (2019)	0		8	7	30
Beattie et al. (2017)	8	2	8	7	30
Bedard et al. (2013)	10	0.5	10	7	30
Benn et al. (2012)	10	2.5	5	7	10
Bergen-Cico et al. (2014)	4	1.5	4	7	45
Bhayee et al. (2016)	0		6	7	10
Black et al. (2015)	6	2	6	7	20

Study	No. face-to-face sessions	Duration of a face-to-face session (in hours)	Program length (in weeks)	Frequency recommended practice (a week)	Duration of a recommended practice (in minutes)
Boettcher et al. (2014)	0		2	6	20
Bogosian et al. (2015)	8	8	8	7	20
Bostock et al. (2018)	0		8	7	20
Bower et al. (2015)	6	2	6	7	20
Braenstroem et al. (2010)	8	2	8	6	60
Britton et al. (2012)	8	3	8	7	45
Brotto et al. (2012)	3	1.5	12	7	20
Brown et al. (2016)	8	2	8	6	60
Carletto et al. (2017)	8	3	8	7	45
Cavanagh et al. (2013)	0		2	7	10
Cavanagh et al. (2018)	0		2	7	10
Chacko et al. (2016)	10	1.5	10	6	60
Chadwick et al. (2016)	12	1.5	12	7	10
Chen et al. (2017)	8	2.5	8	6	45
Cherkin et al. (2016)	8	2	8	7	60
Chiesa et al. (2012;2015)	8	2	8	6	45
Cladder-Micus et al. (2018)	8	2.5	8	6	40
Cox et al. (2019)	0		4	7	8
Chu et al. (2010)	8	0.333	8		
Churcher Clarke et al. (2017)	10	1	5	7	10
Cludius et al. (2015)	0		6	7	42

Study	No. face-to-face sessions	Duration of a face-to-face session (in hours)	Program length (in weeks)	Frequency recommended practice (a week)	Duration of a recommended practice (in minutes)
Danilewitz et al. (2016)	8	1.5	8	7	45
Dimidjian et al. (2016)	8	2	8	6	45
Duncan et al. (2012)	8	3	8	7	60
Duncan et al. (2017)	3	3.5	0.357		
Dvorakova et al. (2017)	8	1.33	6	7	15
Dykens et al. (2014)	6	1.5	6	7	60
Falsafi (2016)	8	1.25	8	7	20
Faramarzi et al. (2015)	8	0.8333	3		
Farver-Vestergaard et al. (2018)	8	1.75	8	7	30
Fissler et al. (2016)	3	1.5	3	7	25
Foley et al. (2010)	8	2	8	7	60
Fordham et al. (2015)	8	2	8	7	45
Franco et al. (2010)	10	1.5	10	7	40
Gallegos et al. (2013)	7	2	8	7	30
Gambrel & Piercy (2015)	4	2	4	7	15
Garland et al. (2016)	10	2	10	7	15
Gayner et al. (2012)	8	3	8	7	60
Geschwind et al. (2011)	8	2.5	8	7	60
Glasner et al. (2017)	8	1.25	8	7	30
Godfrin & van Heeringen (2010)	8	2.75	8	6	45

Study	No. face-to-face sessions	Duration of a face-to-face session (in hours)	Program length (in weeks)	Frequency recommended practice (a week)	Duration of a recommended practice (in minutes)
Goldberg et al. (2013)	10	3.2	10	7	30
Gonzalez-Garcia et al. (2014)	8	2.5	8	6	45
Gross et al. (2010)	8	2.5	8	6	45
Gross et al. (2011)	8	2.5	8	7	40
Gross et al. (2017)	1	3	1	7	41
Gu et al. (2018)	6	1	6	7	30
Hall et al. (2018)	2	2.6	4	2	25
Hauge et al. (2015)	8	2.5	8	7	45
Hazlett-Stevens & Oren (2017)	0		10	7	60
Hearn & Finlay (2018)	0		8	6	45
Henderson et al. (2012; 2013)	7	3.5	7	7	45
Hepburn et al. (2009)	8	2	8	7	60
Hoffman et al. (2012)	8	2	8	6	45
Hosseinzadeh & Barahmand (2014)	8	2	8	6	45
Hou et al. (2013)	8	2	8	7	45
Howells et al. (2016)	0		1.42857	7	10
Ivtzan et al. (2016)	0		8	7	22
James & Rimes (2018)	8	2	8	6	40
Jang et al. (2016)	12	0.75	12	7	45
Jansen et al. (2017)	15	1	8		
Jasbi et al. (2018)	8	1	8	6	40
Jazaieri et al. (2012)	8	2.5	8	7	60
Jedel et al. (2014)	8	2.5	8	6	45

Study	No. face-to-face sessions	Duration of a face-to-face session (in hours)	Program length (in weeks)	Frequency recommended practice (a week)	Duration of a recommended practice (in minutes)
Jennings et al. (2017)	5	6	16	7	30
Johannsen et al. (2016)	8	2	8	7	45
Johns et al. (2016)	7	2	7	7	20
Joseffson et al. (2014)	8	0.75	4	7	20
Kang et al. (2009)	8	2	8	7	45
Kaviani et al. (2011)	8	2.5	8	7	40
Kearney et al. (2013)	8	2.5	8	6	45
Kearney et al. (2016)	8	2.5	8	7	45
Kelly & Garland (2016)	8	2	8	7	45
Key et al. (2017)	8	2	8	7	25
Kitsumban et al. (2009)	11	3	4		
Kolahkaj & Zargar (2015)	8	2	8		
Koszycki et al. (2016)	8	2.5	8	7	30
Kreuzer et al. (2012)	4	2	7	6	60
Kristeller et al. (2014)	12	1.5	9	7	40
Kubo et al. (2019)*	0		8	8	10
Kulz et al. (2018)	8	2	8	7	45
Kuyken et al. (2015)	8	2.5	8	7	45
Kvillemo et al. (2016)	0		8	6	40
Lacerda et al. (2018)	8	1.125	8	5	30

Study	No. face-to-face sessions	Duration of a face-to-face session (in hours)	Program length (in weeks)	Frequency recommended practice (a week)	Duration of a recommended practice (in minutes)
la Cour & Petersen (2015)	8	3	8	7	45
Lee & Jung (2018)	0		4	5	10
Lengacher et al. (2016)	6	2	6	6	45
Liu et al. (2019)	8	2.5	8		
Ly et al. (2014)	0		8	7	30
Ma et al. (2018)	0		8	6	40
Majid et al. (2012)	8	2	8	7	30
Mak et al. (2015)	0		8	6	30
Mallya & Fiocco (2015)	8	2.5	8	7	30
Manicavasagar et al. (2012)	8	2.5	8	7	40
Mann et al. (2016)	8	2	8		
Manotas et al. (2014)	4	2	4	7	25
Matvienko-Sikar & Dockray (2017)	0		3	7	6
McIndoo et al. (2016)	4	1	4	7	40
McManus et al. (2012)	8	2	8	6	60
Meize-Grochowski et al. (2015)	0		6	7	15
Michalak et al. (2015)	8	2.5	8	7	40
Moir et al. (2016)	19	1	19	7	15
Mongrain et al. (2016)	0		3	3.5	10
Moritz et al. (2006)	8	1.5	8	7	40
Moss et al. (2015)	8	2	8	7	30
Nakamura et al. (2013)	3	2	3	7	45
Nathan et al. (2017)	8	2.5	8		

Study	No. face-to-face sessions	Duration of a face-to-face session (in hours)	Program length (in weeks)	Frequency recommended practice (a week)	Duration of a recommended practice (in minutes)
Oken et al. (2010)	6	1.5	6	7	45
Oken et al. (2017)	6	1.5	6	7	45
O'Leary & Dockray (2015)	0		3	4	15
Omidi et al. (2013) MDD	8	2	8	7	60
Omidi et al. (2013) PTSD	8	2	8	7	60
Pan et al. (2019)	8	3	8	6	30
Panahi & Faramarzi (2016)	8	2	8	7	30
Parra-Delgado & Latorre-Postigo (2013)	8	2.5	8	6	45
Parswani et al. (2013)	8	1.5	8	7	30
Pearson et al. (2018)	0		8	7	30
Perez-Blasco et al. (2013)	8	2	8	7	20
Perich et al. (2013)	8	2.5	8	7	40
Philippot et al. (2012)	6	2.25	6	7	40
Pinniger et al. (2012)	6	1.5	6	7	30
Pinniger et al. (2013)	8	1.5	8	7	30
Pots et al. (2014)	11	1.5	11	7	15
Pradhan et al. (2007)	8	2.5	8	6	45
Querstet et al. (2018)	0		4	7	30
Raja-Khan et al. (2017)	8	2.5	8	7	30
Rayan & Ahmad (2017)	5	2.5	5	7	30

Study	No. face-to-face sessions	Duration of a face-to-face session (in hours)	Program length (in weeks)	Frequency recommended practice (a week)	Duration of a recommended practice (in minutes)
Rimes & Wingrove (2013)	8	2.25	8	6	45
Rodgers et al. (2019)	6	2	8		
Roeser et al. (2013)	8	3.27	11	7	15
Sarenmalm et al. (2017)	8	2	8	6	20
Sasikumar & Latheef (2017)	8	0.5	8		
Schellekens et al. (2017)*	8	2.5	8	7	45
Schmidt et al. (2011)	8	2.5	8	7	60
Schoultz et al. (2015)	8	2	8	6	45
Schroevers et al. (2015)	8	1	8	7	30
Shahar et al. (2010)	8	3	8	6	45
Shapiro et al. (1998)	7	2.5	7	7	45
Shearer et al. (2015)	4	1	4	7	15
Skovbjerg et al. (2012)	8	2.5	8	6	45
Snippe et al. (2015)	8	1	8	7	45
Song & Lindquist (2015)	8	2	8		
Spahn et al. (2013)	10	6	10	1	30
Speca et al. (2000)	7	1.5	7	7	45
Spek et al. (2013)	9	2.5	9	6	60
Stefanaki et al. (2015)	0		8	1	30
Strauss et al. (2012)	12	1.5	12	7	10
Strauss et al. (2018)	10	2	10		

Study	No. face-to-face sessions	Duration of a face-to-face session (in hours)	Program length (in weeks)	Frequency recommended practice (a week)	Duration of a recommended practice (in minutes)
Sundquist et al. (2015)	8	2	8	7	20
Tang et al. (2015)	4	2.5	6	7	45
Taylor et al. (2014)	0		8	1	30
Thomas et al. (2017)	24	0.25	8	2	15
Tovote et al. (2014)	8	1	8	7	30
van Aalderen et al. (2012)	8	2.5	8	6	45
van Dam et al. (2014)	8	2	8	7	40
van den Hurk et al. (2012)	8	2.5	8	7	30
van der Zwan et al. (2015)	1	2	5	7	20
van Ravensteijn et al. (2013)	8	2.5	8	6	45
van Son et al. (2013)	8	2	8	4	30
Vieten & Astin (2008)	8	2	8	7	20
Vollestad et al. (2011)	8	2.5	8	7	60
Wahbeh et al. (2016) OA	6	1	6	7	20
Wahbeh et al. (2016) PTSD	6	1.5	6	7	45
Warnecke et al. (2011)	0		8	7	30
Weissbecker et al. (2002)	8	2.5	8	6	45
Wells et al. (2014)	8	2	8	5	45
Whitebird et al. (2013)	8	2.5	8	7	60
Williams et al. (2008)*	8	2	8	6	45

Study	No. face-to-face sessions	Duration of a face-to-face session (in hours)	Program length (in weeks)	Frequency recommended practice (a week)	Duration of a recommended practice (in minutes)
Winnebeck et al. (2017)	2	1.5	2		
Wolever et al. (2012)	12	1	12	7	15
Wong et al. (2011)	8	2.5	8	7	45
Wong et al. (2016)	8	2	8	7	45
Wong et al. (2018)	8	2.5	8	7	40
Woolhouse et al. (2014)	6	2	6	7	45
Yang et al. (2019)	0		8	7	40
Yazdanimehr et al. (2016)	8	1.5	8		
Younge et al. (2015)	0		12		
Zautra et al. (2008)*	8	2	8	7	10
Zemestani & Ottaviani (2016)	8	2	8	7	15
Zhang et al. (2015a)	8	2	8	7	45
Zhang & Emory (2015b)	4	2	4	7	60
Zhang et al. (2017)	0		5	5	30

* These studies compared two participant groups, which were both included in the analysis and had the same doses for both groups; fields are left blank where this dose could not be calculated due to insufficient information available from published papers.

Appendix Table 3.2.3

Composite doses of included studies

Study	Total face-to-face contact (in hours)**	Total recommended program use (in hours)	Total actual program use (in hours)	Program intensity (sessions/ week)	Program intensity incl. retreats	Face-to-face contact/week (in hours)**	Recommended program use (in hours)/ week	Actual program use (in hours)/ week
Abholgaseni et al. (2015)	12			1	1	1		
Advocat et al. (2016)	12	13.67		1	1	2	2.2783	
Aemla-Or (in press)	27.5	69.5		1	1.125	3.4375	8.6875	
Ahmadpanah et al. (2017)	12			2	2	3		
Alsaraireh & Aloush (2017)	30			3	3	3		
Anderson et al. (2007)	12	61		1	1	1.5	7.625	
Arch et al. (2013)	18	49.5		1	1.1	1.8	4.95	
Arif et al. (2017)	3.35	38.3456		0.3333	0.3333	0.2233	2.556	
Armstrong & Rimes (2016)	16	48.83	40.069	1	1	2	6.10375	5.0086
Astin et al. (2003)	20			1	1	2.5		
Asuero et al. (2014)	28	56.14		1	1.125	3.5	7.0175	
Bahrani et al. (2017)	16	52.75		1	1	2	6.5938	
Barnhofer et al. (2009)	16	65		1	1	2	8.125	
Barnhofer et al. (2015)	16	65		1	1	2	8.125	
Barry et al. (2019)	0	28	17.5			0	3.5	2.5
Beattie et al. (2017)	16	40.5	55.34	1	1	2	5.0625	6.9175
Bedard et al. (2013)	5	33		1	1	0.5	3.3	
Benn et al. (2012)	36	40.676		2	2.4	7.2	8.1352	
Bergen-Cico et al. (2014)	6	21.75		1	1	1.5	5.4375	
Bhayee et al. (2016)	7	7	7	1	1	1.167	1.1667	1.1667
Black et al. (2015)	12	23.6655		1	1	2	3.944	

Study	Total face-to-face contact (in hours)**	Total recommended program use (in hours)	Total actual program use (in hours)	Program intensity (sessions/ week)	Program intensity incl. retreats	Face-to-face contact/week (in hours)**	Recommended program use (in hours)/ week	Actual program use (in hours)/ week
Boettcher et al. (2014)	0	21	7.3			0	2.625	0.9125
Bogosian et al. (2015)	0	16.33				0	2.04125	
Bostock et al. (2018)	0	18.665	5.6			0	2.3331	0.7
Bower et al. (2015)	12	23.67		1	1	2	3.945	
Braenstroem et al. (2010)	16	58		1	1	2	7.25	
Britton et al. (2012)	32	68.75	37.0315	1	1.125	4	8.59375	4.6289
Brotto et al. (2012)	4.5	30.17		0.25	0.25	0.375	2.5142	
Brown et al. (2016)	24	66		1	1.125	3	8.25	
Carletto et al. (2017)	31	67.75		1	1.125	3.875	8.46875	
Cavanagh et al. (2013)	0	2.33				0	1.165	
Cavanagh et al. (2018)	0	2.338				0	1.167	
Chacko et al. (2016)	19	73		1	1.1	1.9	7.3	
Chadwick et al. (2016)	18	30.859		1	1	1.5	2.5716	
Chen et al. (2017)	20	51.5		1	1.125	2.5	6.4375	
Cherkin et al. (2016)	32	32		1	1.125	4	4	
Chiesa et al. (2012;2015)	16	47.5		1	1	2	5.9375	
Chu et al. (2010)	2.67			1	1	0.33375		
Churcher Clarke et al. (2017)	10	5.845	10.9556	2	2	2	1.169	2.19112
Cladder-Micus et al. (2018)	20	32.0016		1	1.125	2.5	4.0002	
Cludius et al. (2015)	0	42	42			0	7	7
Cox et al. (2019)	0	3.7324	3.434			0	0.9331	0.891
Danilewitz et al. (2016)	12	61		1	1	1.5	7.625	
Dimidjian et al. (2016)	16	51		1	1	2	6.376	
Duncan et al. (2012)	30	75		1	1.125	3.75	9.375	

Study	Total face-to-face contact (in hours)**	Total recommended program use (in hours)	Total actual program use (in hours)	Program intensity (sessions/ week)	Program intensity incl. retreats	Face-to-face contact/week (in hours)**	Recommended program use (in hours)/ week	Actual program use (in hours)/ week
Duncan et al. (2017)	18			2.5	2.5	18		
Dvorakova et al. (2017)	10.6664	10.5		1.3333	1.3333	1.7777	1.75	
Dykens et al. (2014)	9	9		1	1	1.5	1.5	
Falsafi (2016)	10	26.33	14.7234	1	1	1.125	3.29125	1.8404
Faramarzi et al. (2015)	6.67			2.67	2.67	2.2233		
Farver-Vestergaard et al. (2018)	14	28		1	1	1.75	3.5	
Fissler et al. (2016)	4.5	10.33	4.5	1	1	1.5	3.4433	1.5
Foley et al. (2010)	21	70	37.56	1	1.125	2.625	8.75	4.695
Fordham et al. (2015)	16	52.75	15.96	1	1	2	6.59375	1.995
Franco et al. (2010)	15	57.21		1	1	1.5	5.721	
Gallegos et al. (2013)	21	45.5	40.52	1	1	2.625	5.6875	5.065
Gambrel & Piercy (2015)	8	13.25	19.28	1	1	2	3.3125	4.82
Garland et al. (2016)	20	35.75		1	1	2	3.575	
Gayner et al. (2012)	32	81		1	1.125	4	10.125	
Geschwind et al. (2011)	20	69		1	1	2.5	8.625	
Glasner et al. (2017)	10	24.5	6.16667	1	1	1.25	3.0625	0.7708
Godfrin & van Heeringen (2010)	22	53.5		1	1	2.75	6.6875	
Goldberg et al. (2013)	32	56.5	31.64	1	1	4	7.0625	3.955
Gonzalez-Garcia et al. (2014)	20	51.5		1	1	2.5	6.4375	
Gross et al. (2010)	26	75	36.1817	1	1.125	3.25	9.375	4.523
Gross et al. (2011)	26	75	41.265	1	1.125	3.25	9.375	5.158
Gross et al. (2017)	6	38		1	1	0.75	4.75	
Gu et al. (2018)	6	27		1	1	1	4.5	

Study	Total face-to-face contact (in hours)**	Total recommended program use (in hours)	Total actual program use (in hours)	Program intensity (sessions/ week)	Program intensity incl. retreats	Face-to-face contact/week (in hours)**	Recommended program use (in hours)/ week	Actual program use (in hours)/ week
Hall et al. (2018)	3	3.8667	2.937	0.5	0.5	1.5	2.366	0.734
Hauge et al. (2015)	28	60.83		1	1.125	3.5	7.60375	
Hazlett-Stevens & Oren (2017)	0	47.25	36.288			0	4.725	3.6288
Hearn & Finlay (2018)	0	16	11.52			0	2	1.44
Henderson et al. (2012; 2013)	32	81		1	1	4	10.125	
Hepburn et al. (2009)	22	71		1	1.125	2.75	8.875	
Hoffman et al. (2012)	22.5	54	35.58	1	1.125	2.85	6.75	4.4475
Hosseinzadeh & Barahmand (2014)	16	65		1	1	2	8.125	
Hou et al. (2013)	16	52.75	34.1067	1	1	2	6.59375	4.2633
Howells et al. (2016)	1.67	1.67	1.67	1	1	1.169	1.169	1.168
Ivtzan et al. (2016)	0	3				0	0.375	
James & Rimes (2018)	16	20.002	21.3	1	1	2	4.533	2.227
Jang et al. (2016)	9	9		1	1	0.6923	0.75	
Jansen et al. (2017)	15		12.6	1.875	1.875	1.875		1.575
Jasbi et al. (2018)	8	40		1	1	1	5	
Jazaieri et al. (2012)	22	54.83		1	1.125	2.75	6.85375	
Jedel et al. (2014)	20	51.5		1	1	2.5	6.4375	
Jennings et al. (2017)	30	30	26.94	0.3125	0.3125	1.875	2.5	2.245
Johannsen et al. (2016)	16	52.75	31.2	1	1	2	6.59375	3.9
Johns et al. (2016)	16	65	41.32	1	1	2	8.125	5.165
Joseffson et al. (2014)	6	13		2	2	1.5	3.25	
Kang et al. (2009)	12	12		1	1	1.5	1.5	
Kaviani et al. (2011)	20	52.83		1	1	2.5	6.60375	

Study	Total face-to-face contact (in hours)**	Total recommended program use (in hours)	Total actual program use (in hours)	Program intensity (sessions/ week)	Program intensity incl. retreats	Face-to-face contact/week (in hours)**	Recommended program use (in hours)/ week	Actual program use (in hours)/ week
Kearney et al. (2013)	27	58.5		1	1.125	3.375	7.3125	
Kearney et al. (2016)	27	63.75		1	1.125	3.375	7.96875	
Kelly & Garland (2016)	20	56.75		1	1	2.5	7.09375	
Key et al. (2017)	16	30	26.36	1	1	2	3.75	3.295
Kitsumban et al. (2009)	48			2.75	2.75	12		
Kolahkaj & Zargar (2015)	16			1	1	2		
Koszycki et al. (2016)	24	53.75	46.24	1	1	2	4.479	3.8533
Kreuzer et al. (2012)	22	64		0.57	0.57	3.1428	9.1429	
Kristeller et al. (2014)	14.5	52.02	38.557	1.3333	1.3333	1.6111	5.78	4.284
Kubo et al. (2019)*	0	9.333	5.6			0	1.167	0.7
Kulz et al. (2018)	16	42		1	1	1	2.5	
Kuyken et al. (2015)	20	69		1	1	2.5	8.625	
Kvillemo et al. (2016)	0	28.14	19.296			0	3.5175	2.412
Lacerda et al. (2018)	9	11.5		1	1	1	2.5	
la Cour & Petersen (2015)	28.5	65.25		1	1.125	3.5625	8.15625	
Lee & Jung (2018)	0	3.3334				0	0.83334	
Lengacher et al. (2016)	12	39		1	1	2	6.5	
Liu et al. (2019)	20			1	1	2.5		
Ly et al. (2014)	0	24.5	5.8			0	3.0625	0.725
Ma et al. (2018)	0	37.334	15.992			0	4.6669	1.999
Majid et al. (2012)	16	40.5		1	1	2	5.0625	
Mak et al. (2015)	0	21				0	2.625	
Mallya & Fiocco (2015)	20	44.5		1	1	2.5	5.5625	
Manicavasagar et al. (2012)	20	48.14		1	1	2.5	6.0175	
Mann et al. (2016)	20			1	1.125	2.5		

Study	Total face-to-face contact (in hours)**	Total recommended program use (in hours)	Total actual program use (in hours)	Program intensity (sessions/ week)	Program intensity incl. retreats	Face-to-face contact/week (in hours)**	Recommended program use (in hours)/ week	Actual program use (in hours)/ week
Manotas et al. (2014)	8	16.75		1	1	2	4.1875	
Matvienko-Sikar & Dockray (2017)	0	1.2				0	0.4	
McIndoo et al. (2016)	4	18.07		1	1	1	4.5175	
McManus et al. (2012)	16	58		1	1	2	7.25	
Meize-Grochowski et al. (2015)	1.39	1.39		1	1	0.2317	0.2317	
Michalak et al. (2015)	20	52.83		1	1	2.5	6.60375	
Moir et al. (2016)	12	40.14		1	0.89	0.6316	2.1126	
Mongrain et al. (2016)	0	1.75	1.75			0	0.5833	0.5833
Moritz et al. (2006)	0	12				0	2	
Moss et al. (2015)	16	40.5	10	1	1	2	5.0625	1.25
Nakamura et al. (2013)	6	15.38		4	4	2	5.1267	
Nathan et al. (2017)	26		17.238	1	1.125	3.25		2.15475
Oken et al. (2010)	9	32.45		1	1	1.5	5.4083	
Oken et al. (2017)	9	35.25	11.525	1	1	1.5	5.875	1.9208
O'Leary & Dockray (2015)	0	3	3			0	1	1
Omidi et al. (2013) MDD	16	65		1	1	2	8.125	
Omidi et al. (2013) PTSD	16	65		1	1	2	8.125	
Pan et al. (2019)	24	48		1	1.125	3	6	
Panahi & Faramarzi (2016)	16	65		1	1	2	8.125	
Parra-Delgado & Latorre-Postigo (2013)	20	69		1	1	2.5	8.625	
Parswani et al. (2013)	12	36.5		1	1	1.5	4.5625	

Study	Total face-to-face contact (in hours)**	Total recommended program use (in hours)	Total actual program use (in hours)	Program intensity (sessions/ week)	Program intensity incl. retreats	Face-to-face contact/week (in hours)**	Recommended program use (in hours)/ week	Actual program use (in hours)/ week
Pearson et al. (2018)	0	28				0	3.5	
Perez-Blasco et al. (2013)	16	32.33		1	1	2	4.0413	
Perich et al. (2013)	20	52.83		1	1	2.5	6.60375	
Philippot et al. (2012)	13.5	36.95		1	1	2.25	6.1583	
Pinniger et al. (2012)	9	26.5		1	1	1.5	4.4167	
Pinniger et al. (2013)	12	36.5		1	1	1.5	4.5625	
Pots et al. (2014)	15.5	26		1	1	1.4091	2.3636	
Pradhan et al. (2007)	27	58.5		1	1.125	3.375	7.3125	
Querstet et al. (2018)	0	14				0	3.5	
Raja-Khan et al. (2017)	26	50.5		1	1.125	3.25	6.3125	
Rayan & Ahmad (2017)	5	14	21.61	1	1	1	2.8	4.322
Rimes & Wingrove (2013)	18	50.83	47.57	1	1	2.25	6.35375	5.94625
Rodgers et al. (2019)	12			1	1	2		
Roeser et al. (2013)	36	48.25	42.8	1.375	1.375	4.5	6.03125	5.35
Sarenmalm et al. (2017)	16	14		1	1	2	1.75	
Sasikumar & Latheef (2017)	20			1	1	2.5		
Schellekens et al. (2017)*	26	36.75		1	1.125	3.25	4.594	
Schmidt et al. (2011)	27	76		1	1.125	3.375	9.5	
Schoultz et al. (2015)	16	47.5		1	1.125	2	5.9375	
Schroevers et al. (2015)	8	32.5		1	1	1	4.0625	
Shahar et al. (2010)	31	31		1	1.125	3.875	3.875	
Shapiro et al. (1998)	24.5	66.5		1	1.14	4.5	9.5	
Shearer et al. (2015)	4	9.25	8.92	1	1	1	2.3125	2.23
Skovbjerg et al. (2012)	20	51.5		1	1	2.5	6.4375	

Study	Total face-to-face contact (in hours)**	Total recommended program use (in hours)	Total actual program use (in hours)	Program intensity (sessions/ week)	Program intensity incl. retreats	Face-to-face contact/week (in hours)**	Recommended program use (in hours)/ week	Actual program use (in hours)/ week
Snippe et al. (2015)	15	51.75	28.12	1	1	1.875	6.4688	3.515
Song & Lindquist (2015)	6			1	1	0.75		
Spahn et al. (2013)	60	73.5		1	1	6	7.35	
Speca et al. (2000)	10.5	34.62		1	1	1.5	4.947	
Spek et al. (2013)	22.5	70.5		1	1	2.5	7.8333	
Stefanaki et al. (2015)	0	4				0	0.5	
Strauss et al. (2012)	18	30.869		1	1	1.5	2.5724	
Strauss et al. (2018)	20			1	1	2		
Sundquist et al. (2015)	16	32.33		1	1	2	4.04125	
Tang et al. (2015)	10	36.25		0.67	0.67	1.667	6.04167	
Taylor et al. (2014)	0	10				0	1.25	
Thomas et al. (2017)	6	12.25		3	3	0.75	1.53125	
Tovote et al. (2014)	8	32.5		1	1	1	4.0625	
van Aalderen et al. (2012)	26	57.5		1	1.125	3.25	7.1875	
van Dam et al. (2014)	22	54.83		1	1.125	2.75	6.85375	
van den Hurk et al. (2012)	27	27		1	1.125	3.375	3.375	
van der Zwan et al. (2015)	9.92	9.92		1	1	1.984	1.984	
van Ravensteijn et al. (2013)	26	57.5		1	1.125	3.25	7.1875	
van Son et al. (2013)	16	33.5		1	1	2	4.1875	
Vieten & Astin (2008)	16	32.33	24.1719	1	1	2	4.04125	3.0215
Vollestad et al. (2011)	24	60.75	30.25	1	1.125	3	7.594	3.78125
Wahbeh et al. (2016) OA	6	35.25	25.34	1	1	1	5.875	4.2233
Wahbeh et al. (2016) PTSD	9	17.76		1	1	1.5	2.945	
Warnecke et al. (2011)	0	18.67	21.42			0	2.33376	2.6775

Study	Total face-to-face contact (in hours)**	Total recommended program use (in hours)	Total actual program use (in hours)	Program intensity (sessions/ week)	Program intensity incl. retreats	Face-to-face contact/week (in hours)**	Recommended program use (in hours)/ week	Actual program use (in hours)/ week
Weissbecker et al. (2002)	20	51.5		1	1	2.5	6.4375	
Wells et al. (2014)	22	48.25	42.3683	1	1.125	2.75	6.03125	5.296
Whitebird et al. (2013)	25	74	69.96	1	1.125	3.125	9.25	8.745
Williams et al. (2008)*	22	53.5		1	1.125	2.75	6.6875	
Winnebeck et al. (2017)	3.83333			1	1.5	1.9167		
Wolever et al. (2012)	14	33.25		1	1.083	1.1667	2.7703	
Wong et al. (2011)	27	63.75	68.45	1	1.125	3.375	7.9688	8.556
Wong et al. (2016)	16	52.75		1	1	2	6.59375	
Wong et al. (2018)	20	37.33		1	1	2.5	4.667	
Woolhouse et al. (2014)	12	35.45		1	1	2	5.9083	
Yang et al. (2019)	0	9.9994	4.6			0	2.333	1.15
Yazdanimehr et al. (2016)	12			1	1	1.5		
Younge et al. (2015)	0					0		
Zautra et al. (2008)*	16	24.183		1	1	2	3.0229	
Zemestani & Ottaviani (2016)	16	45		1	1	2	5.625	
Zhang et al. (2015a)	18	54.75		1	1.125	2.25	6.84375	
Zhang & Emory (2015b)	16	31.75		2	2	4	7.9375	
Zhang et al. (2017)	23.3	23.3		1	1	4.66	4.66	

*These studies compared two participant groups, which were both included in the analysis and had the same doses for both groups; **for the contact hours and contact hours/week doses, this analysis was repeated with studies that had zero hours of contact excluded; fields are left blank where this dose could not be calculated due to insufficient information available from published paper

Appendix Table 3.2.4

Comparison of means in doses between MBSR/MBCT and other MBPs

Dose	*k*	M(SD)	M diff.	F	t	*p*
Total no. of face-to-face sessions	**MBSR/CT: 107** **Other: 96**	**MBSR/CT: 7.74 (1.68)** **Other: 5.66 (5.27)**	**2.08**	**79.89**	**3.87**	**<.001**
Duration of a face-to-face session (in hours)	MBSR/CT: 106 Other: 67	MBSR/CT: 2.14 (.81) Other: 1.94 (.99)	2.02	2.6	3.71	.11
Program length (in weeks)	**MBSR/CT: 107** **Other: 96**	**MBSR/CT: 7.78 (1.42)** **Other: 7.24 (3.06)**	**5.38**	**43.79**	**1.47**	**<.001**
Frequency recommended practice (no. recommended practices/week)	**MBSR/CT: 96** **Other: 87**	**MBSR/CT: 6.6 (.59)** **Other: 6.47 (1.42)**	**.14**	**9.34**	**1.4**	**.003**
Duration of recommended home practice (one practice in minutes)	MBSR/CT: 96 Other: 87	MBSR/CT: 42.65 (12.26) Other: 28.5 (13.91)	14.14	4.09	7.31	.05
Total amount of face-to-face facilitator contact (in hours)	**MBSR/CT: 107** **Other: 96**	**MBSR/CT: 18.31 (7)** **Other: 11.14 (11.44)**	**7.17**	**14.33**	**5.44**	**<.001**
Total amount of face-to-face facilitator contact (in hours, excl. 0 hours)	**MBSR/CT: 106** **Other: 96**	**MBSR/CT: 18.48 (6.81)** **Other: 15.5 (10.7)**	**2.98**	**6.84**	**2.25**	**.01**
Total recommended use of the program (in hours)	MBSR/CT: 96 Other: 87	MBSR/CT: 49.49 (17.89) Other: 27.6 (18.49)	21.89	.27	8.13	.6
Total actual use of the program (in hours)	MBSR/CT: 24 Other: 32	MBSR/CT: 33.99 (15.98) Other: 19.37 (14.6)	14.62	.003	3.4	.96
Program intensity (sessions a week)	**MBSR/CT: 106** **Other: 67**	**MBSR/CT: 1.06 (.36)** **Other: 1.14 (.51)**	**.09**	**11.54**	**1.3**	**.001**
Program intensity (sessions a week) incl. retreats	**MBSR/CT: 106** **Other: 67**	**MBSR/CT: 1.1 (.36)** **Other: 1.16 (.52)**	**.06**	**11.84**	**.92**	**.001**

Dose	*k*	M(SD)	M diff.	F	t	*p*
Amount of face-to-face facilitator contact a week (in hours)	**MBSR/CT: 107** **Other: 96**	**MBSR/CT: 2.34 (.86)** **Other: 1.74 (2.42)**	**.6**	**10.3**	**2.41**	**.002**
Amount of face-to-face facilitator contact a week (in hours, excl. 0 hours)	**MBSR/CT: 106** **Other: 96**	**MBSR/CT:2.36 (.83)** **Other: 2.41 (2.56)**	**.05**	**10.08**	**.2**	**.002**
Recommended use of the program a week (in hours)	MBSR/CT: 96 Other: 87	MBSR/CT: 6.26 (2.12) Other: 3.76 (2.22)	2.5	.61	7.82	.44
Actual use of the program a week (in hours)	MBSR/CT: 24 Other: 32	MBSR/CT: 4.27 (1.97) Other: 2.64 (1.83)	1.6	.01	3.04	.92

k= Number of studies; M=Mean; SD=Standard Deviation; M diff=Mean difference between program types; MBSR/CT= MBSR and MBCT programs; other=programs other than MBSR and MBCT; significant results in bold.

Appendix 3.2.5

Risk of bias summary for each domain and study separately

	Random sequence generation (selection bias)	Allocation concealment (selection bias)	Blinding of participants and personnel (performance bias)	Blinding of outcome assessment (detection bias)	Incomplete outcome data (attrition bias)	Selective reporting (reporting bias)	Other bias
Abholgasemi et al. (2015)	?	−	−	−	?	?	+
Advocat et al. (2016)	+	+	−	?	?	+	+
Aemla-Or (in press)	+	+	−	+	+	+	+
Ahmadpanah et al. (2017)	+	+	+	+	+	?	+
Alsaraireh & Aloush (2017)	+	?	+	?	−	?	+
Anderson et al. (2007)	?	?	−	?	+	?	+
Arch et al. (2013)	+	?	+	+	+	+	+
Arif et al. (2017)	+	+	+	?	−	?	+
Armstrong & Rimes (2016)	+	+	+	?	+	?	+
Astin et al. (2003)	+	+	−	+	−	?	+
Asuero et al. (2014)	?	?	−	?	+	?	+
Bahrani et al. (2017)	+	+	−	+	−	+	+
Barnhofer et al. (2009)	+	+	−	+	+	?	+

Barnhofer et al. (2015)	?	+	−	+	+	?	+
Barry et al. (2019)	+	+	−	−	+	?	+
Beattie et al. (2017)	+	+	+	+	−	+	+
Bedard et al. (2013)	−	+	−	?	+	+	+
Benn et al. (2012)	+	?	−	?	−	?	+
Bergen-Cico et al. (2014)	?	?	−	?	+	?	+
Bhayee et al. (2016)	+	+	+	?	+	?	+
Black et al. (2015)	+	+	−	+	+	+	+
Boettcher et al. (2014)	+	+	−	?	+	+	+
Bogosian et al. (2015)	+	+	−	+	+	?	+
Bostock et al. (2018)	+	+	−	?	+	?	+
Bower et al. (2015)	+	+	−	?	+	?	+
Braenstroem et al. (2010, 2012)	+	?	−	+	+	?	+
Britton et al. (2012)	+	+	−	+	−	?	+
Brotto et al. (2012)	?	?	−	?	−	?	+
Brown et al. (2016)	?	?	?	+	+	?	+
Carletto et al. (2017)	+	+	+	+	+	+	+
Cavanagh et al. (2013)	+	?	−	+	+	?	+
Cavanagh et al. (2019)	+	+	−	?	+	?	+
Chacko et al. (2016)	+	+	?	?	+	?	+
Chadwick et al. (2016)	+	+	?	+	+	?	+
Chen et al. (2017)	+	+	−	+	−	?	+
Cherkin et al. (2016)	+	+	−	+	+	+	+

Chiesa et al. (2012, 2015)	+	?	?	?	+	?	+
Chu et al. (2010)	?	?	−	−	?	?	+
Churcher Clarke et al. (2017)	+	?	−	+	+	?	+
Cladder-Micus et al. (2018)	+	+	−	−	+	?	+
Cludius et al. (2015)	−	?	?	−	+	?	+
Cox et al. (2019)	+	?	+	?	−	+	+
Danilewitz et al. (2016)	?	−	−	?	+	?	+
Dimidjian et al. (2016)	?	?	−	?	+	?	+
Duncan et al. (2012)	+	?	−	?	+	?	+
Duncan et al.(2017)	+	+	+	+	+	+	+
Dvorakova et al. (2017)	+	+	−	+	+	?	+
Dykens et al. (2014)	+	?	?	−	−	+	+
Falsafi et al. (2016)	+	?	−	−	−	?	+
Faramarziet al. (2015)	+	+	−	+	+	+	+
Farver-Vestergaard et al. (2018)	+	−	−	+	?	+	+
Fissler et al. (2016)	+	+	−	?	−	?	+
Foley et al. (2010)	?	?	−	+	+	?	+
Fordham et al. (2015)	+	?	−	?	−	?	+
Franco et al. (2010)	?	?	+	?	?	?	+
Gallegos et al. (2013)	+	?	−	?	−	?	+
Gambrel & Piercy (2015)	+	?	−	+	+	?	+

Garland et al. (2016)	+	+	−	+	+	?	+
Gayner et al. (2012)	+	+	−	?	+	?	+
Geschwind et al. (2011)	+	+	−	−	+	+	+
Glasner et al. (2017)	+	?	+	+	+	?	+
Godfrin &Heeringen (2010)	+	+	−	?	+	+	+
Goldberg et al. (2013)	?	?	−	?	−	?	+
Gonzalez-Garcia et al. (2014)	+	?	−	?	?	?	+
Gross et al. (2010)	+	?	−	−	−	?	+
Gross et al. (2011)	+	?	?	?	−	?	+
Gross et al. (2017)	+	+	+	?	+	+	+
Gu et al. (2018)	?	?	−	+	+	?	+
Hall et al. (2018)	+	?	−	?	−	?	+
Hauge et al. (2015)	+	+	−	?	?	+	+
Hazlett-Stevens & Oren (2017)	+	?	−	+	−	?	+
Hearn & Finlay (2018)	+	?	+	−	+	+	+
Henderson et al. (2012/13)	+	?	−	?	+	?	+
Hepburn et al. (2009)	?	?	?	+	−	?	+
Hoffman et al. (2012)	+	+	−	+	+	?	+
Hosseinzadeh et al. (2014)	?	?	−	?	?	?	+
Hou et al. (2014)	+	?	−	+	+	+	+
Howells et al. (2016)	+	+	?	+	+	?	+
Ivtzan et al. (2016)	+	+	−	+	+	?	+

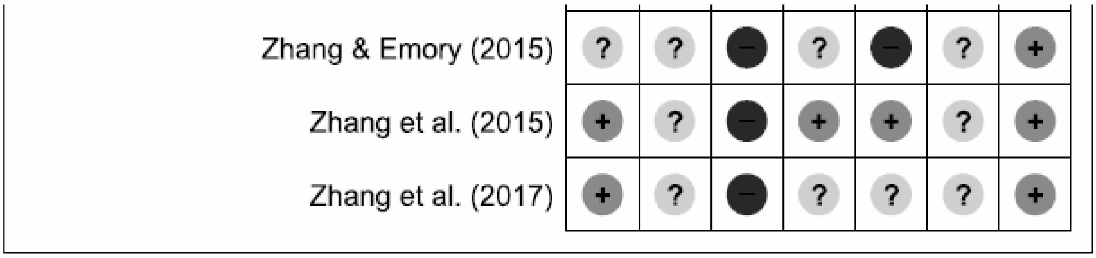
James & Rimes (2018)	+	+	+	?	+	?	+
Jang et al. (2016)	?	?	−	?	+	?	+
Jansen et al. (2017)	?	?	−	?	−	?	+
Jasbi et al. (2018)	+	−	+	?	+	?	+
Jazaieri et al. (2012)	+	?	−	?	+	?	+
Jedel et al. (2014)	+	+	+	+	+	?	+
Jennings et al. (2017)	+	?	−	+	+	?	+
Johannsen et al. (2016)	+	?	−	+	+	+	+
Johns et al. (2016)	+	+	+	+	+	+	+
Joseffson et al. (2014)	+	?	−	?	−	?	+
Kang et al. (2009)	+	+	−	+	−	?	+
Kaviani et al (2011)	?	?	−	?	?	?	+
Kearney et al. (2013)	?	+	−	?	+	?	+
Kearney et al. (2016)	?	+	−	+	+	?	+
Kelly & Garland (2016)	?	+	−	−	+	?	+
Key et al. (2017)	?	?	−	?	+	?	+
Kitsumban et al. (2009)	?	?	−	?	+	?	+
Kolahkaj & Zargar (2015)	+	?	−	?	?	?	+
Koszycki et al. (2016)	+	+	−	+	+	+	+
Kreuzer et al. (2012)	+	?	−	?	?	?	+
Kristeller et al. (2014)	−	?	−	?	+	?	+
Kubo et al. (2019)	+	+	−	−	+	+	+
Kulz et al. (2018)	+	?	+	+	+	+	+
Kuyken et al. (2015)	+	+	−	+	+	+	+

Kvillemo et al. (2016)	+	+	+	+	+	+	+
Lacerda et al. (2018)	+	−	−	?	+	+	+
La Cour & Peterson (2015)	+	+	−	+	+	?	+
Lee & Jung (2018)	+	+	−	+	−	?	+
Lengacher et al. (2016)	+	−	−	−	−	?	+
Liu et al. (2019)	+	+	−	−	−	?	+
Ly et al. (2014)	+	+	+	+	+	+	+
Ma et al. (2018)	?	+	+	?	−	?	+
Majid et al. (2012)	?	?	+	?	+	?	+
Mak et al. (2015)	+	+	+	+	+	?	+
Mallya & Fiocco et al. (2015)	+	?	+	+	+	?	+
Manicavasagar et al. (2012)	?	?	+	+	−	?	+
Mann et al. (2016)	+	+	−	+	−	?	+
Manotas et al. (2014)	?	?	−	+	−	?	+
Matvienko-Sikar et al. (2016)	+	+	+	+	−	?	+
McIndoo et al. (2016)	+	−	−	?	+	?	+
McManus et al. (2012)	+	?	−	+	+	?	+
Meize-Grochowski et al. (2015)	+	?	−	−	−	+	+
Michalak et al. (2015)	+	+	−	+	+	+	+
Moir et al. (2016)	+	+	−	?	−	?	+
Mongrain et al. (2016)	?	?	+	+	−	?	+
Moritz et al. (2006)	+	+	−	+	+	?	+
Moss et al. (2015)	+	+	−	+	+	?	+
Nakamura et al. (2013)	+	?	+	?	?	?	+
Nathan et al. (2017)	+	+	−	+	−	+	+

O'Leary et al. (2015)	?	+	-	+	-	?	+
Oken et al. (2010)	+	+	-	+	-	?	+
Oken et al. (2017)	+	?	-	+	-	+	+
Omidi et al. (2013) MDD	?	?	-	?	?	?	+
Omidi et al. (2013) PTSD	?	?	-	?	?	?	+
Panahi & Faramarzi (2016)	+	?	-	+	?	?	+
Pan et al. (2019)	+	?	-	?	-	+	+
Parra-Delgado & Latorre-Postigo (2013)	+	?	-	?	-	?	+
Parswani et al. (2013)	+	?	-	?	+	?	+
Pearson et al. (2018)	+	+	-	?	+	?	+
Perez-Blasco et al. (2013)	?	?	?	?	-	?	+
Perich et al. (2013)	+	+	-	+	+	+	+
Philippot et al. (2012)	?	?	?	?	-	?	+
Pinniger et al. (2012)	+	-	-	?	-	?	+
Pinniger et al. (2013)	?	?	-	?	-	?	+
Pots et al. (2014)	+	?	-	?	+	+	+
Pradhan et al. (2007)	+	?	-	+	+	+	+
Querstet et al. (2018)	+	+	-	?	+	?	+
Raja-Khan et al. (2017)	+	+	+	+	+	+	+
Rayan & Ahmad (2017)	?	?	-	?	-	?	+
Rimes & Wingrove (2013)	+	+	-	?	+	?	+
Rodgers et al. (2019)	+	?	-	?	-	+	+
Roeser et al. (2013)	?	?	-	?	-	?	+
Sarenmalm et al. (2017)	+	+	-	?	?	?	+
Sasikumar & Latheef (2017)	+	+	-	?	-	?	+

Schellekens et al. (2017)	+	+	−	?	+	+	+
Schmidt et al. (2011)	+	+	−	−	+	?	+
Schoultz et al. (2015)	+	+	−	+	?	?	+
Schroevers et al. (2015)	+	+	−	?	+	?	+
Shahar et al. (2010)	+	+	−	+	−	?	+
Shapiro et al. (1998)	?	?	−	+	?	?	+
Shearer et al. (2015)	−	?	−	+	?	?	+
Skovbjerg et al. (2012)	+	?	−	+	−	+	+
Snippe et al. (2015)	?	?	+	?	+	?	+
Song & Lindquist (2015)	?	?	−	?	−	?	+
Spahn et al. (2013)	+	+	−	?	+	?	+
Speca et al. (2000)	+	?	−	?	+	?	+
Spek et al. (2013)	+	+	−	+	+	?	+
Stefanaki et al. (2015)	+	+	−	+	+	?	+
Strauss et al. (2012)	+	?	−	+	+	?	+
Strauss et al. (2018)	+	+	+	+	+	+	+
Sundquist et al. (2015)	?	+	−	?	+	+	+
Tang et al. (2015)	+	?	+	+	+	?	+
Taylor et al. (2014)	+	?	−	+	+	?	+
Thomas et al. (2017)	+	+	−	+	+	+	+
Tovote et al. (2014)	+	?	−	−	+	+	+
van Aalderen et al. (2012)	+	?	−	?	+	+	+
van Dam et al. (2014)	?	?	−	+	−	?	+
van den Hurk et al. (2012)	?	?	−	?	?	?	+

van der Zwan et al. (2015)	?	?	+	?	+	?	+
van Ravestein et al. (2013)	+	+	−	?	+	?	+
van Son et al. (2013)	+	?	−	?	+	+	+
Vieten & Astin (2008)	?	?	−	−	−	?	+
Vollestad et al. (2011)	?	?	−	?	−	?	+
Wahbeh et al. (2016) OA	?	+	+	+	−	?	+
Wahbeh et al. (2016) PTSD	+	+	−	+	−	?	+
Warnecke et al. (2011)	+	+	−	+	−	?	+
Weissbecker et al. (2002)	?	?	−	?	?	?	+
Wells et al. (2014)	+	+	−	+	+	+	+
Whitebird et al. (2013)	+	?	+	?	+	?	+
Williams et al. (2008)	?	+	−	?	?	?	+
Winnebeck et al. (2017)	+	+	+	+	−	+	+
Wolever et al. (2012)	?	?	−	?	+	?	+
Wong et al. (2011)	+	+	+	+	+	+	+
Wong et al. (2016)	+	+	−	?	?	+	+
Wong et al. (2018)	+	+	+	?	+	+	+
Woolhouse et al. (2014)	?	+	−	?	−	?	+
Yang et al. (2019)	+	+	−	+	+	?	+
Yazdanimehr et al. (2016)	+	?	−	?	−	?	+
Younge et al. (2015)	+	+	−	+	+	+	+
Zautra et al. (2008)	+	?	+	+	+	?	+
Zemestani & Ottaviani (2016)	+	?	−	+	+	?	+


Zhang & Emory (2015)
Zhang et al. (2015)
Zhang et al. (2017)

Appendix 3.3: Meta-Analysis Results

Appendix Table 3.3.1

Meta-analysis results using leave1out() function repeatedly fitting the model leaving out one study at a time for depression outcome at post-program compared to inactive controls

#	d	z	p	CI_{lower}	CI_{upper}	Q	Q_p	tau^2	I^2	H^2
1	0.547	-12.196	0.000	0.497	0.603	675.800	0.000	0.280	83.885	6.205
2	0.547	-12.189	0.000	0.496	0.603	675.372	0.000	0.280	83.814	6.178
3	0.547	-12.219	0.000	0.496	0.602	674.883	0.000	0.280	83.838	6.187
4	0.548	-12.164	0.000	0.497	0.603	676.278	0.000	0.281	83.910	6.215
5	0.549	-12.134	0.000	0.498	0.604	675.752	0.000	0.281	83.922	6.220
6	0.549	-12.134	0.000	0.499	0.605	675.066	0.000	0.279	83.892	6.208
7	0.548	-12.167	0.000	0.497	0.603	676.245	0.000	0.281	83.901	6.212
8	0.548	-12.127	0.000	0.498	0.604	675.626	0.000	0.281	83.901	6.212
9	0.548	-12.147	0.000	0.497	0.604	676.252	0.000	0.281	83.907	6.214
10	0.549	-12.128	0.000	0.498	0.605	675.514	0.000	0.281	83.913	6.216
11	0.547	-12.198	0.000	0.497	0.603	675.714	0.000	0.280	83.878	6.203
12	0.549	-12.142	0.000	0.498	0.604	675.928	0.000	0.280	83.926	6.221
13	0.548	-12.146	0.000	0.497	0.604	676.295	0.000	0.281	83.729	6.146
14	0.563	-13.034	0.000	0.517	0.614	596.774	0.000	0.208	79.503	4.879
15	0.548	-12.157	0.000	0.497	0.604	676.292	0.000	0.281	83.918	6.218
16	0.550	-12.136	0.000	0.500	0.606	673.150	0.000	0.277	83.797	6.172
17	0.549	-12.149	0.000	0.498	0.604	675.938	0.000	0.280	83.921	6.219
18	0.547	-12.214	0.000	0.496	0.602	675.340	0.000	0.280	83.863	6.197
19	0.544	-12.492	0.000	0.495	0.599	654.247	0.000	0.269	83.259	5.973
20	0.547	-12.172	0.000	0.497	0.603	676.106	0.000	0.281	83.872	6.200
21	0.548	-12.133	0.000	0.497	0.604	675.990	0.000	0.281	83.834	6.186
22	0.547	-12.165	0.000	0.497	0.603	675.963	0.000	0.281	83.755	6.156
23	0.546	-12.278	0.000	0.496	0.602	674.337	0.000	0.278	83.802	6.174
24	0.548	-12.161	0.000	0.497	0.603	676.250	0.000	0.281	83.879	6.203
25	0.547	-12.247	0.000	0.496	0.602	675.216	0.000	0.279	83.850	6.192
26	0.548	-12.165	0.000	0.497	0.603	676.281	0.000	0.281	83.916	6.218
27	0.547	-12.227	0.000	0.496	0.602	674.690	0.000	0.279	83.833	6.185
28	0.547	-12.204	0.000	0.496	0.602	674.895	0.000	0.280	83.814	6.178
29	0.549	-12.129	0.000	0.498	0.605	675.385	0.000	0.280	83.913	6.216
30	0.552	-12.140	0.000	0.502	0.608	658.782	0.000	0.272	83.482	6.054
31	0.551	-12.116	0.000	0.500	0.607	666.436	0.000	0.276	83.679	6.127
32	0.548	-12.125	0.000	0.498	0.604	675.561	0.000	0.281	83.879	6.203
33	0.546	-12.312	0.000	0.496	0.601	673.405	0.000	0.276	83.746	6.152
34	0.545	-12.340	0.000	0.495	0.600	658.673	0.000	0.275	83.461	6.046
35	0.547	-12.197	0.000	0.496	0.603	675.671	0.000	0.280	83.873	6.201
36	0.547	-12.183	0.000	0.497	0.603	675.688	0.000	0.281	83.835	6.186
37	0.547	-12.200	0.000	0.496	0.603	675.243	0.000	0.280	83.836	6.187
38	0.548	-12.154	0.000	0.497	0.603	676.285	0.000	0.281	83.848	6.191
39	0.552	-12.139	0.000	0.502	0.608	655.348	0.000	0.272	83.455	6.044
40	0.545	-12.379	0.000	0.495	0.600	663.514	0.000	0.274	83.507	6.063
41	0.549	-12.130	0.000	0.498	0.605	675.263	0.000	0.280	83.908	6.214
42	0.549	-12.126	0.000	0.498	0.605	675.292	0.000	0.281	83.910	6.215
43	0.549	-12.113	0.000	0.498	0.605	674.099	0.000	0.280	83.878	6.203
44	0.547	-12.228	0.000	0.496	0.602	674.583	0.000	0.279	83.827	6.183
45	0.548	-12.131	0.000	0.498	0.604	675.767	0.000	0.281	83.915	6.217
46	0.547	-12.193	0.000	0.497	0.603	676.004	0.000	0.280	83.901	6.212

47	0.547	-12.158	0.000	0.497	0.603	676.163	0.000	0.281	83.746	6.152
48	0.549	-12.133	0.000	0.498	0.605	675.289	0.000	0.280	83.905	6.213
49	0.549	-12.110	0.000	0.498	0.605	673.801	0.000	0.281	83.827	6.183
50	0.553	-12.194	0.000	0.503	0.608	664.967	0.000	0.269	83.381	6.017
51	0.547	-12.233	0.000	0.496	0.602	675.317	0.000	0.279	83.862	6.197
52	0.550	-12.113	0.000	0.499	0.605	673.493	0.000	0.280	83.865	6.198
53	0.547	-12.214	0.000	0.496	0.602	672.898	0.000	0.280	83.688	6.130
54	0.547	-12.171	0.000	0.497	0.603	676.011	0.000	0.281	83.839	6.188
55	0.547	-12.183	0.000	0.497	0.603	675.994	0.000	0.280	83.887	6.206
56	0.549	-12.145	0.000	0.498	0.604	675.941	0.000	0.280	83.925	6.221
57	0.551	-12.130	0.000	0.501	0.607	668.613	0.000	0.275	83.669	6.123
58	0.548	-12.143	0.000	0.498	0.604	676.072	0.000	0.281	83.927	6.222
59	0.547	-12.185	0.000	0.497	0.603	676.049	0.000	0.280	83.899	6.211
60	0.548	-12.150	0.000	0.498	0.604	676.165	0.000	0.281	83.930	6.223
61	0.549	-12.126	0.000	0.499	0.605	674.576	0.000	0.279	83.883	6.205
62	0.551	-12.121	0.000	0.500	0.607	668.742	0.000	0.276	83.705	6.137
63	0.556	-12.363	0.000	0.507	0.610	647.217	0.000	0.253	82.496	5.713
64	0.549	-12.135	0.000	0.498	0.605	675.490	0.000	0.280	83.912	6.216
65	0.548	-12.163	0.000	0.497	0.604	676.275	0.000	0.280	83.930	6.223
66	0.549	-12.109	0.000	0.499	0.605	673.288	0.000	0.280	83.858	6.195
67	0.551	-12.135	0.000	0.500	0.606	672.380	0.000	0.277	83.769	6.161
68	0.546	-12.248	0.000	0.496	0.602	673.684	0.000	0.279	83.793	6.170
69	0.547	-12.164	0.000	0.497	0.603	675.689	0.000	0.281	83.580	6.090
70	0.547	-12.175	0.000	0.497	0.603	676.121	0.000	0.281	83.891	6.208
71	0.550	-12.119	0.000	0.499	0.606	673.450	0.000	0.279	83.852	6.193
72	0.547	-12.208	0.000	0.496	0.602	673.973	0.000	0.280	83.745	6.152
73	0.546	-12.245	0.000	0.496	0.602	666.990	0.000	0.279	83.518	6.067
74	0.551	-12.112	0.000	0.500	0.607	663.630	0.000	0.276	83.655	6.118
75	0.547	-12.243	0.000	0.496	0.602	674.878	0.000	0.279	83.841	6.189
76	0.556	-12.353	0.000	0.506	0.610	649.709	0.000	0.254	82.587	5.743
77	0.546	-12.279	0.000	0.496	0.601	667.151	0.000	0.278	83.603	6.099
78	0.546	-12.293	0.000	0.496	0.601	673.468	0.000	0.277	83.766	6.160
79	0.547	-12.182	0.000	0.497	0.603	675.958	0.000	0.281	83.878	6.203
80	0.546	-12.279	0.000	0.496	0.602	674.222	0.000	0.278	83.798	6.172
81	0.549	-12.142	0.000	0.498	0.604	675.854	0.000	0.280	83.923	6.220
82	0.547	-12.199	0.000	0.496	0.603	675.515	0.000	0.280	83.861	6.196
83	0.546	-12.339	0.000	0.496	0.601	672.112	0.000	0.275	83.688	6.130
84	0.548	-12.152	0.000	0.497	0.604	676.282	0.000	0.281	83.910	6.215
85	0.546	-12.306	0.000	0.495	0.601	661.081	0.000	0.277	83.489	6.056
86	0.546	-12.231	0.000	0.496	0.602	664.658	0.000	0.279	83.326	5.997
87	0.548	-12.149	0.000	0.497	0.604	676.291	0.000	0.281	83.873	6.201
88	0.552	-12.158	0.000	0.502	0.608	665.475	0.000	0.272	83.501	6.061
89	0.550	-12.112	0.000	0.499	0.606	672.920	0.000	0.279	83.850	6.192
90	0.548	-12.123	0.000	0.498	0.604	675.423	0.000	0.281	83.858	6.195
91	0.549	-12.152	0.000	0.498	0.605	675.701	0.000	0.279	83.900	6.211
92	0.552	-12.144	0.000	0.502	0.608	663.157	0.000	0.272	83.519	6.068
93	0.550	-12.111	0.000	0.499	0.606	672.735	0.000	0.279	83.845	6.190
94	0.551	-12.124	0.000	0.501	0.607	666.878	0.000	0.275	83.656	6.119
95	0.548	-12.186	0.000	0.497	0.603	676.200	0.000	0.280	83.916	6.218
96	0.550	-12.129	0.000	0.499	0.606	674.241	0.000	0.279	83.862	6.196
97	0.547	-12.228	0.000	0.496	0.602	674.648	0.000	0.279	83.830	6.184
98	0.548	-12.192	0.000	0.497	0.603	676.239	0.000	0.280	83.914	6.217
99	0.547	-12.183	0.000	0.497	0.603	675.826	0.000	0.281	83.858	6.195
100	0.548	-12.150	0.000	0.498	0.604	676.187	0.000	0.281	83.930	6.223

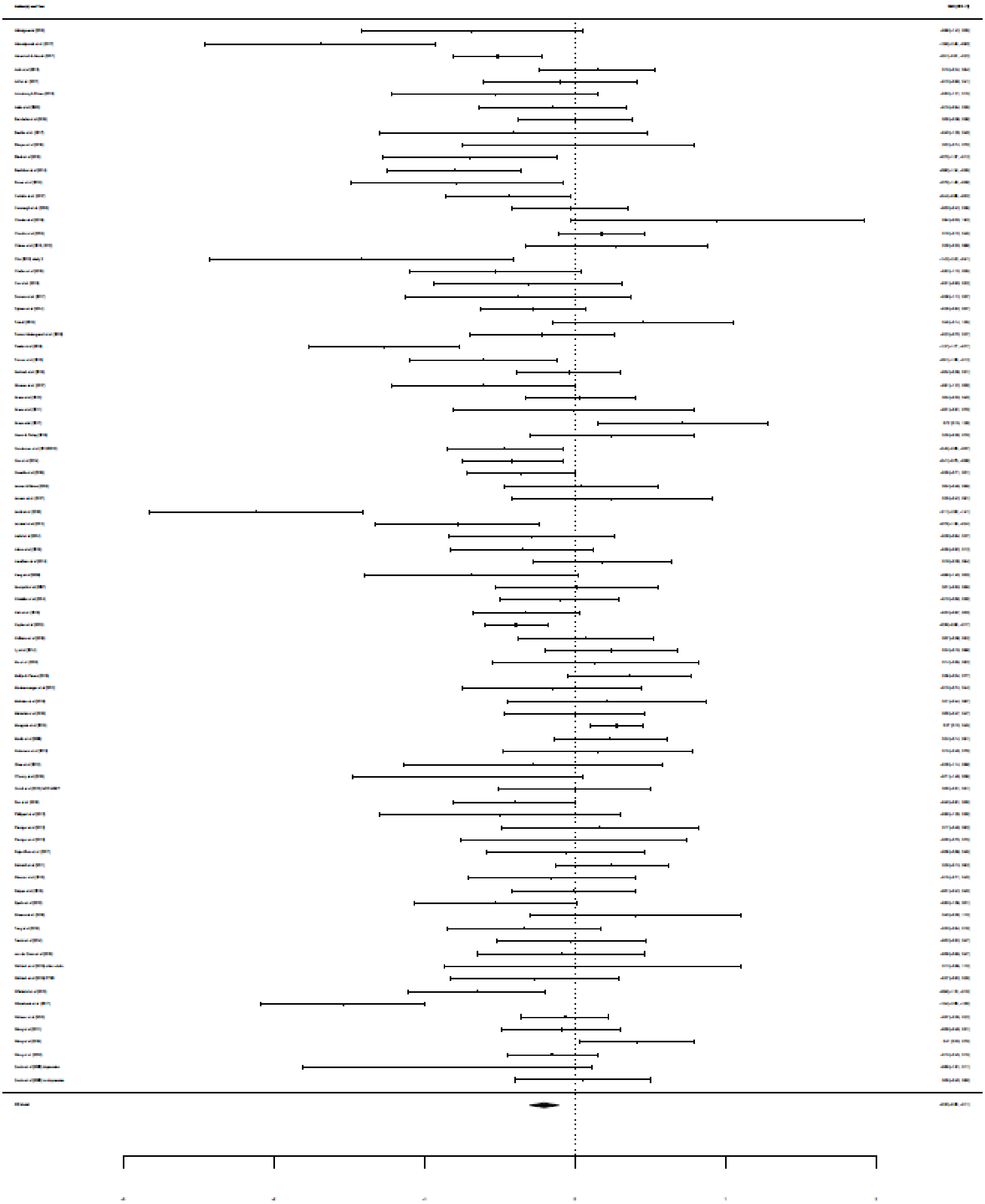
101	0.548	-12.161	0.000	0.497	0.604	676.255	0.000	0.280	83.930	6.223
102	0.548	-12.145	0.000	0.497	0.604	676.275	0.000	0.281	83.829	6.184
103	0.546	-12.295	0.000	0.496	0.601	671.754	0.000	0.277	83.721	6.143
104	0.550	-12.101	0.000	0.500	0.606	667.344	0.000	0.278	83.746	6.152
105	0.548	-12.132	0.000	0.497	0.604	675.927	0.000	0.281	83.883	6.205
106	0.548	-12.147	0.000	0.498	0.604	676.075	0.000	0.280	83.929	6.222
107	0.551	-12.147	0.000	0.501	0.607	669.800	0.000	0.274	83.647	6.115
108	0.550	-12.101	0.000	0.499	0.606	668.454	0.000	0.279	83.765	6.160
109	0.546	-12.235	0.000	0.496	0.602	673.203	0.000	0.279	83.763	6.159
110	0.556	-12.350	0.000	0.506	0.610	648.719	0.000	0.254	82.565	5.735
111	0.546	-12.306	0.000	0.496	0.601	672.841	0.000	0.277	83.738	6.149
112	0.548	-12.175	0.000	0.497	0.603	676.238	0.000	0.280	83.917	6.218
113	0.546	-12.246	0.000	0.496	0.602	672.532	0.000	0.279	83.746	6.152
114	0.547	-12.202	0.000	0.497	0.603	676.083	0.000	0.280	83.906	6.213
115	0.550	-12.135	0.000	0.499	0.606	673.631	0.000	0.278	83.821	6.181
116	0.550	-12.119	0.000	0.500	0.606	671.973	0.000	0.278	83.800	6.173
117	0.548	-12.158	0.000	0.497	0.604	676.292	0.000	0.281	83.904	6.213
118	0.547	-12.210	0.000	0.496	0.603	675.612	0.000	0.280	83.879	6.203
119	0.547	-12.251	0.000	0.496	0.602	675.064	0.000	0.278	83.844	6.190
120	0.549	-12.135	0.000	0.498	0.605	675.735	0.000	0.281	83.922	6.220
121	0.548	-12.151	0.000	0.497	0.604	676.287	0.000	0.281	83.895	6.209
122	0.549	-12.123	0.000	0.499	0.605	674.572	0.000	0.280	83.887	6.206
123	0.553	-12.178	0.000	0.502	0.608	663.728	0.000	0.270	83.400	6.024
124	0.552	-12.165	0.000	0.502	0.608	667.194	0.000	0.272	83.520	6.068
125	0.548	-12.145	0.000	0.497	0.604	676.280	0.000	0.281	83.809	6.176
126	0.548	-12.157	0.000	0.497	0.604	676.293	0.000	0.281	83.899	6.211
127	0.545	-12.359	0.000	0.496	0.601	670.982	0.000	0.275	83.641	6.113
128	0.548	-12.143	0.000	0.497	0.604	676.151	0.000	0.281	83.919	6.219
129	0.549	-12.115	0.000	0.498	0.604	674.443	0.000	0.281	83.773	6.163
130	0.550	-12.127	0.000	0.500	0.606	672.323	0.000	0.277	83.786	6.168
131	0.549	-12.120	0.000	0.498	0.605	674.983	0.000	0.281	83.898	6.210
132	0.546	-12.266	0.000	0.496	0.601	671.264	0.000	0.278	83.713	6.140
133	0.549	-12.115	0.000	0.498	0.605	674.548	0.000	0.281	83.840	6.188
134	0.547	-12.229	0.000	0.496	0.602	675.435	0.000	0.279	83.869	6.199
135	0.548	-12.140	0.000	0.497	0.604	676.121	0.000	0.281	83.909	6.215
136	0.547	-12.222	0.000	0.496	0.602	675.085	0.000	0.279	83.852	6.193
137	0.547	-12.179	0.000	0.497	0.603	675.965	0.000	0.281	83.869	6.199
138	0.550	-12.142	0.000	0.499	0.606	674.322	0.000	0.278	83.839	6.188
139	0.547	-12.251	0.000	0.497	0.602	675.364	0.000	0.278	83.851	6.192
140	0.547	-12.228	0.000	0.496	0.602	675.383	0.000	0.279	83.867	6.198
141	0.544	-12.490	0.000	0.495	0.599	648.707	0.000	0.269	83.217	5.958
142	0.546	-12.319	0.000	0.496	0.601	673.037	0.000	0.276	83.730	6.146
143	0.550	-12.101	0.000	0.499	0.606	670.282	0.000	0.280	83.796	6.171
144	0.555	-12.253	0.000	0.505	0.610	649.426	0.000	0.261	82.937	5.861
145	0.546	-12.245	0.000	0.496	0.602	668.918	0.000	0.279	83.598	6.097
146	0.552	-12.131	0.000	0.501	0.607	662.823	0.000	0.274	83.566	6.085
147	0.546	-12.323	0.000	0.496	0.601	672.299	0.000	0.276	83.709	6.138
148	0.551	-12.127	0.000	0.501	0.607	666.245	0.000	0.275	83.634	6.110
149	0.553	-12.154	0.000	0.502	0.608	660.543	0.000	0.271	83.444	6.040

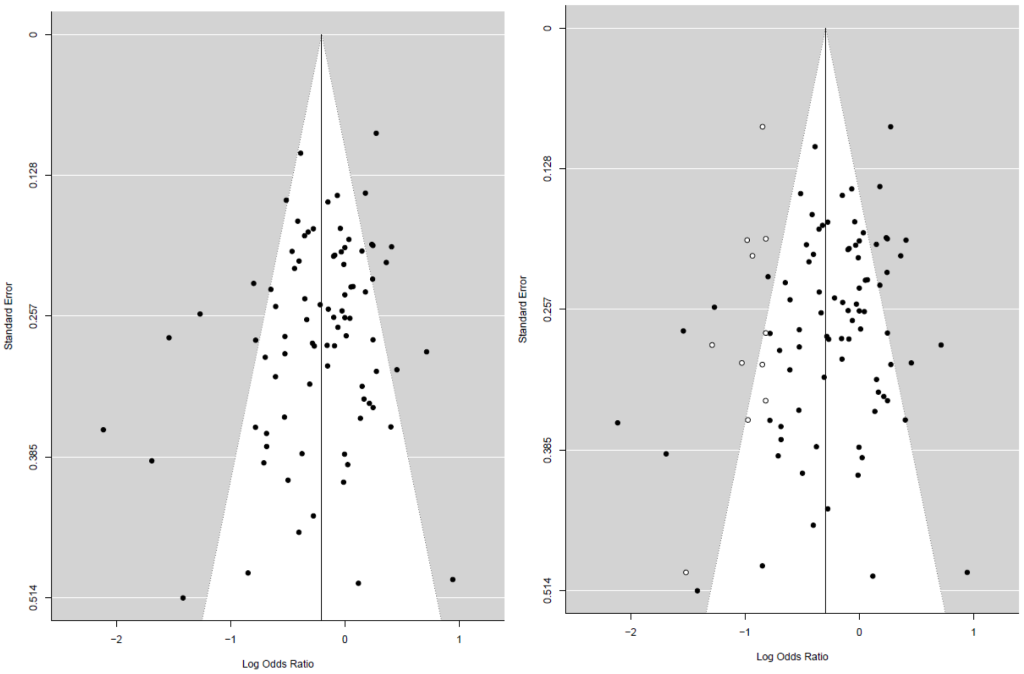
#=Number of included study (in alphabetical order); d=effect size; z=standardized coefficient; p=significance level; CI= confidence intervals; Q=between-study heterogeneity; Q_p=Q significance level; tau^2= variance of the underlying true effect sizes; I^2=percentage of between-study heterogeneity; H^2=heterogeneity statistic.

Appendix Figure 3.3.2

Forest and funnel plots depression compared to active controls, post-program

Top: Forest plot; bottom: funnel plot (left), trim-and-fill funnel plot (right)



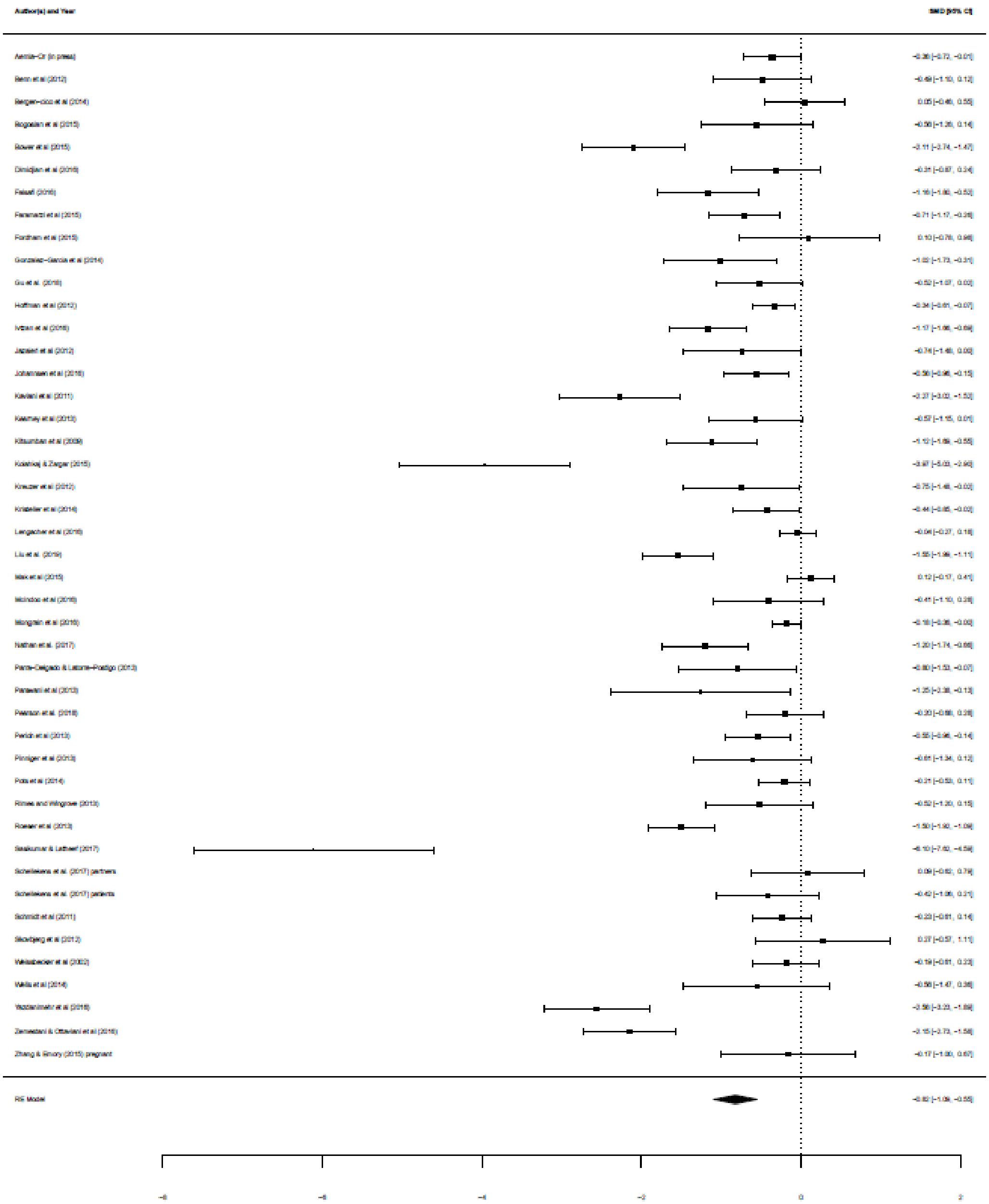


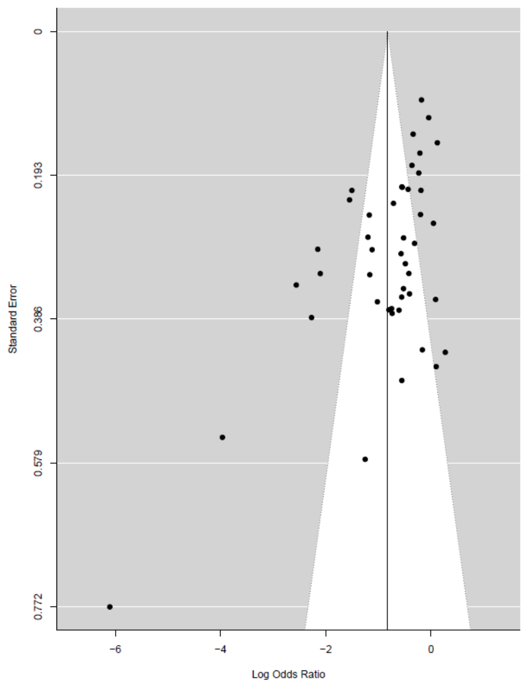
0
0.128
0.257
0.385
0.514
Standard Error
−2
−1
0
1
Log Odds Ratio
0
0.128
0.257
0.385
0.514
Standard Error
−2
−1
0
1
Log Odds Ratio

Appendix Figure 3.3.3

Forest and funnel plots depression compared to inactive controls, 1-4 months follow-up

Top: Forest plot; bottom: funnel plot (left), trim-and-fill funnel plot (right)





Standard Error
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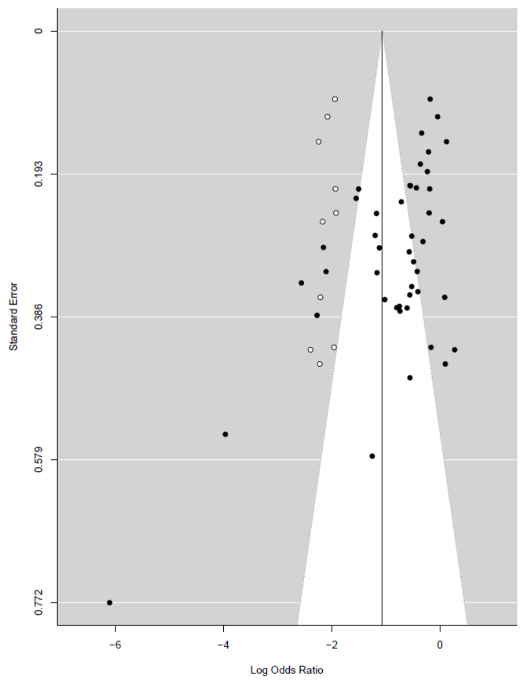
0.193
0.386
0.579
0.772
−6
−4
−2
0
Log Odds Ratio

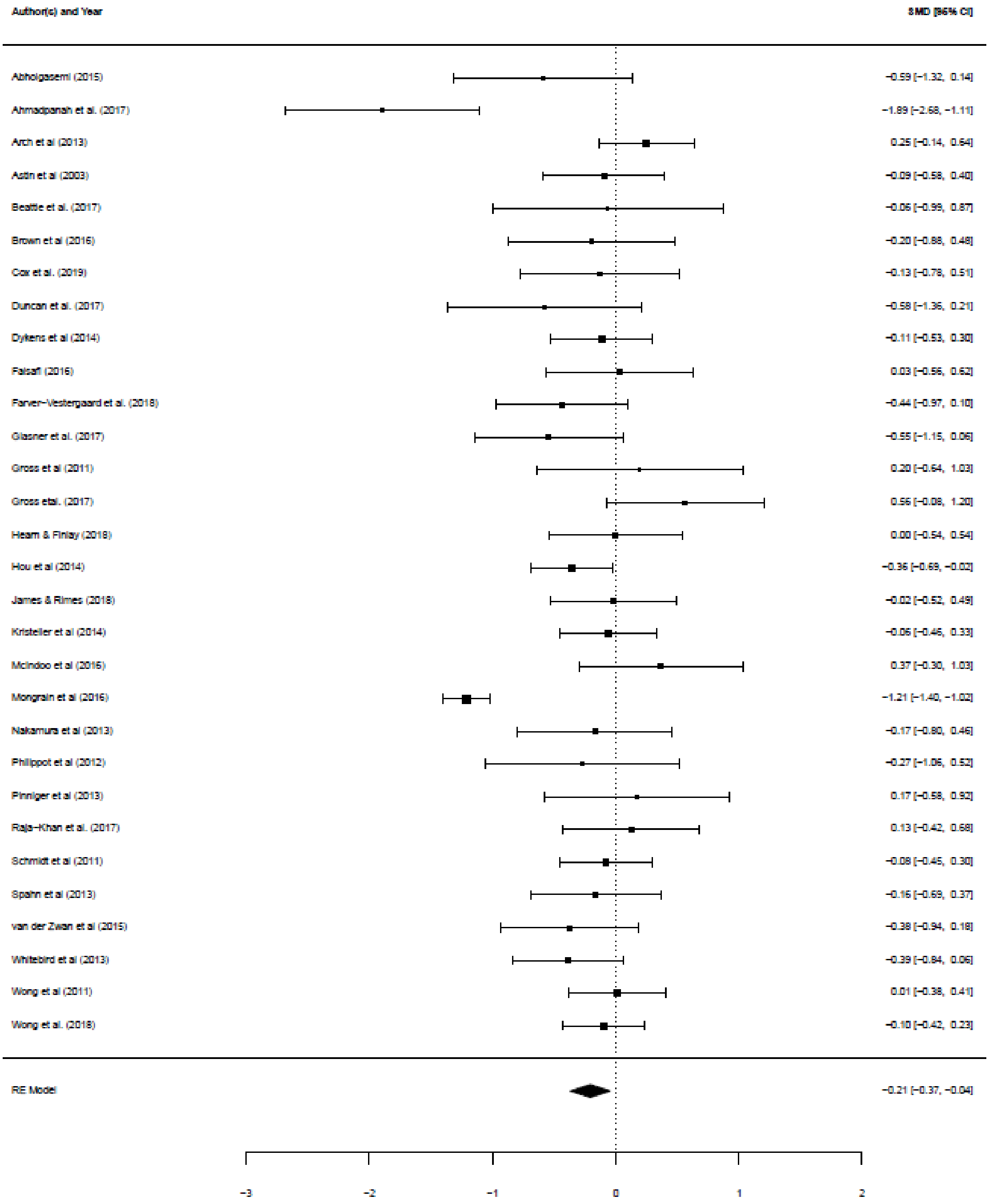


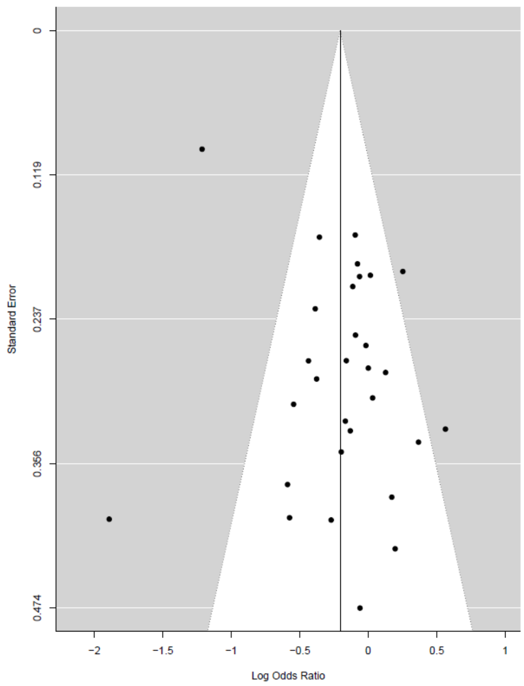
Standard Error
0
0.193
0.386
0.579
0.772
−6
−4
−2
0
Log Odds Ratio

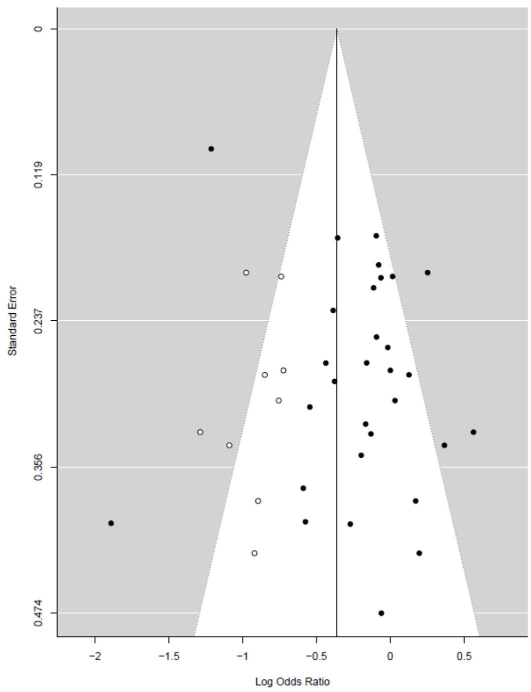
Appendix Figure 3.3.4

Forest and funnel plots depression compared to active controls, 1-4 months follow-up

Top: Forest plot; bottom: funnel plot (left), trim-and-fill funnel plot (right)



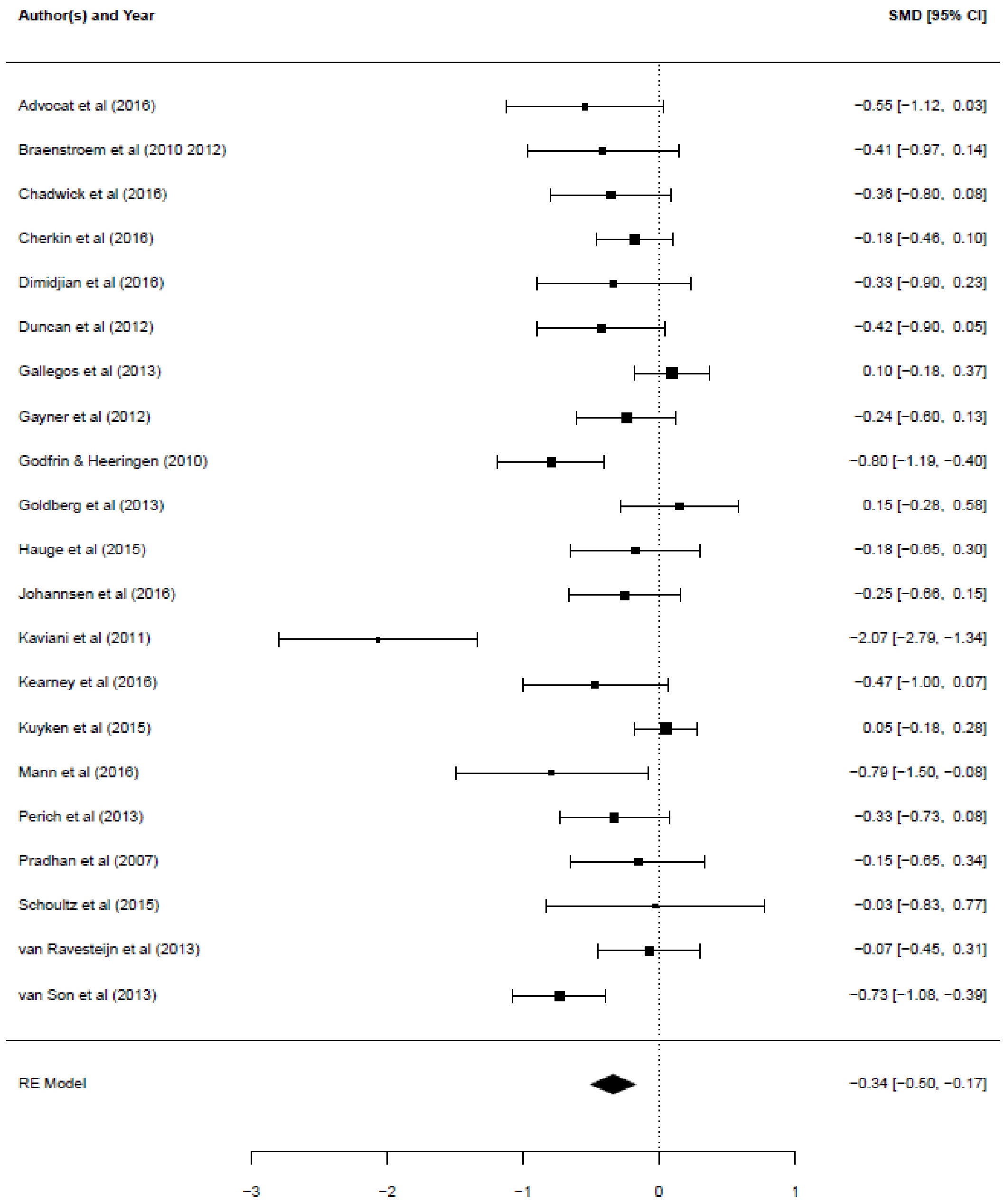

Standard Error
0
0.119
0.237
0.356
0.474
-2
-1.5
-1
-0.5
0
0.5
1
Log Odds Ratio

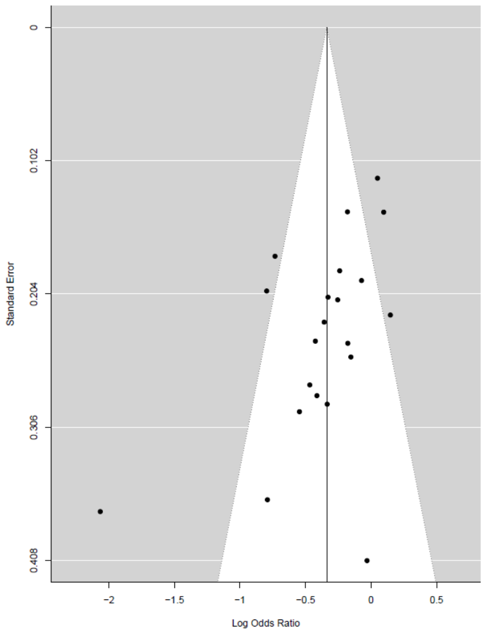

Standard Error
0
0.119
0.237
0.356
0.474
-2
-1.5
-1
-0.5
0
0.5
Log Odds Ratio

Appendix Figure 3.3.5

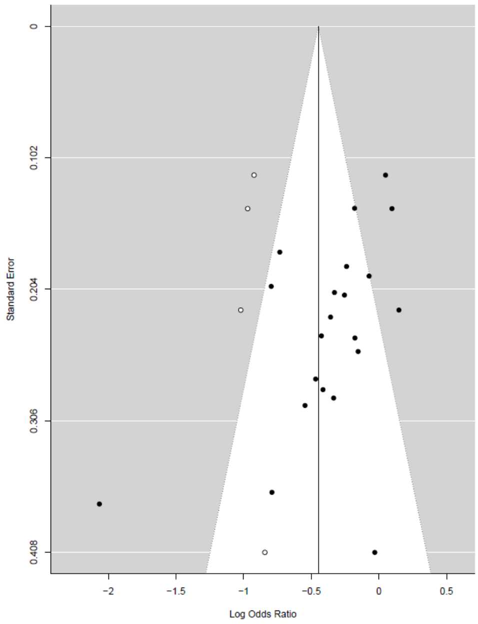
Forest and funnel plots depression compared to inactive controls, 5-10 months follow-up

Top: Forest plot; bottom: funnel plot (left), trim-and-fill funnel plot (right)





Standard Error
0
0.102
0.204
0.306
0.408
−2
−1.5
−1
−0.5
0
0.5
Log Odds Ratio

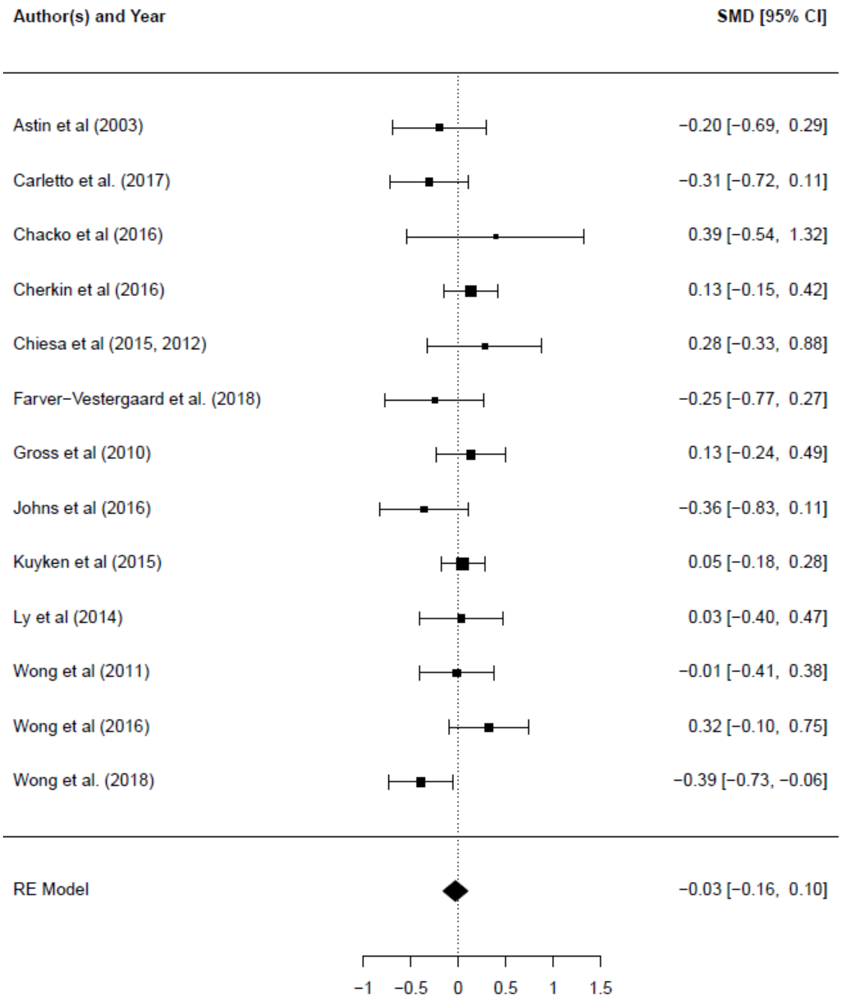


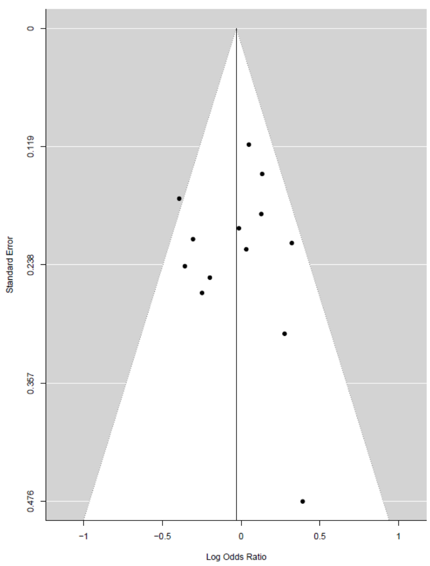
Standard Error
0
0.102
0.204
0.306
0.408
−2
−1.5
−1
−0.5
0
0.5
Log Odds Ratio

Appendix Figure 3.3.6

Forest and funnel plots depression compared to active controls, 5-10 months follow-up

Top: Forest plot; bottom: funnel plot

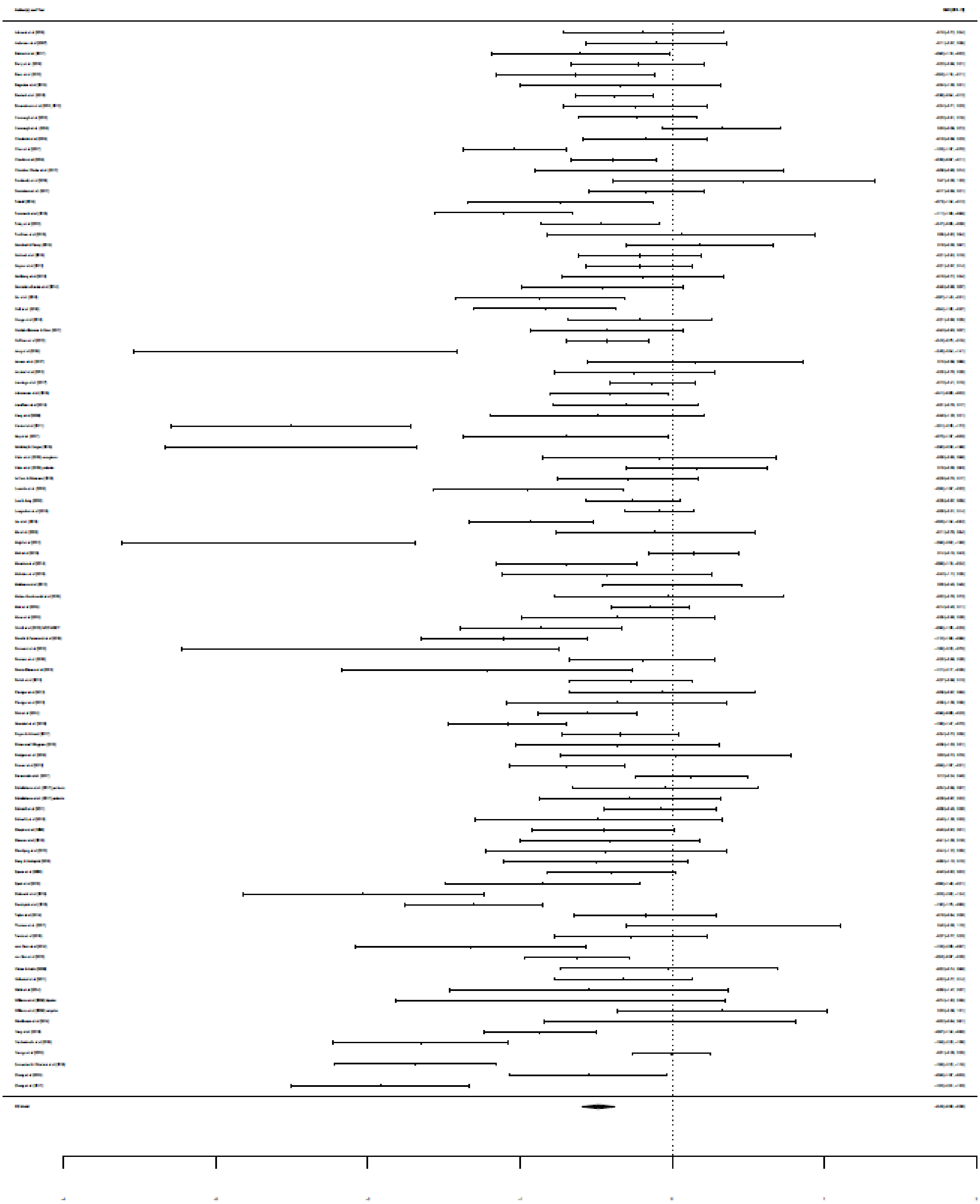


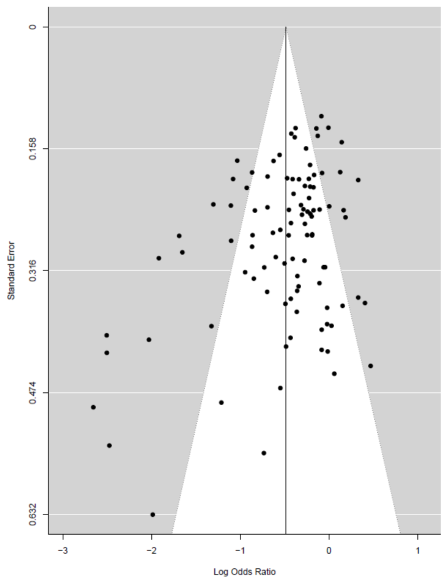


Appendix Figure 3.3.7

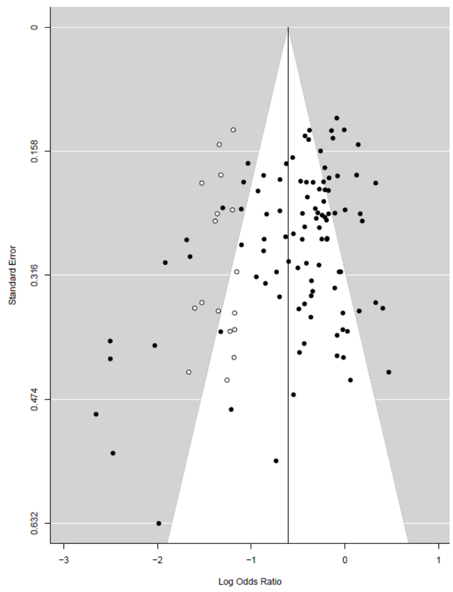
Forest and funnel plots anxiety compared to inactive controls, post-program

Top: Forest plot; bottom: funnel plot (left), trim-and-fill funnel plot (right)





Standard Error
0
0.158
0.316
0.474
0.632
−3
−2
−1
0
1
Log Odds Ratio

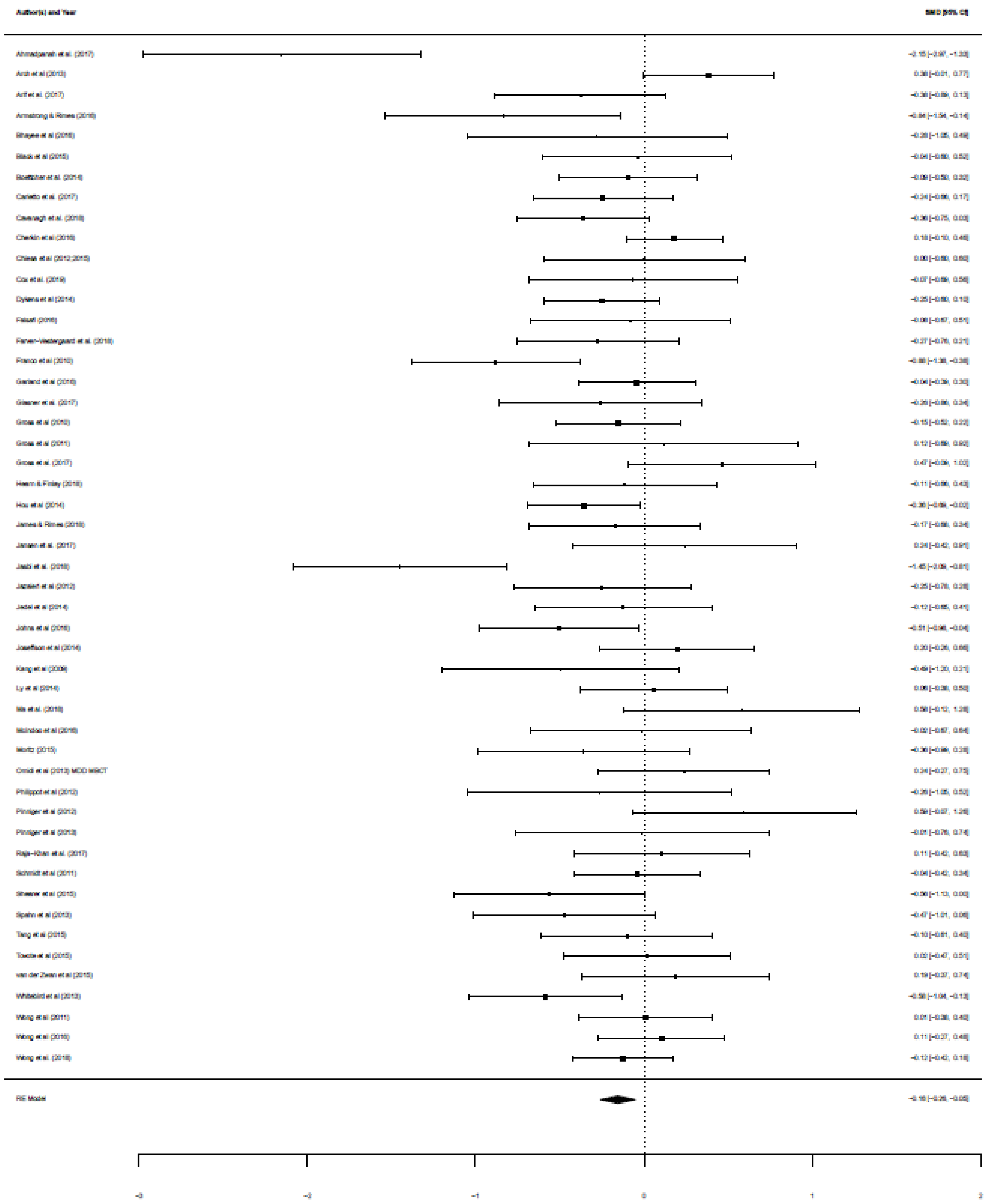


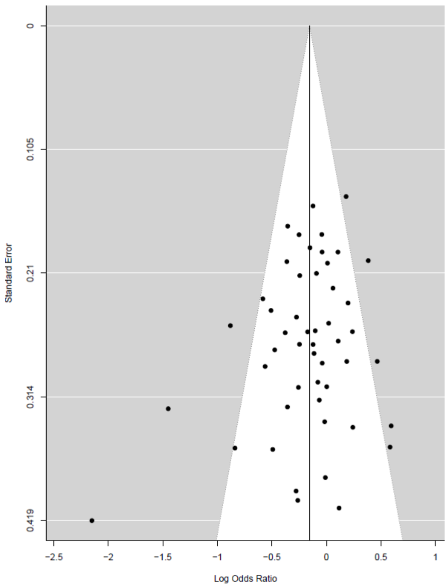
Standard Error
0
0.158
0.316
0.474
0.632
−3
−2
−1
0
1
Log Odds Ratio

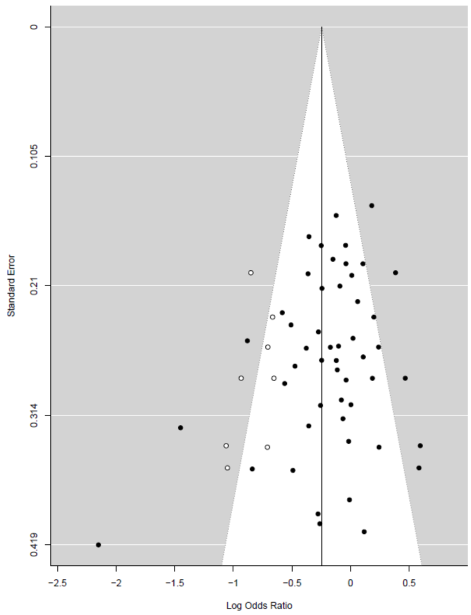
Appendix Figure 3.3.8

Forest and funnel plots anxiety compared to active controls, post-program

Top: Forest plot; bottom: funnel plot (left), trim-and-fill funnel plot (right)



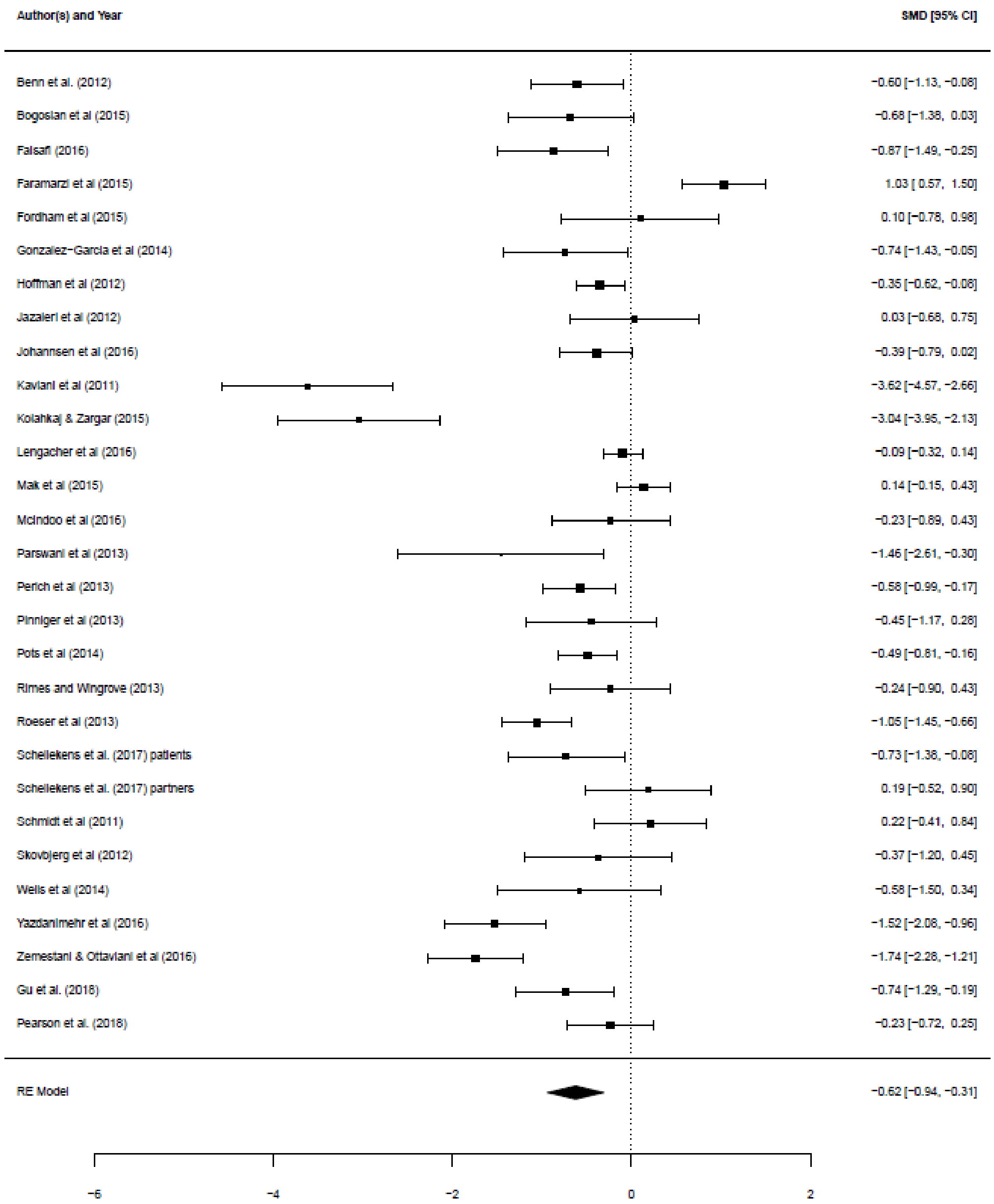

Standard Error
0
0.105
0.21
0.314
0.419
-2.5
-2
-1.5
-1
-0.5
0
0.5
1
Log Odds Ratio

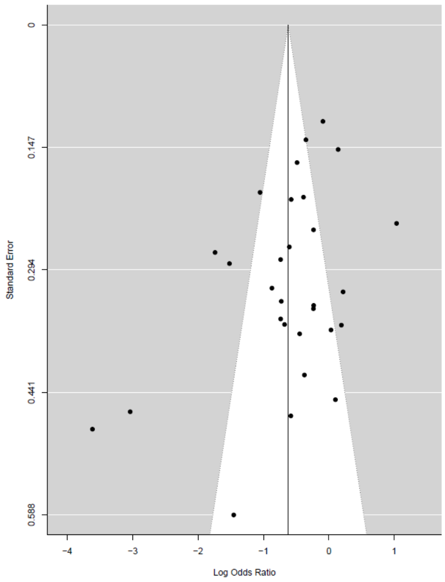

Standard Error
0
0.105
0.21
0.314
0.419
-2.5
-2
-1.5
-1
-0.5
0
0.5
Log Odds Ratio

Appendix Figure 3.3.9

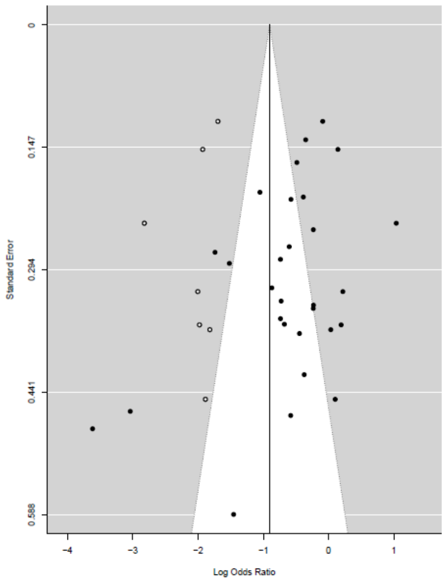
Forest and funnel plots anxiety compared to inactive controls, 1-4 months follow-up

Top: Forest plot; bottom: funnel plot (left), trim-and-fill funnel plot (right)





0
0.147
0.294
0.441
0.588
Standard Error
−4
−3
−2
−1
0
1
Log Odds Ratio

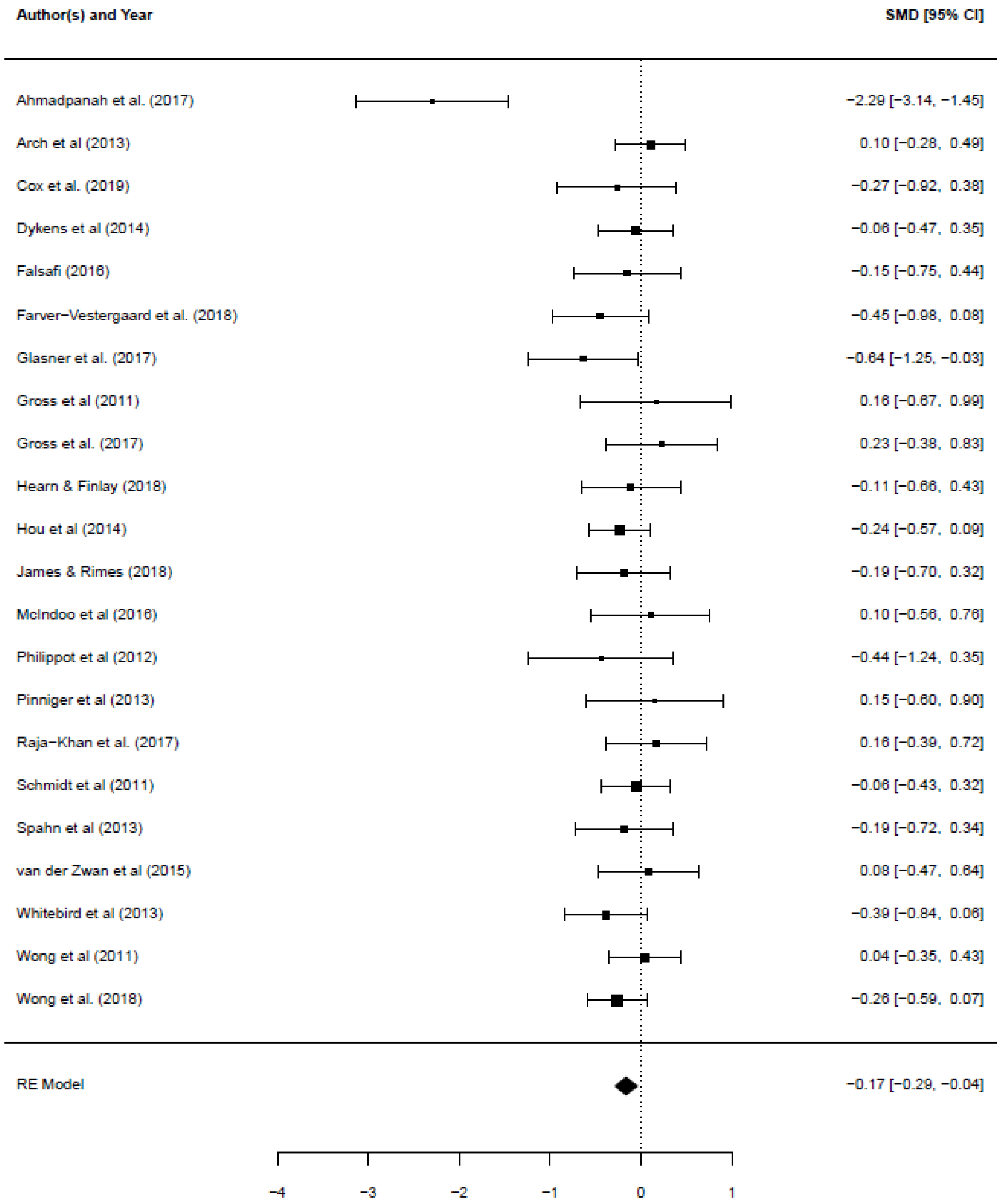


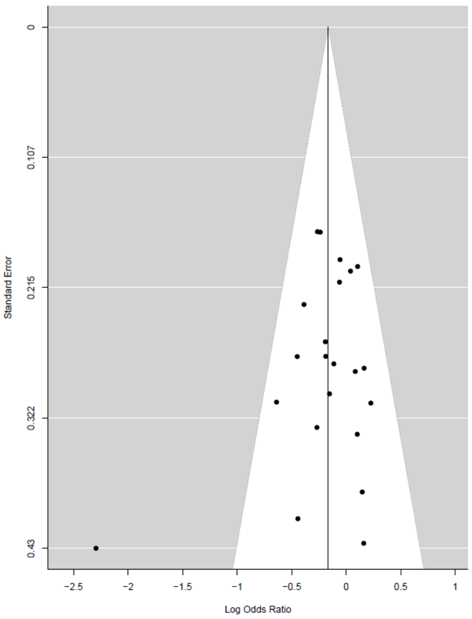
0
0.147
0.294
0.441
0.588
Standard Error
−4
−3
−2
−1
0
1
Log Odds Ratio

Appendix Figure 3.3.10

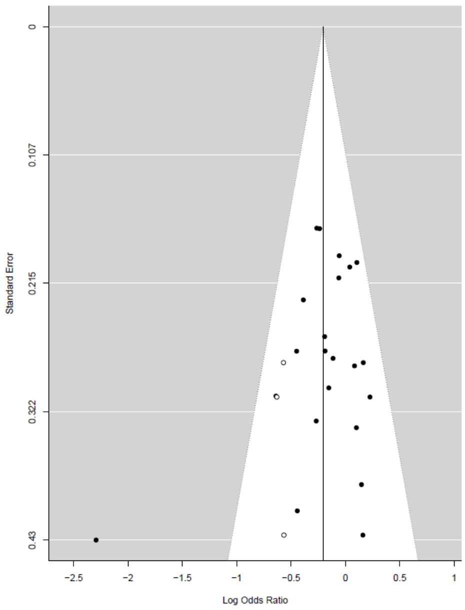
Forest and funnel plots anxiety compared to active controls, 1-4 months follow-up

Top: Forest plot; bottom: funnel plot (left), trim-and-fill funnel plot (right)





0
0.107
0.215
0.322
0.43
Standard Error
-2.5
-2
-1.5
-1
-0.5
0
0.5
1
Log Odds Ratio

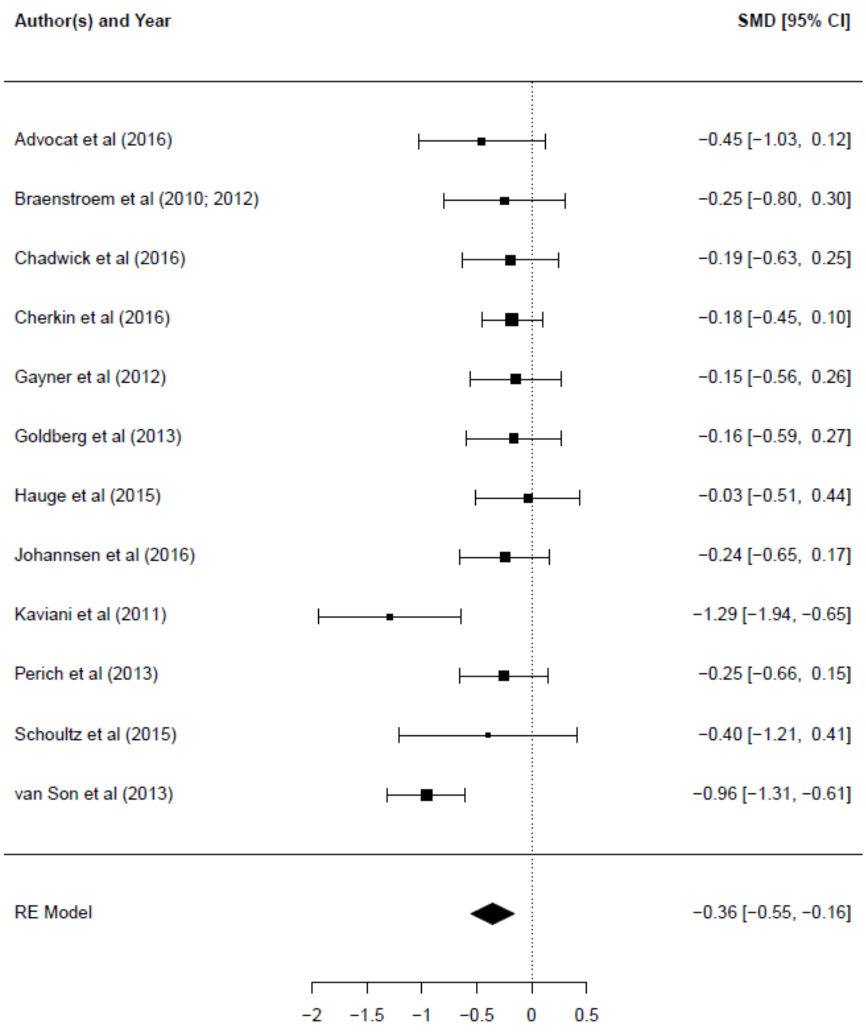


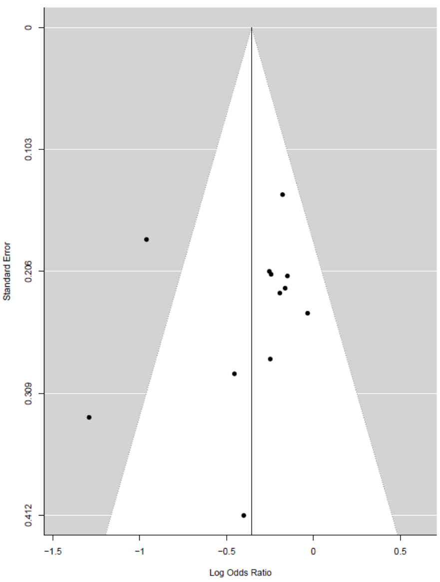
0
0.107
0.215
0.322
0.43
Standard Error
-2.5
-2
-1.5
-1
-0.5
0
0.5
1
Log Odds Ratio

Appendix Figure 3.3.11

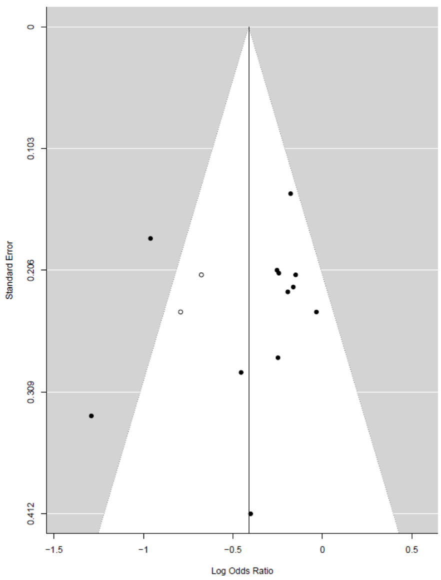
Forest and funnel plots anxiety compared to inactive controls, 5-10 months follow-up

Top: Forest plot; bottom: funnel plot (left), trim-and-fill funnel plot (right)





0
0.103
0.206
0.309
0.412
Standard Error
-1.5
-1
-0.5
0
0.5
Log Odds Ratio

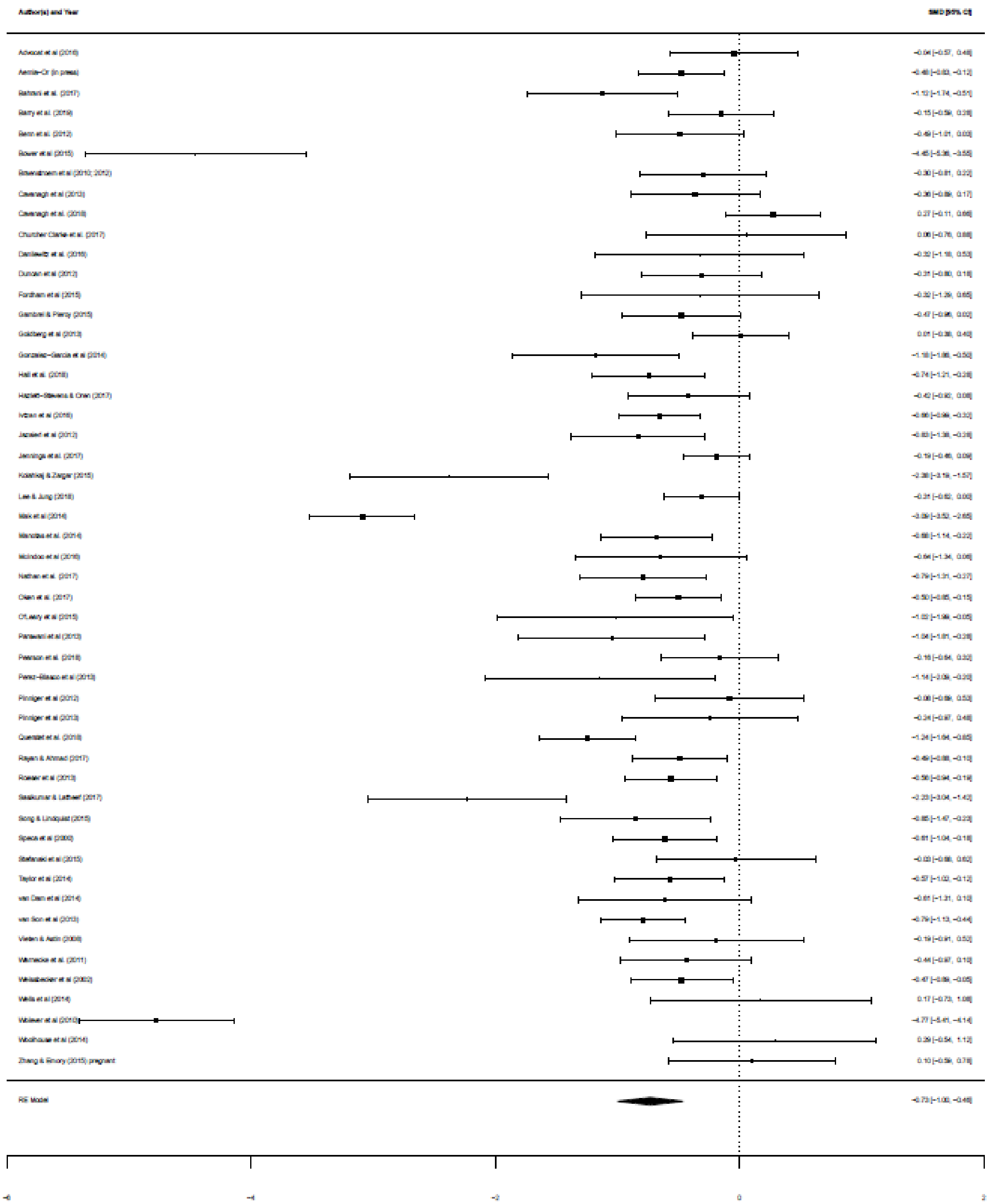


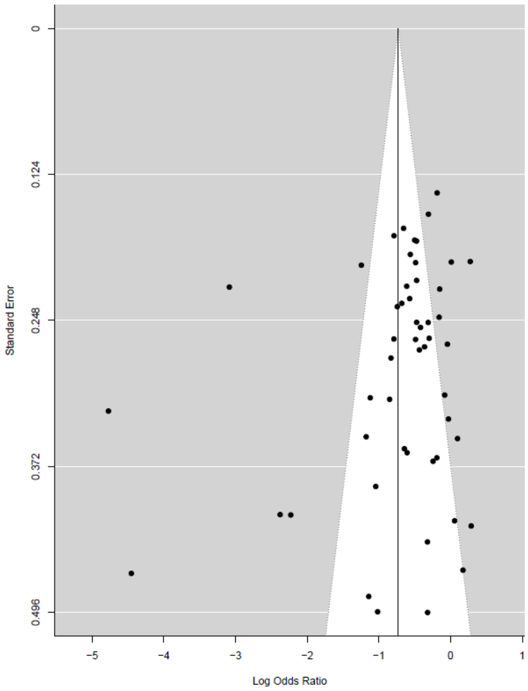
0
0.103
0.206
0.309
0.412
Standard Error
-1.5
-1
-0.5
0
0.5
Log Odds Ratio

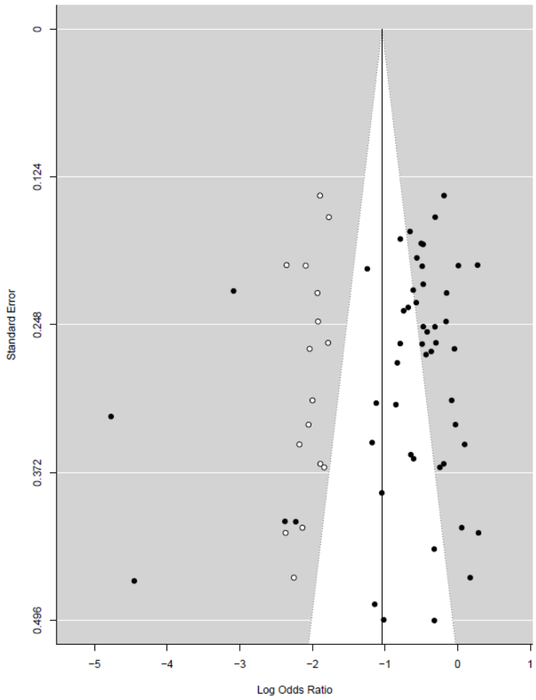
Appendix Figure 3.3.12

Forest and funnel plots stress compared to inactive controls, post-program

Top: Forest plot; bottom: funnel plot (left), trim-and-fill funnel plot (right)




Standard Error
0
0.124
0.248
0.372
0.496
−5
−4
−3
−2
−1
0
1
Log Odds Ratio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0.248
0.372
0.496
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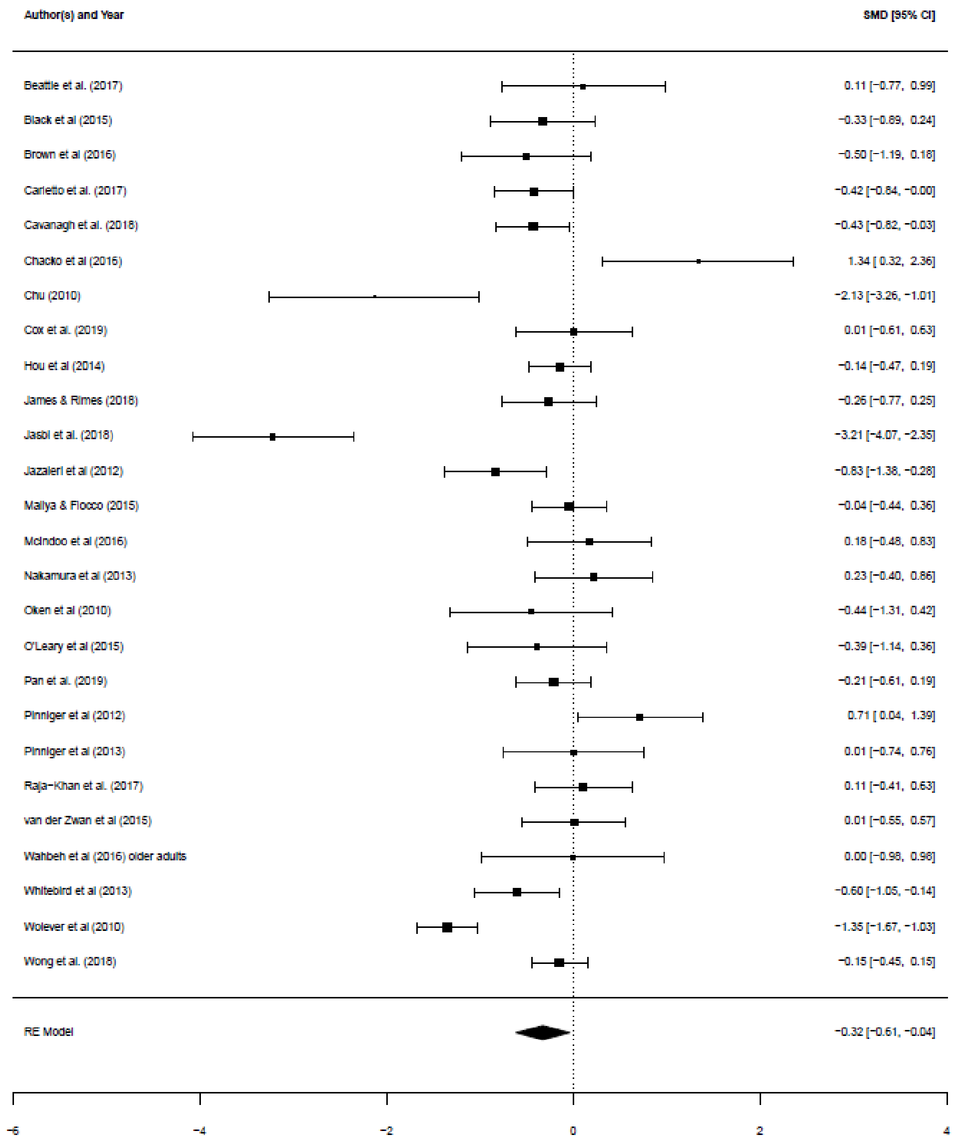
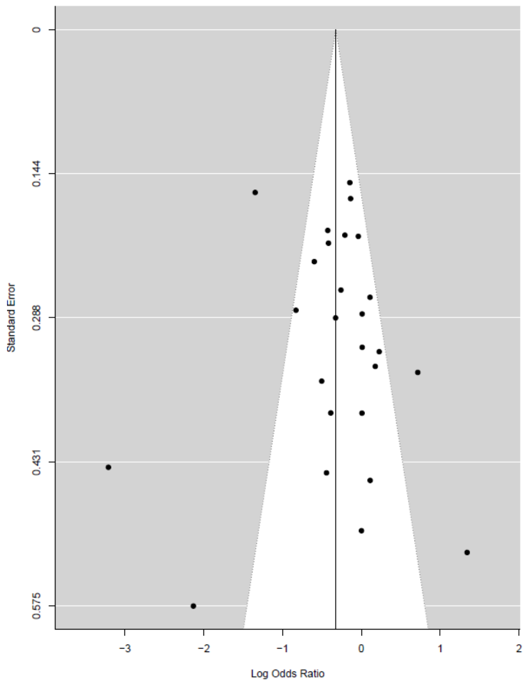
−4
−3
−2
−1
0
1
Log Odds Ratio

Appendix Figure 3.3.13

Forest and funnel plots stress compared to active controls, post-program

Top: Forest plot; bottom: funnel plot (left), trim-and-fill funnel plot (right)





Standard Error
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0.431
0.575
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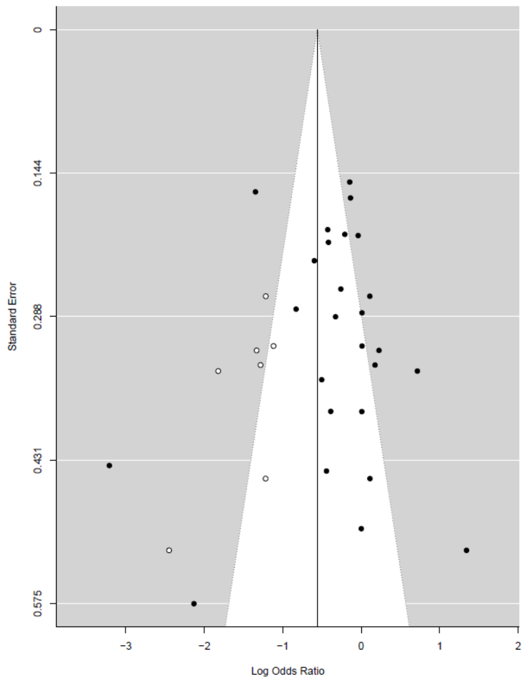
−2
−1
0
1
2
Log Odds Ratio



Standard Error
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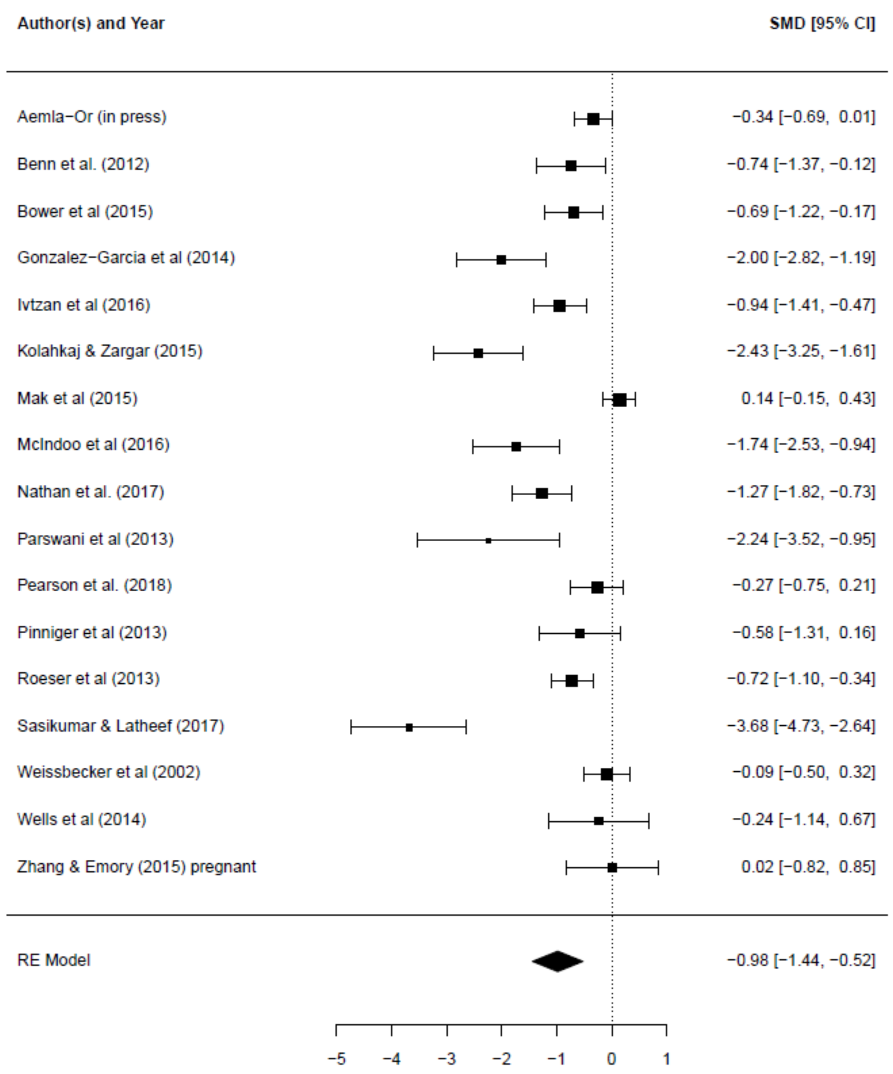
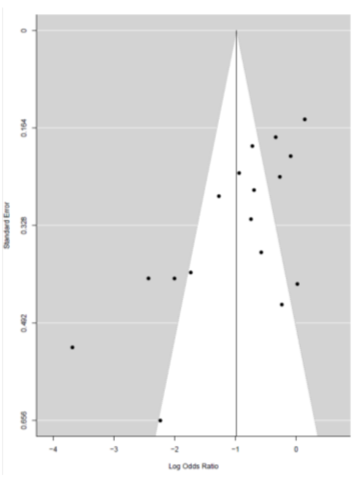
0.288
0.431
0.575
−3
−2
−1
0
1
2
Log Odds Ratio

Appendix Figure 3.3.14

Forest and funnel plots stress compared to inactive controls, 1-4 months follow-up

Top: Forest plot; bottom: funnel plot

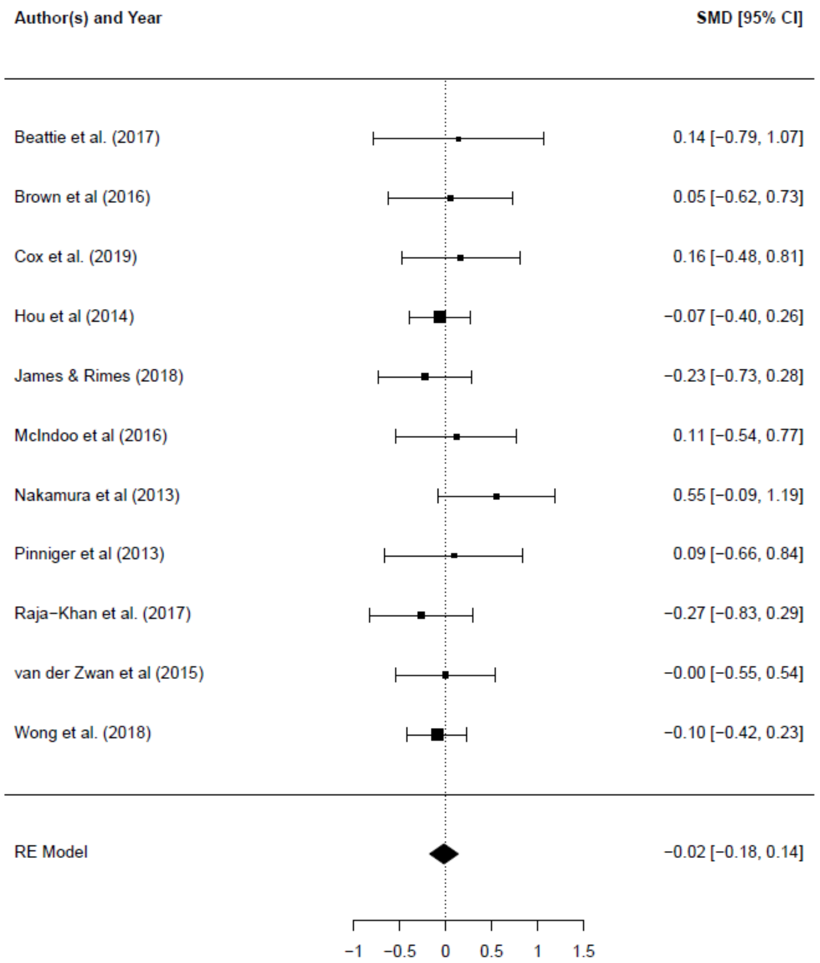


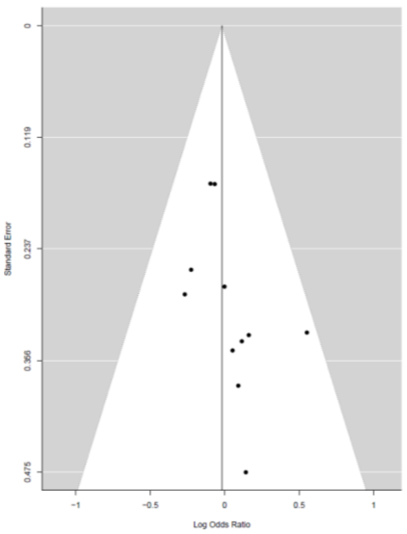


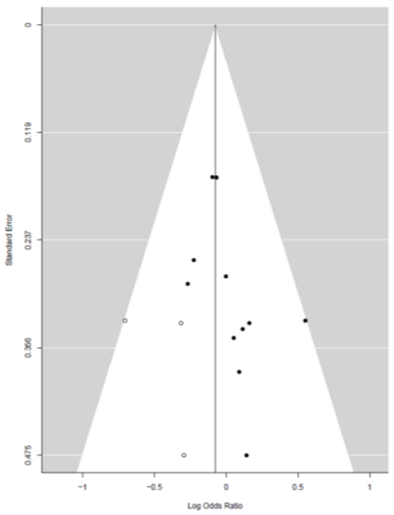
Appendix Figure 3.3.15

Forest and funnel plots stress compared to active controls, 1-4 months follow-up

Top: Forest plot; bottom: funnel plot (left), trim-and-fill funnel plot (right)



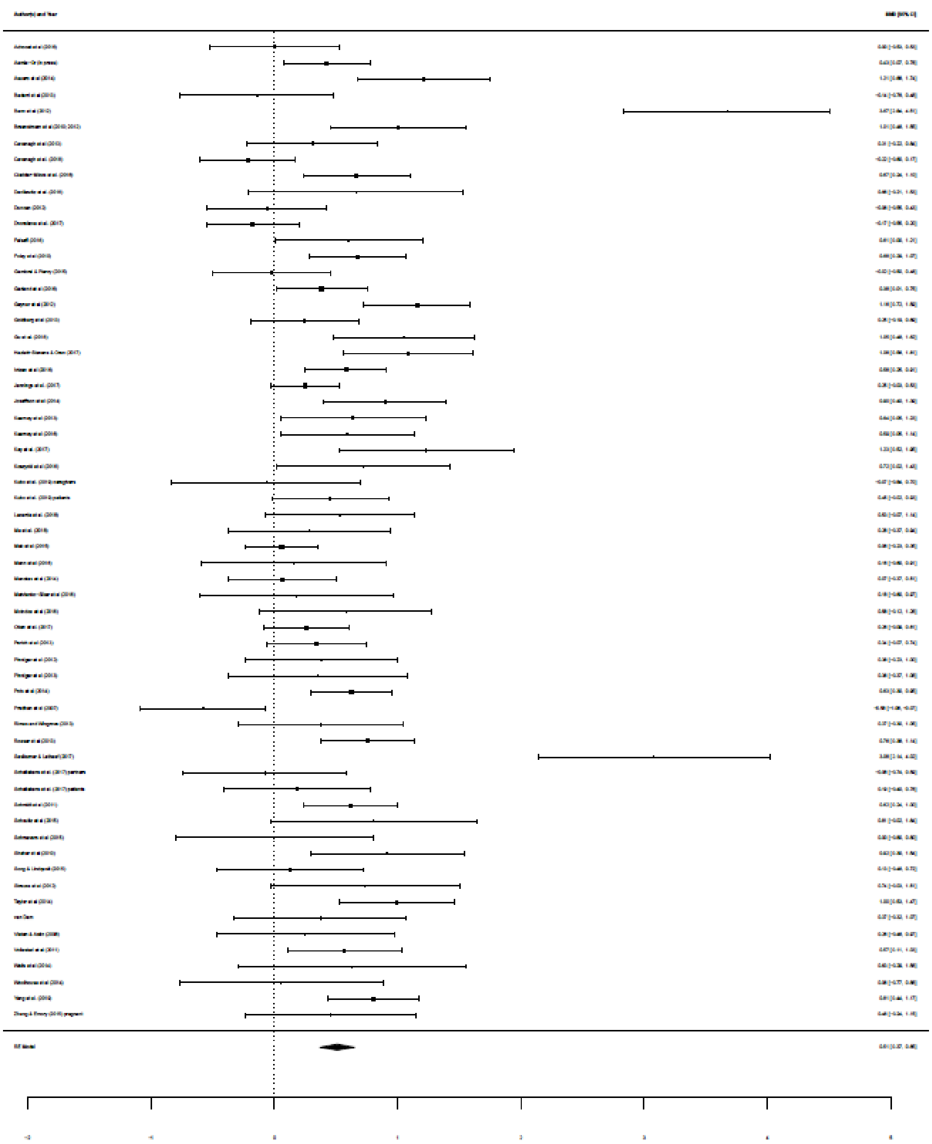


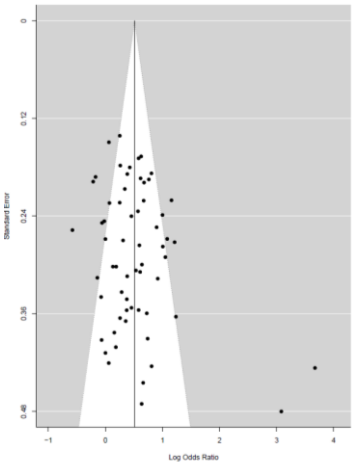


Appendix Figure 3.3.16

Forest and funnel plots mindfulness compared to inactive controls, post-program

Top: Forest plot; bottom: funnel plot

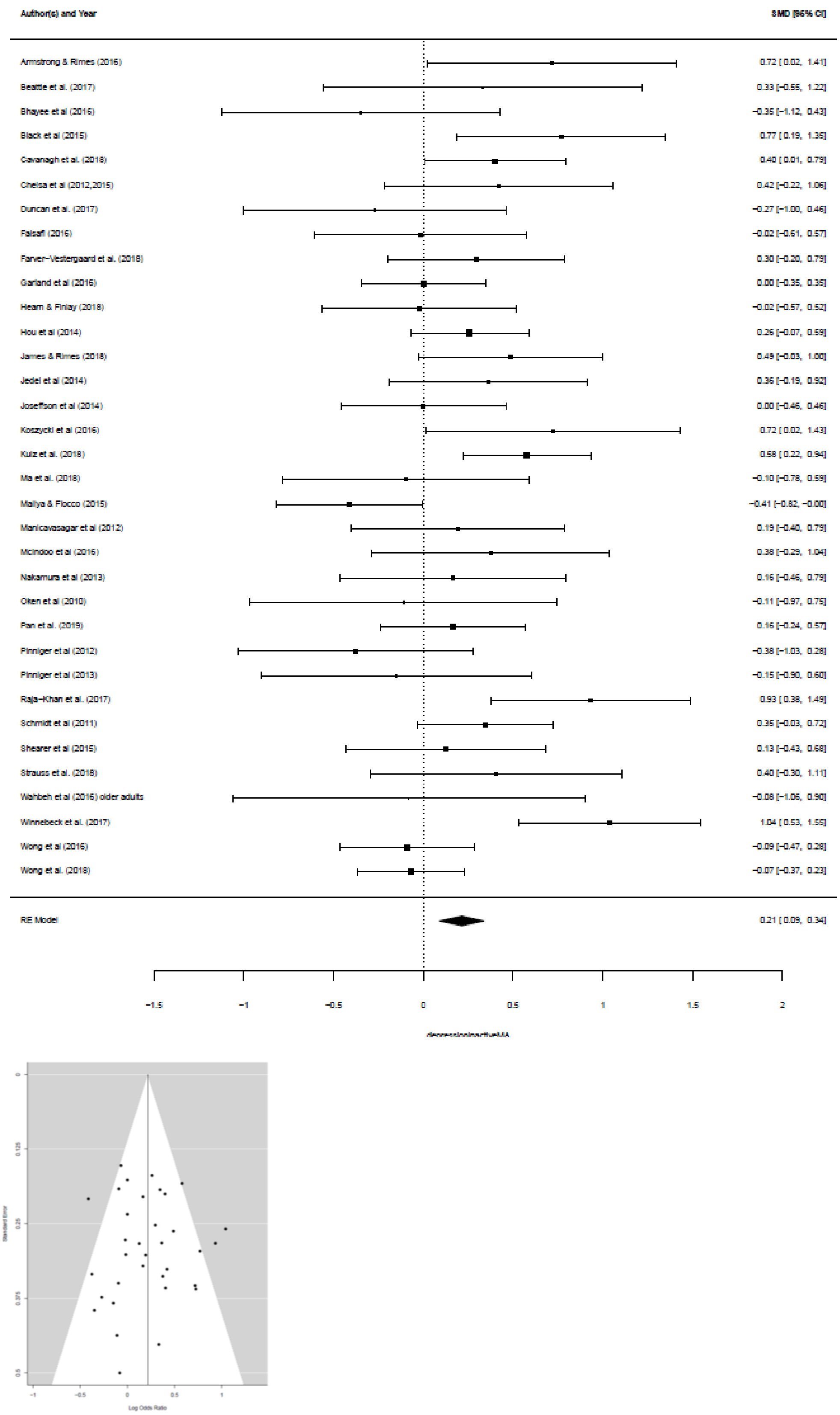




Standard Error
0
0.12
0.24
0.36
0.48
-1
0
1
2
3
4
Log Odds Ratio

Appendix Figure 3.3.17

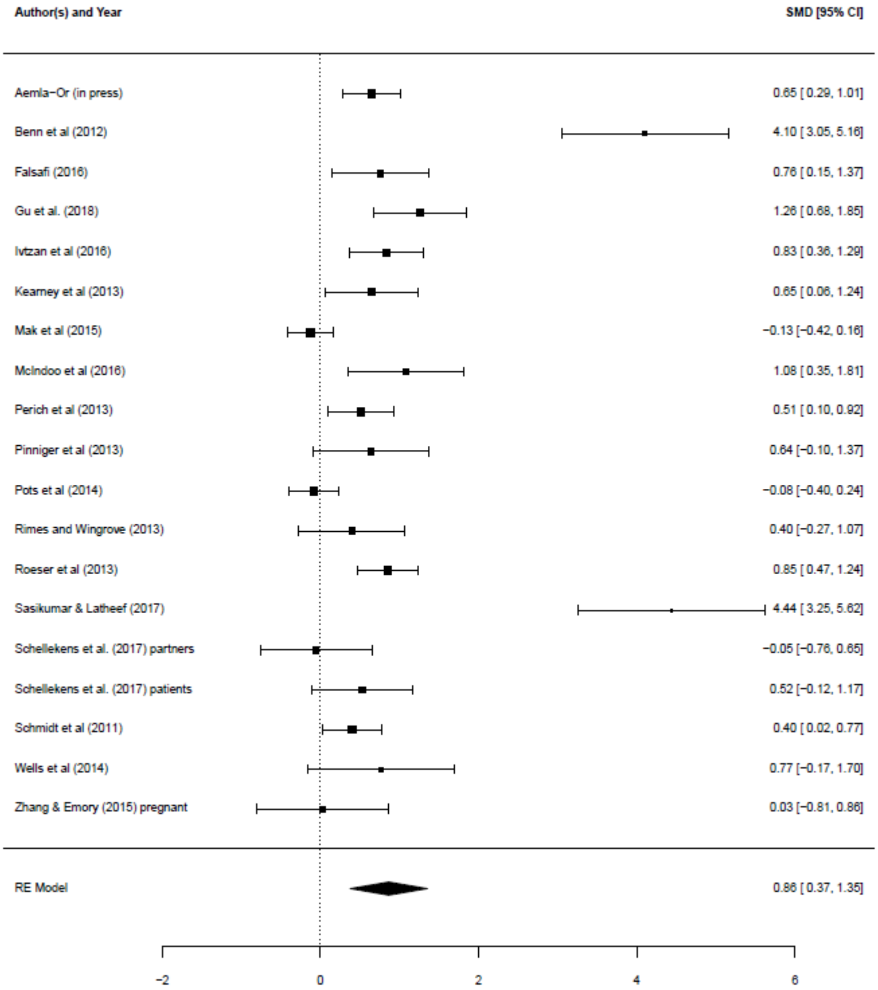
Forest and funnel plots mindfulness compared to active controls, post-program; Top: Forest plot; bottom: funnel plot

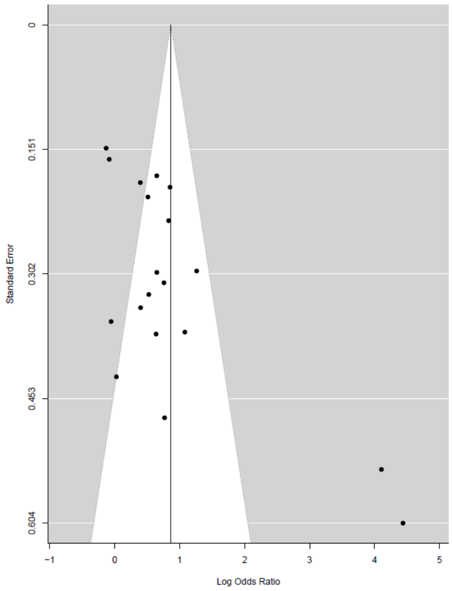


Appendix Figure 3.3.18

Forest and funnel plots mindfulness compared to inactive controls, 1-4 months follow-up

Top: Forest plot; bottom: funnel plot

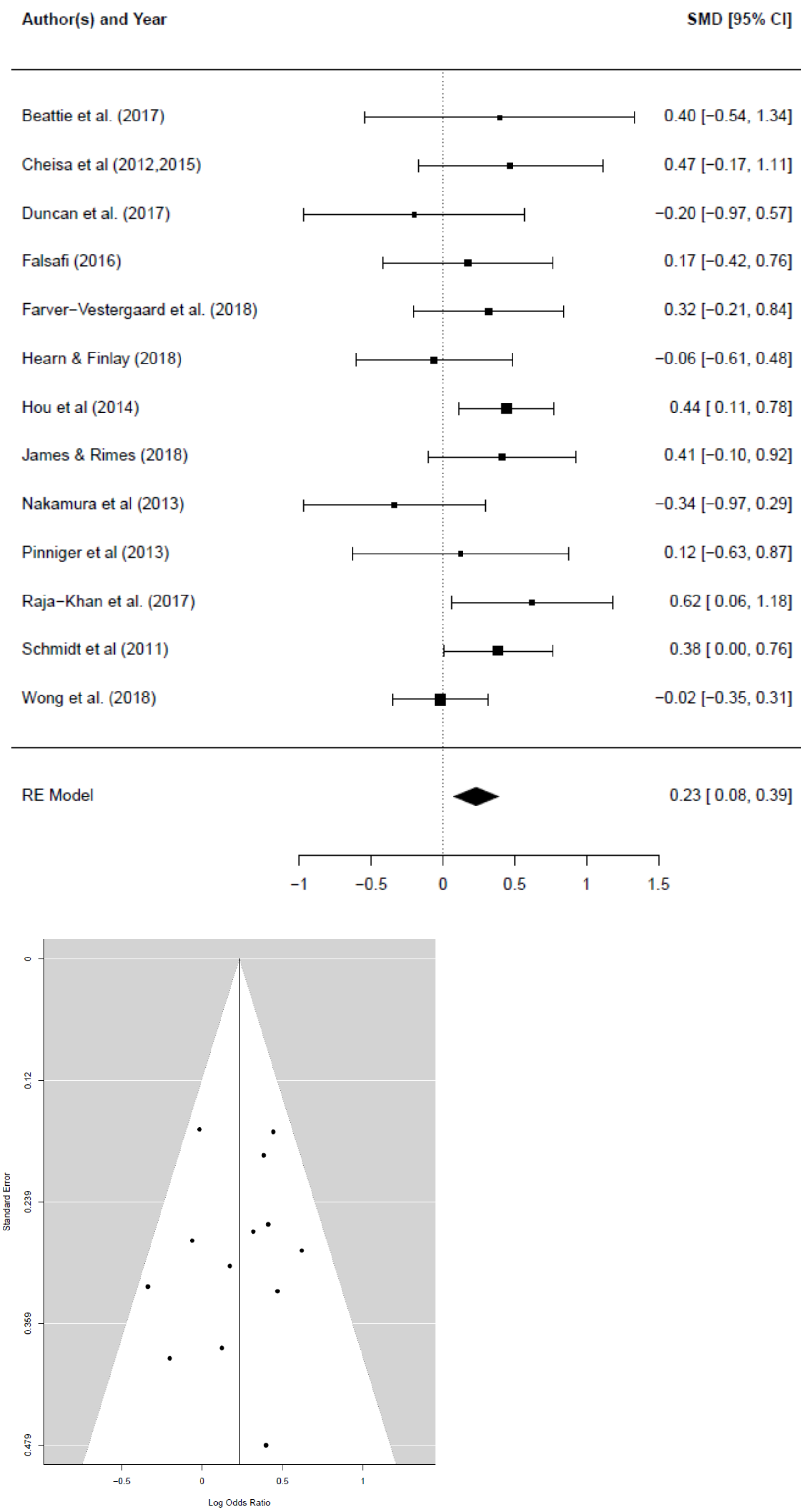




Appendix Figure 3.3.19

Forest and funnel plots mindfulness compared to active controls, 1-4 months follow-up

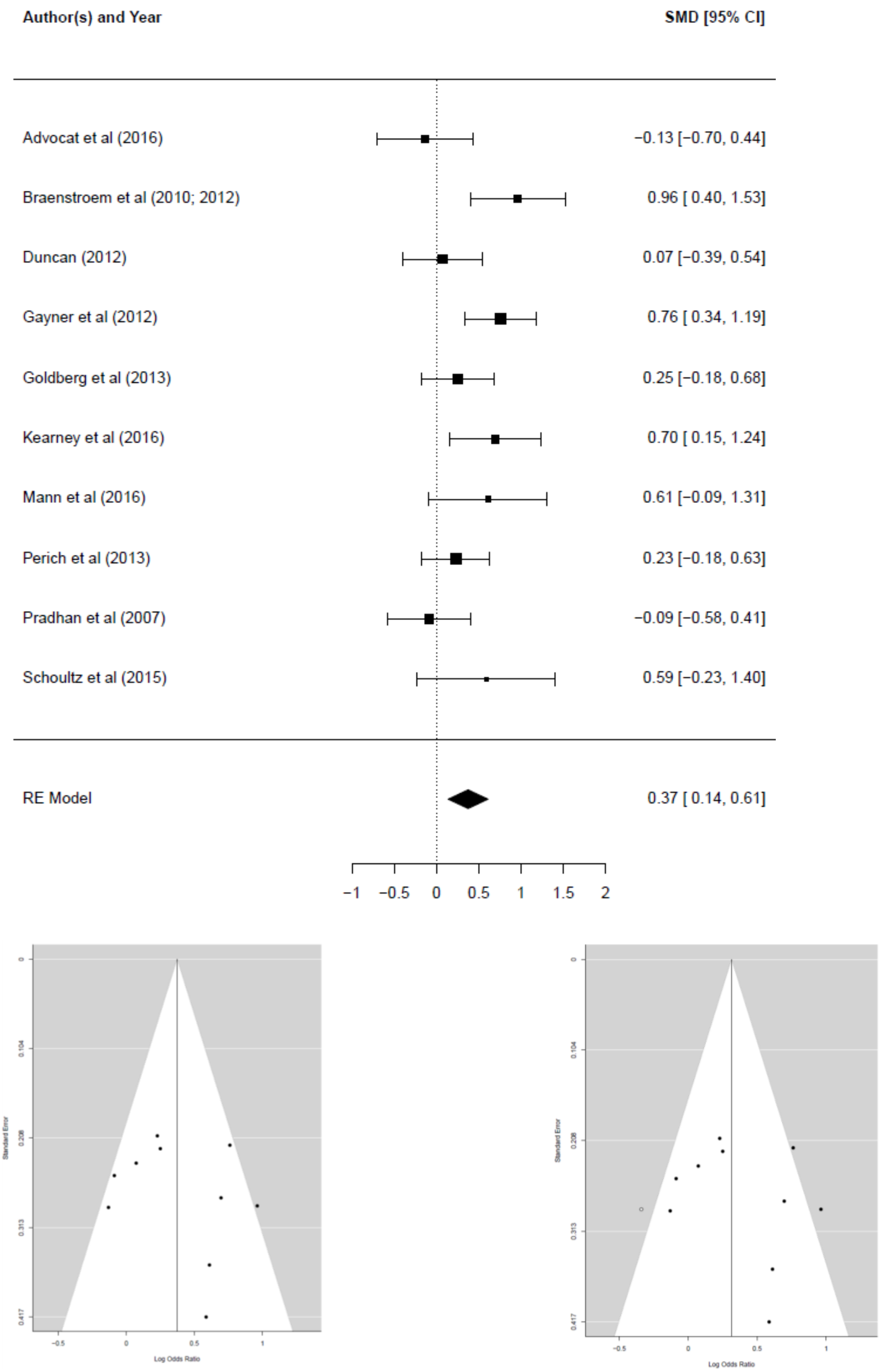
Top: Forest plot; bottom: funnel plot



Appendix Figure 3.3.20

Forest and funnel plots mindfulness compared to active controls, 5-10 months follow-up

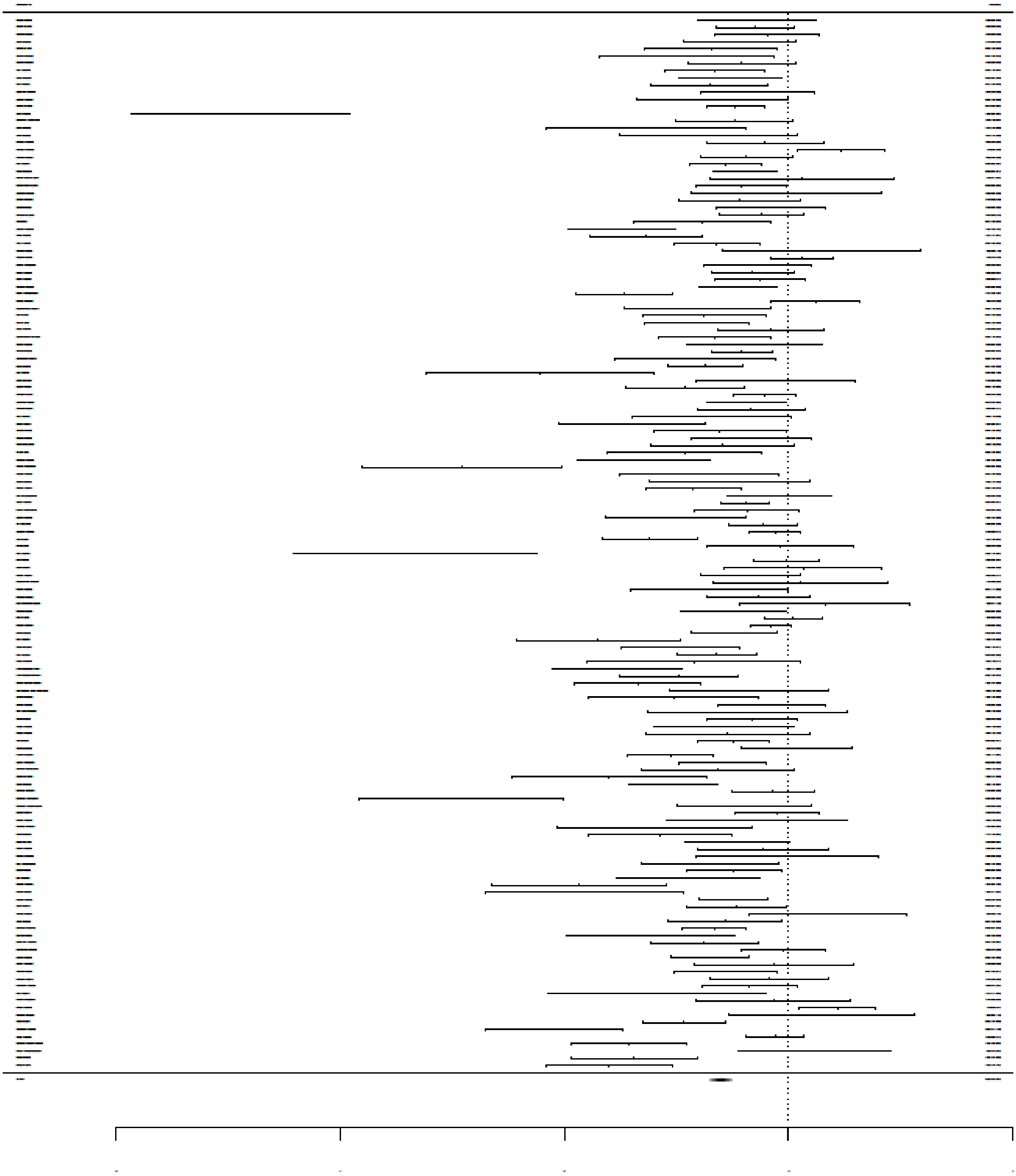
Top: Forest plot; bottom: funnel plot (left), trim-and-fill funnel plot (right)

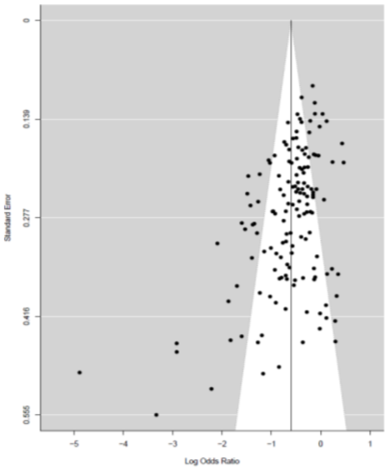


Appendix Figure 3.3.21

Forest and funnel plots depression compared to inactive controls at post-program one sample/study

Top: Forest plot; bottom: funnel plot (left), trim-and-fill funnel plot (right)





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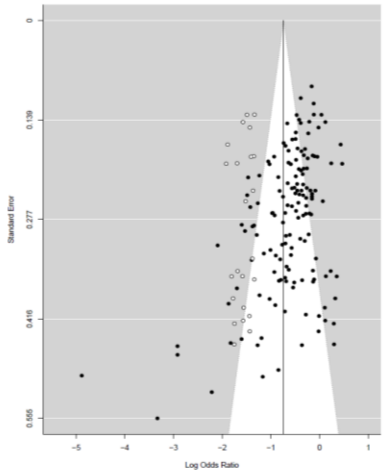
0.416
0.555
−5
−4
−3
−2
−1
0
1
Log Odds Ratio

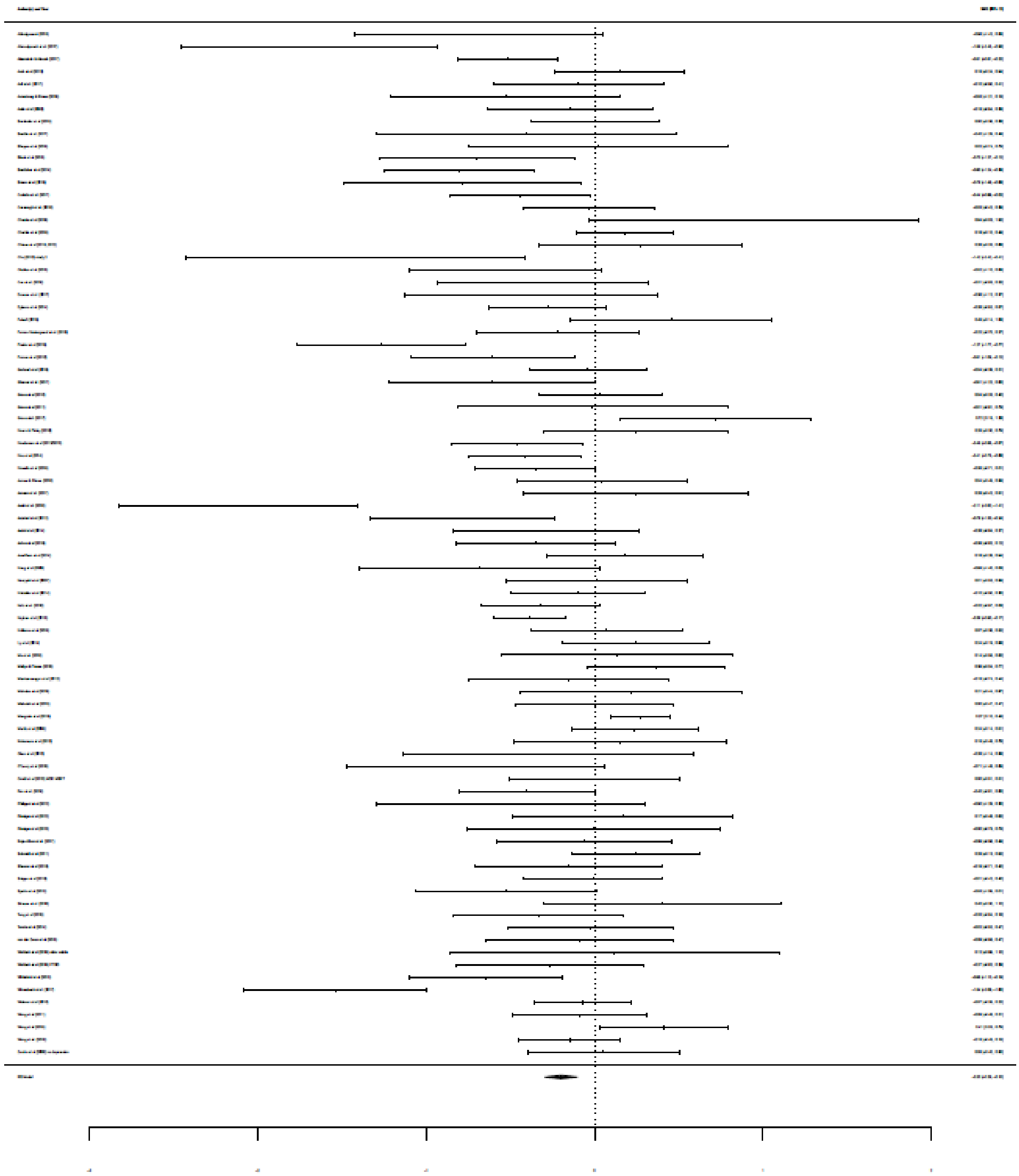


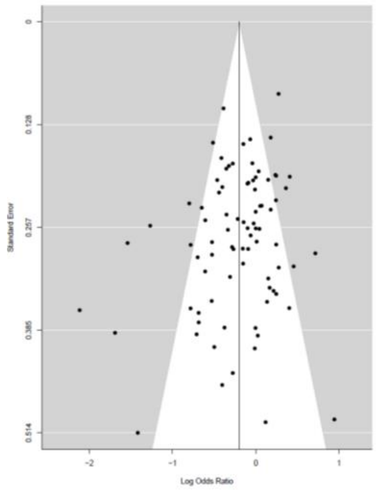
Standard Error
0
0.139
0.277
0.416
0.555
−5
−4
−3
−2
−1
0
1
Log Odds Ratio

Appendix Figure 3.3.22

Forest plot depression compared to active controls at post-program one sample/study

Top: Forest plot; bottom: funnel plot (left), trim-and-fill funnel plot (right)

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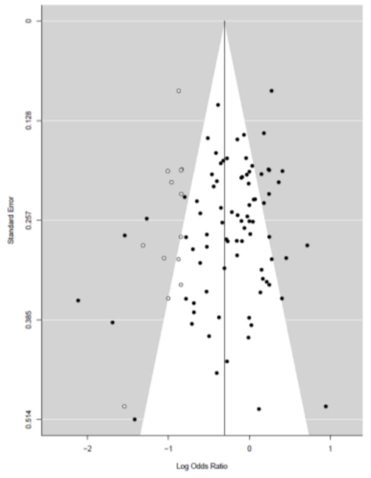
0.257
0.385
0.514
-2
-1
0
1
Log Odds Ratio


Standard Error
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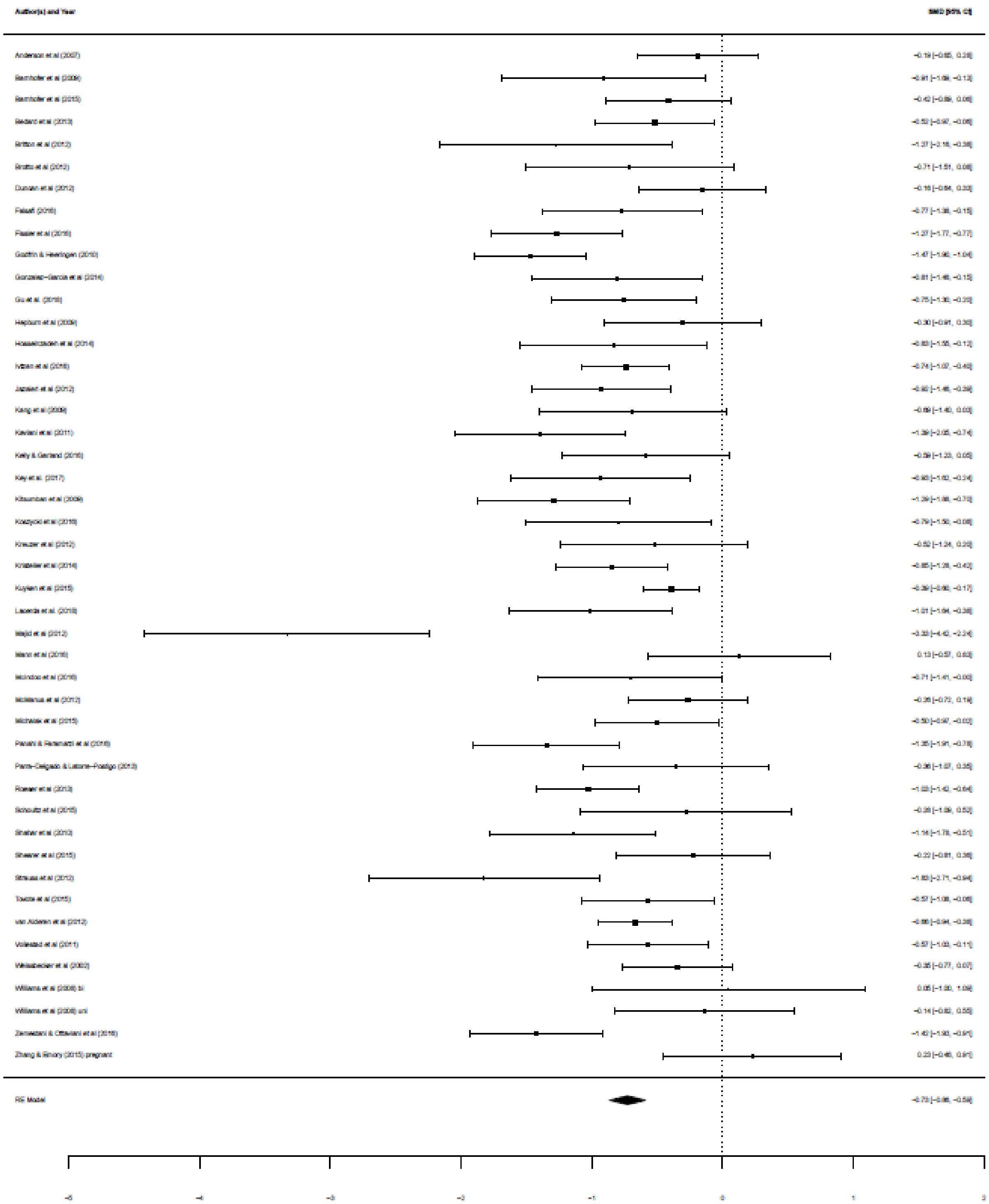
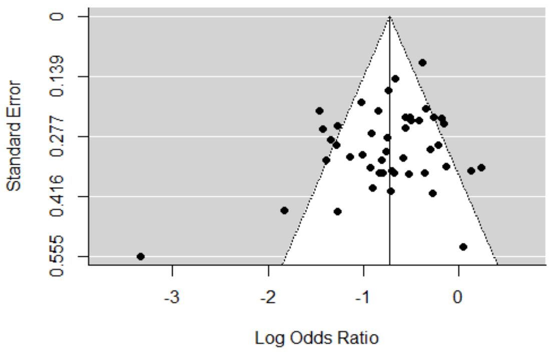
0.257
0.385
0.514
-2
-1
0
1
Log Odds Ratio

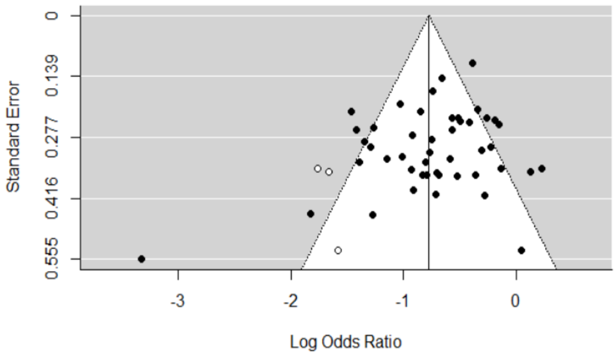
Appendix Figure 3.3.23

Forest and funnel plots BDI depression compared to inactive controls, post-program

Top: Forest plot; bottom: funnel plot (left), trim-and-fill funnel plot (right)




0
0.139
0.277
0.416
0.555
Standard Error
-3
-2
-1
0
Log Odds Ratio


0
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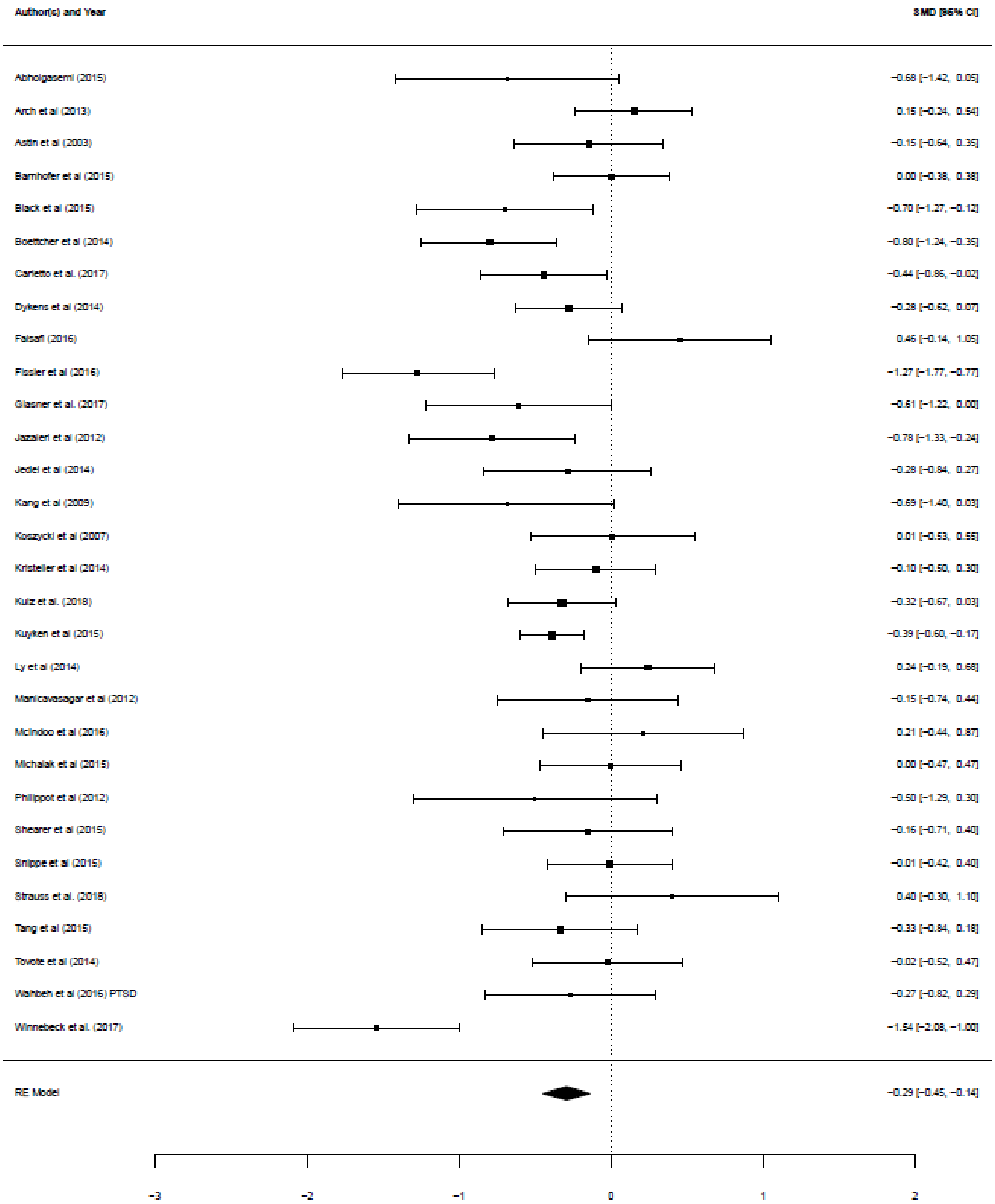
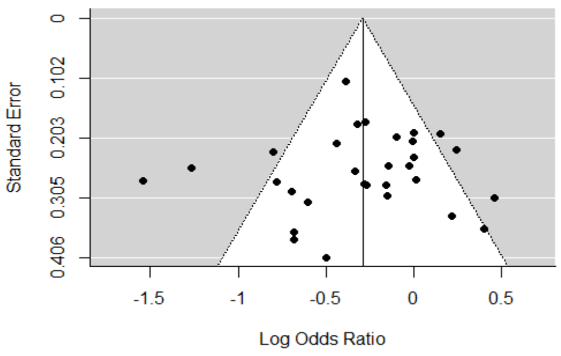
0.416
0.555
Standard Error
-3
-2
-1
0
Log Odds Ratio

Appendix Figure 3.3.24

Forest and funnel plots BDI depression compared to active controls, post-program

Top: Forest plot; bottom: funnel plot (left), trim-and-fill funnel plot (right)




Standard Error
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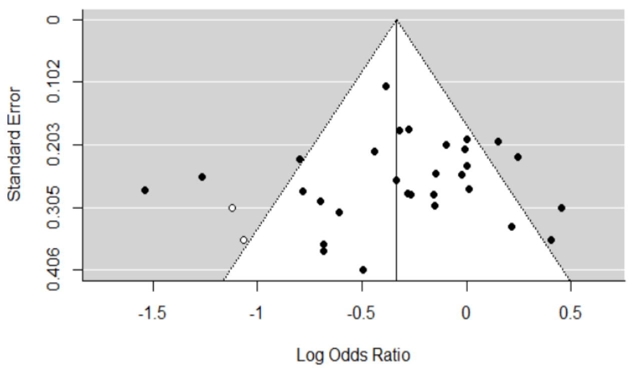
0.203
0.305
0.406
-1.5
-1
-0.5
0
0.5
Log Odds Ratio


Standard Error
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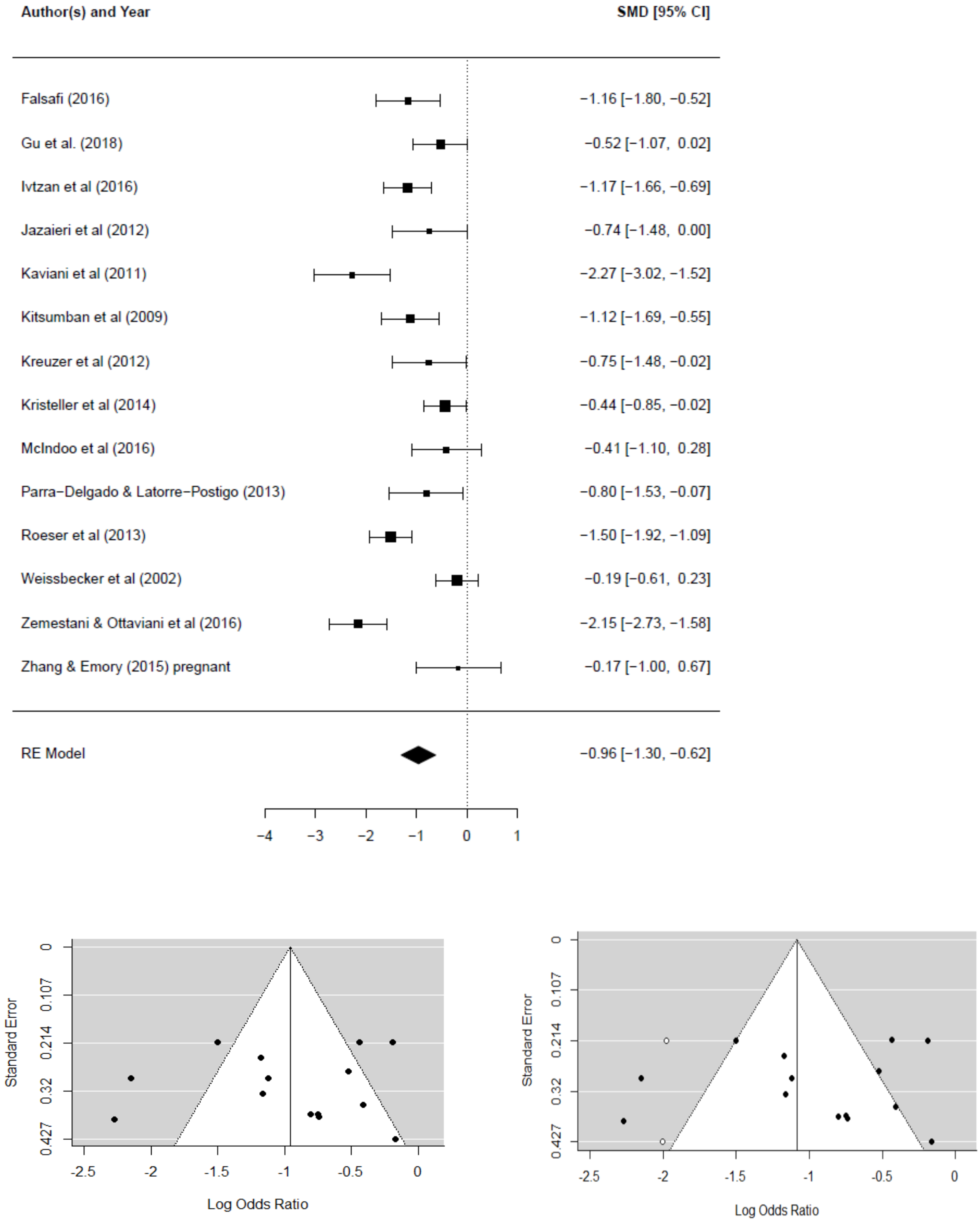
0.203
0.305
0.406
-1.5
-1
-0.5
0
0.5
Log Odds Ratio

Appendix Figure 3.3.25

Forest and funnel plots BDI depression compared to inactive controls, 1-4 months follow-up

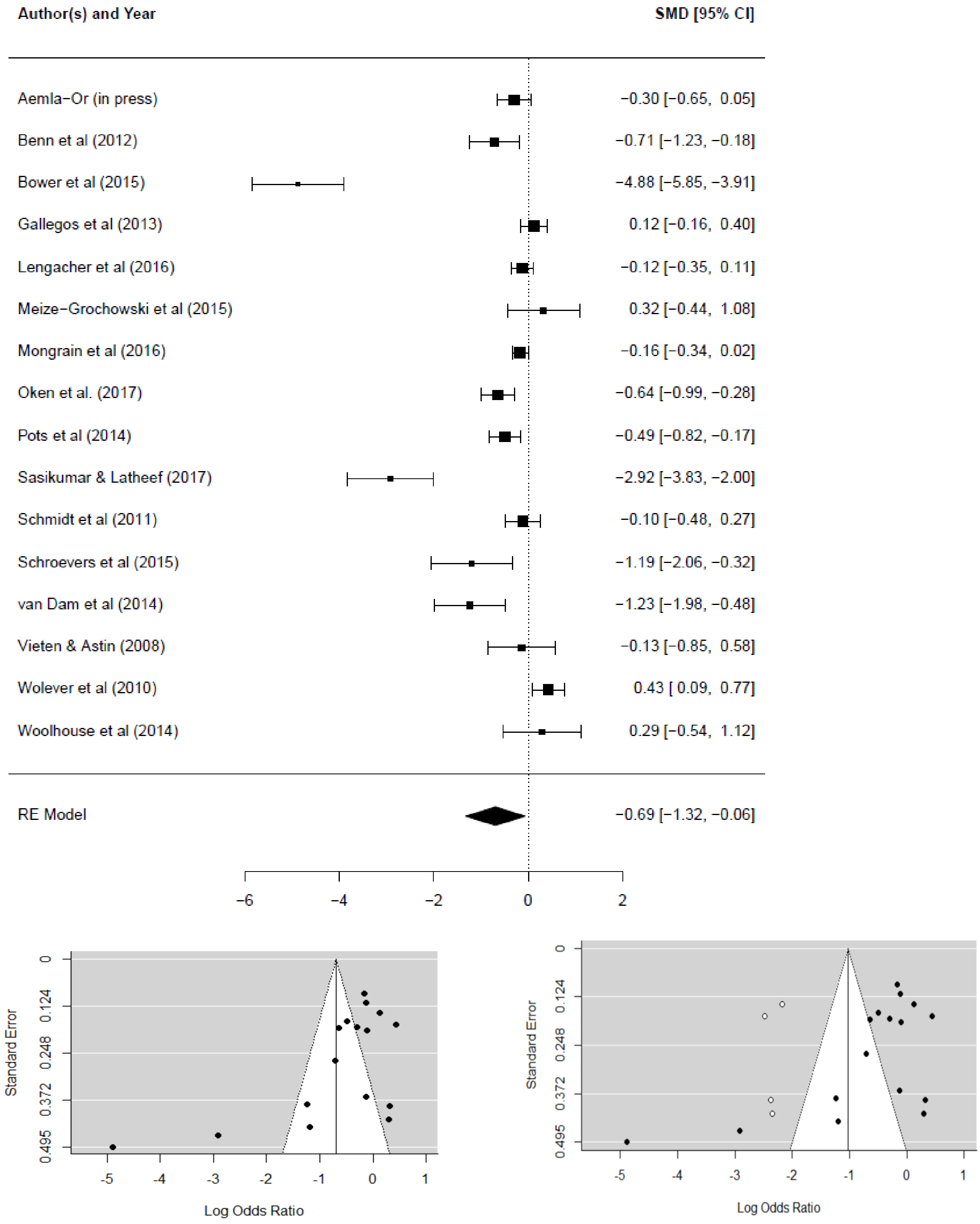
Top: Forest plot; bottom: funnel plot (left), trim-and-fill funnel plot (right)



Appendix Figure 3.3.26

Forest and funnel plots CES-D depression compared to inactive controls, post-program

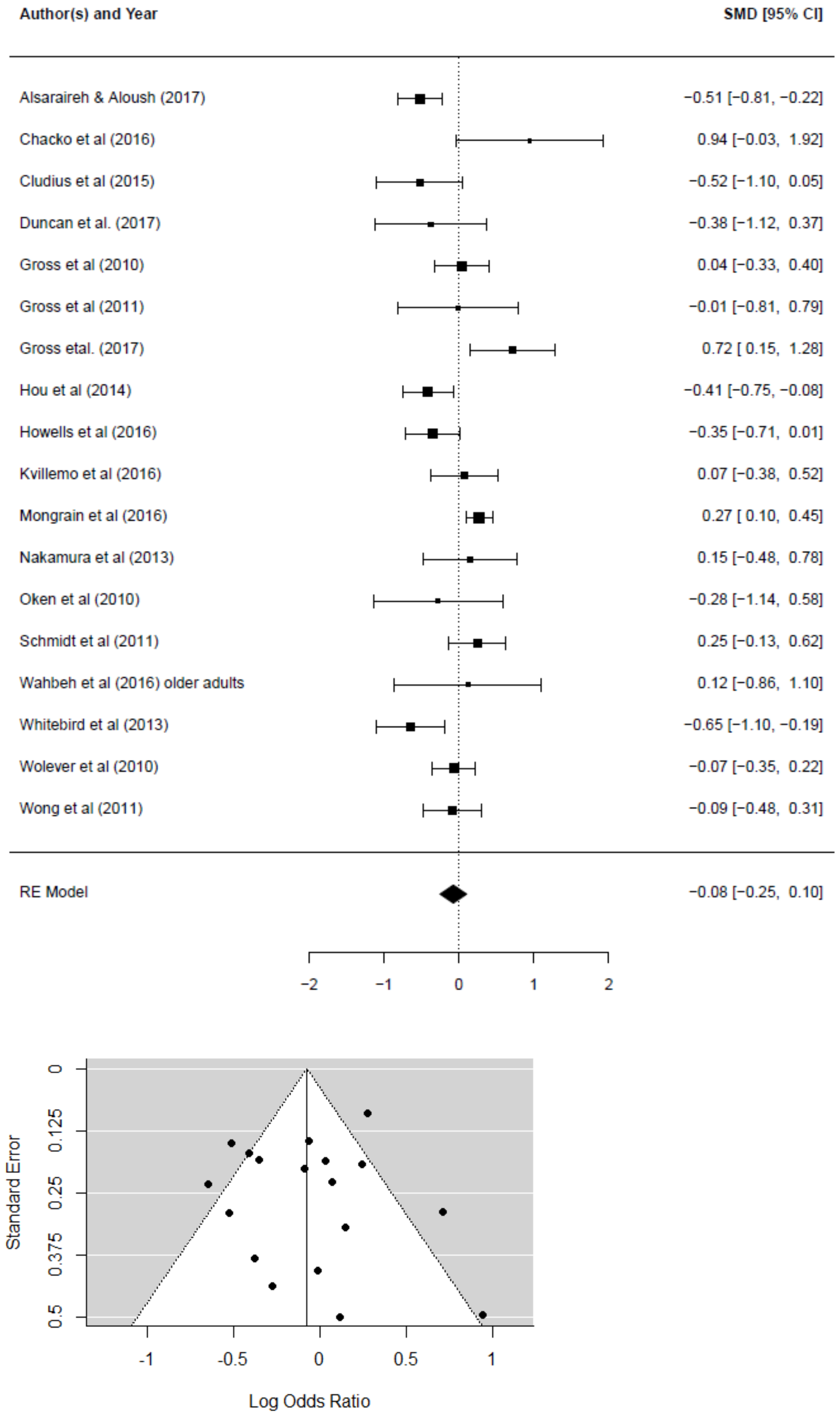
Top: Forest plot; bottom: funnel plot (left), trim-and-fill funnel plot (right)



Appendix Figure 3.3.27

Forest and funnel plots CES-D depression compared to active controls, post-program

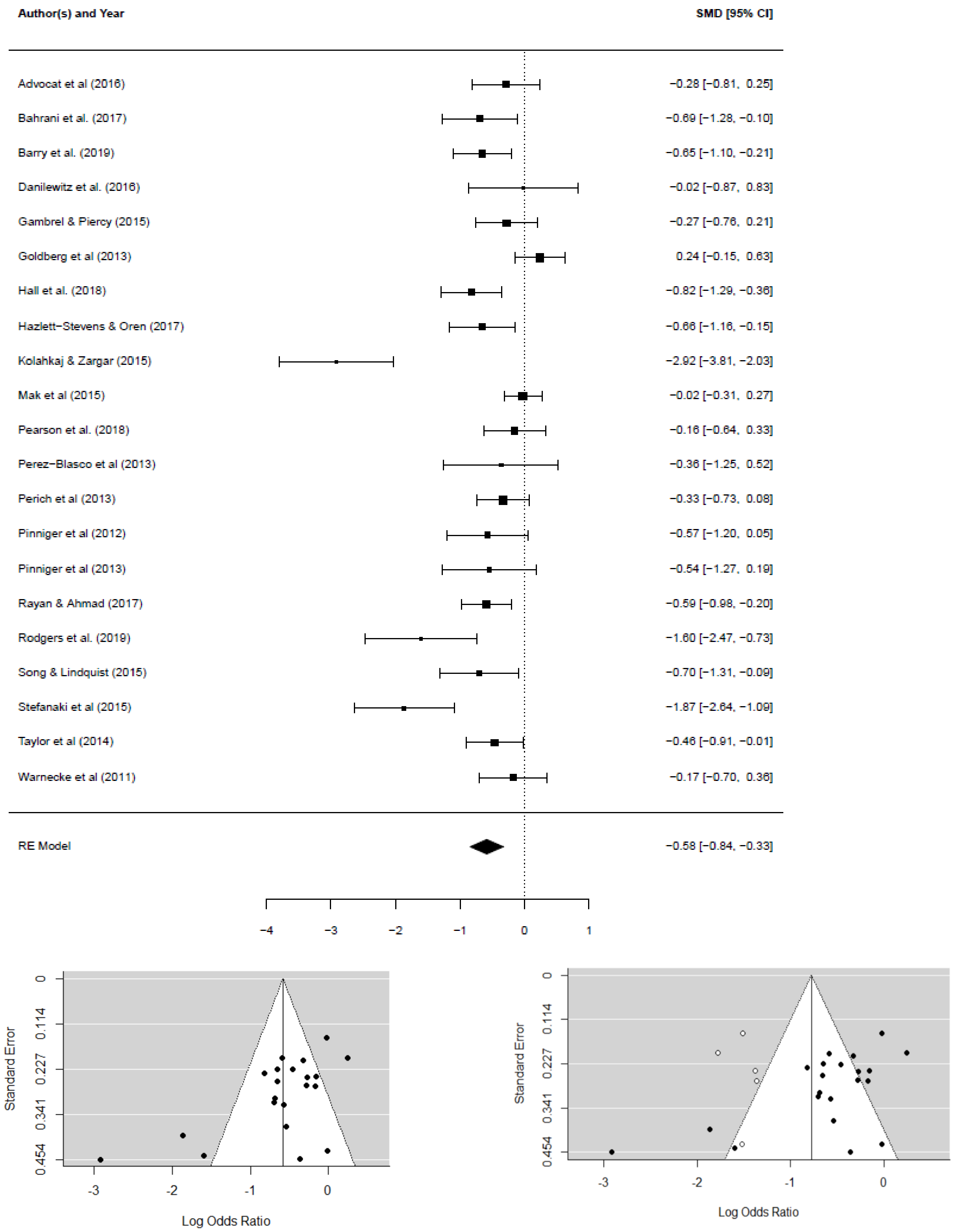
Top: Forest plot; bottom: funnel plot



Appendix Figure 3.3.28

Forest and funnel plots DASS-D depression compared to inactive controls, post-program

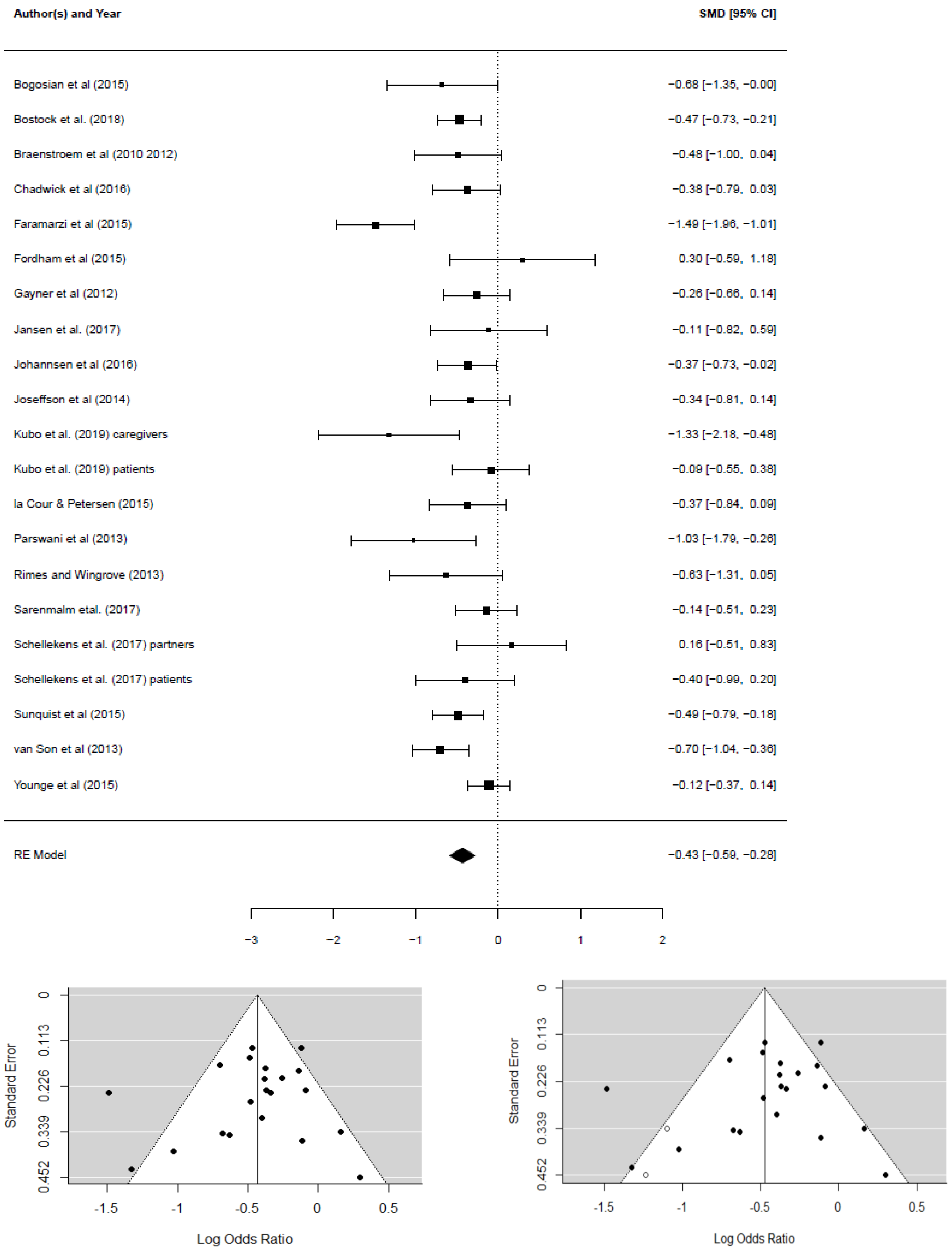
Top: Forest plot; bottom: funnel plot (left), trim-and-fill funnel plot (right)



Appendix Figure 3.3.29

Forest and funnel plots HADS-D depression compared to inactive controls, post-program

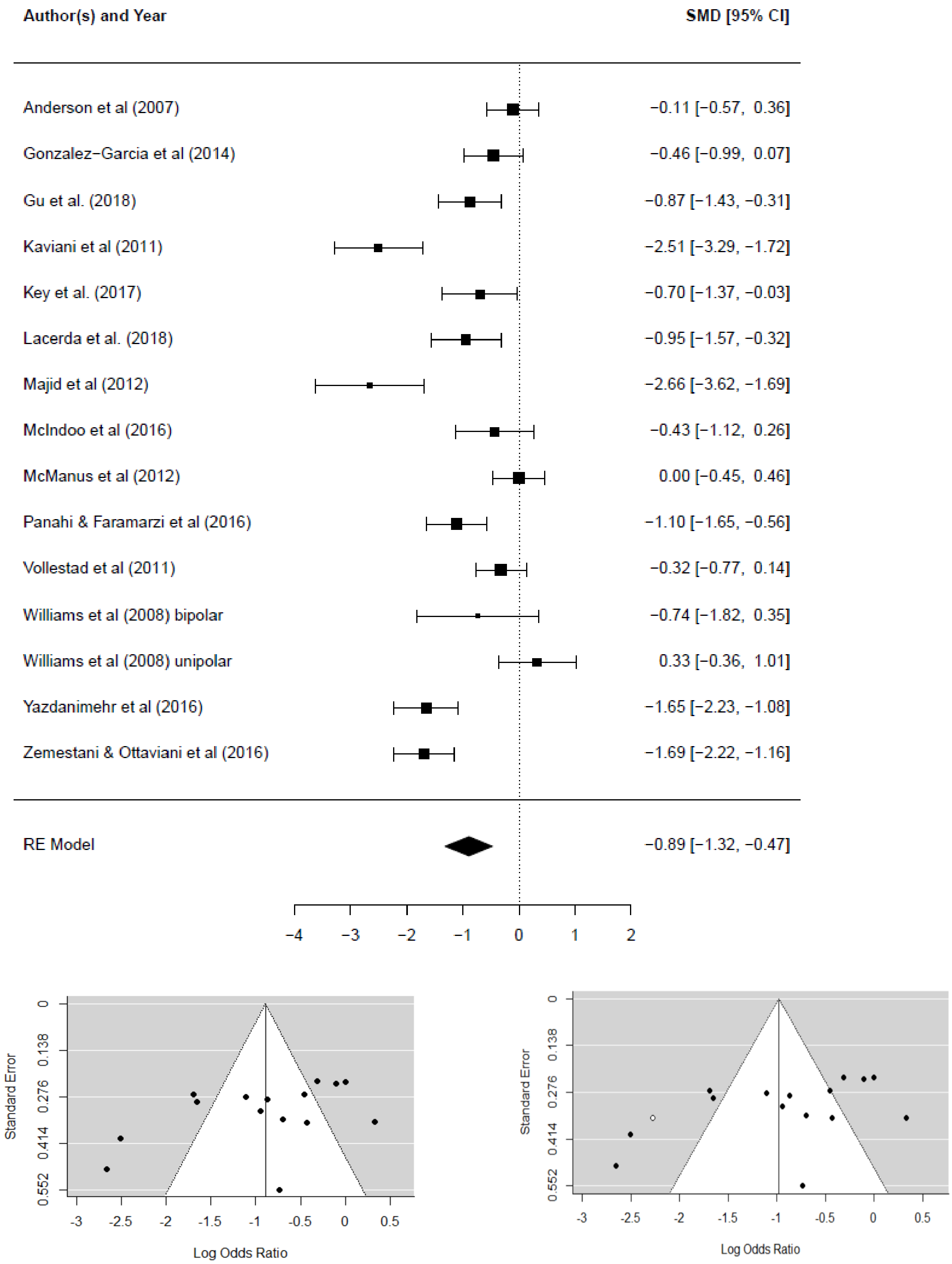
Top: Forest plot; bottom: funnel plot (left), trim-and-fill funnel plot (right)



Appendix Figure 3.3.30

Forest and funnel plots BAI anxiety compared to inactive controls, post-program

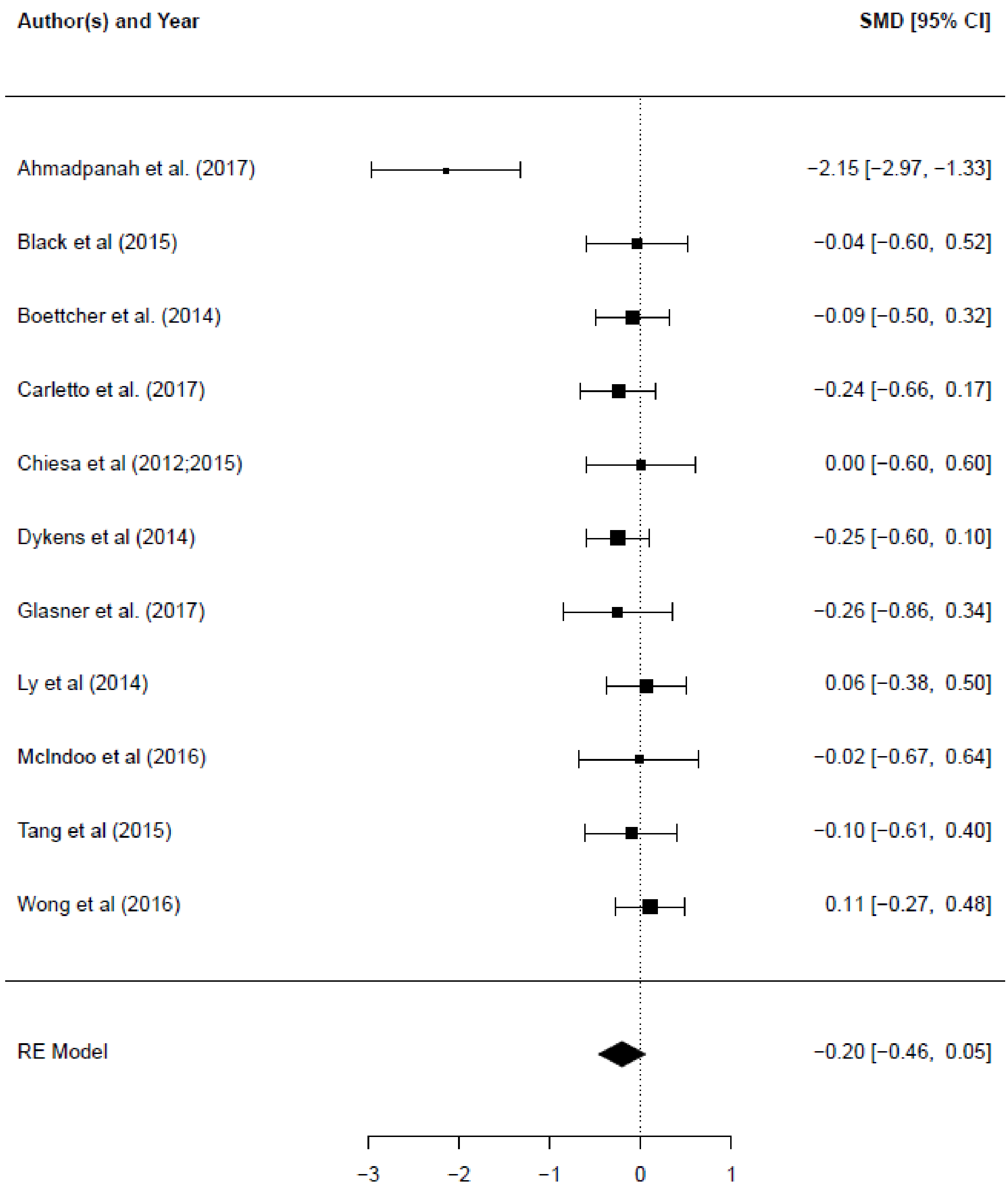
Top: Forest plot; bottom: funnel plot (left), trim-and-fill funnel plot (right)

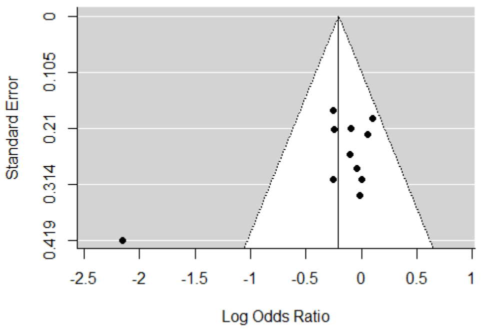


Appendix Figure 3.3.31

Forest and funnel plots BAI anxiety compared to active controls, post-program

Top: Forest plot; bottom: funnel plot (left), trim-and-fill funnel plot (right)

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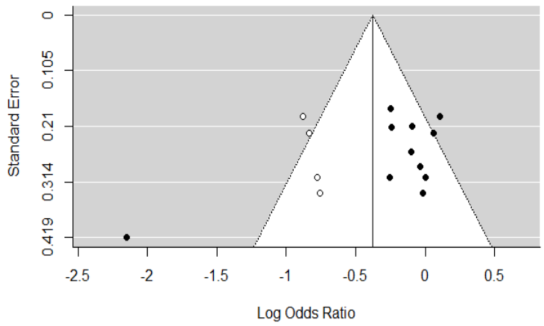
-1.5
-1
-0.5
0
0.5
1
Log Odds Ratio


Standard Error
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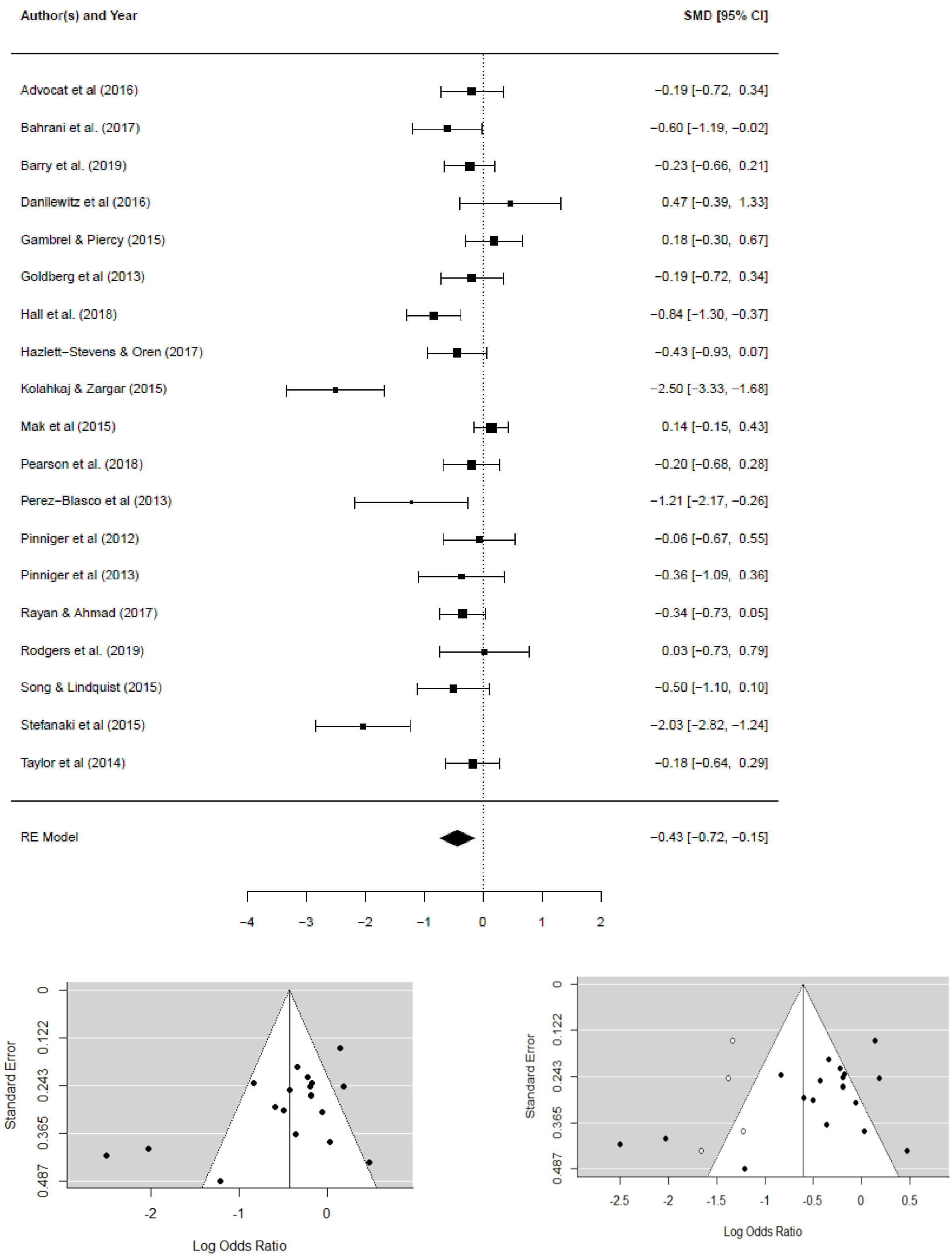
0.21
0.314
0.419
-2.5
-2
-1.5
-1
-0.5
0
0.5
Log Odds Ratio

Appendix Figure 3.3.32

Forest and funnel plots DASS-A anxiety compared to inactive controls, post-program

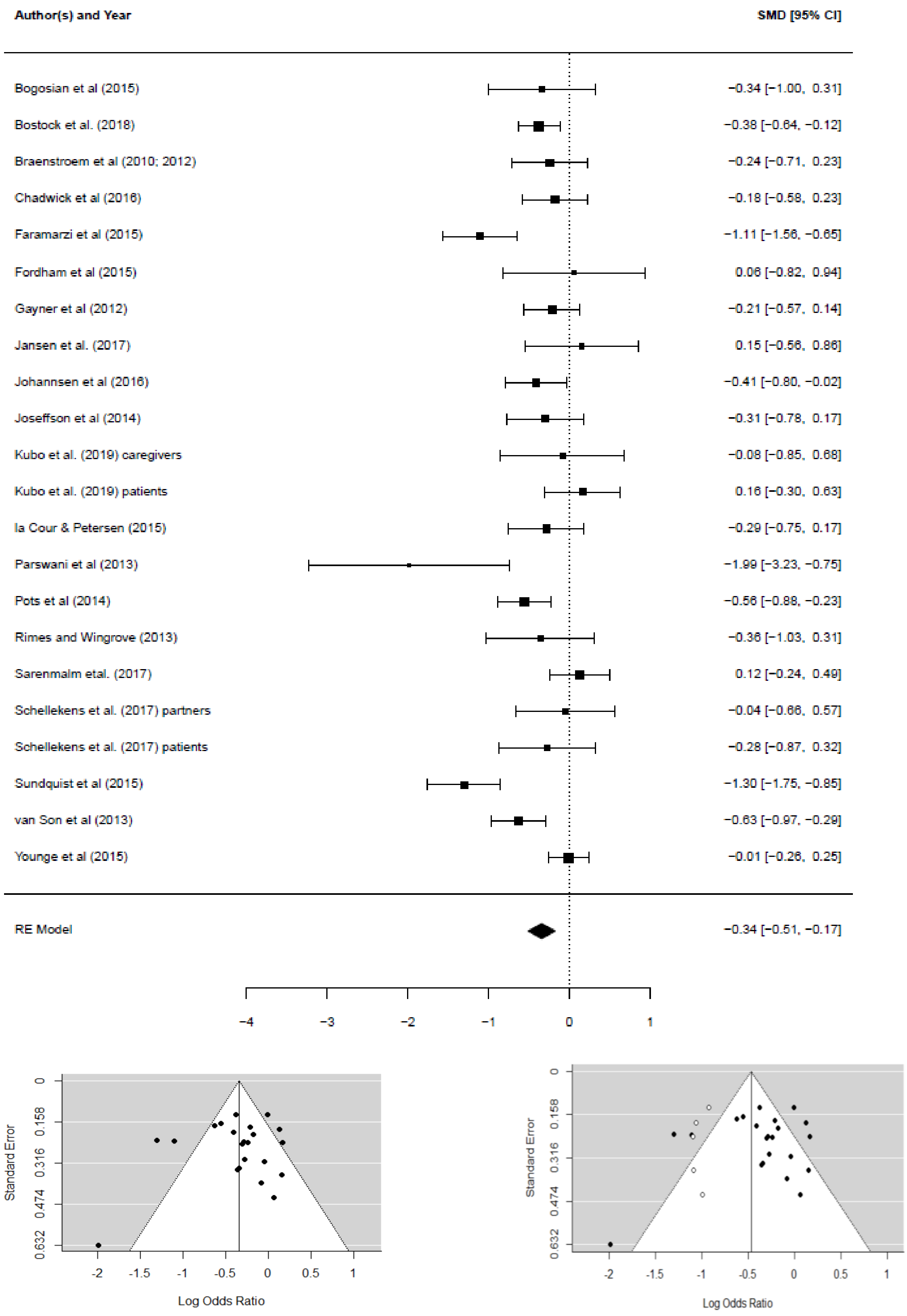
Top: Forest plot; bottom: funnel plot (left), trim-and-fill funnel plot (right)



Appendix Figure 3.3.33

Forest and funnel plots HADS-A anxiety compared to inactive controls, post-program

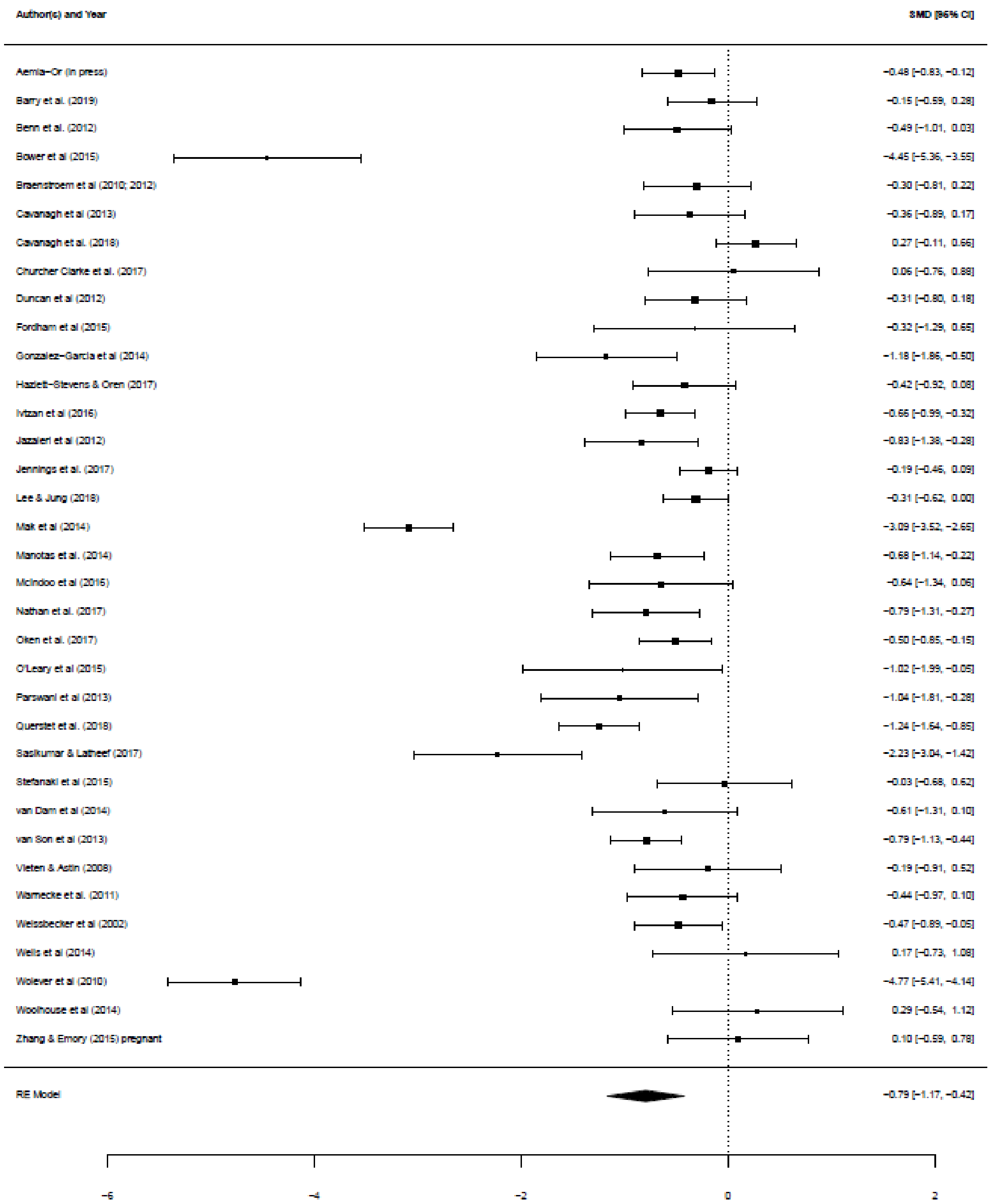
Top: Forest plot; bottom: funnel plot (left), trim-and-fill funnel plot (right)

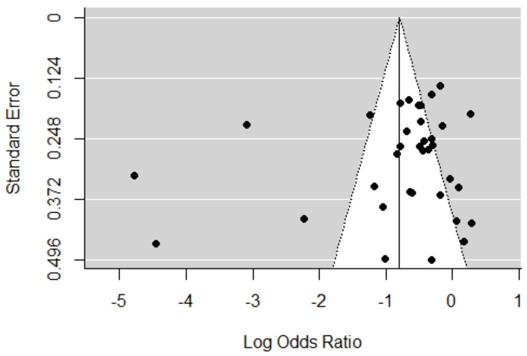


Appendix Figure 3.3.34

Forest and funnel plots PSS stress compared to inactive controls, post-program

Top: Forest plot; bottom: funnel plot (left), trim-and-fill funnel plot (right)





Standard Error
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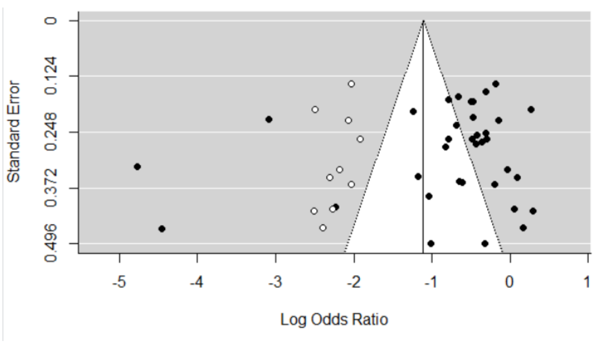
0.248
0.372
0.496
-5
-4
-3
-2
-1
0
1
Log Odds Ratio



Standard Error
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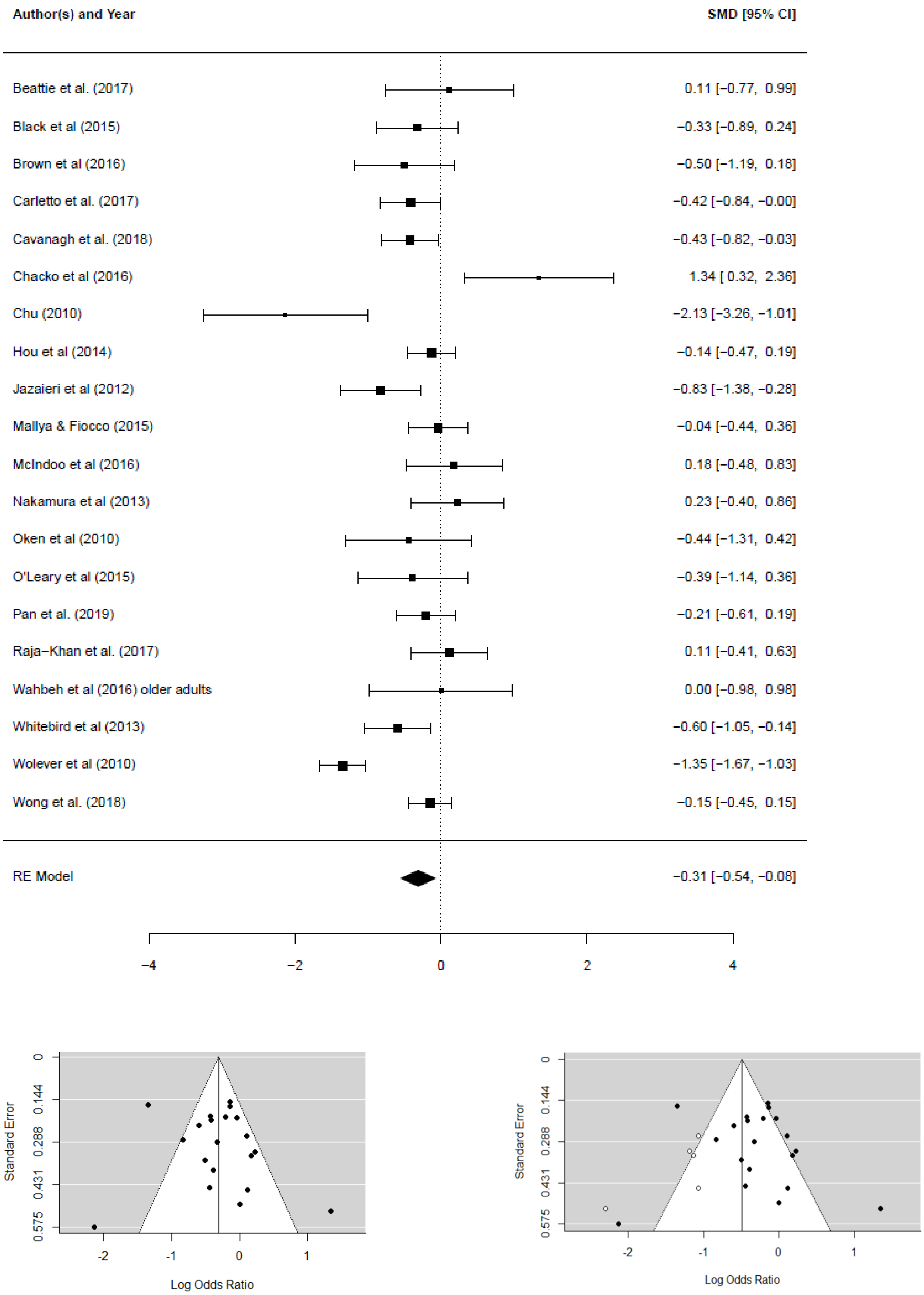
0.248
0.372
0.496
-5
-4
-3
-2
-1
0
1
Log Odds Ratio

Appendix Figure 3.3.35

Forest and funnel plots PSS stress compared to active controls, post-program

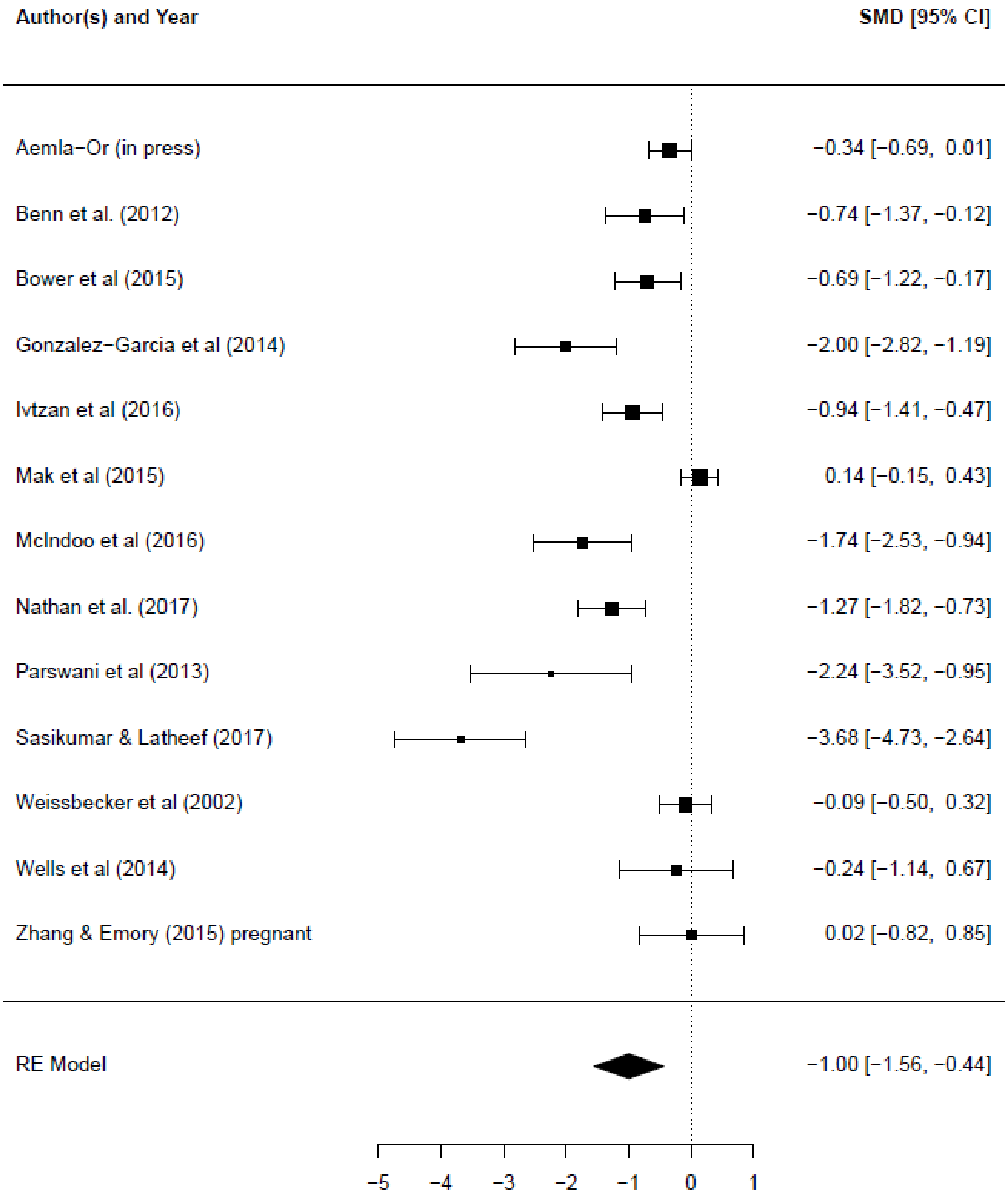
Top: Forest plot; bottom: funnel plot (left), trim-and-fill funnel plot (right)

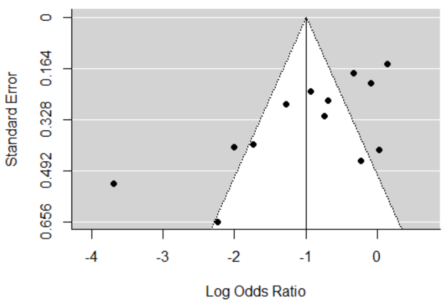


Appendix Figure 3.3.36

Forest and funnel plots PSS stress compared to inactive controls, 1-4 months follow-up

Top: Forest plot; bottom: funnel plot

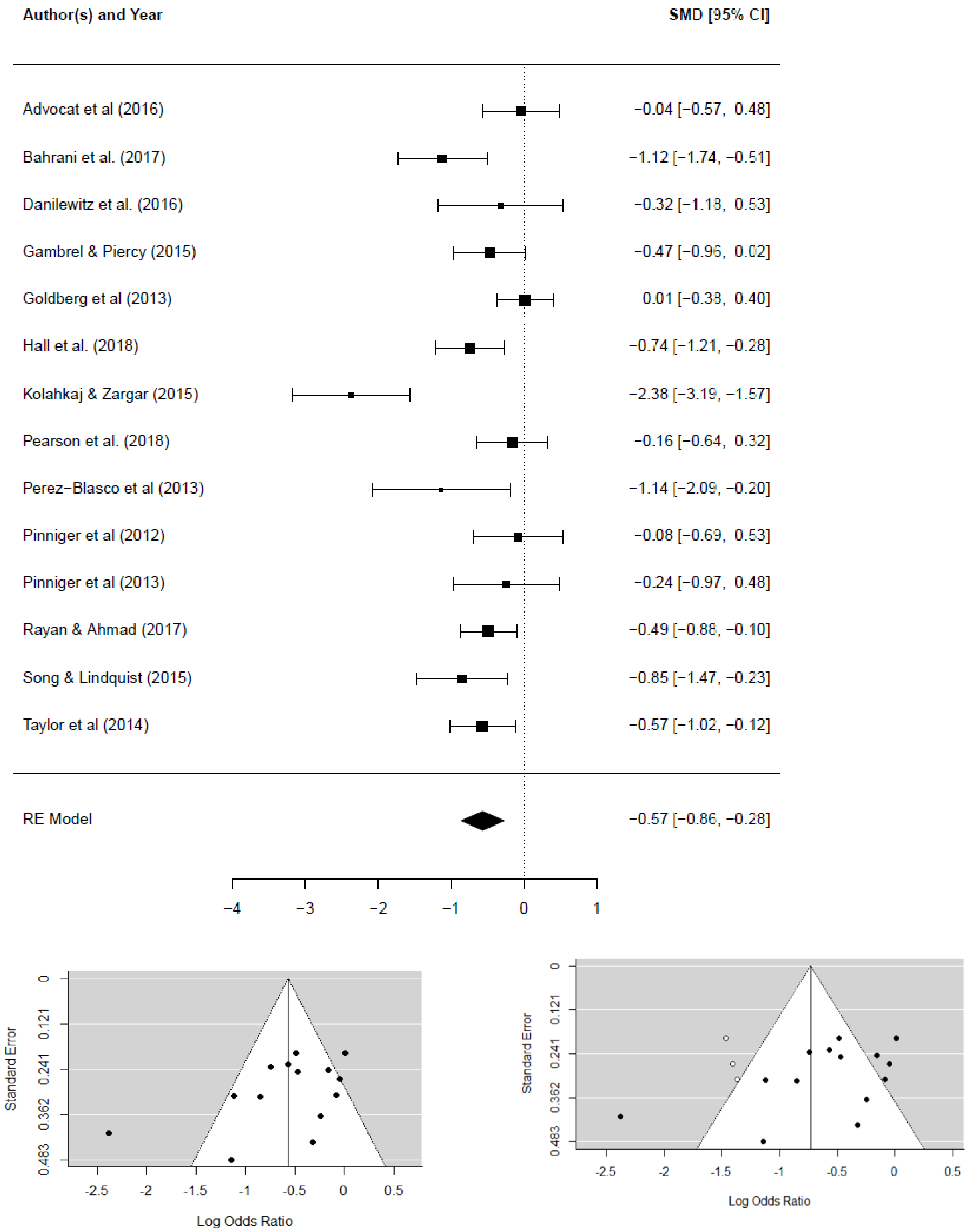




Appendix Figure 3.3.37

Forest and funnel plots DASS-S stress compared to inactive controls, post-program

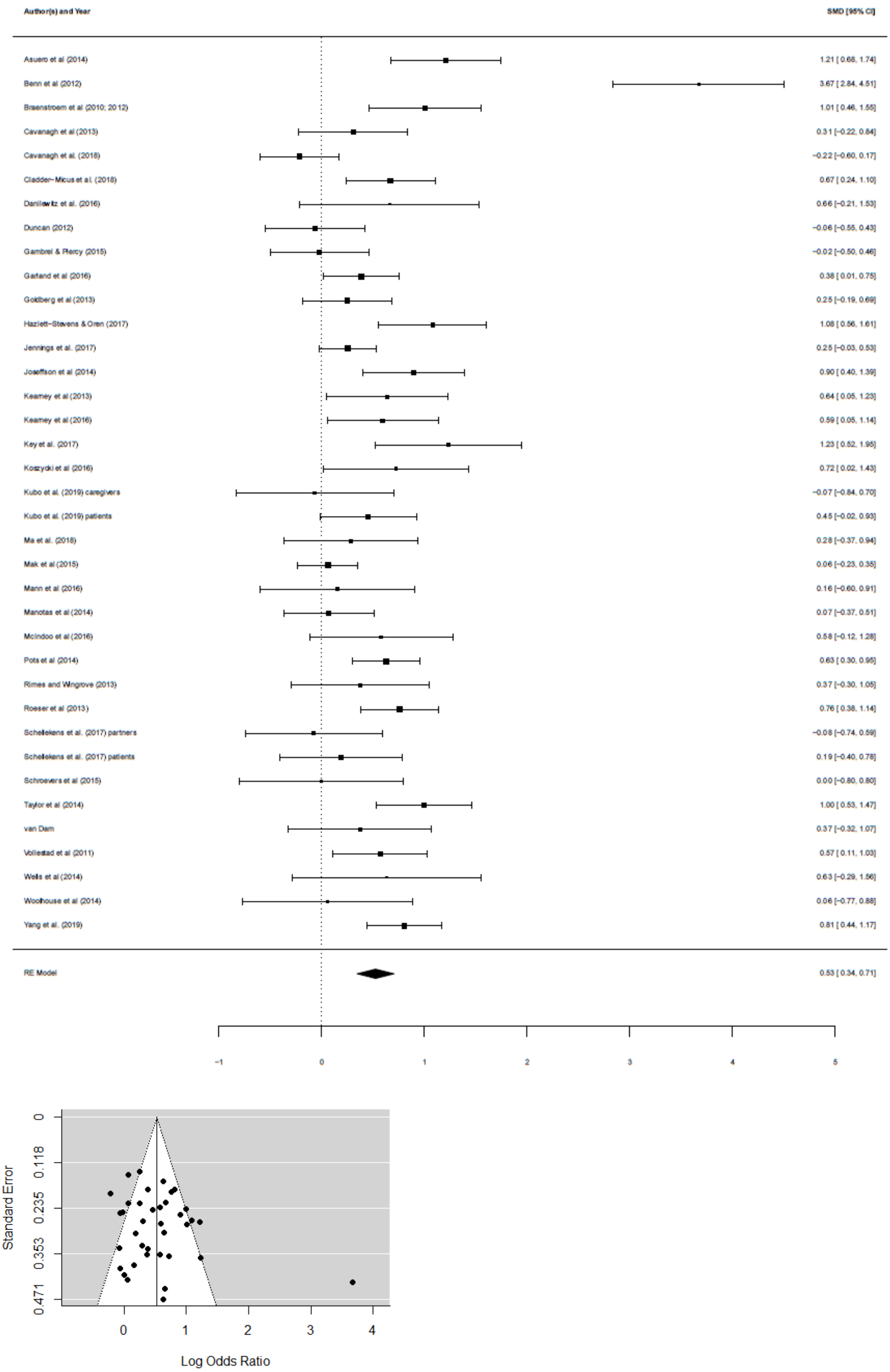
Top: Forest plot; bottom: funnel plot (left), trim-and-fill funnel plot (right)



Appendix Figure 3.3.38

Forest and funnel plots FFMQ mindfulness compared to active controls, post-program

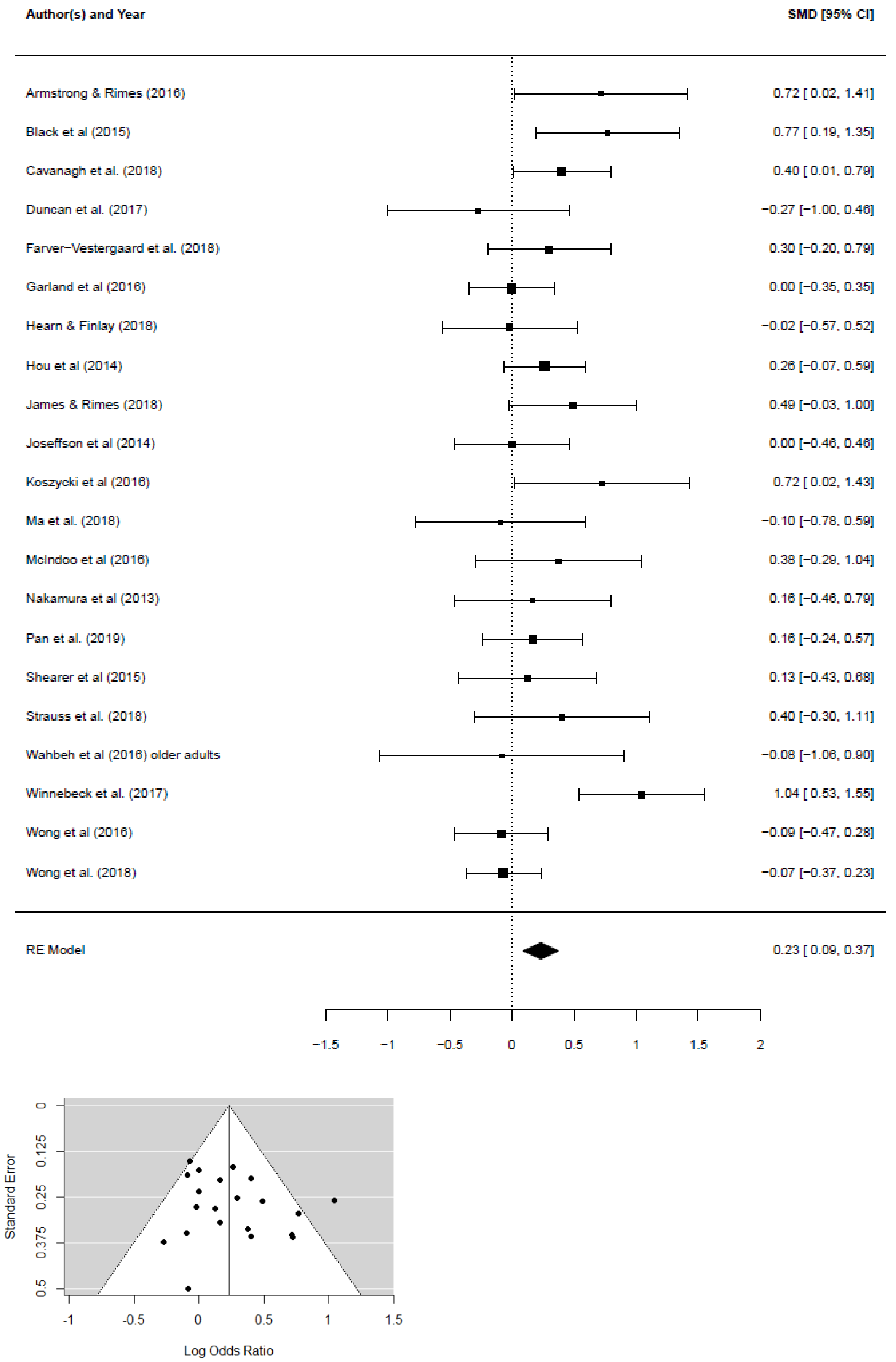
Top: Forest plot; bottom: funnel plot



Appendix Figure 3.3.39

Forest and funnel plots FFMQ mindfulness compared to active controls, post-program

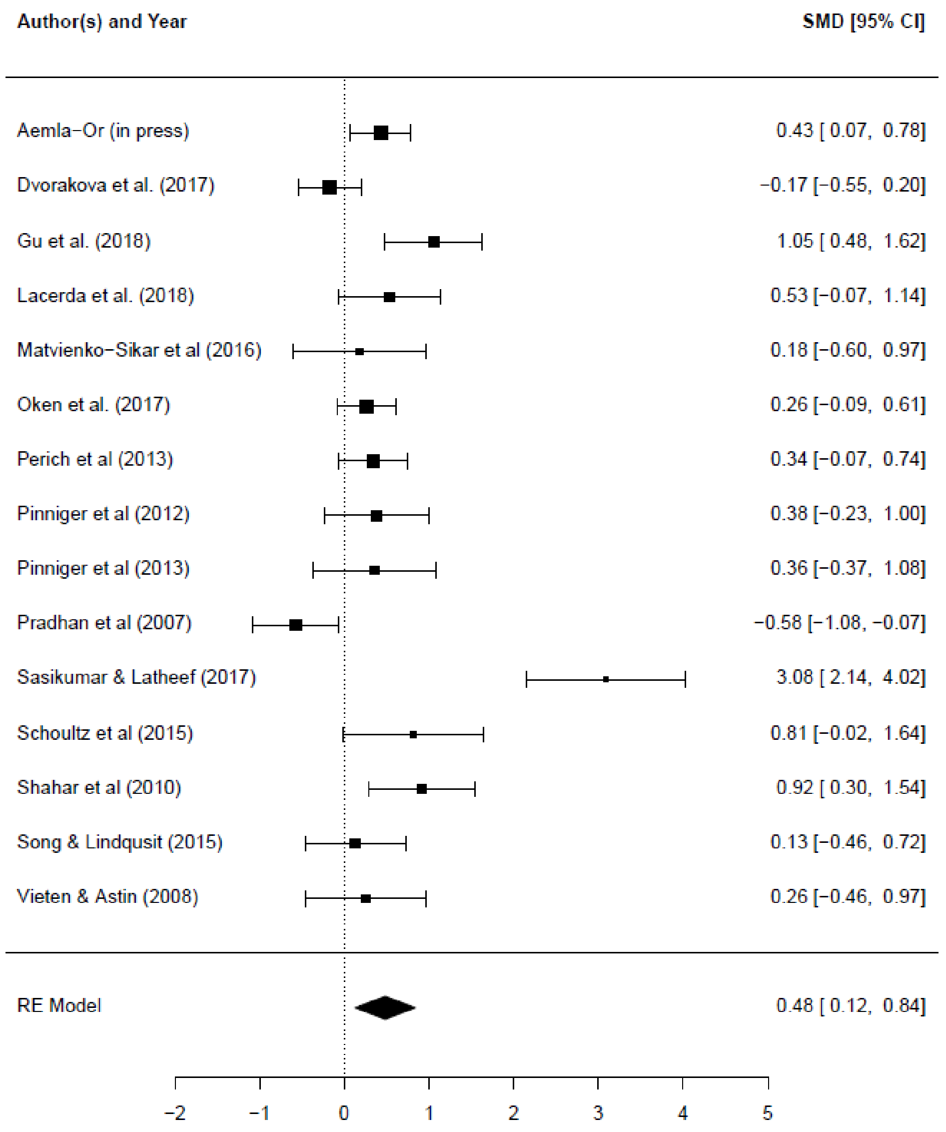
Top: Forest plot; bottom: funnel plot

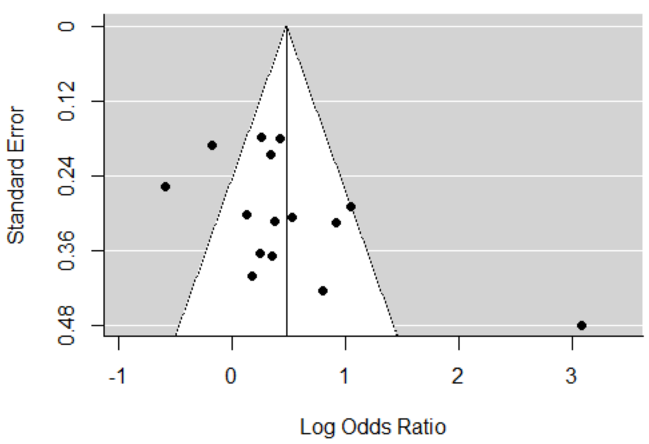


Appendix Figure 3.3.40

Forest and funnel plots MAAS mindfulness compared to inactive controls, post-program

Top: Forest plot; bottom: funnel plot





Appendices Chapter 4

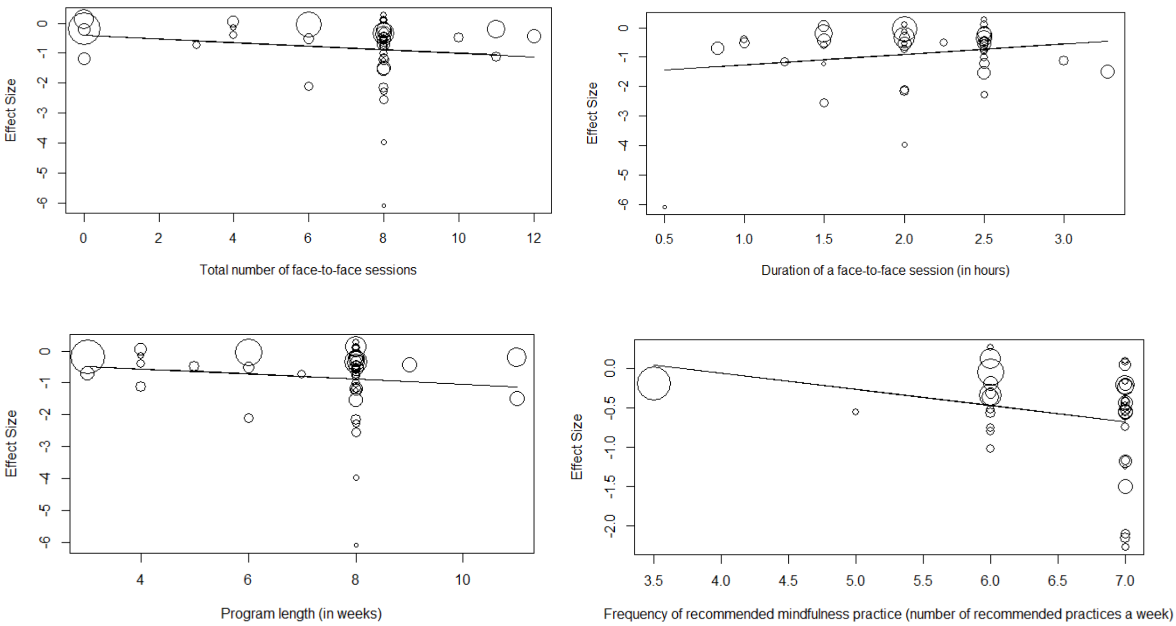
4.2 Dose-Response Meta-Regression Results Psychological Distress Outcomes

Appendix 4.2.1 Dose-Response Meta-Regression Results: Depression

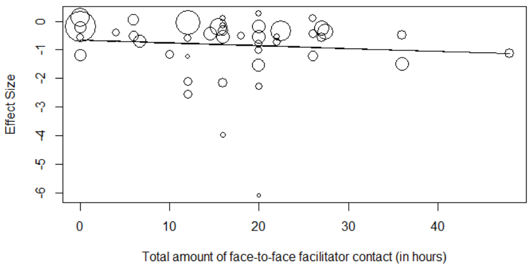
Appendix Figure 4.2.1.1

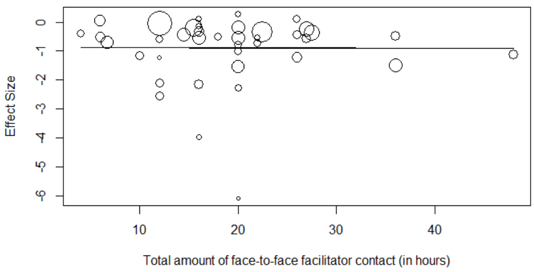
Meta-regression plots for depression at 1-4 months follow-up compared to inactive and active controls for the doses total number of face-to-face sessions, duration of a face-to-face session (in hours), program length (in weeks), frequency (number) of recommended home practices a week, duration of a recommended home practice (one practice in minutes; compared to active controls only), total amount of face-to-face facilitator contact (in hours), total amount of face-to-face facilitator contact with zero hours of contact excluded (in hours), total recommended use of the program (in hours), total actual use of the program (in hours; compared to active controls only), program intensity (number of sessions a week excluding all-day retreats), program intensity (number of sessions a week including all-day retreats), amount of face-to-face facilitator contact a week (in hours), amount of face-to-face facilitator contact excluding zero contact a week (in hours), recommended use of the program a week (in hours) and actual use of the program a week (in hours; compared to active controls only)

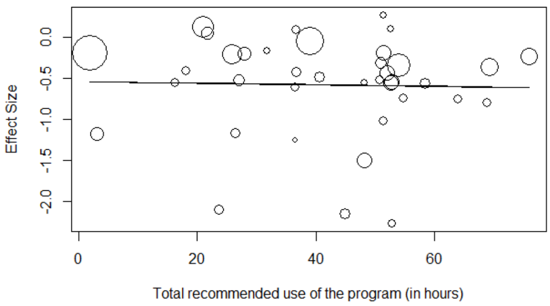
Compared to inactive controls – primary dose variables

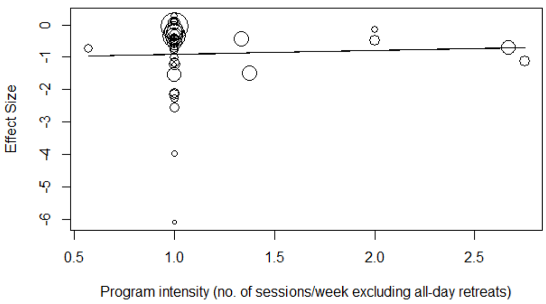


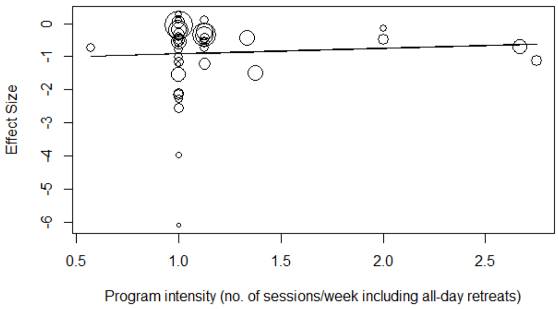
Compared to inactive controls – composite dose variables

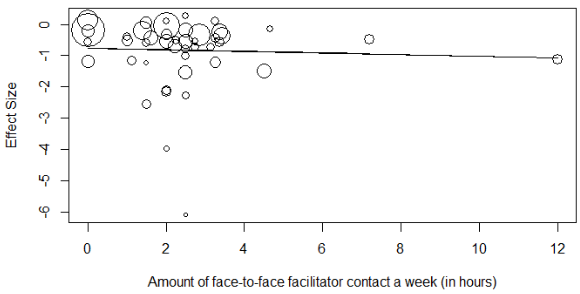


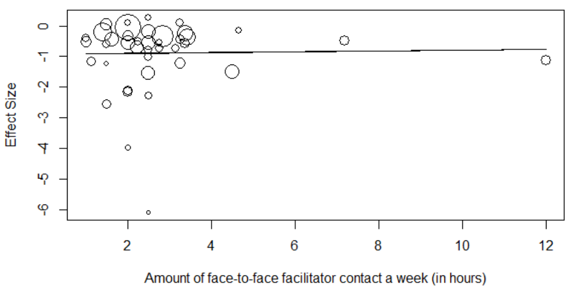


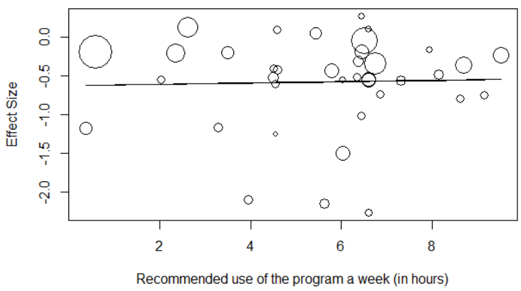




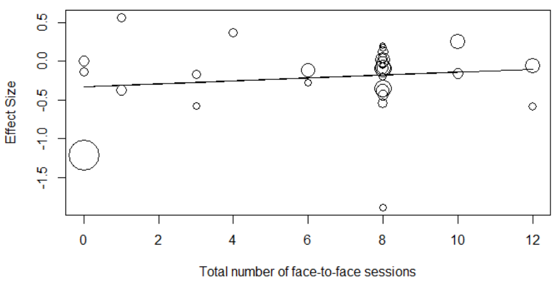


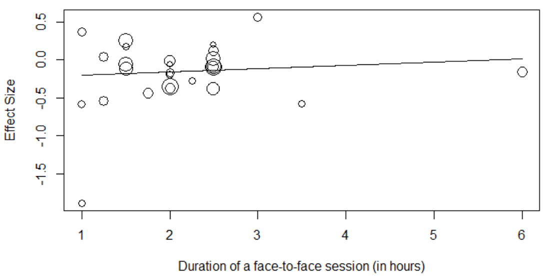


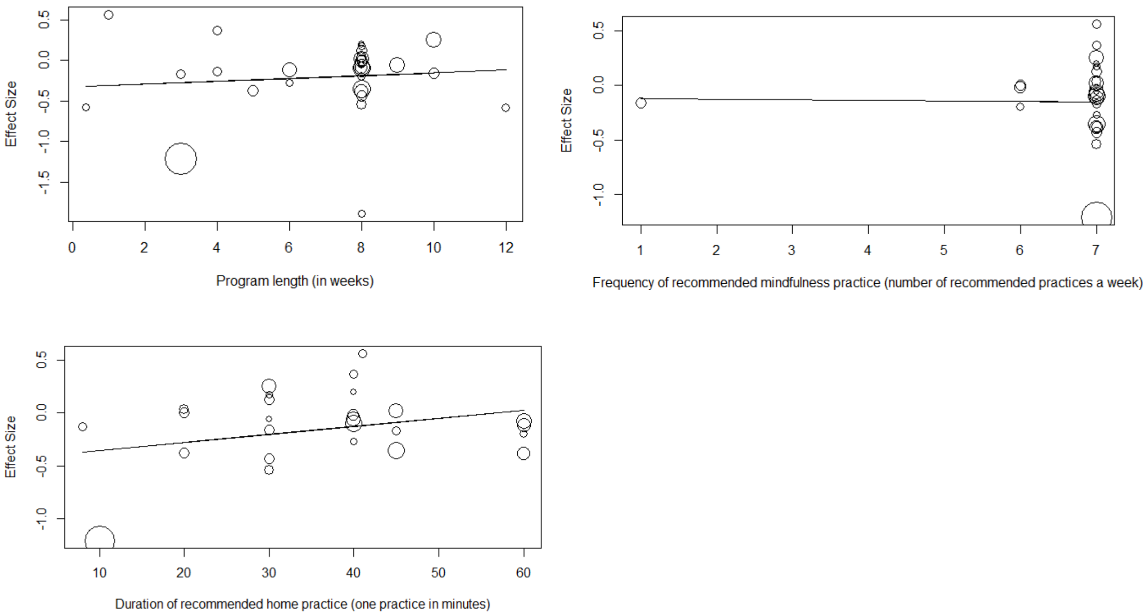




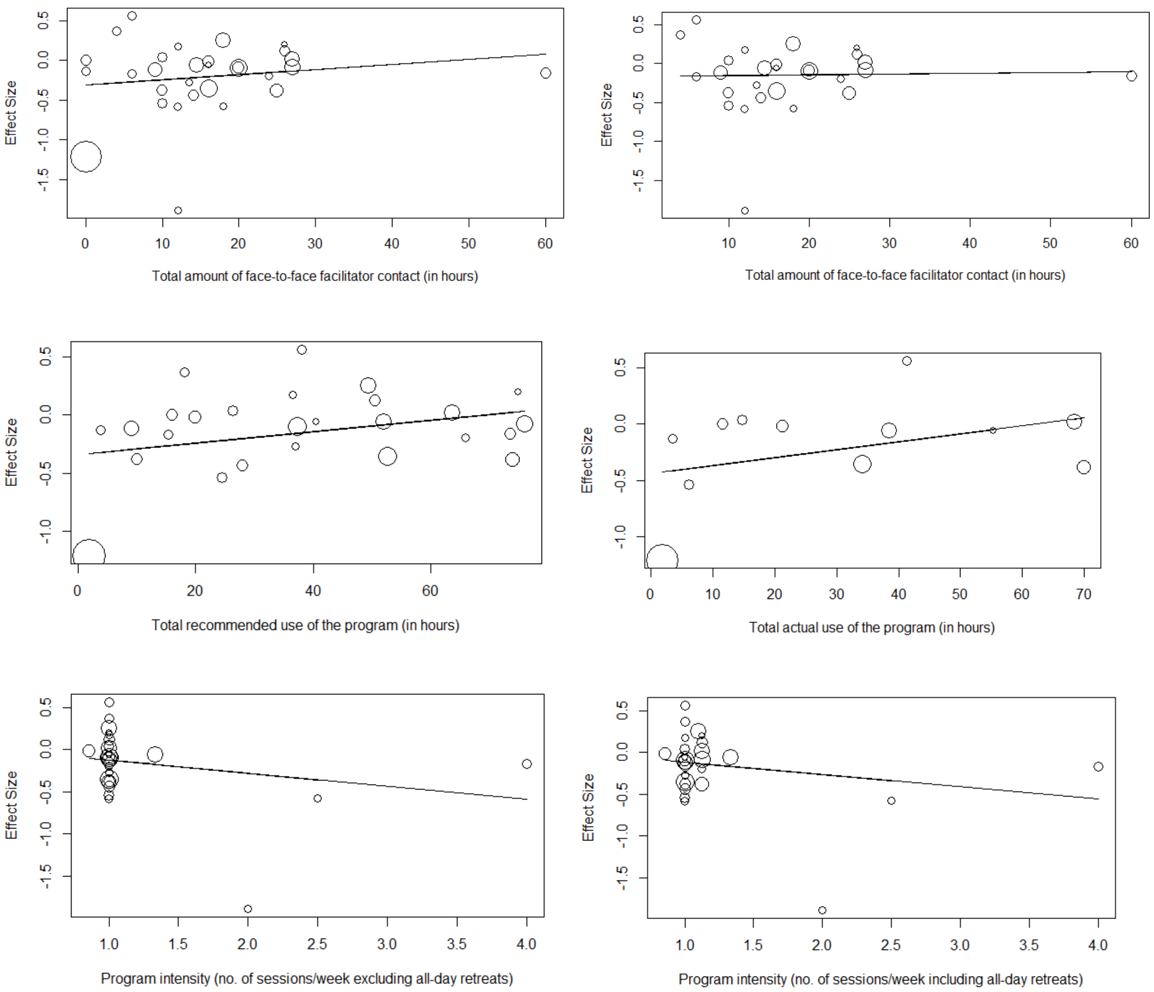
Compared to active controls – primary dose variables

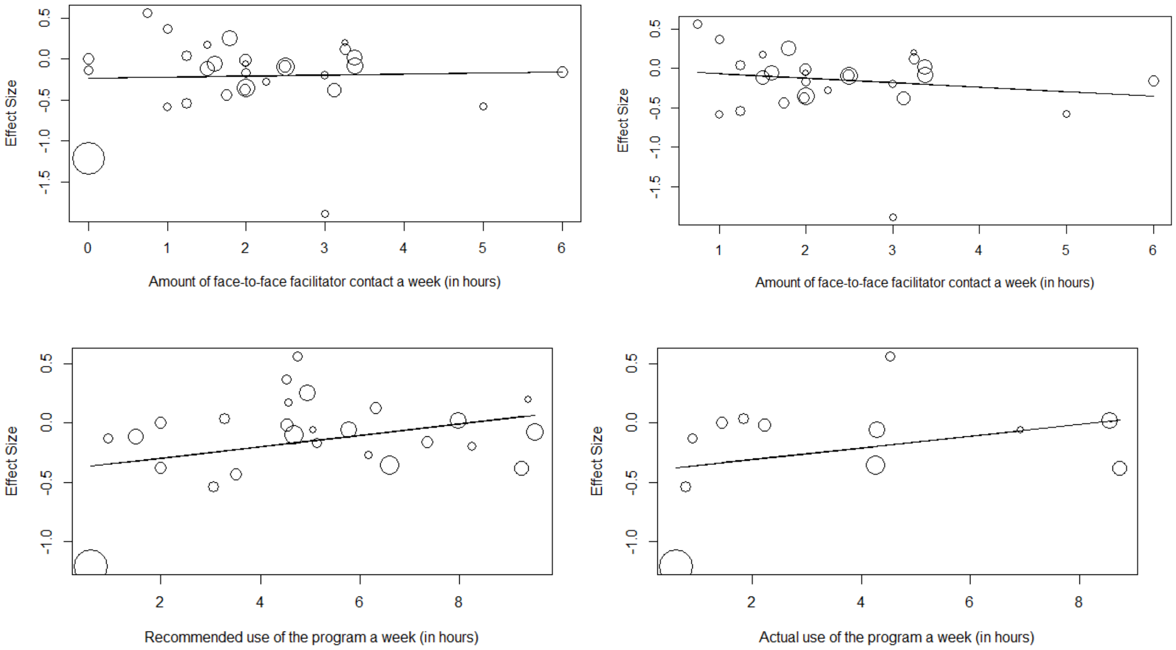






Compared to active controls – composite dose variables





Appendix Table 4.2.1.2

Step-by-step False Discovery Rate correction (Benjamini-Hochberg procedure) for depression at 1-4 months follow-up compared to inactive controls

p	**.037**	.105	.226	.284	.357	.515	.662	.743	.784	.858	.865	.873	.978
Rank	1	2	3	4	5	6	7	8	9	10	11	12	13
***p*$_{adj}$**	.325	.325	.327	.327	.327	.327	.945	.945	.945	.945	.945	.945	.978

Significant results in bold.

Appendix Table 4.2.1.3

Meta-regression analysis results by MBP dose for between-group depression effect sizes at 5-10 months follow-up compared to inactive and active controls

Compared to inactive control groups												
Dose	**Meta-regression model**						**Heterogeneity statistics**					
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	*SE* tau^2	Q_E	$p\ (Q_E)$
Total no. face-to-face sessions	0.03	.09	[-0.15, 0.21]	0.34	.74	21	0.12	0.00%	0.1	0.05	61.68	<.001
Duration of a face-to-face session	0.06	0.22	[-0.4, 0.53]	0.29	.77	21	0.09	0.00%	0.1	0.05	61.4	<.001
Program length	0.06	0.09	[-0.13, 0.24]	0.63	.54	21	0.4	0.00%	0.1	0.05	61.14	<.001
Frequency of recommended practice	0.12	0.12	[-0.12, 0.37]	1.05	.31	20	1.11	13.42%	0.08	0.04	49.14	<.001
Duration of a recommended practice	0.001	0.01	[-0.01, 0.02]	0.19	.85	20	0.04	0.00%	0.1	0.05	59.4	<.001
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	*SE* tau^2	Q_E	$p\ (Q_E)$
Total amount of contact	0.02	0.02	[-0.01, 0.05]	1.27	.22	21	1.62	6.39%	0.09	0.05	58.33	<.001
Total amount of contact (excl. 0 hours)**	-	-	-	-	-	-	-	-	-	-	-	-
Total recommended use of program	0.004	0.01	[-0.01, 0.2]	0.7	.5	20	0.48	0.00%	0.1	0.05	56.42	<.001
Total actual use of program*	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity excl. retreats***	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity incl. retreats	1.01	1.51	[-2.15, 4.17]	0.67	.5	21	0.45	0.00%	0.1	0.05	61.59	<.001
Amount of contact/week	0.13	0.11	[-0.11, 0.37]	1.13	.27	21	1.29	3.36%	0.09	0.05	58.84	<.001
Amount of contact (excl. 0 hours)/week**	-	-	-	-	-	-	-	-	-	-	-	-
Recommended use of program/week	0.03	0.05	[-0.07, 0.13]	0.65	.52	20	0.43	0.00%	0.1	0.05	56.53	<.001
Actual use of program/week*	-	-	-	-	-	-	-	-	-	-	-	-

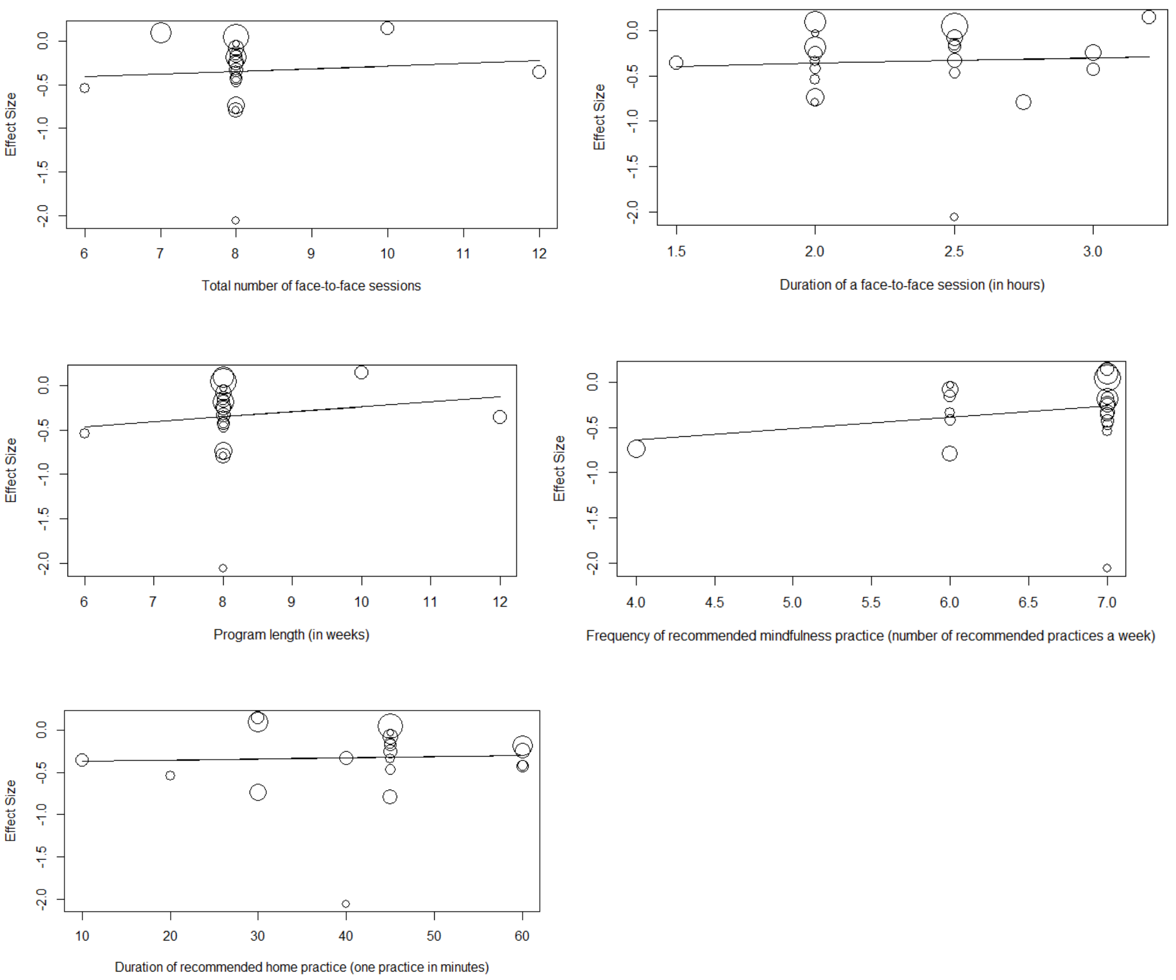
Compared to active control groups												
Dose	**Meta-regression model**						**Heterogeneity statistics**					
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total no. face-to-face sessions	<.001	0.03	[-0.07, 0.07]	0.01	.99	13	<.001	0.00%	0.02	0.03	16.1	.14
Duration of a face-to-face session	-0.27	0.2	[-0.7, 0.18]	-1.33	.21	12	1.76	16.865	0.02	0.03	13.71	.19
Program length	0.27	0.17	[-0.12, 0.65]	1.53	.15	13	2.35	25.98%	0.01	0.02	13.52	.26
Frequency of recommended practice	-0.27	0.28	[-0.67, 0.14]	-1.46	.18	12	2.12	0.00%	0.02	0.03	13.46	.2
Duration of a recommended practice	0.01	0.01	[-0.001, 0.03]	2.09	.06	12	4.35	69.1%	0.01	0.02	10.87	.37
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total amount of contact	<.001	.01	[-0.02, 0.02]	-0.003	.99	13	<.001	0.00%	0.02	0.03	16.08	.14
Total amount of contact (excl. 0 hours)	0.003	0.01	[-0.03, 0.03]	0.22	.83	12	0.05	0.00%	0.03	0.03	15.82	.11
Total recommended use of program	0.001	0.004	[-0.01, 0.01]	0.3	.77	12	0.09	0.00%	0.02	0.03	15.43	.12
Total actual use of program*	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity excl. retreats***	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity incl. retreats***	-	-	-	-	-	-	-	-	-	-	-	-
Amount of contact/week	-0.004	0.07	[-0.15, 0.15]	-0.06	.95	13	0.004	0.00%	0.02	0.03	16.09	.14
Amount of contact (excl. 0 hours)/week	0.01	0.1	[-0.21, 0.24]	0.14	.89	12	0.02	0.00%	0.03	0.03	15.88	.1
Recommended use of program/week	0.01	0.04	[-0.07, 0.08]	0.2	.84	12	0.04	0.00%	0.02	0.03	15.48	.12
Actual use of program/week*	-	-	-	-	-	-	-	-	-	-	-	-

*$k<10$; **no studies had zero hours of contact; ***all studies had the same score dose for this variable; *d*=effect size of the standardized regression coefficient, SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value= test statistic of slope, *p*-value= significance level; *k*=number of studies; *F*-distribution= test for the overall model; R^2= percentage of heterogeneity accounted for, tau^2/τ^2= variance of the underlying true effect sizes; *SE* tau^2= standard error of tau^2; Q_E= between-study heterogeneity; $p(Q_E)$)=Q_E significance level.

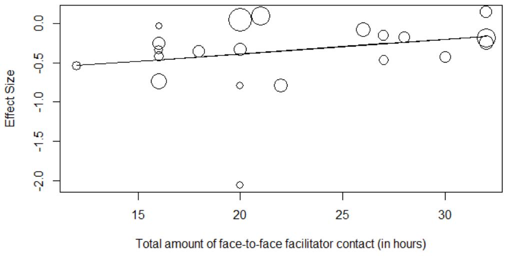
Appendix Figure 4.2.1.4

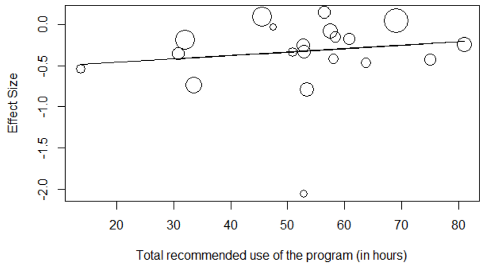
Meta-regression plots for depression at 5-10 months follow-up compared to inactive and active controls for the doses total number of face-to-face sessions, duration of a face-to-face session (in hours), program length (in weeks), frequency (number) of recommended home practices a week, duration of a recommended home practice (one practice in minutes), total amount of face-to-face facilitator contact (in hours), total amount of face-to-face facilitator contact with zero hours of contact excluded (in hours; compared to active controls only), total recommended use of the program (in hours), program intensity (number of sessions a week including all-day retreats; compared to inactive controls only), amount of face-to-face facilitator contact a week (in hours), amount of face-to-face facilitator contact excluding zero contact a week (in hours; compared to active controls only) and recommended use of the program a week (in hours).

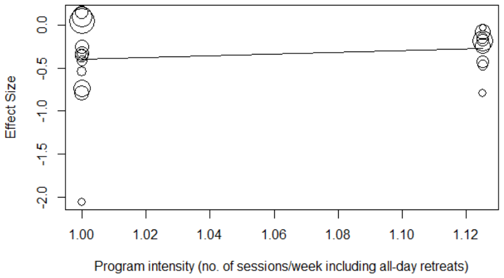
Compared to inactive controls – primary dose variables

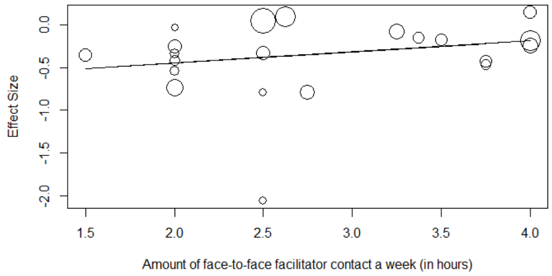


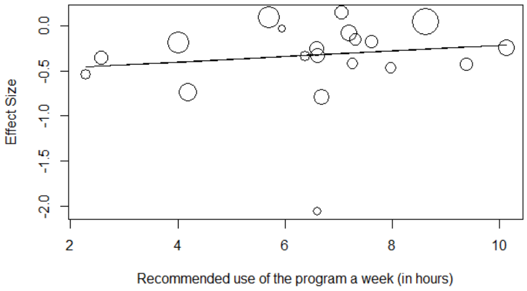
Compared to inactive controls – composite dose variables



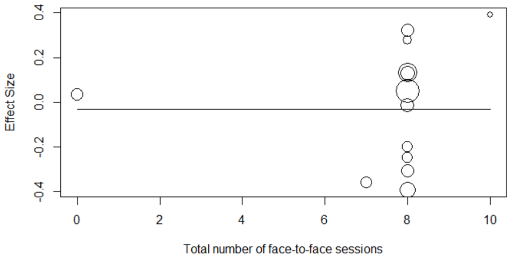


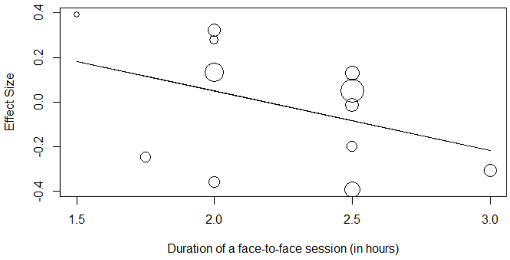


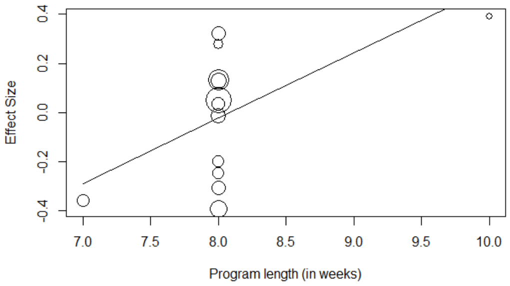


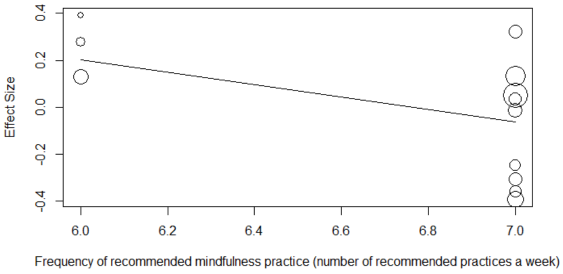


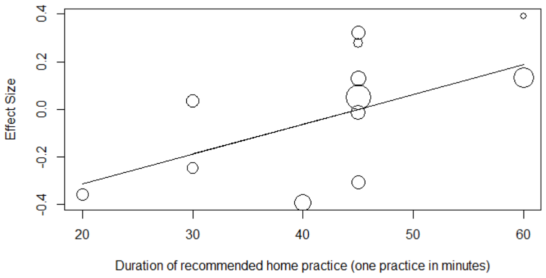
Compared to active controls – primary dose variables



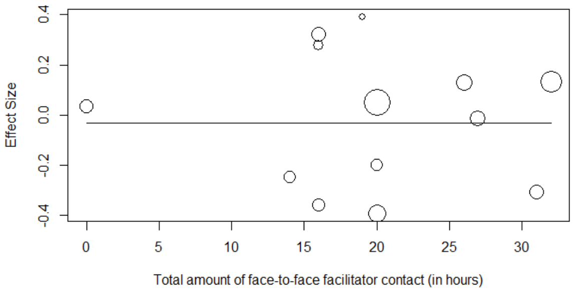


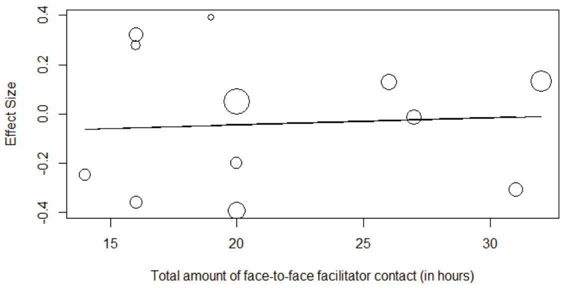


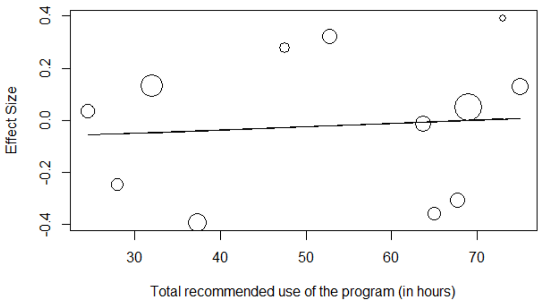


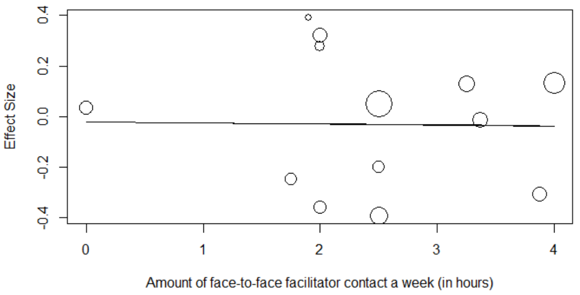


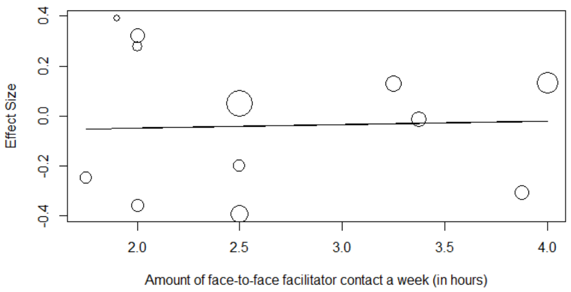
Compared to active controls – composite dose variables

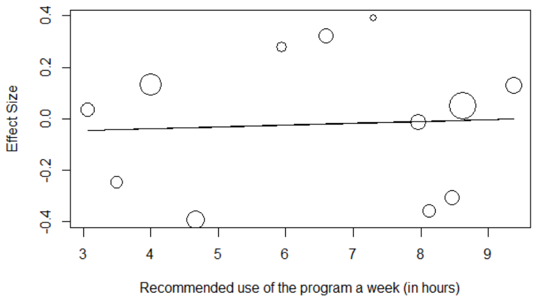








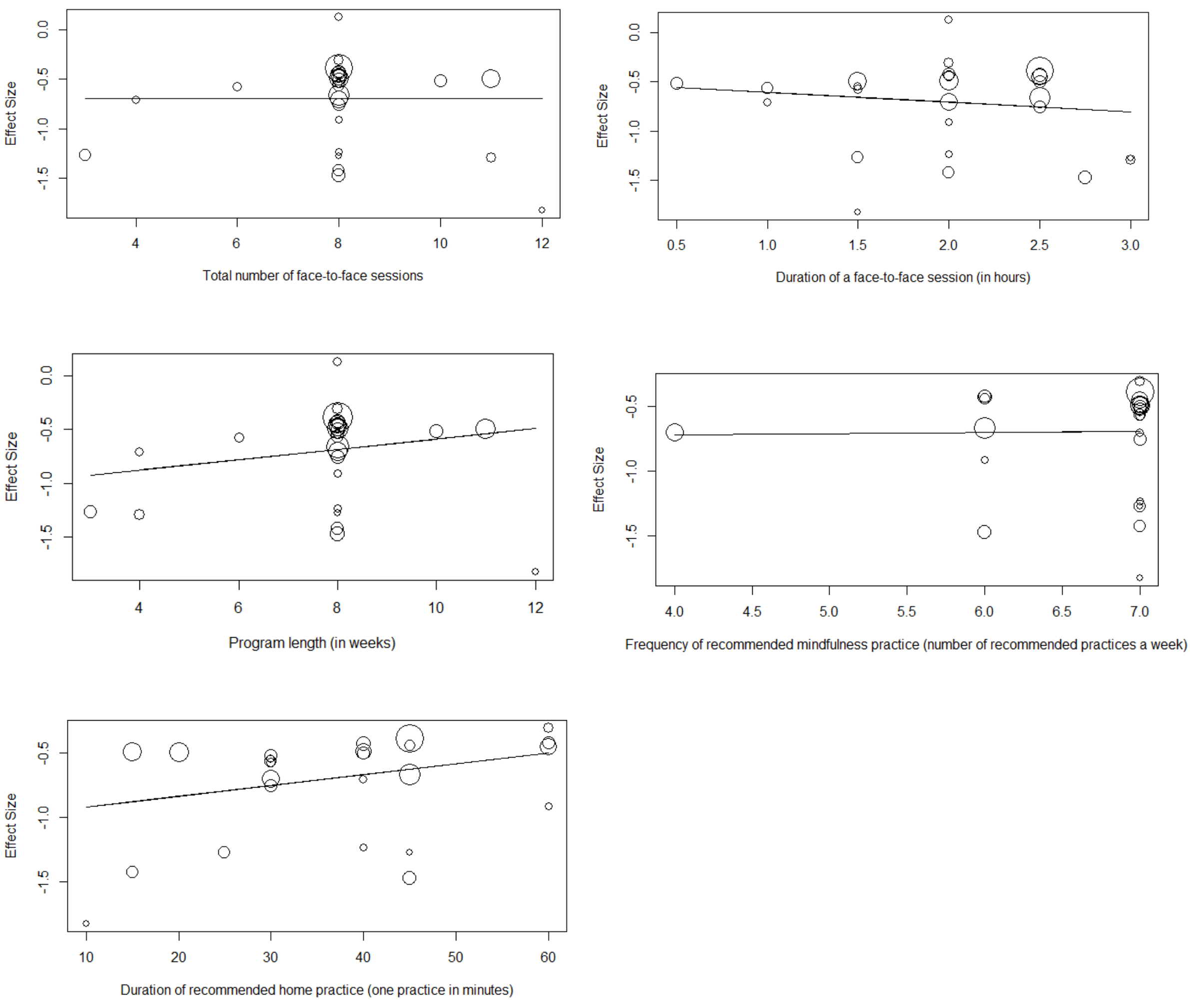




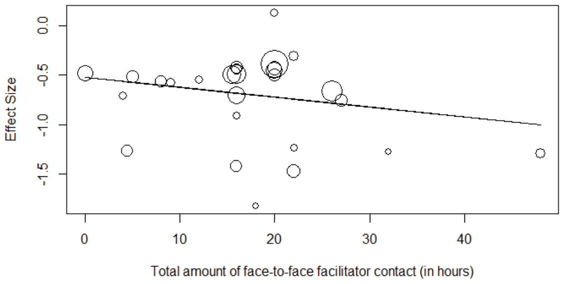
Appendix Figure 4.2.1.5

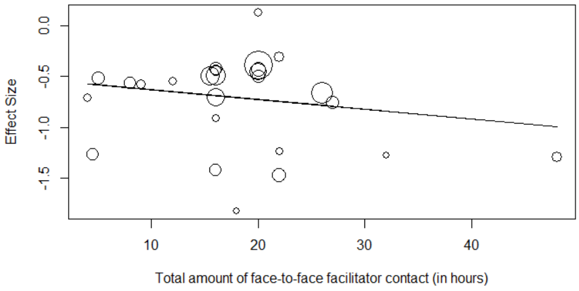
Meta-regression plots for depression at post-program compared to inactive controls for the depression population for the doses total number of face-to-face sessions, duration of a face-to-face session (in hours), program length (in weeks), frequency (number) of recommended home practices a week, duration of a recommended home practice (one practice in minutes), total amount of face-to-face facilitator contact (in hours), total amount of face-to-face facilitator contact with zero hours of contact excluded (in hours), total recommended use of the program (in hours), program intensity (number of sessions a week including all-day retreats), amount of face-to-face facilitator contact a week (in hours), amount of face-to-face facilitator contact excluding zero contact a week (in hours) and recommended use of the program a week (in hours).

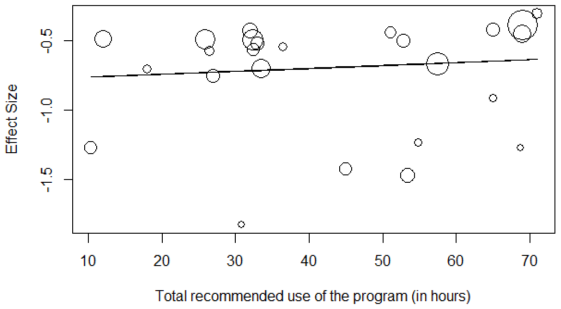
Primary dose variables

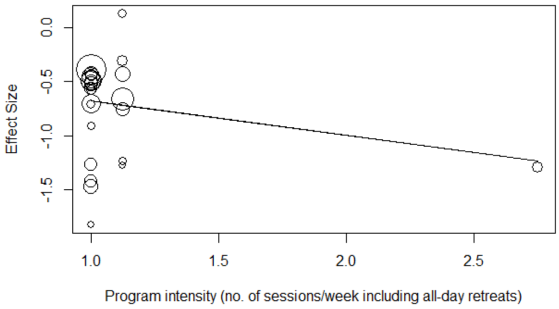


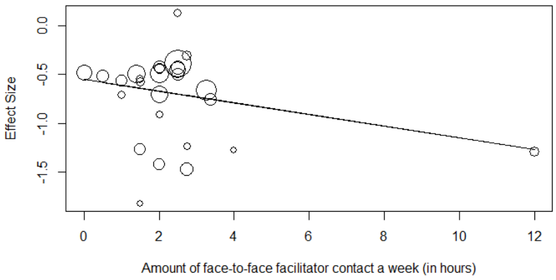
Composite dose variables

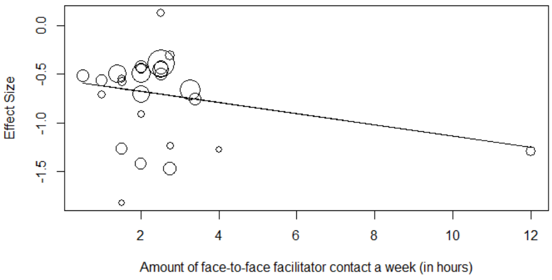


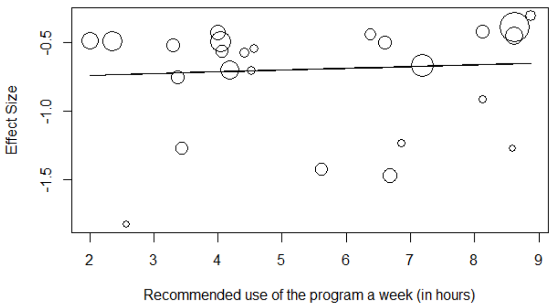








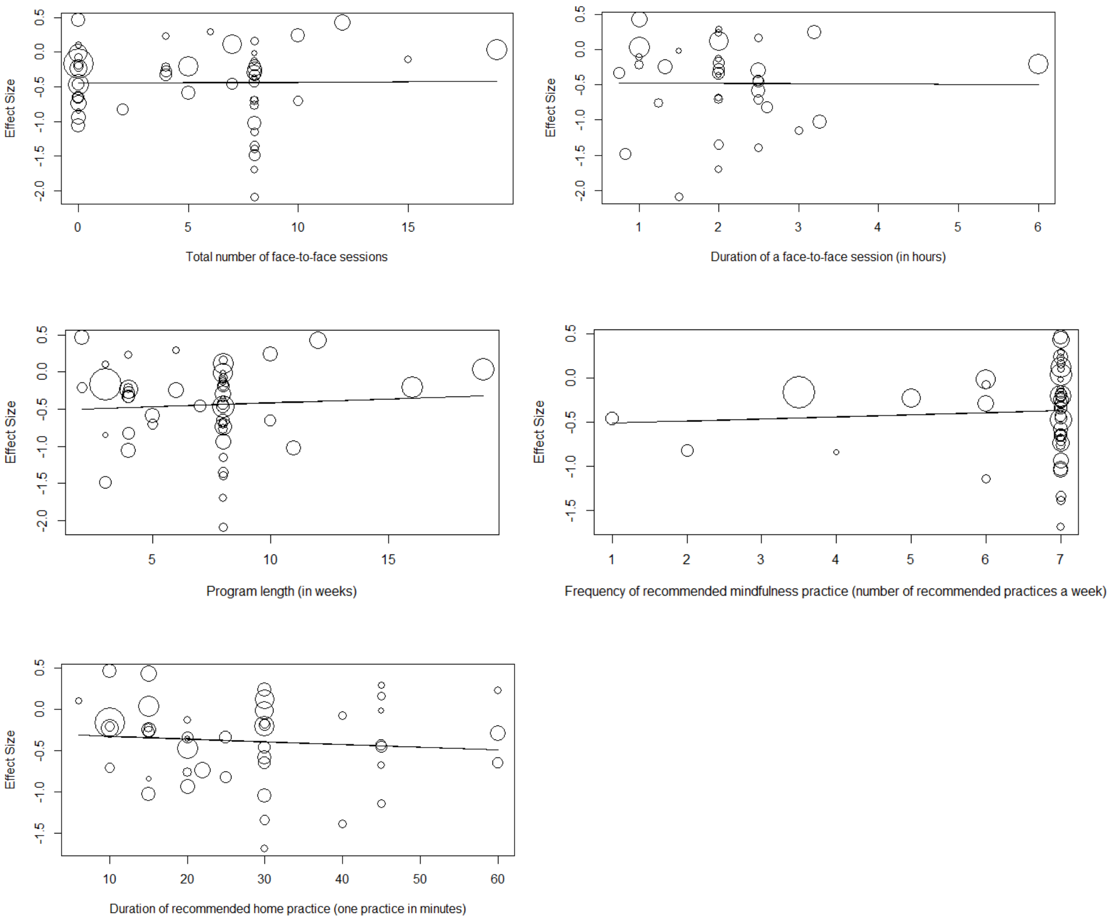




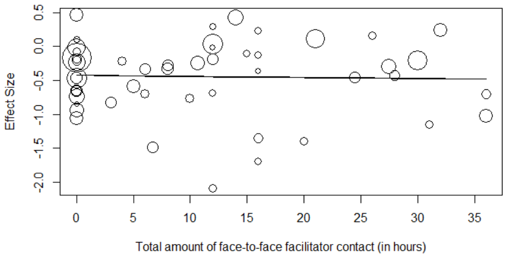
Appendix Figure 4.2.1.6

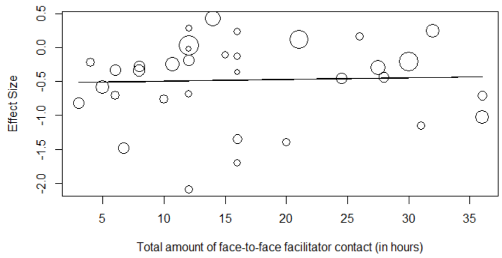
Meta-regression plots for depression at post-program compared to inactive controls for the general population for the doses total number of face-to-face sessions, duration of a face-to-face session (in hours), program length (in weeks), frequency (number) of recommended home practices a week, duration of a recommended home practice (one practice in minutes), total amount of face-to-face facilitator contact (in hours), total amount of face-to-face facilitator contact with zero hours of contact excluded (in hours), total recommended use of the program (in hours), total actual use of the program (in hours), program intensity (number of sessions a week excluding all-day retreats), program intensity (number of sessions a week including all-day retreats), amount of face-to-face facilitator contact a week (in hours), amount of face-to-face facilitator contact excluding zero contact a week (in hours), recommended use of the program a week (in hours) and actual use of the program a week (in hours)

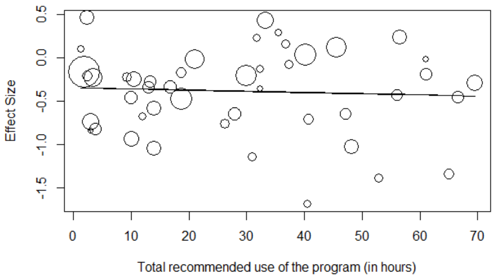
Primary dose variables

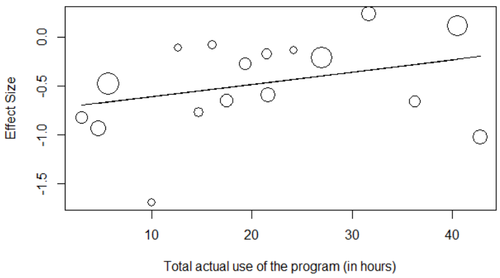


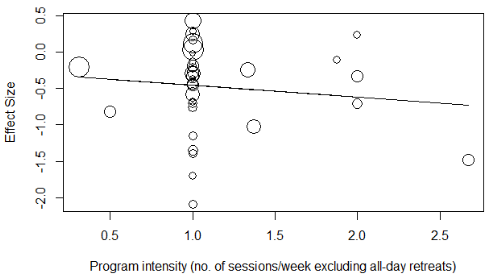
Composite dose variables

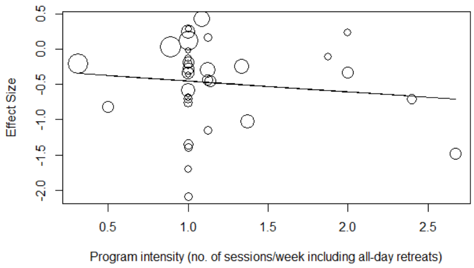


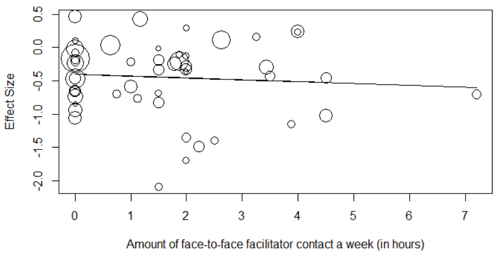


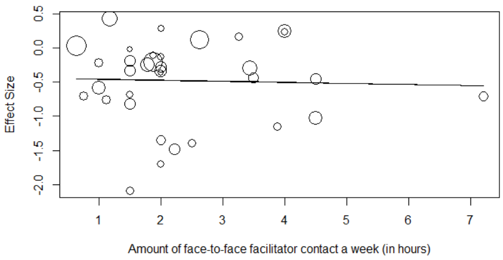


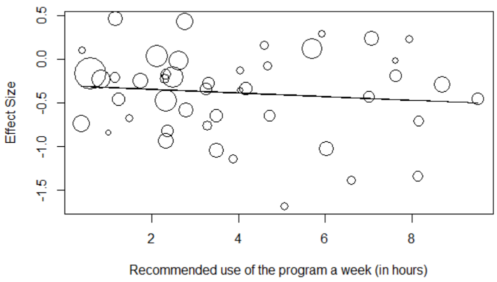


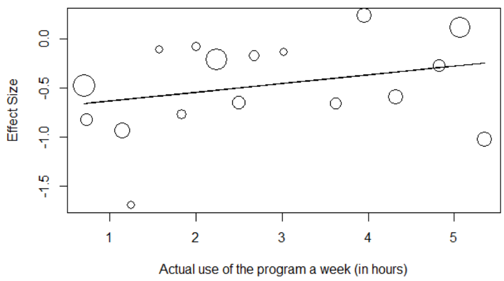












Appendix Table 4.2.1.7

Clinical significance analysis for BDI depression; top: classification of low and high doses for each dose, bottom: clinical significance analysis for low and high dose at baseline (BL), post-program (Post) and follow-up (FU)

Dose	Low dose	High dose
Total no. face-to-face sessions	≤8	>8
Duration of a face-to-face session	≤2	>2
Program length	<8	≥8
Frequency of recommended practice	6	7
Duration of a recommended practice	<45	≥45
Total amount of contact	<20	≥20
Total recommended use of program	<55	>55
Program intensity incl. retreats	1	>1
Amount of contact/week	≤2	>2
Recommended use of program/week	<6	>6

k<2 studies for actual use of program doses; no doses had zero hours of contact;
all studies had the same dose for intensity (excl. retreats).

Dose	Low dose	High dose
Total no. face-to-face sessions	Bl: 17.416 Post: 11.44 FU: 17.74	Bl:25.67 Post: 17.66 FU**
Duration of a face-to-face session	Bl: 23.06 Post: 14.36 FU: 17.74	Bl: 15.98 Post: 10.619 FU**
Program length	Bl: 24.86 Post: 11.56 FU: 12.43	Bl: 17.44 Post: 12.24 FU**
Frequency of recommended practice	Bl: 13.08 Post: 9.62 FU*	Bl: 21.46 Post: 13.75 FU:17.74
Duration of a recommended practice	Bl: 28.89 Post: 17.91 FU: 17.74	Bl: 13.44 Post: 9.54 FU*
Total amount of contact	Bl: 24.66 Post: 15.29 FU: 17.74	Bl: 14.93 Post: 10.43 FU**
Total recommended use of program	Bl: 24.6 Post: 15.73 FU: 17.74	Bl: 14.34 Post: 9.93 FU*
Program intensity incl. retreats	Bl: 19.62 Post: 13.09 FU: 17.74	Bl: 14.86 Post: 9.56 FU**
Amount of contact/week	Bl: 24.66 Post: 15.29 FU: 17.74	Bl: 14.9 Post: 10.43 FU**
Recommended use of program/week	Bl: 28.57 Post: 16.95 FU: 17.74	Bl: 14.76 Post: 10.56 FU*

*k=0; **k=1

Appendix Table 4.2.1.8

Interaction effects between doses and depression severity (mild vs. severe) for depression at post-program

Dose x severity	Meta-regression model						Heterogeneity statistics					
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total no. face-to-face sessions	-0.03	0.11	[-0.26, 0.21]	-0.22	.82	23	2.66	30.09%	0.07	0.05	42.28	<.01
Duration of a face-to-face session	0.01	0.3	[-0.61, 0.64]	0.05	.96	23	3.47	40.66%	0.06	0.04	39.49	<.01
Program length	0.01	0.11	[-0.22, 0.24]	0.09	.93	23	2.71	36.13%	0.07	0.05	40.96	<.01
Frequency of recommended practice	-0.01	0.29	[-0.61, 0.6]	-0.02	.99	22	1.55	13.83%	0.07	0.04	39.52	<.01
Duration of a recommended practice	0.01	0.01	[-0.01, 0.04]	1.13	.27	22	2.5	27.45%	0.06	0.04	37.49	<.01
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total amount of contact	0.001	0.02	[-0.05, 0.5]	0.05	.96	23	3.52	43.34%	0.06	0.04	39.02	<.01
Total amount of contact (excl. 0 hours)**	-	-	-	-	-	-	-	-	-	-	-	-
Total recommended use of program	-0.01	0.01	[-0.03, 0.01]	-0.91	.37	22	1.85	24.75%	0.06	0.04	37.88	<.01
Total actual use of program*	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity excl. retreats***	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity incl. retreats	-0.16	1.95	[-4.23, 3.92]	-0.08	.94	23	2.88	36.47%	0.07	0.04	40.87	<.01
Amount of contact/week	0.07	0.16	[-0.28, 0.4]	0.37	0.71	23	3.38	42.92%	0.06	0.04	39.21	<.01
Amount of contact (excl. 0 hours)/week**	-	-	-	-	-	-	-	-	-	-	-	-
Recommended use of program/week	-0.09	0.08	[-0.25, 0.07]	-1.15	.27	22	2.13	36.31%	0.05	0.04	35.71	<.01
Actual use of program/week*	-	-	-	-	-	-	-	-	-	-	-	-

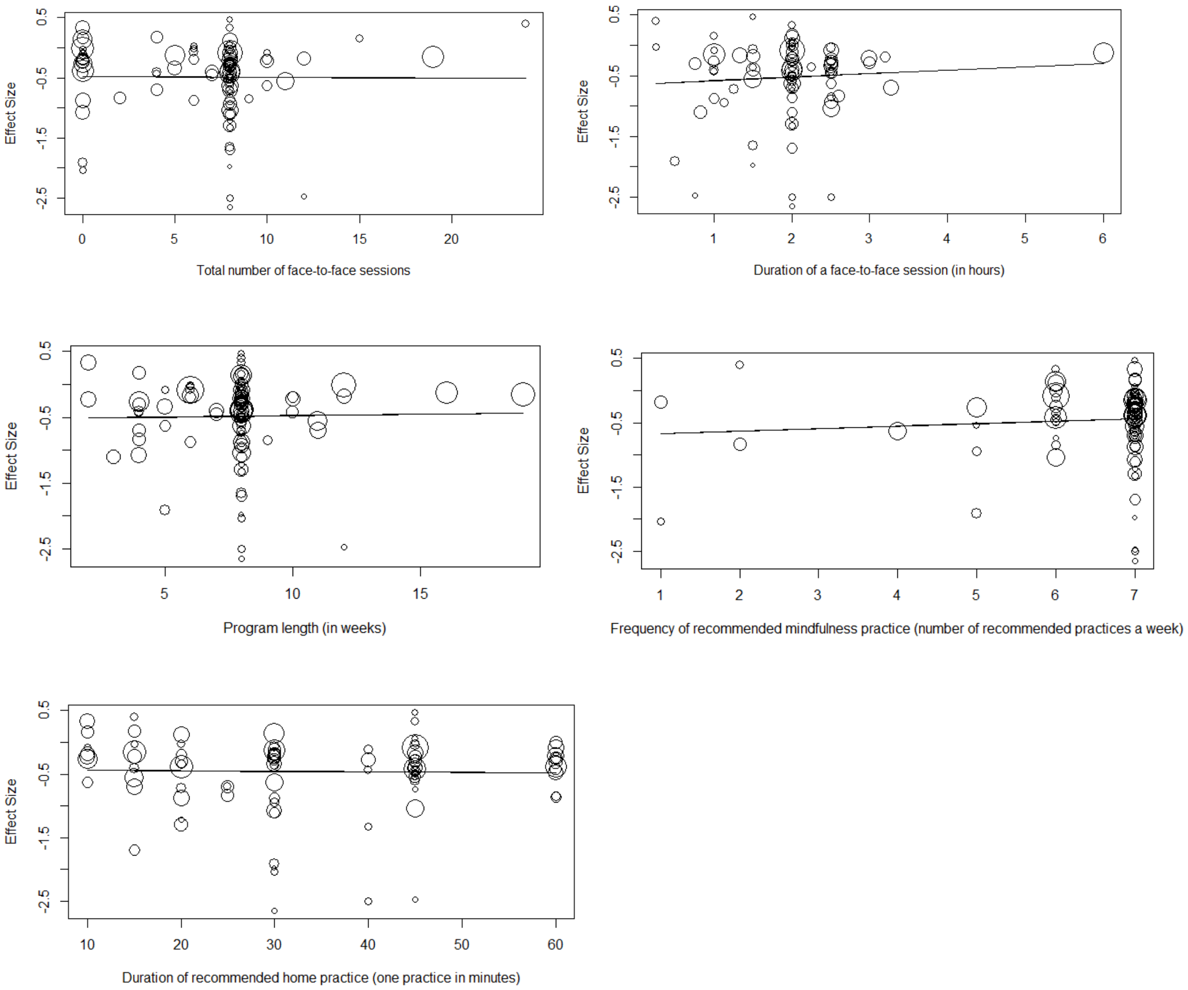
*k<10; **no studies had zero hours of contact;***all but 2 studies had the same score on this dose;*d*=effect size of the standardized regression coefficient, SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value= test statistic of slope, *p*-value= significance level; *k*=number of studies; *F*-distribution= test for the overall model; R^2= percentage of heterogeneity accounted for, tau^2/τ^2= variance of the underlying true effect sizes; *SE* tau^2= standard error of tau^2; Q_E= between-study heterogeneity; $p(Q_E)$)=Q_E significance level.

Appendix 4.2.2 Dose-Response Meta-Regression Results: Anxiety

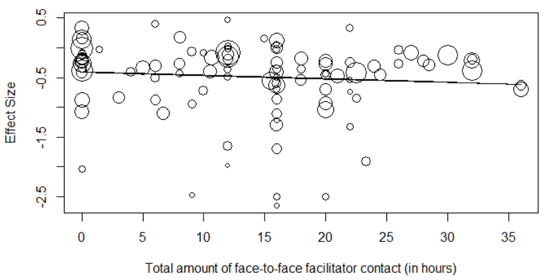
Appendix Figure 4.2.2.1

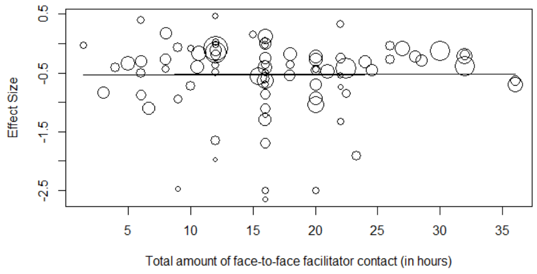
Meta-regression plots for anxiety at post-program compared to inactive and active controls for the doses total number of face-to-face sessions, duration of a face-to-face session (in hours), program length (in weeks), frequency (number) of recommended home practices a week, duration of recommended home practice (one practice in minutes), total amount of face-to-face facilitator contact (in hours), total amount of face-to-face facilitator contact with zero hours of contact excluded (in hours), total recommended use of the program (in hours), total actual use of the program (in hours), program intensity (number of sessions a week excluding all-day retreats), program intensity (number of sessions a week including all-day retreats), amount of face-to-face facilitator contact a week (in hours), amount of face-to-face facilitator contact excluding zero contact a week (in hours), recommended use of the program a week (in hours) and actual use of the program a week (in hours).

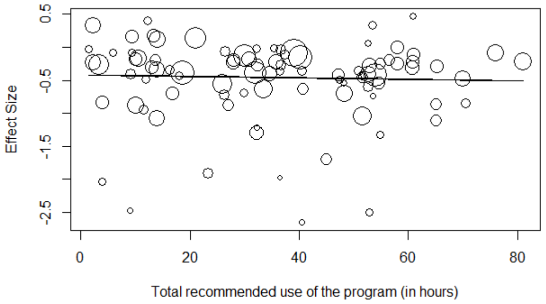
Compared to inactive controls – primary dose variables

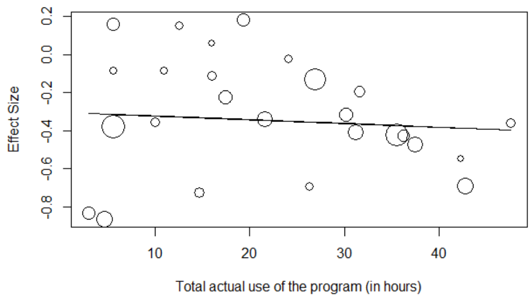


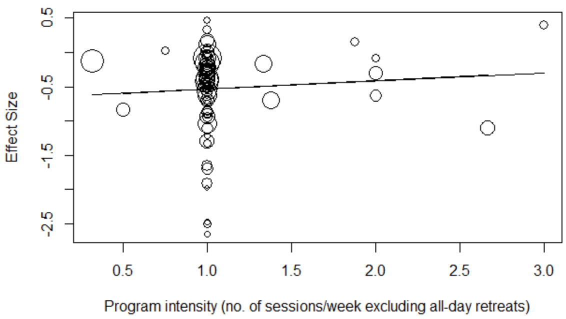
Compared to inactive controls – composite dose variables

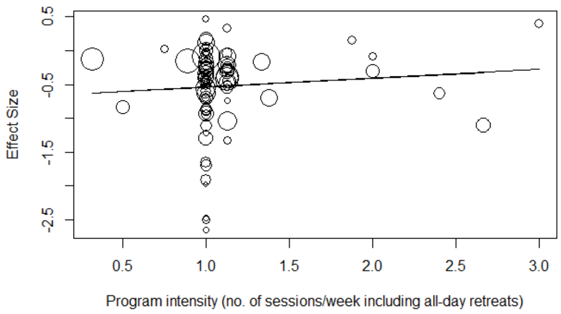


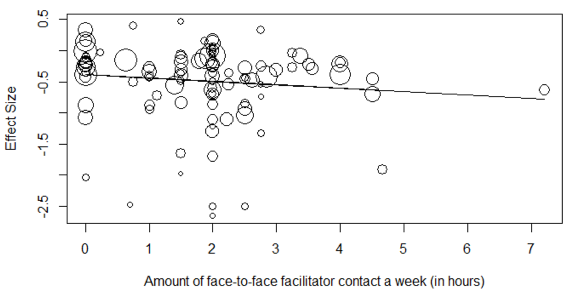


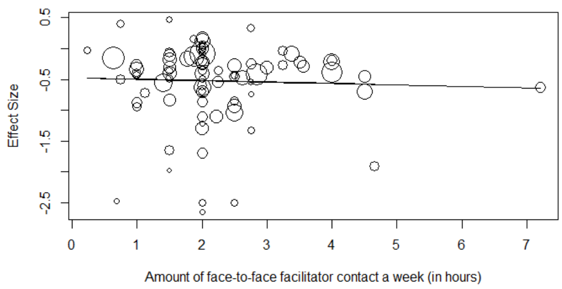


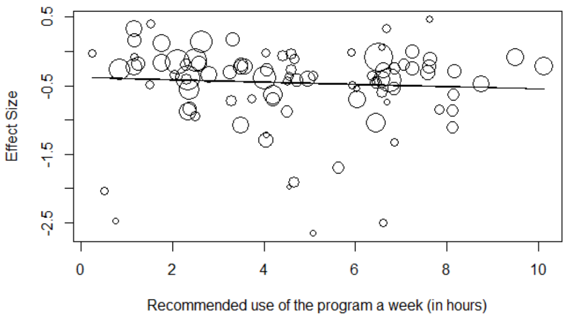


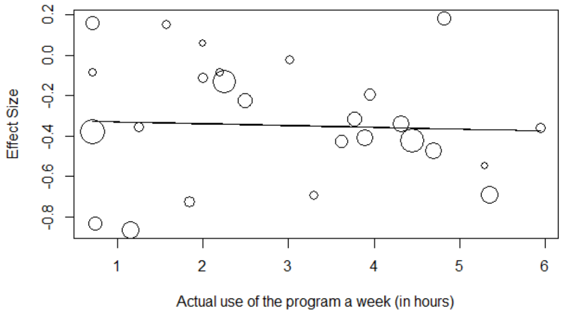




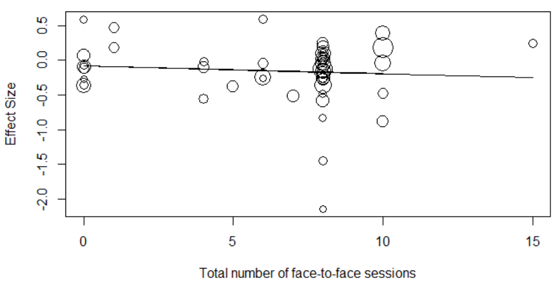


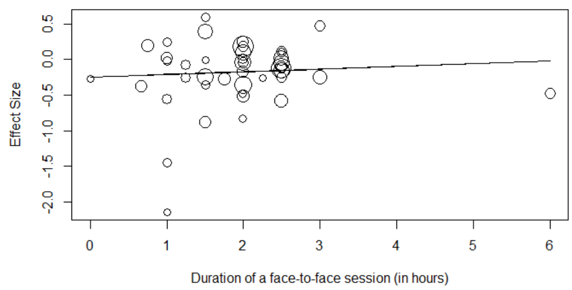


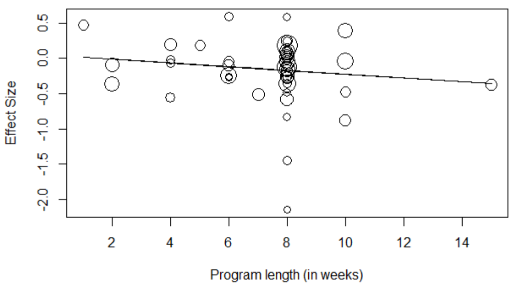


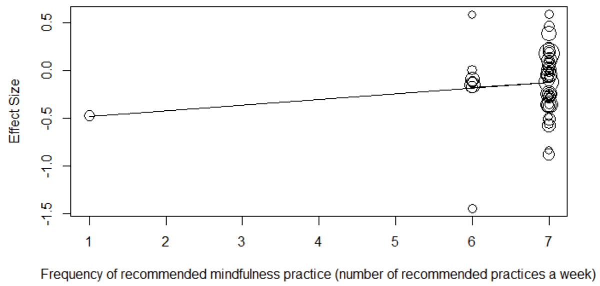


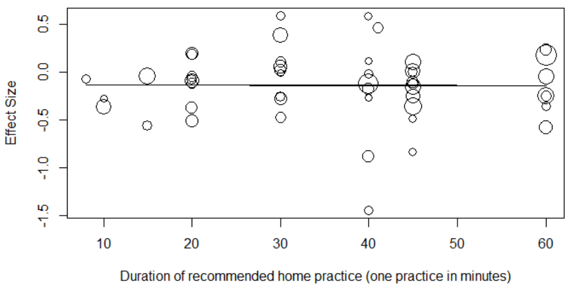
Compared to active controls – primary dose variables



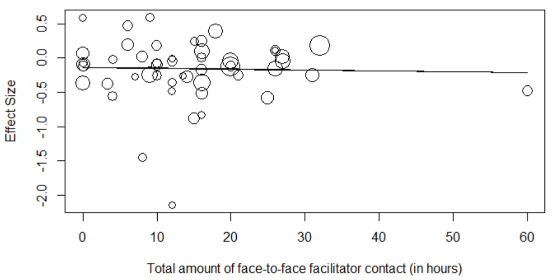


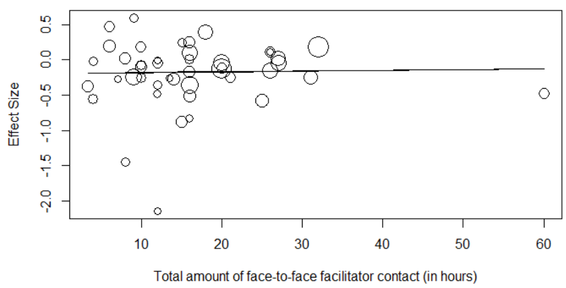


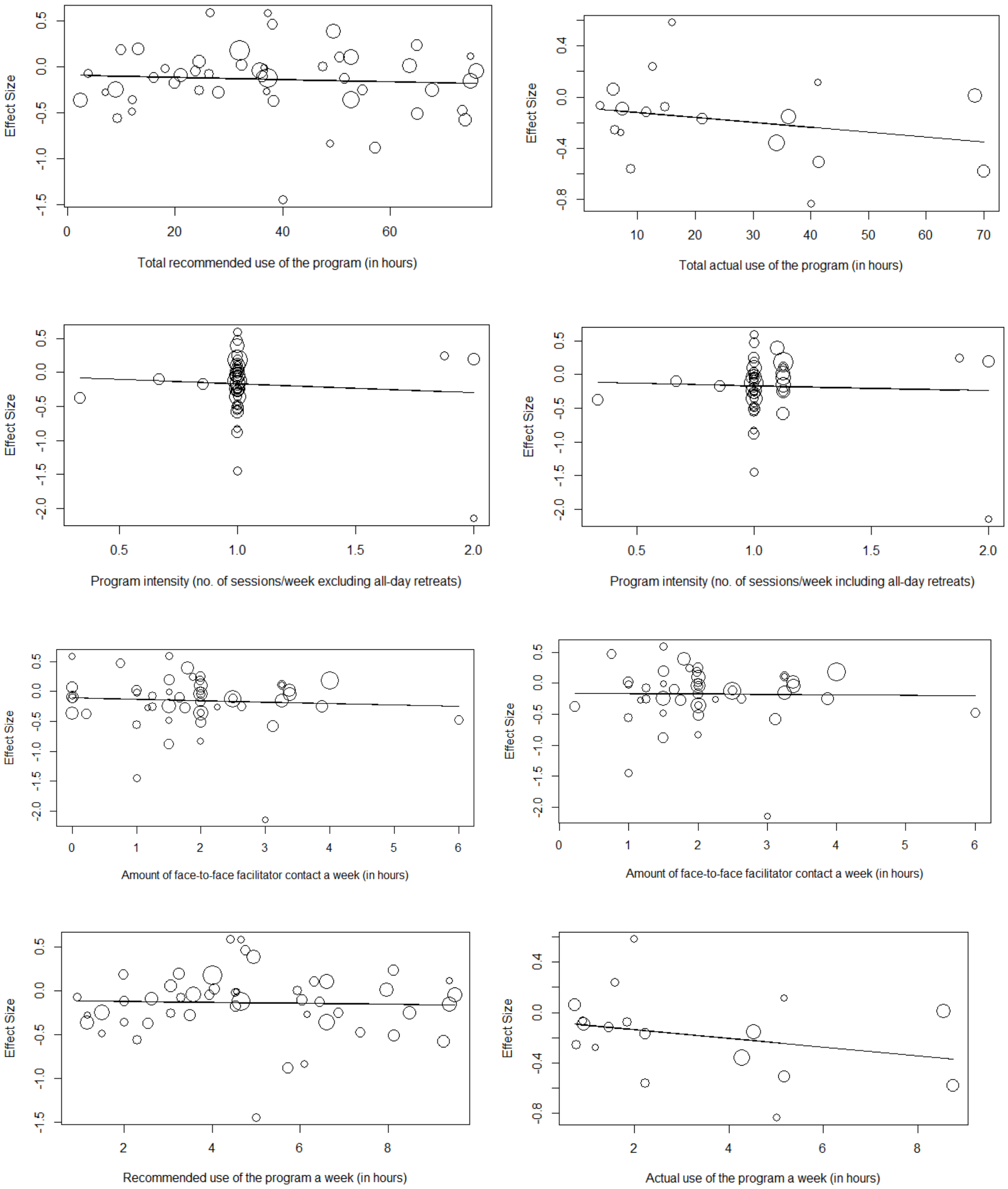




Compared to active controls – composite dose variables



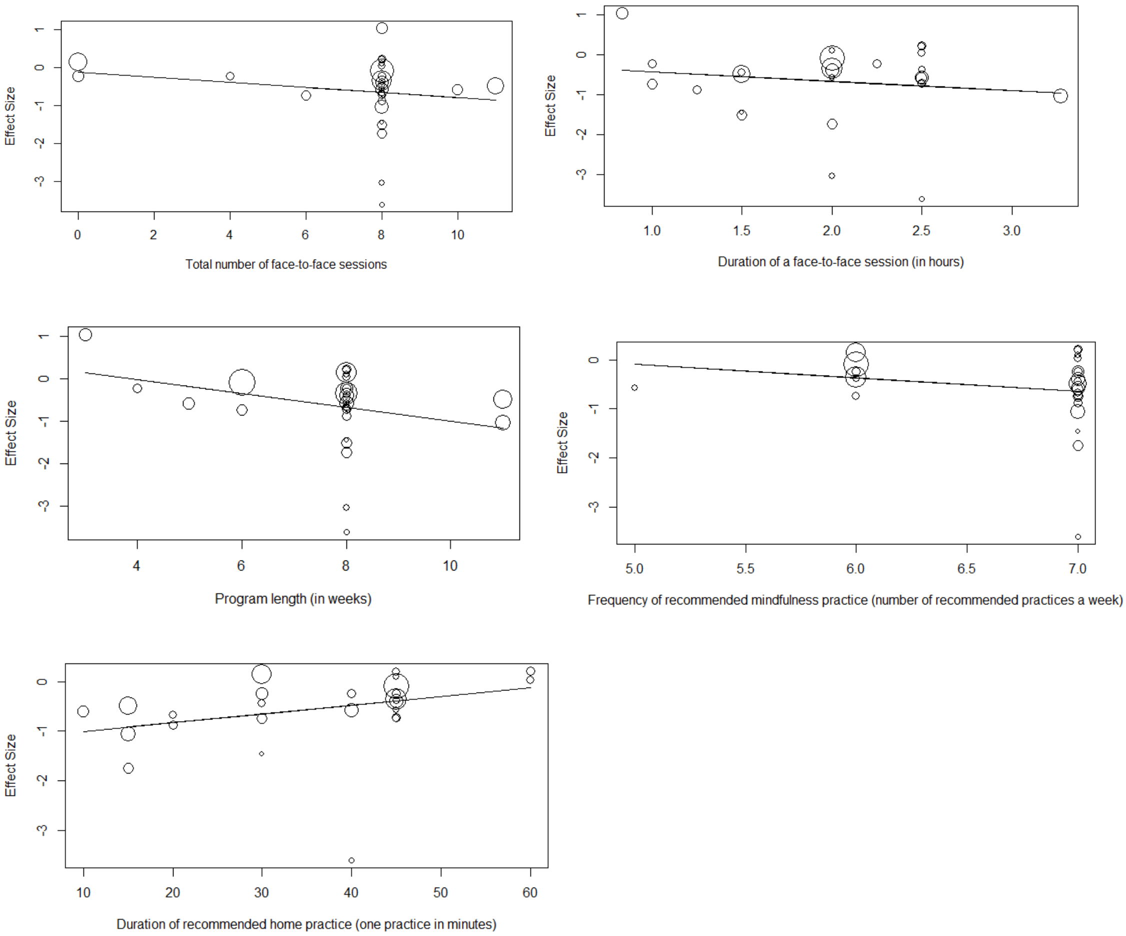



Effect Size
Total recommended use of the program (in hours)
Effect Size
Total actual use of the program (in hours)
Effect Size
Program intensity (no. of sessions/week excluding all-day retreats)
Effect Size
Program intensity (no. of sessions/week including all-day retreats)
Effect Size
Amount of face-to-face facilitator contact a week (in hours)
Effect Size
Amount of face-to-face facilitator contact a week (in hours)
Effect Size
Recommended use of the program a week (in hours)
Effect Size
Actual use of the program a week (in hours)

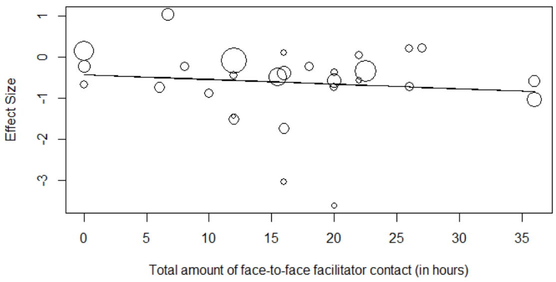
Appendix Figure 4.2.2.2

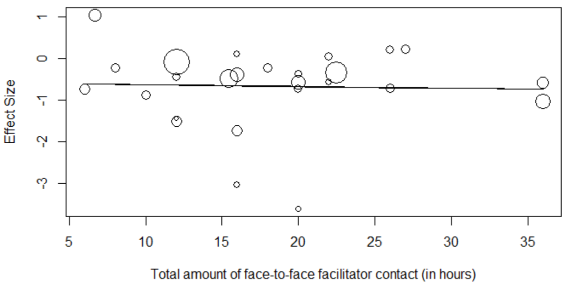
Meta-regression plots for anxiety at 1-4 months follow-up compared to inactive and active controls for the doses total number of face-to-face sessions, duration of a face-to-face session (in hours), program length (in weeks), frequency (number) of recommended home practices a week (compared to inactive controls only), duration of recommended home practice (one practice in minutes), total amount of face-to-face facilitator contact (in hours), total amount of face-to-face facilitator contact with zero hours of contact excluded (in hours), total recommended use of the program (in hours), total actual use of the program (in hours; compared to active controls only), program intensity (number of sessions a week excluding all-day retreats; compared to inactive controls only), program intensity (number of sessions a week including all-day retreats; compared to inactive controls only), amount of face-to-face facilitator contact a week (in hours), amount of face-to-face facilitator contact excluding zero contact a week (in hours), recommended use of the program a week (in hours) and actual use of the program a week (in hours; compared to active controls only).

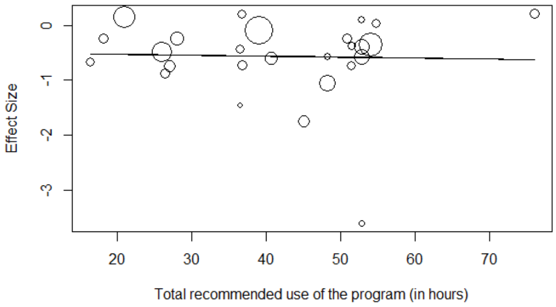
Compared to inactive controls – primary dose variables

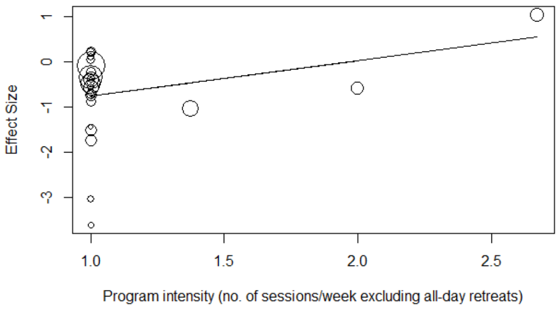


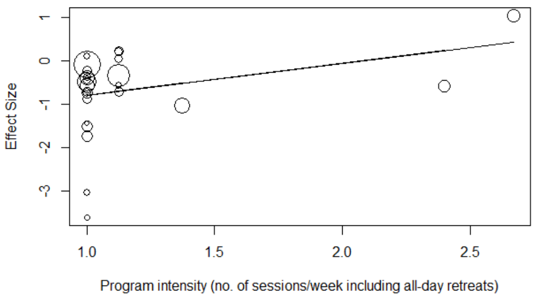
Compared to inactive controls – composite dose variables

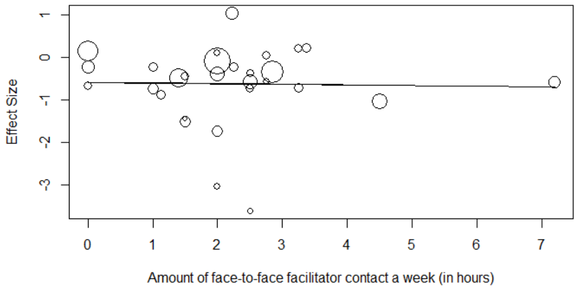


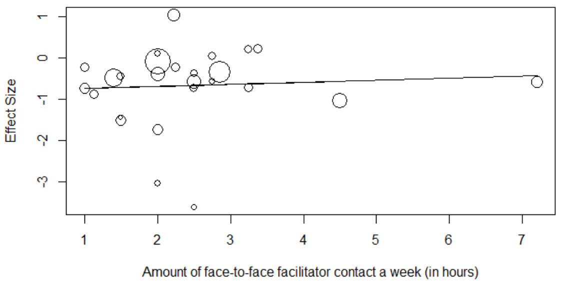


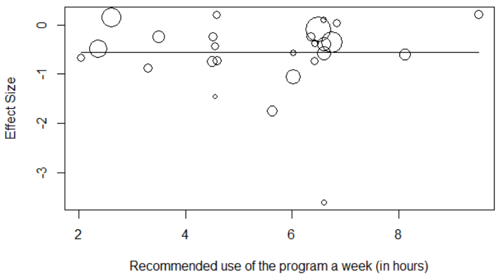




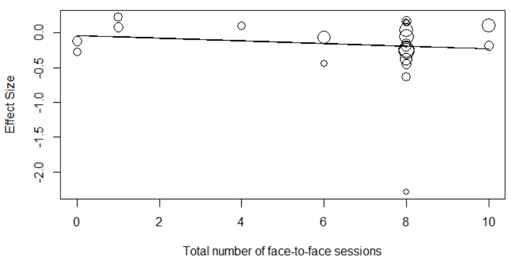


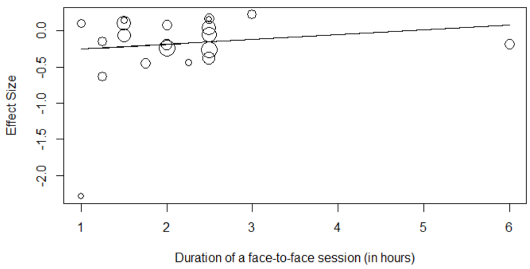


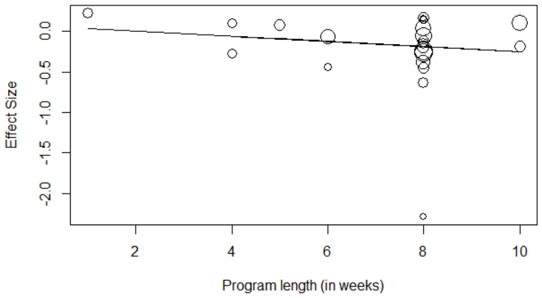


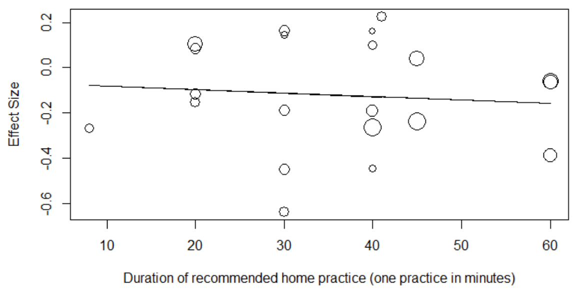


Compared to active controls – primary dose variables

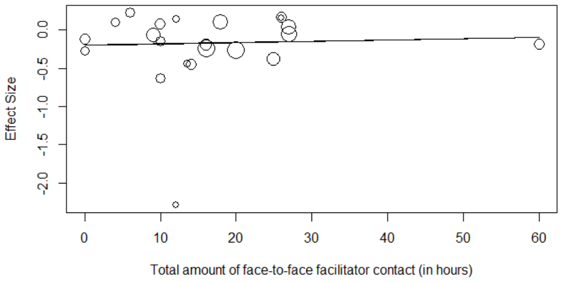


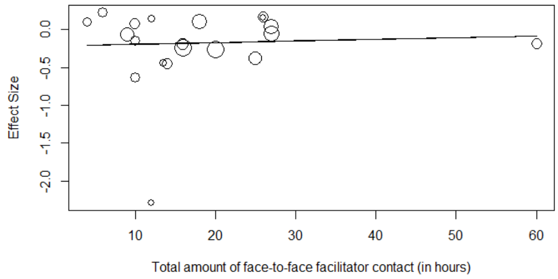


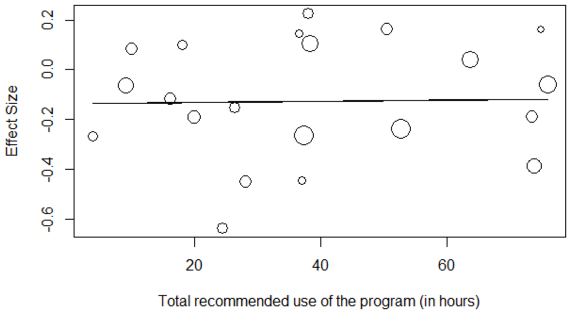


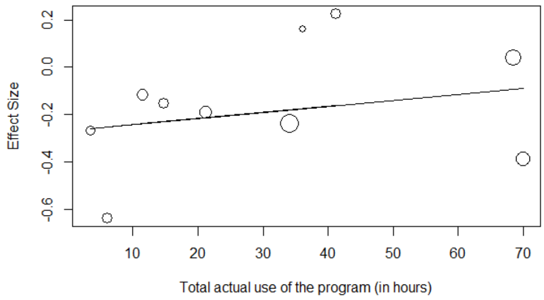


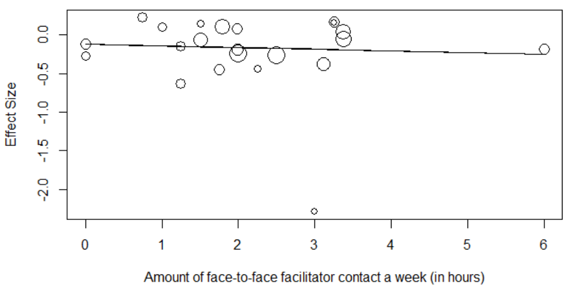
Compared to active controls – composite dose variables

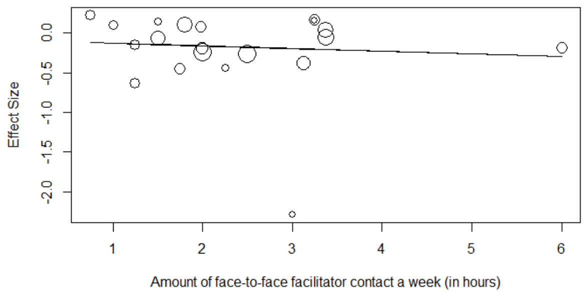


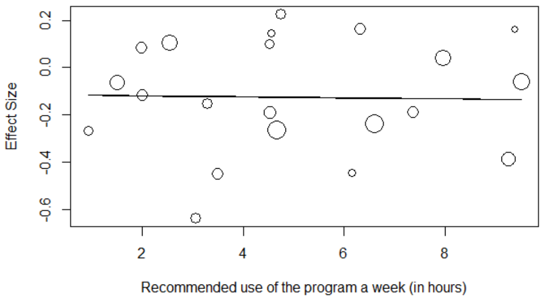












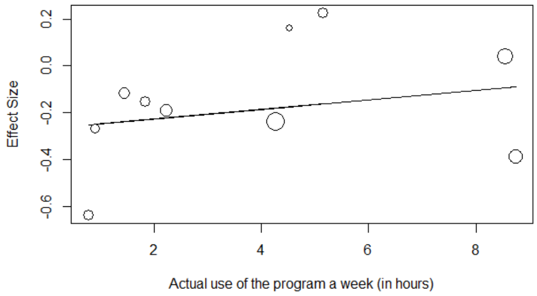


Table 4.2.2.3

Step-by-step False Discovery Rate correction (Benjamini-Hochberg procedure) for anxiety at 1-4 months follow-up compared to active controls

p	**.001**	.43	.44	.47	.48	.51	.65	.7	.77	.81	.81	.9	.91
Rank	1	2	3	4	5	6	7	8	8	10	11	12	13
p_{adj}	**.01**	.07	.1	.14	,19	.23	.35	.43	.53	.62	.69	.84	.91

Significant results in bold.

Appendix Table 4.2.2.4

Meta-regression analysis results by MBP dose for between-group anxiety effect sizes at 5-10 months follow-up compared to inactive controls

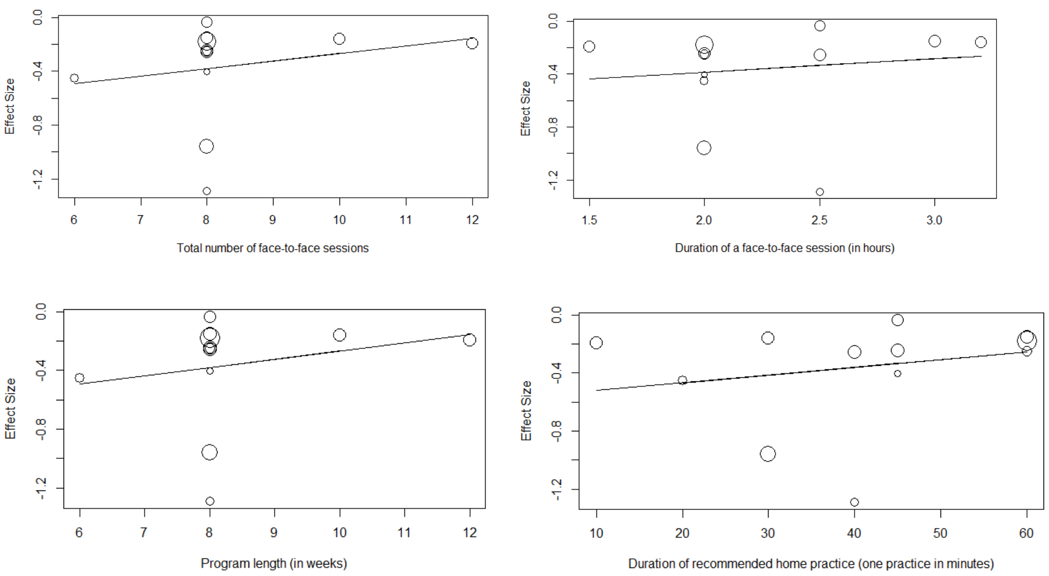
Dose	**Meta-regression model**						**Heterogeneity statistics**					
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total no. face-to-face sessions	0.06	0.08	[-0.11, 0.23]	0.73	.48	12	0.54	0.00%	0.07	.06	24.69	<.01
Duration of a face-to-face session	0.1	0.22	[-0.4, 0.6]	0.46	.66	12	0.21	0.00%	0.07	0.06	25.05	<.01
Program length	0.06	0.08	[-0.11, 0.23]	0.73	.48	12	0.54	0.00%	0.07	0.06	24.7	<.01
Frequency of recommended practice****	-	-	-	-	-	-	-	-	-	-	-	-
Duration of a recommended practice	0.01	0.01	[-0.01, 0.02]	0.8	.44	12	0.64	0.00%	0.07	0.06	23.19	.01
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total amount of contact	0.02	0.01	[-0.-1, 0.05]	1.65	.13	12	2.72	32.42%	0.04	0.04	19.03	.04
Total amount of contact (excl. 0 hours)**	-	-	-	-	-	-	-	-	-	-	-	-
Total recommended use of program	0.01	0.01	[-.01, 0.02]	0.9	.39	12	0.81	0.00%	0.07	0.05	23.8	.01
Total actual use of program*	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity excl. retreats***	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity incl. retreats	2.34	1.64	[-1.34, 5.99]	1.41	.19	12	2	18.1%	0.05	0.05	20.69	.02
Amount of contact/week	0.14	0.1	[-0.09, 0.38]	1.38	.2	12	1.91	19.58%	0.05	0.05	20.44	.03
Amount of contact (excl. 0 hours)/week**	-	-	-	-	-	-	-	-	-	-	-	-
Recommended use of program/week	0.04	0.05	[-0.07, 0.14]	0.76	.46	12	0.58	0.00%	0.07	0.06	24.29	.01
Actual use of program/week*	-	-	-	-	-	-	-	-	-	-	-	-

*k<10; , **no included studies had zero hours of contact; ***all included studies had the same score on this dose; ****all but one had the same score on this dose, *****all but 2 studies had the same score on this dose; *d*=effect size of the standardized regression coefficient, SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value= test statistic of slope, *p*-value= significance level; *k*=number of studies; *F*-distribution= test for the overall model; R^2= percentage of heterogeneity accounted for, tau^2/τ^2= variance of the underlying true effect sizes; *SE* tau^2= standard error of tau^2; Q_E= between-study heterogeneity; $p(Q_E)$=Q_E significance level.

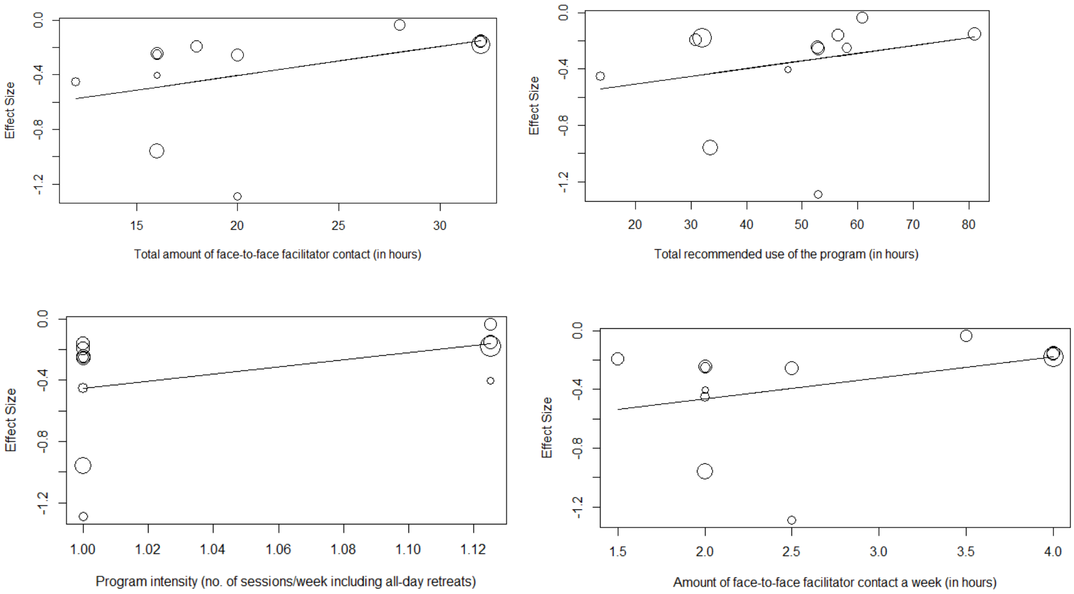
Appendix Figure 4.2.2.5

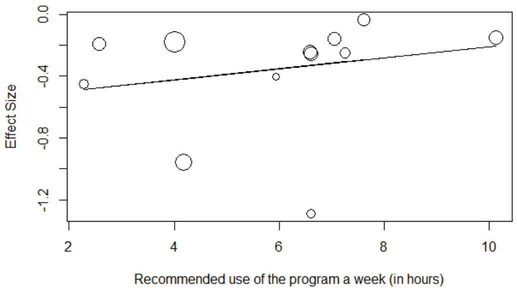
Meta-regression plots for anxiety at 5-10 months follow-up compared to inactive controls for the doses total number of face-to-face sessions, duration of a face-to-face session (in hours), program length (in weeks), duration of recommended home practice (one practice in minutes), total amount of face-to-face facilitator contact (in hours), total recommended use of the program (in hours), program intensity (number of sessions a week including all-day retreats), amount of face-to-face facilitator contact a week (in hours) and recommended use of the program a week (in hours).

Primary dose variables



Composite dose variables





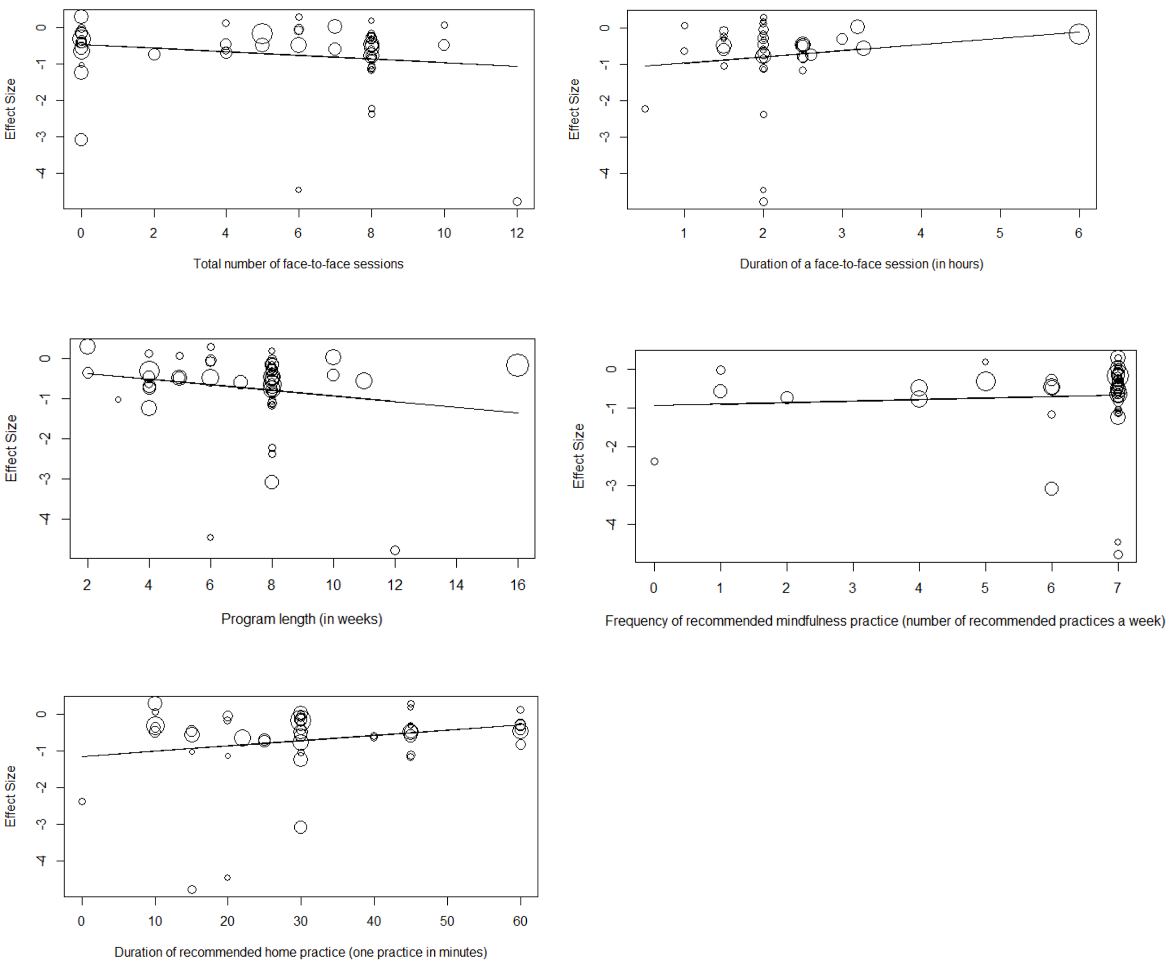
Effect Size
0.0
-0.4
-0.8
-1.2
2
4
6
8
10
Recommended use of the program a week (in hours)

Appendix 4.2.3 Dose-Response Meta-Regression Results: Stress

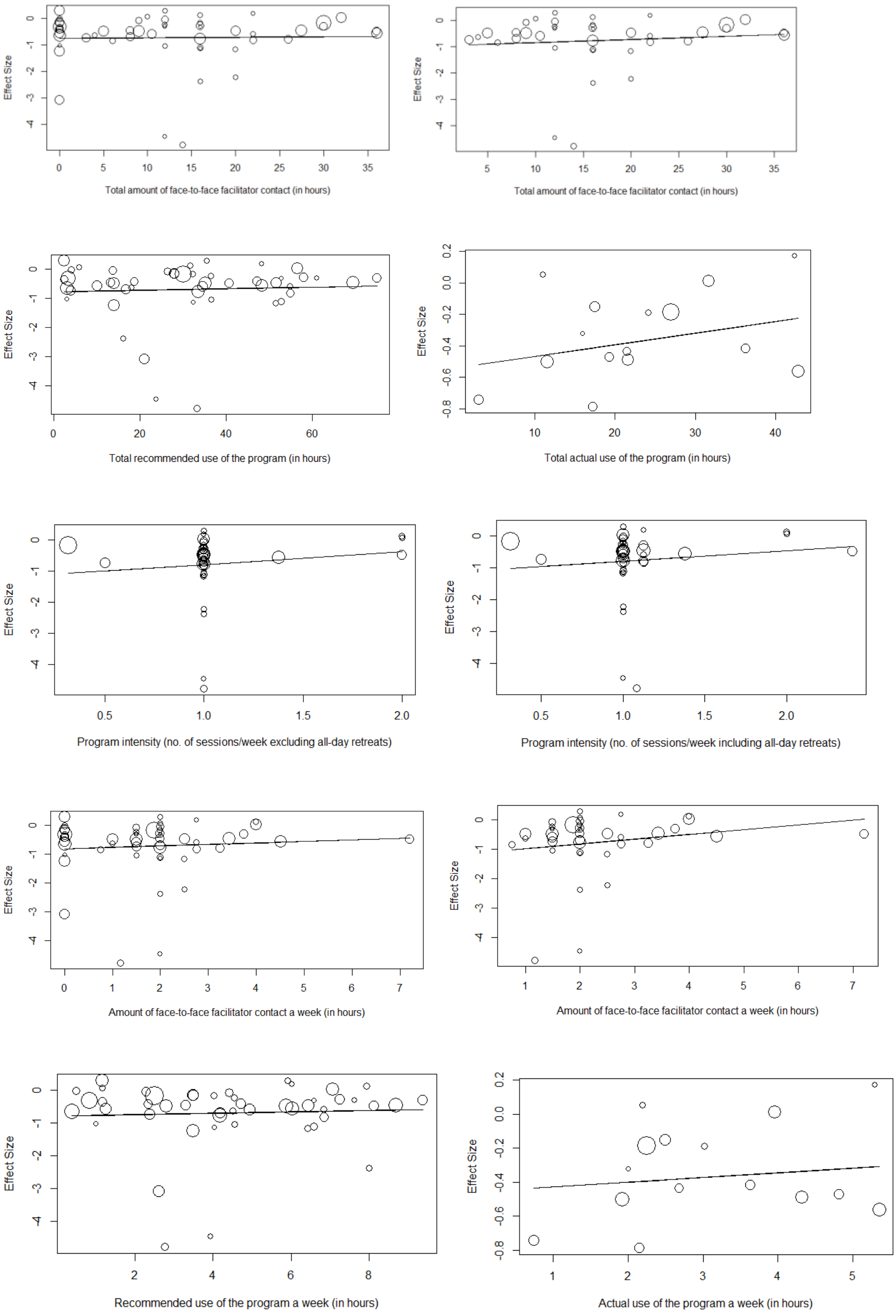
Appendix Figure 4.2.3.1

Meta-regression plots for stress at post-program compared to inactive and active controls for the doses total number of face-to-face sessions, duration of a face-to-face session (in hours), program length (in weeks), frequency (number) of recommended home practices a week, duration of recommended home practice (one practice in minutes), total amount of face-to-face facilitator contact (in hours), total amount of face-to-face facilitator contact with zero hours of contact excluded (in hours), total recommended use of the program (in hours), total actual use of the program (in hours; compared to inactive controls only), program intensity (number of sessions a week excluding all-day retreats; compared to inactive controls only), program intensity (number of sessions a week including all-day retreats), amount of face-to-face facilitator contact a week (in hours), amount of face-to-face facilitator contact excluding zero contact a week (in hours), recommended use of the program a week (in hours) and actual use of the program a week (in hours; compared to inactive controls only).

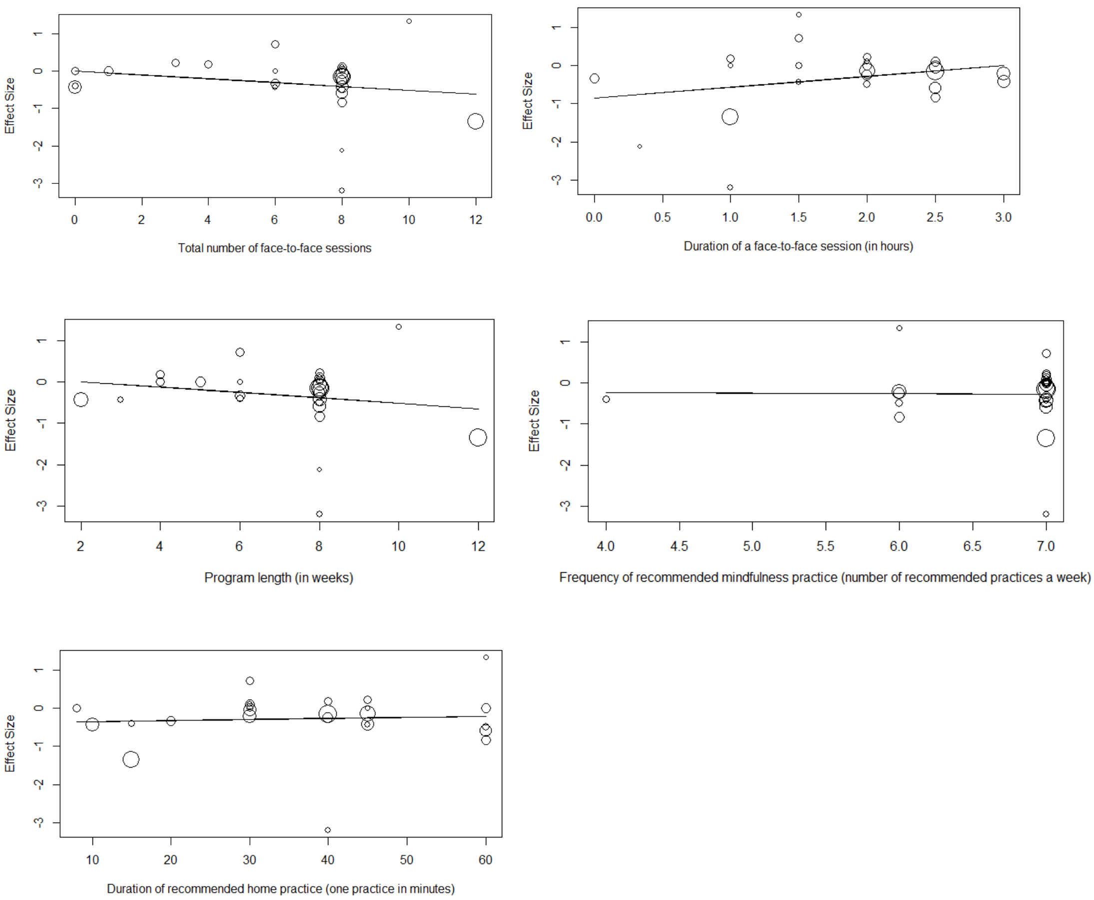
Compared to inactive controls – primary dose variables



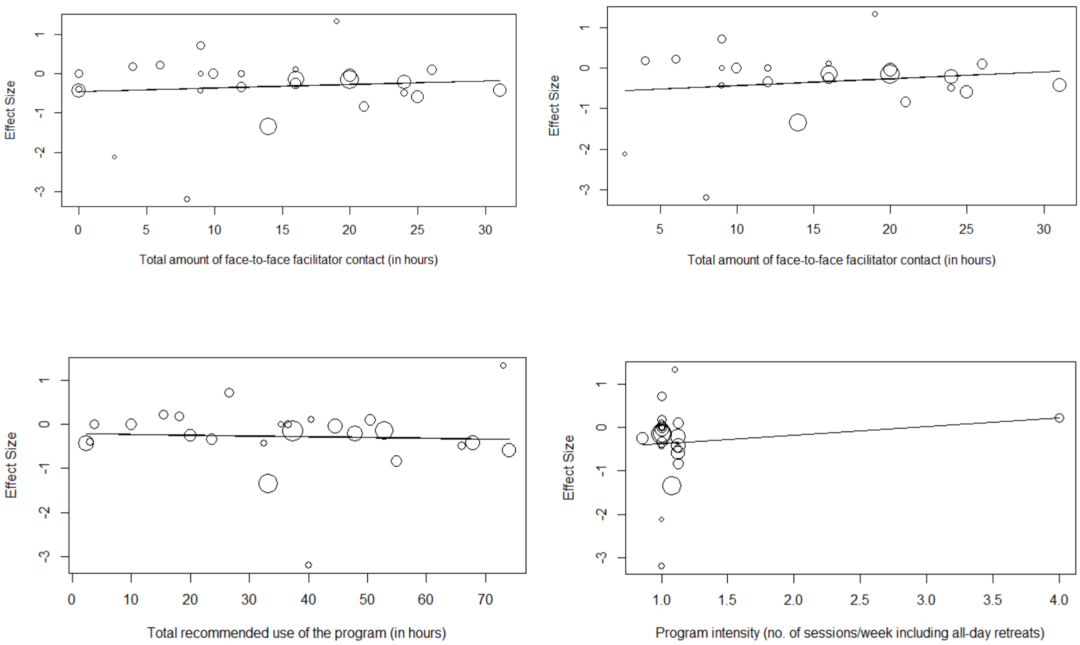
Compared to inactive controls – composite dose variables

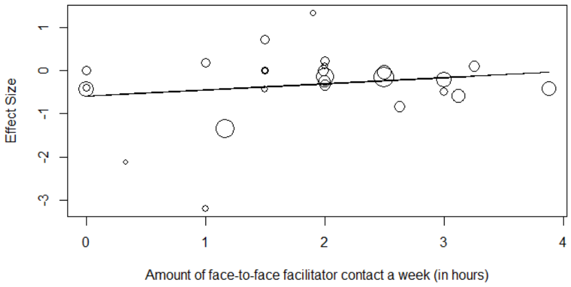


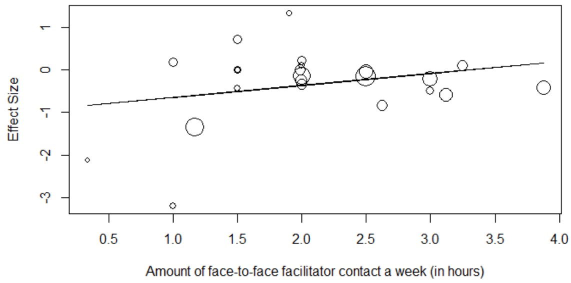
Compared to active controls – primary dose variables

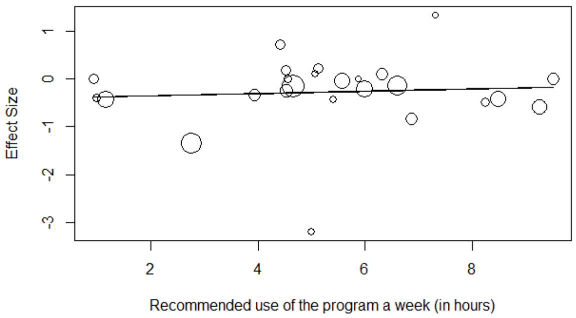


Compared to active controls – composite dose variables




Effect Size
Amount of face-to-face facilitator contact a week (in hours)

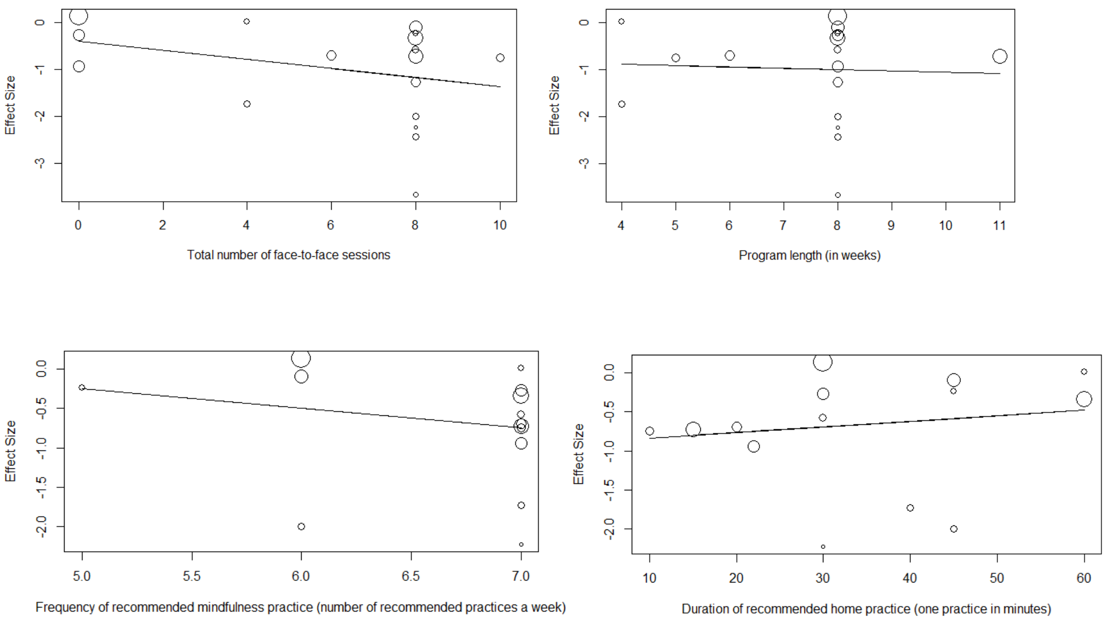

Effect Size
Amount of face-to-face facilitator contact a week (in hours)


Effect Size
Recommended use of the program a week (in hours)

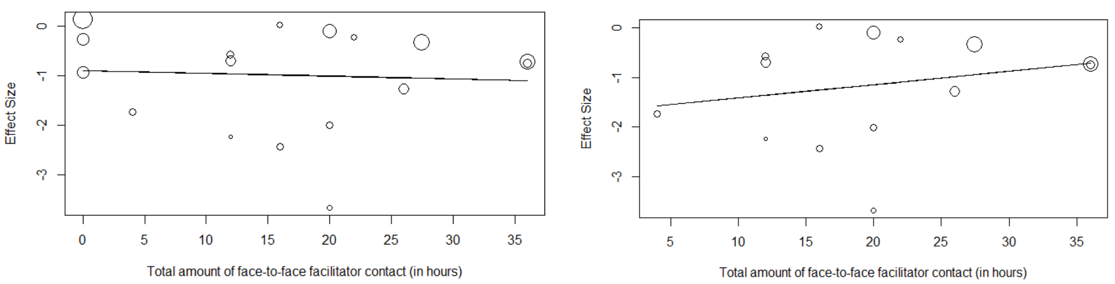
Appendix Figure 4.2.3.2

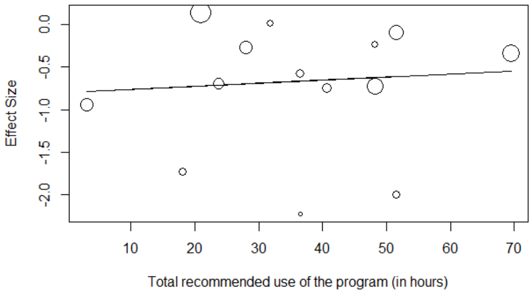
Meta-regression plots for stress at 1-4 months follow-up compared to inactive and active controls for the doses total number of face-to-face sessions, duration of a face-to-face session (in hours; compared to active controls only), program length (in weeks), frequency (number) of recommended home practices a week, duration of recommended home practice (one practice in minutes), total amount of face-to-face facilitator contact (in hours; compared to inactive controls only), total amount of face-to-face facilitator contact with zero hours of contact excluded (in hours; compared to inactive controls only), total recommended use of the program (in hours), program intensity (number of sessions a week excluding all-day retreats; compared to inactive controls only), program intensity (number of sessions a week including all-day retreats; compared to inactive controls only), amount of face-to-face facilitator contact a week (in hours), amount of face-to-face facilitator contact excluding zero contact a week (in hours) and recommended use of the program a week (in hours).

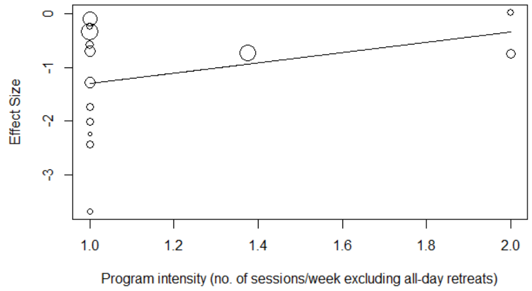
Compared to inactive controls – primary dose variables

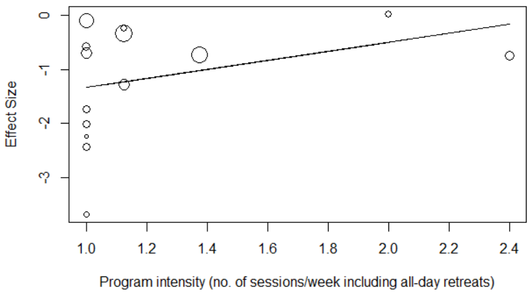


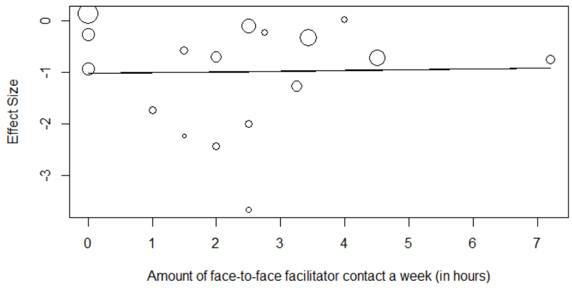
Compared to inactive controls – composite dose variables

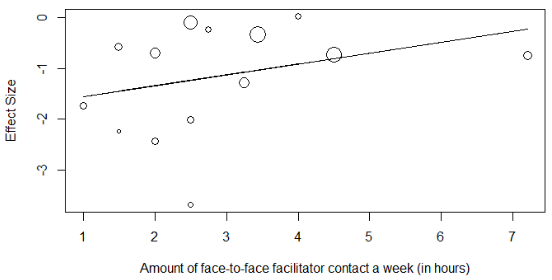


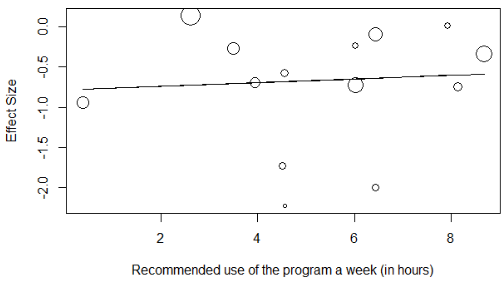




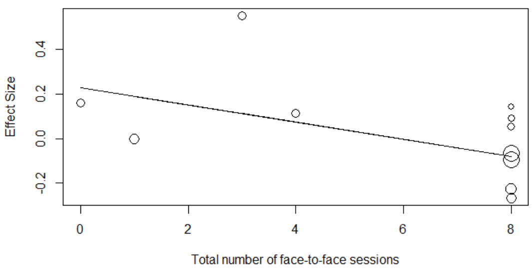


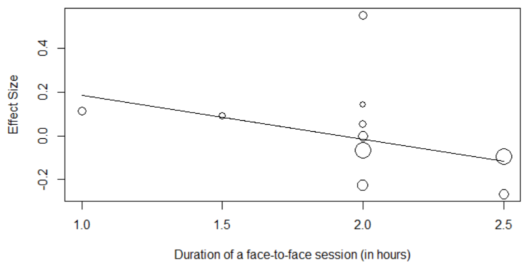


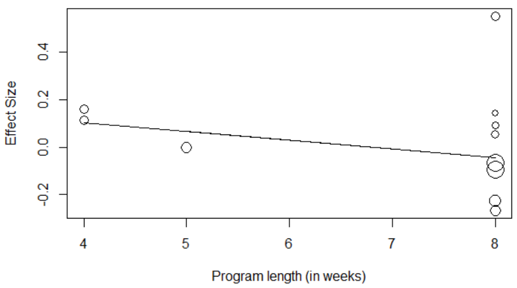


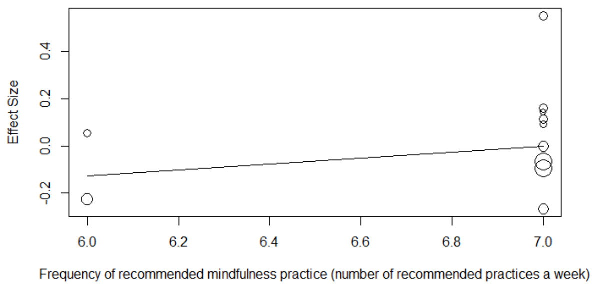


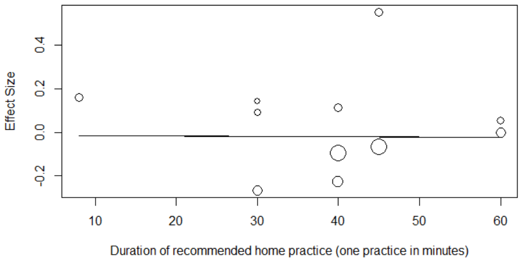
Compared to active controls – primary dose variables



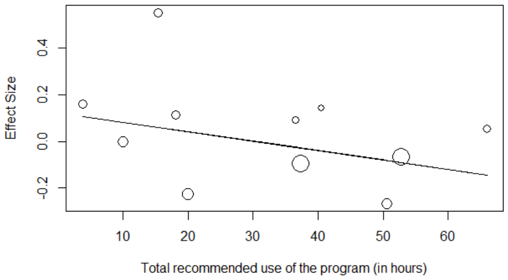


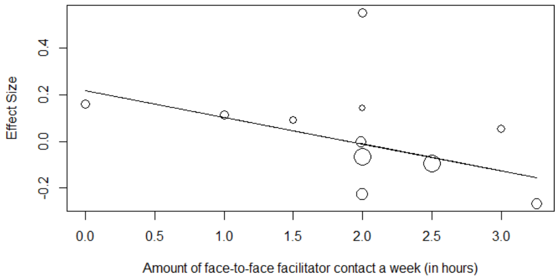


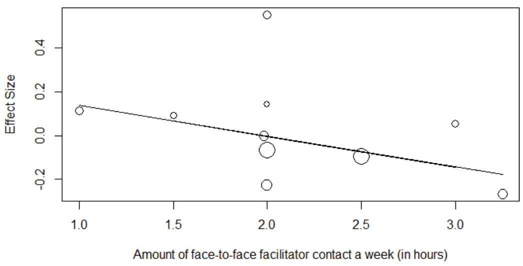


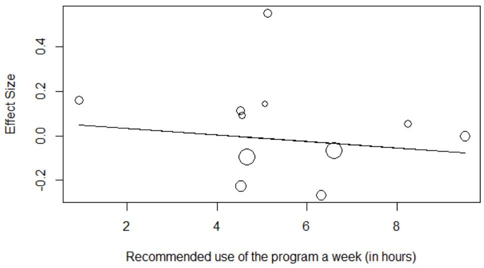


Compared to active controls – composite dose variables









Appendix Table 4.2.3.3

Step-by-step False Discovery Rate correction (Benjamini-Hochberg procedure) for stress at 1-4 months follow-up compared to inactive controls

p	**.03**	.2	.22	.23	.26	.41	.45	.58	.76	.78	.8	.85	.92
Rank	1	2	3	4	5	6	7	8	9	10	11	12	13
p**adj**	.49	.67	.67	. 67	. 67	.83	.83	.91	.91	.91	.91	.91	.92

Significant results in bold.

Appendix 4.2.3.4 Holm-Bonferroni sequential rejective test procedure for stress at 1-4 months follow-up compared to active controls

<u>Step 1: Rank-order significant *p*-values from smallest to largest</u>

Rank 1: Program intensity (when including all-day retreats): H_1: p=.004

Rank 2: Total amount of face-to-face facilitator contact: H_2: p=.023

Rank 3: Total amount of face-to-face facilitator contact (when excl. no contact): H_3: p=.034

<u>Step 2: Holm-Bonferroni formula for first rank</u>

HB=Target α / (n – rank + 1)

HB=.05 / (12 – 1 +1) = .0041

H_1<.0041

<u>Step 3: Holm-Bonferroni formula for second rank</u>

HB=Target α / (n – rank + 1)

HB=.05 / (12 – 2 +1) = .0045

H_2>.0045

<u>Step 4: Holm-Bonferroni formula for third rank</u>

HB=Target α / (n – rank + 1)

HB=.05 / (12 – 3 +1) = .005

H_3>.005

Table 4.2.3.5

Step-by-step False Discovery Rate correction (Benjamini-Hochberg procedure) for stress at 1-4 months follow-up compared to active controls

p	**.004**	**.023**	**.034**	.066	.17	.24	.25	.25	.39	.48	.66	.98
Rank	1	2	3	4	5	6	7	8	9	10	11	12
$\boldsymbol{p_{adj}}$	**.048**	.14	.14	.2	.38	.38	.38	.38	.52	.58	.98	.98

Significant results in bold.

Appendix 4.3: Measure-by-Measure Meta-Regression Results

Appendix 4.3.1: Measure-by-Measure Meta-Regression Results: Depression

Appendix Table 4.3.1.1

BDI measure-by-measure meta-regression analysis results for BDI depression by MBP dose for between group depression effect sizes at immediately post-program compared to inactive and active controls

Compared to inactive control groups												
Dose	**Meta-regression model**						**Heterogeneity statistics**					
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	SE tau^2	Q_E	$p\ (Q_E)$
Total no. face-to-face sessions	-0.03	0.03	[-0.1, 0.04]	-0.99	.33	46	0.98	0.00%	0.14	0.05	126.46	<.001
Duration of a face-to-face session	-0.08	0.13	[-0.33, 0.17]	-0.64	.53	45	0.41	0.00%	0.15	0.05	126.2	<.001
Program length	-0.03	0.05	[-0.12, 0.06]	-0.67	.51	46	0.45	0.00%	0.14	0.05	126.56	<.001
Frequency of recommended practice	-0.09	0.15	[-0.39, 0.21]	-0.63	.53	44	0.4	0.00%	0.13	0.05	117.58	<.001
Duration of a recommended practice	**0.01**	**0.01**	**[0.004, 0.02]**	**2.78**	**.008**	**44**	**7.73**	**27.55%**	**0.09**	**0.04**	**97.87**	**<.001**
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	SE tau^2	Q_E	$p\ (Q_E)$
Total amount of contact	-0.01	0.01	[-0.02, 0.01]	-0.67	.51	46	0.45	0.00%	0.14	0.05	125.95	<.001
Total amount of contact (excl. 0 hours)	-0.01	0.01	[-0.02, 0.01]	-0.71	.48	45	0.5	0.00%	0.15	0.05	125.16	<.001
Total recommended use of program	0.004	0.004	[-0.03, .01]	1.15	.26	44	1.33	3.64%	0.12	0.05	109.98	<.001
Total actual use of program*	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity excl. retreats	-0.09	0.25	[-0.58, 0.4]	-0.37	.72	45	0.13	0.00%	0.15	0.05	125.06	<.001
Program intensity incl. retreats	-0.03	0.25	[-0.52, 0.47]	-0.11	.92	45	0.01	0.00%	0.15	0.05	125.85	<.001
Amount of contact/week	-0.03	0.04	[-0.12, 0.06]	-0.66	.52	46	0.43	0.00%	0.14	0.05	125.19	<.001
Amount of contact (excl. 0 hours)/week	-0.03	0.05	[-0.12, 0.06]	-0.67	.51	45	0.45	0.00%	0.15	0.05	124.47	<.001
Recommended use of program/week	0.06	0.03	[-0.01, 0.12]	1.7	.096	44	2.89	9.64%	0.11	0.04	106.89	<.001
Actual use of program/week*	-	-	-	-	-	-	-	-	-	-	-	-

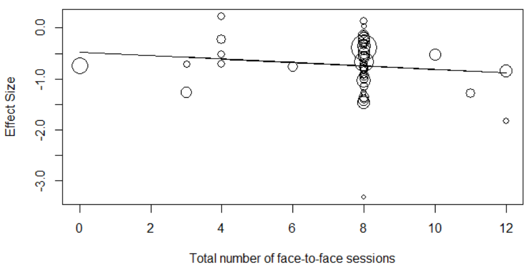
Compared to active control groups												
Dose	**Meta-regression model**						**Heterogeneity statistics**					
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	**Q_E**	***p (Q_E)***
Total no. face-to-face sessions	0.05	0.03	[-0.01, 0.1]	1.87	.072	30	3.45	14.59	0.1	0.05	73.08	<.001
Duration of a face-to-face session	-0.05	0.14	[-0.33, 0.23]	-0.38	.71	28	0.15	0.00%	0.12	0.05	70.32	<.001
Program length	**0.12**	**0.03**	**[0.06, 0.18]**	**3.9**	**<.001**	**30**	**15.17**	**65%**	**0.04**	**0.03**	**51.78**	**.004**
Frequency of recommended practice***	-	-	-	-	-	-	-	-	-	-	-	-
Duration of a recommended practice	<.001	0.01	[-0.01, 0.01]	0.05	.96	26	0.002	0.00%	0.08	0.04	56.24	<.001
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	**Q_E**	***p (Q_E)***
Total amount of contact	0.01	0.01	[-0.01, 0.03]	1.15	.26	30	1.32	0.91%	0.12	0.05	79.89	<.001
Total amount of contact (excl. 0 hours)	0.02	0.01	[-0.01, 0.04]	1.39	.18	28	1.94	5.26%	0.11	0.05	67.97	<.001
Total recommended use of program	0.004	0.004	[-0.004, 0.01]	1.05	.31	26	1.1	0.00%	0.08	0.04	55.22	<.001
Total actual use of program*	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity excl. retreats**	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity incl. retreats	-1	0.59	[-2.2, 0.23]	-1.67	.11	28	2.77	7.93%	0.1	0.05	66.92	<.001
Amount of contact/week	-0.04	0.1	[-0.25, 0.17]	-0.37	.72	30	0.14	0.00%	0.13	0.05	81.5	<.001
Amount of contact (excl. 0 hours)/week	-0.05	0.13	[-0.31, 0.21]	-0.43	.67	28	0.19	0.00%	0.12	0.05	70.46	<.001
Recommended use of program/week	0.01	0.04	[-0.06, 0.09]	0.39	.7	26	0.15	0.00%	0.08	0.04	56.25	<.001
Actual use of program/week*	-	-	-	-	-	-	-	-	-	-	-	-

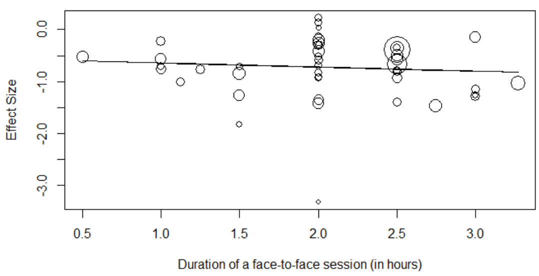
significant results in bold; *k<10, **all but one study had the same score on this dose; **all but two studies had the same score on this dose; d=effect size of the standardized regression coefficient, SE=standard error of the effect size, 95% CI= confidence intervals; t-value= test statistic of slope, p-value= significance level; k=number of studies; F-distribution= test for the overall model; R^2= percentage of heterogeneity accounted for, tau^2/τ^2= variance of the underlying true effect sizes; *SE* tau^2= standard error of tau^2; Q_E= between-study heterogeneity; $p(Q_E)$)=Q_E significance level.

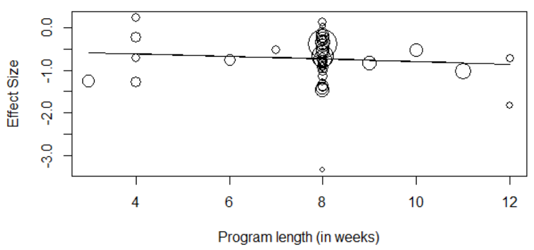
Appendix Figure 4.3.1.2

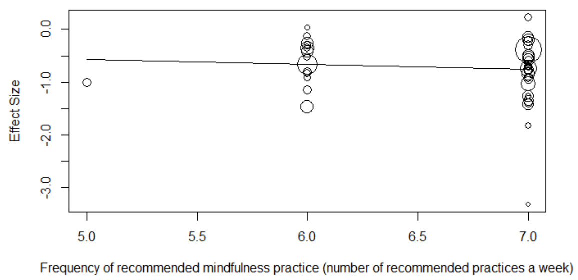
BDI measure-by-measure meta-regression plots for BDI depression at post-program compared to inactive and active controls for the doses total number of face-to-face sessions, duration of a face-to-face session (in hours), program length (in weeks; compared to inactive controls only), frequency (number) of recommended home practices a week (compared to inactive controls only), duration of a recommended home practice (one practice in minutes; compared to active controls only), total amount of face-to-face facilitator contact (in hours), total amount of face-to-face facilitator contact with zero hours of contact excluded (in hours), total recommended use of the program (in hours), program intensity (number of sessions a week excluding all-day retreats; compared to inactive controls only), program intensity (number of sessions a week including all-day retreats), amount of face-to-face facilitator contact a week (in hours), amount of face-to-face facilitator contact excluding zero contact a week (in hours) and recommended use of the program a week (in hours).

Compared to inactive controls – primary dose variables

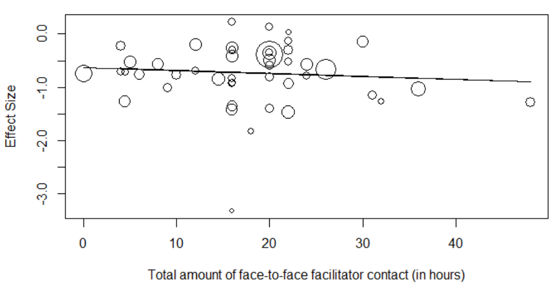


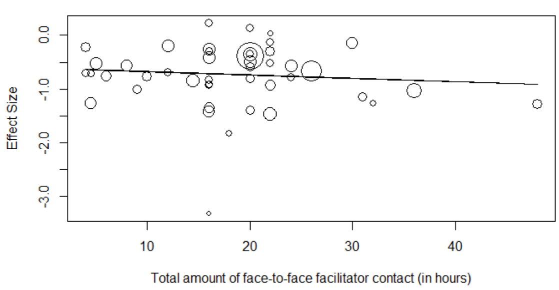


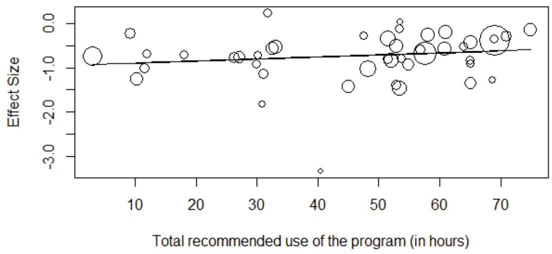


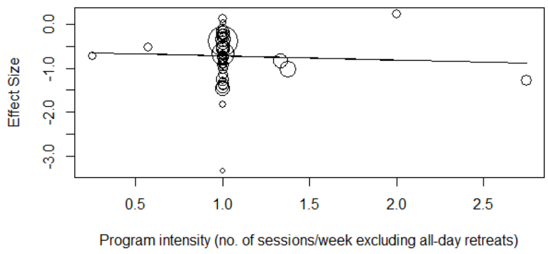


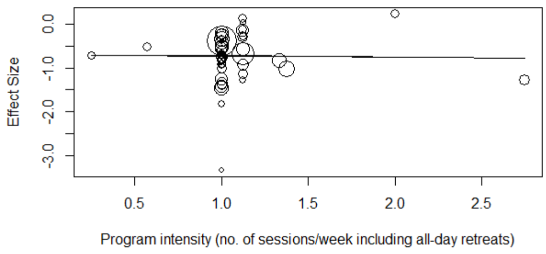
Compared to inactive controls – composite dose variables

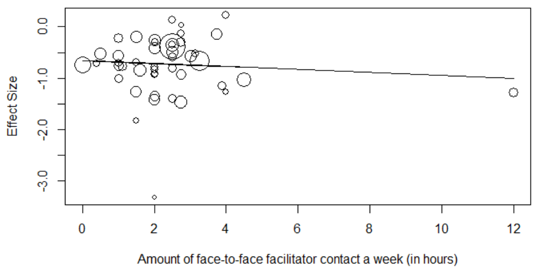


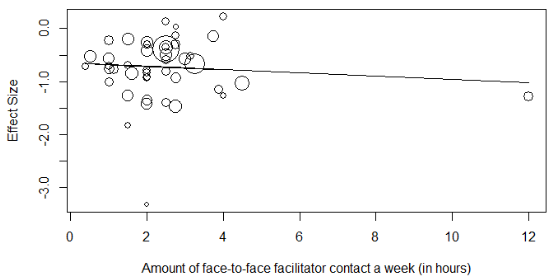


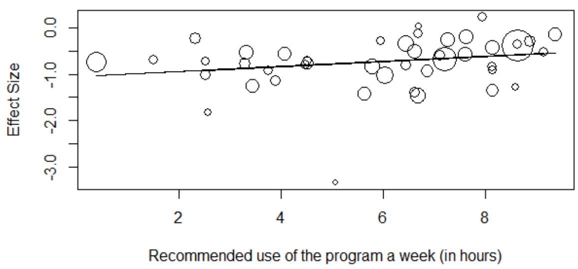




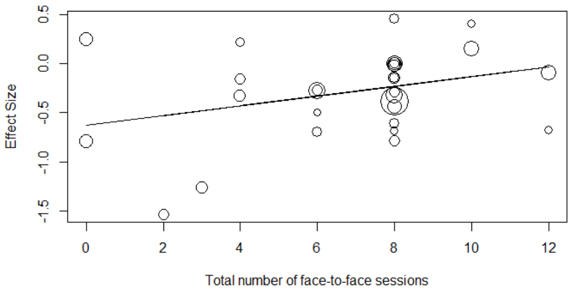


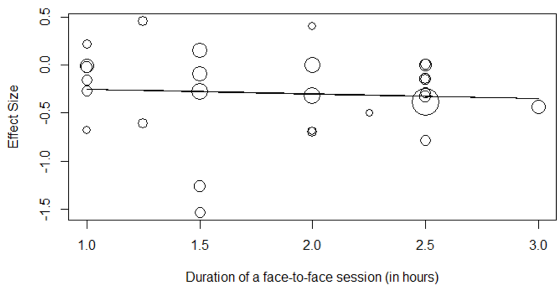


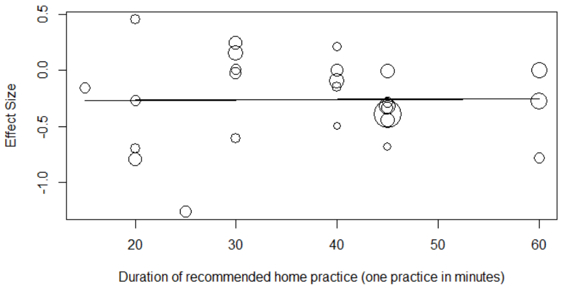




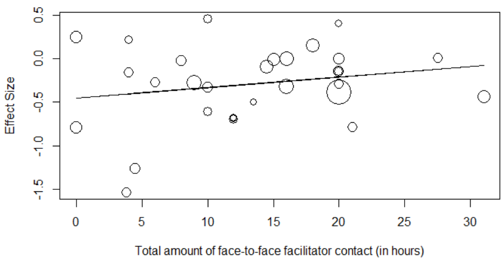
Compared to active controls – primary dose variables

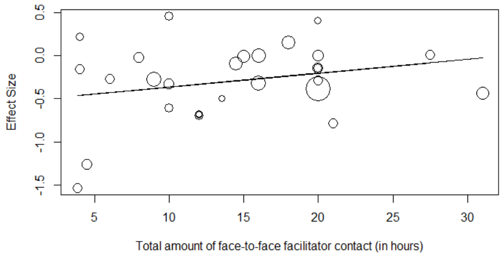


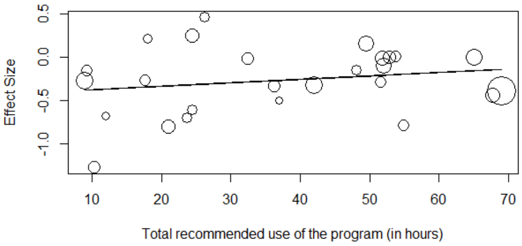


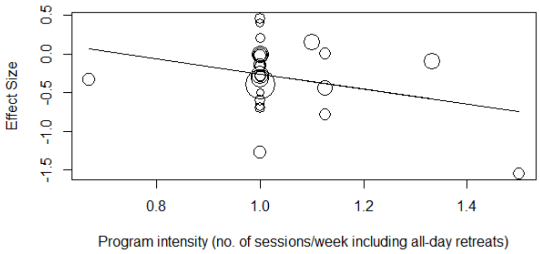


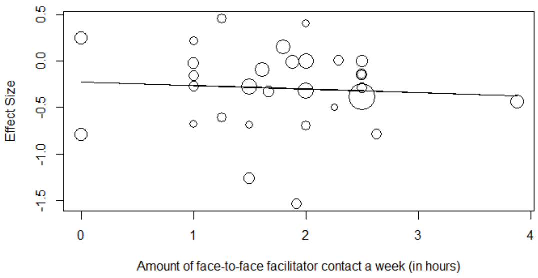
Compared to active controls – composite dose variables

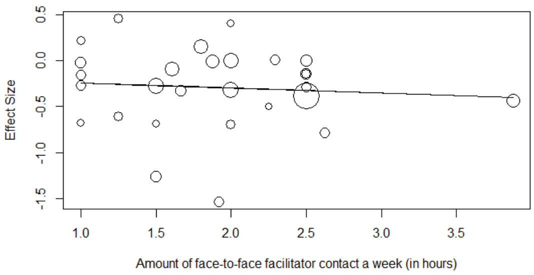


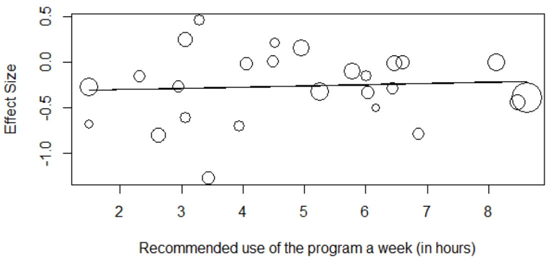












Appendix Table 4.3.1.3

Step-by-step False Discovery Rate correction (Benjamini-Hochberg procedure) for BDI depression at post-program compared to inactive controls

p	**.008**	.096	.255	.328	.482	.506	.507	.507	.515	.526	.531	.716	.917
Rank	1	2	3	4	5	6	7	8	9	10	11	12	13
$\boldsymbol{p}_{\mathbf{adj}}$	.104	.624	.744	.744	.744	.744	.744	.744	.744	.776	.776	.776	.917

Significant results in bold.

Appendix Table 4.3.1.4

Step-by-step False Discovery Rate correction (Benjamini-Hochberg procedure) for BDI depression at post-program compared to active controls

p	**<.001**	.072	.108	.175	.261	.305	.67	.701	.706	.716	.962
Rank	1	2	3	4	5	6	7	8	9	10	11
$\boldsymbol{p}_{\mathbf{adj}}$	**.011**	.396	.396	.481	.559	.559	.789	.789	.789	.789	.962

Significant results in bold.

Appendix Table 4.3.1.5

BDI measure-by-measure meta-regression analysis results for BDI depression by MBP dose for between-group depression effect sizes at 1-4 months follow-up compared to inactive controls

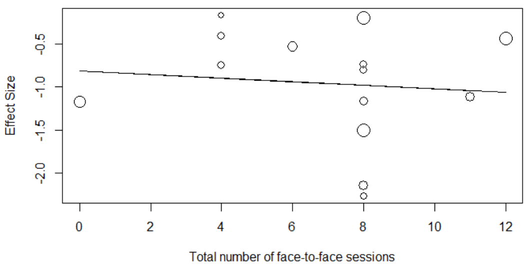
Dose	Meta-regression model						Heterogeneity statistics					
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total no. face-to-face sessions	-0.02	0.06	[-0.15, 0.11]	-0.35	.73	14	0.12	0.00%	0.37	0.19	64.28	<.001
Duration of a face-to-face session	-0.32	0.26	[-0.89, 0.25]	-1.22	.25	13	1.5	4.74%	0.35	0.19	54.69	<.001
Program length	-0.12	0.09	[-0.3, 0.07]	-1.37	.2	14	1.87	5.3%	0.31	0.17	58.5	<.001
Frequency of recommended practice**	-	-	-	-	-	-	-	-	-	-	-	-
Duration of a recommended practice	**0.02**	**0.01**	**[0.002, 0.05]**	**2.37**	**.037**	**13**	**5.61**	**38.22%**	**0.22**	**0.14**	**36.86**	**<.001**
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total amount of contact	-0.01	0.02	[-0.04, 0.02]	-0.75	.47	14	0.56	0.00%	0.35	0.19	60.84	<.001
Total amount of contact (excl. 0 hours)	-0.02	0.02	[-0.05, 0.02]	-0.98	.35	13	0.95	0.00%	0.36	0.2	55.83	<.001
Total recommended use of program	-0.002	0.01	[-0.03, 0.02]	-0.21	.84	13	0.04	0.00%	0.4	0.21	63.91	<.001
Total actual use of program*	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity excl. retreats	0.01	0.36	[-0.79, 0.81]	0.02	.98	13	<.001	0.00%	0.41	0.22	61.8	<.001
Program intensity incl. retreats	0.06	0.37	[-0.75, 0.88]	0.17	.87	13	0.03	0.00%	0.4	0.22	63.29	<.001
Amount of contact/week	-0.02	0.06	[-0.16, 0.12]	-0.3	.77	14	0.09	0.00%	0.36	0.19	63.13	<.001
Amount of contact (excl. 0 hours)/week	-0.03	0.07	[-0.18, 0.13]	-0.4	.69	13	0.16	0.00%	0.4	0.21	60.81	<.001
Recommended use of program/week	0.04	0.09	[-0.15, 0.24]	0.51	.62	13	0.26	0.00%	0.39	0.21	62.36	<.001
Actual use of program/week*	-	-	-	-	-	-	-	-	-	-	-	-

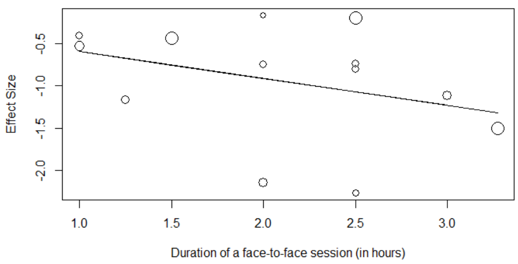
significant results in bold*;* **k*<10, **all but one study had the same score on this dose*;* **all but two studies had the same score on this dose; *d*=effect size of the standardized regression coefficient, SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value= test statistic of slope, *p*-value= significance level; *k*=number of studies; *F*-distribution= test for the overall model; R^2= percentage of heterogeneity accounted for, tau^2/τ^2= variance of the underlying true effect sizes; *SE* tau^2= standard error of tau^2; Q_E= between-study heterogeneity; $p(Q_E)$)=Q_E significance level.

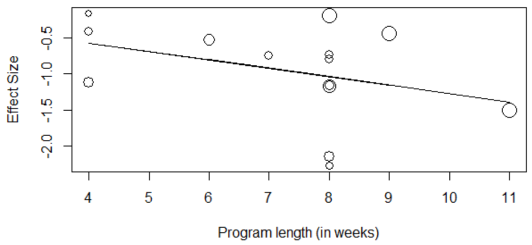
Appendix Figure 4.3.1.6

BDI measure-by-measure meta-regression plots for BDI depression at 1-4 months follow-up compared to inactive controls for the doses total number of face-to-face sessions, duration of a face-to-face session (in hours), program length (in weeks), total amount of face-to-face facilitator contact (in hours), total amount of face-to-face facilitator contact with zero hours of contact excluded (in hours), total recommended use of the program (in hours), program intensity (number of sessions a week excluding all-day retreats), program intensity (number of sessions a week including all-day retreats), amount of face-to-face facilitator contact a week (in hours), amount of face-to-face facilitator contact excluding zero contact a week (in hours) and recommended use of the program a week (in hours).

Primary dose variables







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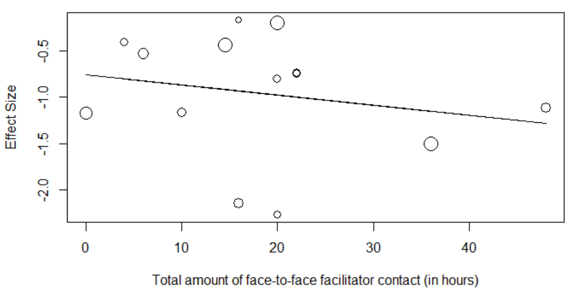
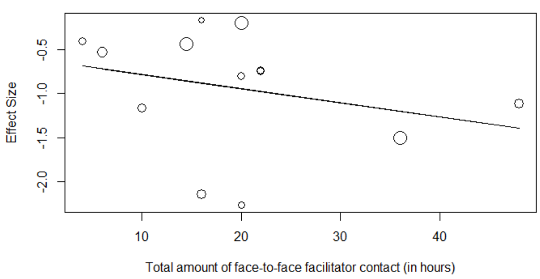
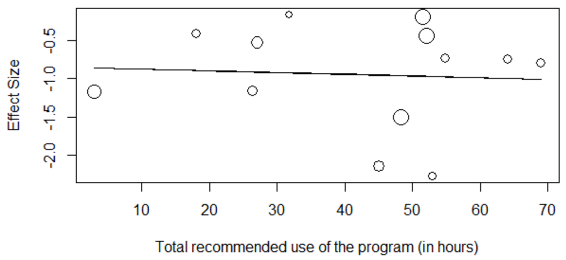
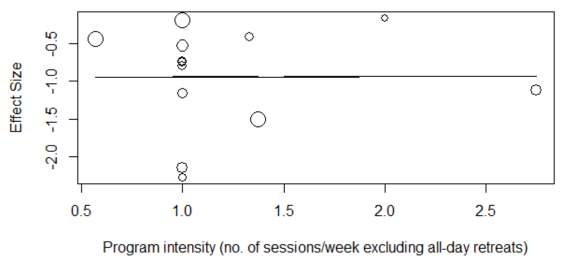
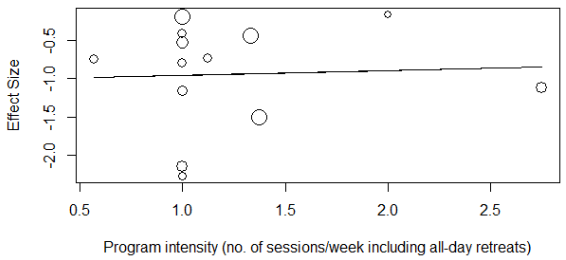
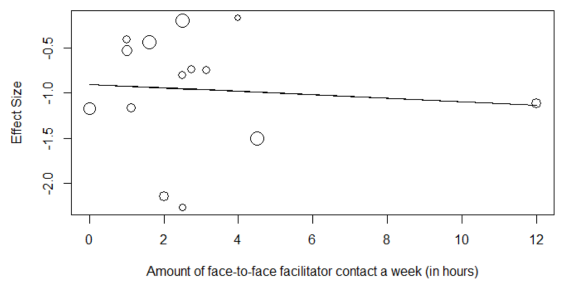


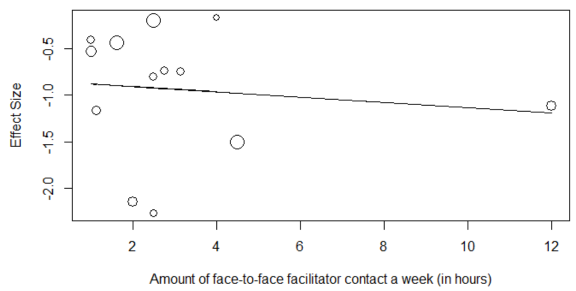


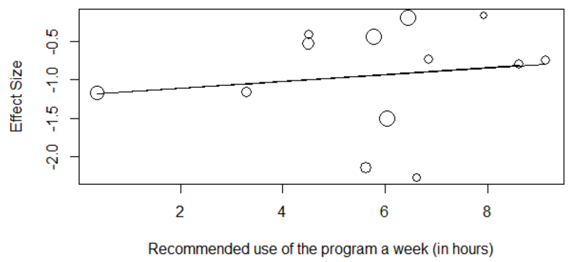












Appendix Table 4.3.1.7

Step-by-step False Discovery Rate correction (Benjamini-Hochberg procedure) for BDI depression measure at 1-4-months follow-up compared to inactive controls

p	**.037**	.197	.247	.351	.471	.62	.694	.733	.769	.841	.865	.984
Rank	1	2	3	4	5	6	7	8	9	10	11	12
$\boldsymbol{p_{adj}}$	.444	.943	.943	.943	.943	.943	.943	.943	.943	.943	.943	.984

Significant results in bold.

Appendix Table 4.3.1.8

CES-D measure-by-measure meta-regression analysis results for CES-D depression by MBP dose for between group depression effect sizes at immediately post-program compared to inactive and active controls

Compared to inactive control groups												
Dose	**Meta-regression model**							**Heterogeneity statistics**				
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total no. face-to-face sessions	-0.02	0.11	[-0.26, 0.21]	-0.19	.85	16	0.04	0.00%	1.71	0.68	169.83	<.001
Duration of a face-to-face session	0.13	0.54	[-1.04, 1.3]	0.24	.82	15	0.06	0.00%	1.84	0.76	168.94	<.001
Program length	0.07	0.16	[-0.27, 0.41]	0.43	.67	16	0.19	0.00%	1.69	0.67	168.93	<.001
Frequency of recommended practice***	-	-	-	-	-	-	-	-	-	-	-	-
Duration of a recommended practice	0.01	0.02	[-0.03, 0.05]	0.56	.59	15	0.31	0.00%	1.41	0.59	136.85	<.001
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total amount of contact	-0.01	0.04	[-0.09, 0.07]	-0.18	.86	16	0.03	0.00%	1.71	0.68	169.1	<.001
Total amount of contact (excl. 0 hours)	<.001	0.04	[-0.09, 0.09]	0.01	.99	15	<.001	0.00%	1.85	0.76	168.88	<.001
Total recommended use of program	0.004	0.02	[-0.03, 0.04]	0.25	.81	15	0.06	0.00%	1.44	0.6	136.92	<.001
Total actual use of program*	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity excl. retreats**	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity incl. retreats	0.21	1.07	[-2.18, 2.42]	0.11	.91	15	0.01	0.00%	1.85	0.76	167.19	<.001
Amount of contact/week	-0.07	0.21	[-0.53, 0.39]	-0.34	.74	16	0.12	0.00%	1.69	0.68	166.65	<.001
Amount of contact (excl. 0 hours)/week	-0.05	0.25	[-0.56, 0.47]	-0.2	.84	15	0.04	0.00%	1.84	0.76	166.57	<.001
Recommended use of program/week	-0.01	0.12	[-0.27, 0.26]	-0.06	.95	15	0.003	0.00%	1.45	0.6	136.07	<.001
Actual use of program/week*	-	-	-	-	-	-	-	-	-	-	-	-

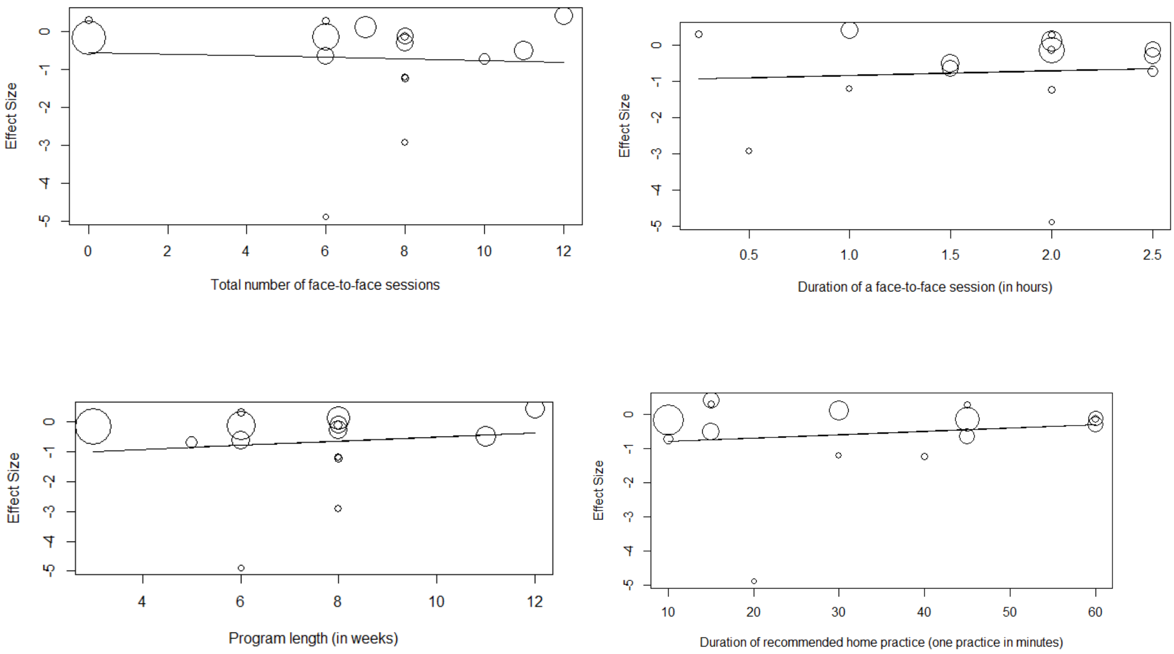
Compared to active control groups												
Dose	**Meta-regression model**						**Heterogeneity statistics**					
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	SE tau^2	Q_E	$p(Q_E)$
Total no. face-to-face sessions	-0.01	0.01	[-0.04, 0.01]	-1.42	.17	18	2.03	18.26%	0.07	0.05	39.17	.001
Duration of a face-to-face session	0.16	0.15	[-0.16, 0.48]	1.08	.3	15	1.16	20.45%	0.07	0.05	27.86	.01
Program length	-0.02	0.03	[-0.08, 0.04]	-0.77	.45	18	0.6	0.6%	0.06	0.05	45.86	<.001
Frequency of recommended practice***	-	-	-	-	-	-	-	-	-	-	-	-
Duration of a recommended practice	<.001	0.01	[-0.01, 0.01]	-0.03	.98	16	0.001	0.00%	0.09	0.06	39.37	<.001
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	SE tau^2	Q_E	$p(Q_E)$
Total amount of contact	-0.01	0.01	[-0.03, 0.01]	-0.87	.4	18	0.77	1.71%	0.08	0.05	43.64	<.001
Total amount of contact (excl. 0 hours)	-0.01	0.01	[-0.03, 0.01]	-0.72	.49	15	0.52	0.00%	0.09	0.06	34.21	<.001
Total recommended use of program	-0.001	0.004	[-0.01, 0.01]	-0.24	.82	16	0.06	0.00%	0.09	0.06	38.51	<.001
Total actual use of program*	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity excl. retreats***	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity incl. retreats	-0.07	0.12	[-0.33, 0.19]	-0.57	.58	15	0.33	0.00%	0.09	0.06	32.65	.002
Amount of contact/week	-0.07	0.07	[-0.21, 0.07]	-1.09	.29	18	1.18	4.99%	0.08	0.05	42.79	<.001
Amount of contact (excl. 0 hours)/week	-0.1	0.1	[-0.31, 0.11]	-1.03	.32	15	1.06	0.00%	0.09	0.06	34.1	.001
Recommended use of program/week	-0.02	0.03	[-0.09, 0.05]	-0.54	.6	16	0.29	0.00%	0.09	0.06	37.6	<.001
Actual use of program/week*	-	-	-	-	-	-	-	-	-	-	-	-

*k<10, **all but one study had the same score on this dose; *** all but two studies had the same score on this dose; *d*=effect size of the standardized regression coefficient, SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value= test statistic of slope, *p*-value= significance level; *k*=number of studies; *F*-distribution= test for the overall model; R^2= percentage of heterogeneity accounted for, tau^2/τ^2= variance of the underlying true effect sizes; *SE* tau^2= standard error of tau^2; Q_E= between-study heterogeneity; $p(Q_E)$)=Q_E significance level.

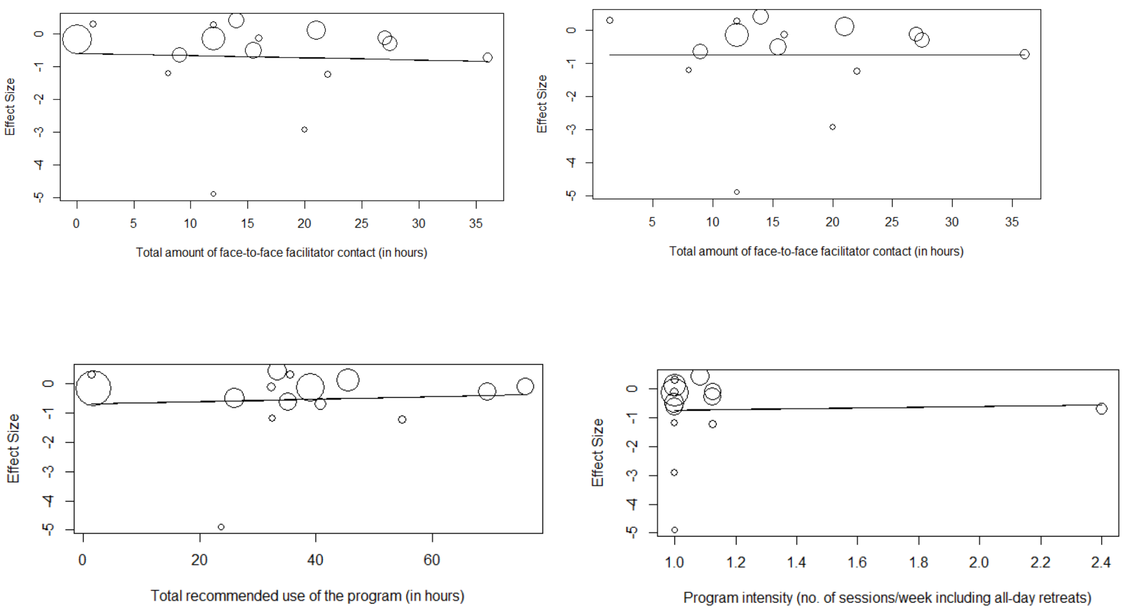
Appendix Figure 4.3.1.9

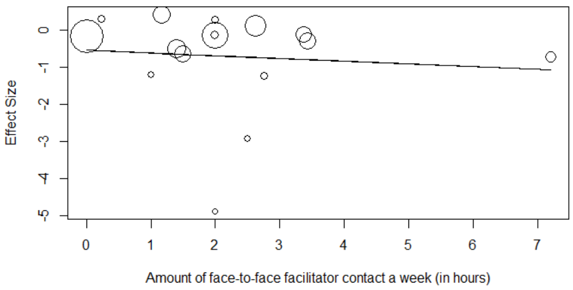
CES-D measure-by-measure meta-regression plots for CES-D depression at post-program compared to inactive and active controls for the doses total number of face-to-face sessions, duration of a face-to-face session (in hours), program length (in weeks), duration of a recommended home practice (one practice in minutes), total amount of face-to-face facilitator contact (in hours), total amount of face-to-face facilitator contact with zero hours of contact excluded (in hours), total recommended use of the program (in hours), program intensity (number of sessions a week including all-day retreats), amount of face-to-face facilitator contact a week (in hours), amount of face-to-face facilitator contact excluding zero contact a week (in hours) and recommended use of the program a week (in hours).

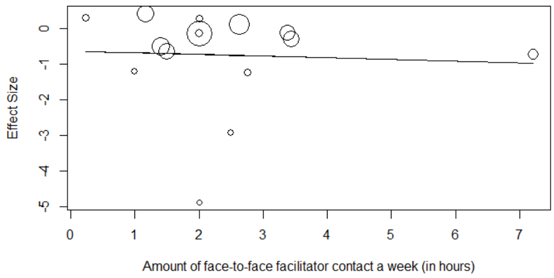
Compared to inactive controls – primary dose variables

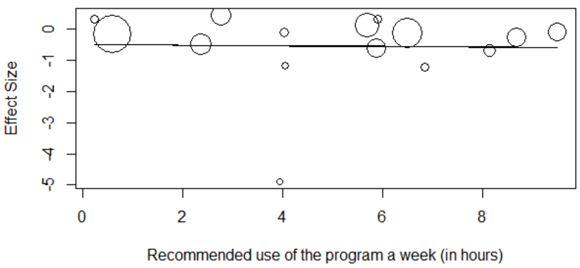


Compared to inactive controls – composite dose variables

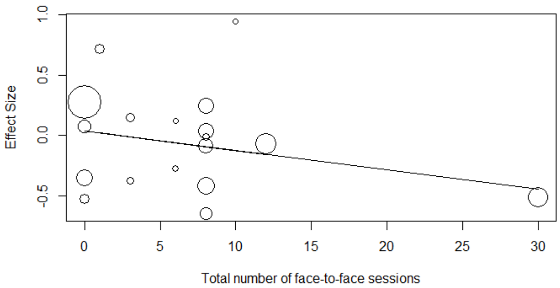


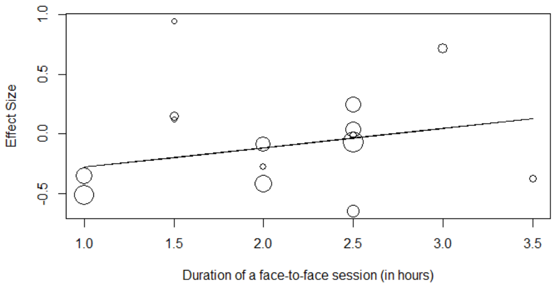


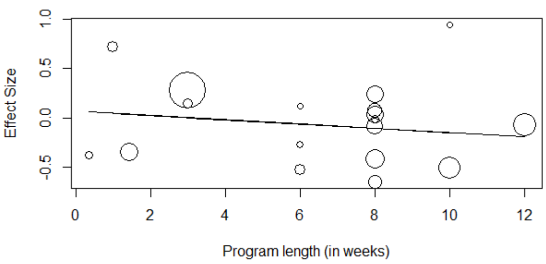


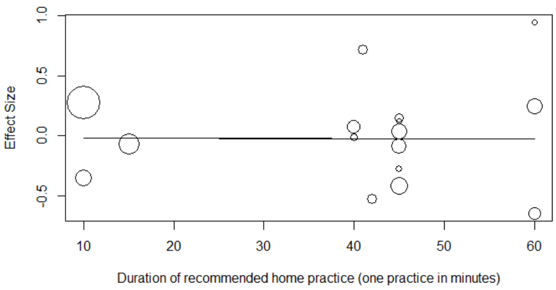


Compared to active controls – primary dose variables

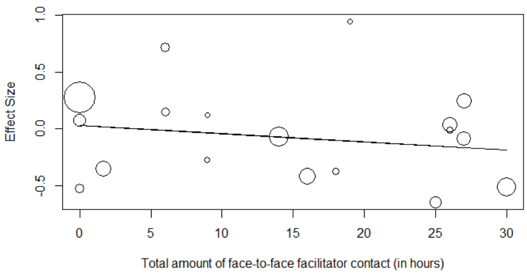


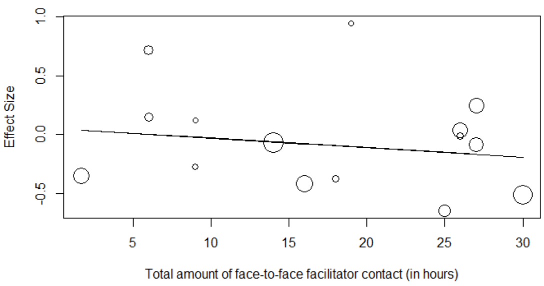


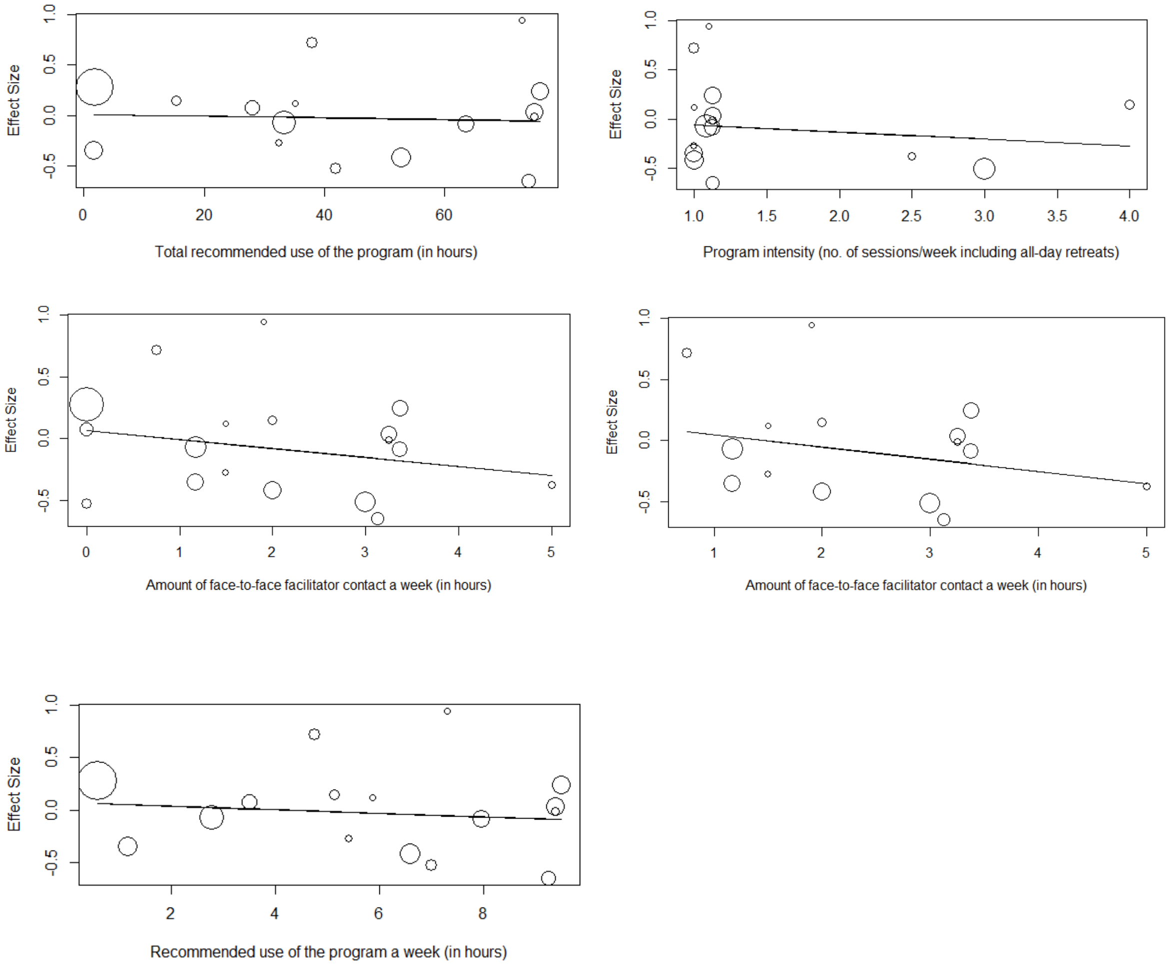




Compared to active controls – composite dose variables






Effect Size
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0.0
0.5
1.0
0
20
40
60
Total recommended use of the program (in hours)
Effect Size
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0.0
0.5
1.0
1.0
1.5
2.0
2.5
3.0
3.5
4.0
Program intensity (no. of sessions/week including all-day retreats)
Effect Size
-0.5
0.0
0.5
1.0
0
1
2
3
4
5
Amount of face-to-face facilitator contact a week (in hours)
Effect Size
-0.5
0.0
0.5
1.0
1
2
3
4
5
Amount of face-to-face facilitator contact a week (in hours)
Effect Size
-0.5
0.0
0.5
1.0
2
4
6
8
Recommended use of the program a week (in hours)

Appendix Table 4.3.1.10

DASS-D measure-by-measure meta-regression analysis results for DASS-D depression by MBP dose for between group depression effect sizes at post-program compared to inactive controls

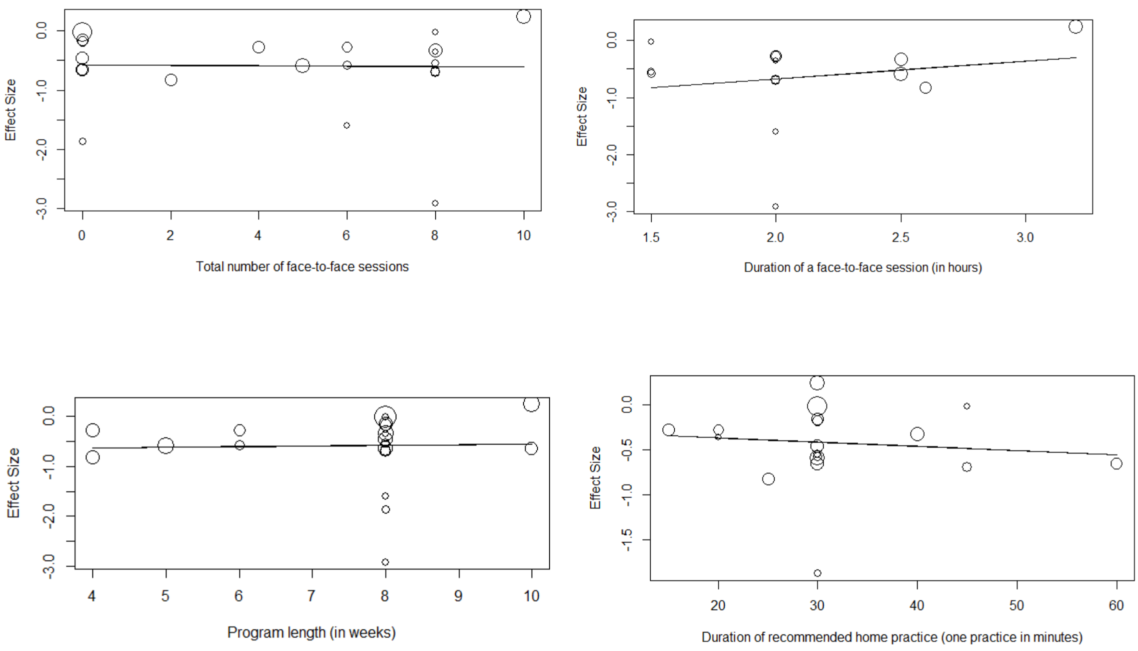
Dose	Meta-regression model						Heterogeneity statistics					
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total no. face-to-face sessions	-0.003	0.04	[-0.09, 0.08]	-0.08	.94	21	0.006	0.00%	0.3	0.13	79.85	<.001
Duration of a face-to-face session	0.31	0.42	[-0.6, 1.21]	0.73	.48	14	0.54	0.00%	0.37	0.19	46.93	<.001
Program length	0.01	0.09	[-0.18, 0.2]	0.15	.88	21	0.02	0.00%	0.3	0.13	76.69	<.001
Frequency of recommended practice	**0.08**	**0.04**	**[0.01, 0.19]**	**2.32**	**.034**	**18**	**5.4**	**21.84%**	**0.07**	**0.05**	**33.2**	**.007**
Duration of a recommended practice	-0.01	0.01	[-0.03, 0.02]	-0.46	.65	18	0.21	0.00%	0.09	0.06	40.14	<.001
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total amount of contact	0.01	0.02	[-0.03, 0.04]	0.39	.7	21	0.15	0.00%	0.3	0.13	77.06	<.001
Total amount of contact (excl. 0 hours)	0.02	0.03	[-0.04, 0.08]	0.8	.44	14	0.65	0.00%	0.36	0.19	42.97	<.001
Total recommended use of program	0.01	0.01	[-0.002, 0.02]	1.68	.11	18	2.83	14.1%	0.07	0.05	35.41	.003
Total actual use of program*	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity excl. retreats**	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity incl. retreats**	-	-	-	-	-	-	-	-	-	-	-	-
Amount of contact/week	0.04	0.13	[-0.24, 0.32]	0.32	.75	21	0.1	0.00%	0.3	0.13	78.35	<.001
Amount of contact (excl. 0 hours)/week	0.24	0.25	[-0.3, 0.78]	0.97	.35	14	0.94	4.52%	0.34	0.18	41.09	<.001
Recommended use of program/week	0.09	0.05	[-0.016, 0.19]	1.78	.094	18	3.18	12.72%	0.07	0.05	35.82	<.001
Actual use of program/week*	-	-	-	-	-	-	-	-	-	-	-	-

significant results in bold*;* **k*<10, **all but two studies had the same score on this dose; *d*=effect size of the standardized regression coefficient, SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value= test statistic of slope, *p*-value= significance level; *k*=number of studies; *F*-distribution= test for the overall model; R^2= percentage of heterogeneity accounted for, tau^2/τ^2= variance of the underlying true effect sizes; *SE* tau^2= standard error of tau^2; Q_E= between-study heterogeneity; $p(Q_E)$=Q_E significance level.

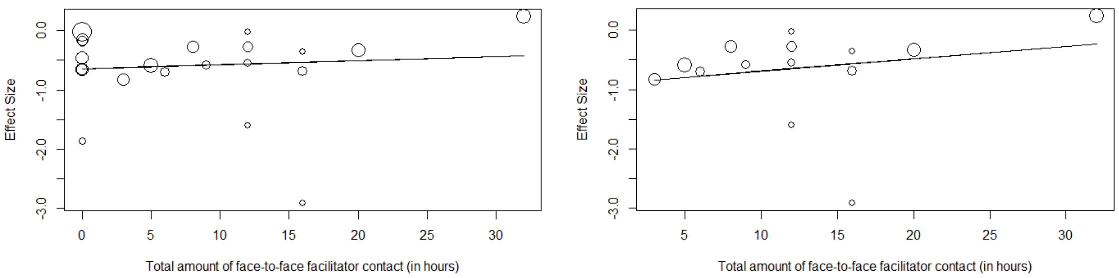
Appendix Figure 4.3.1.11

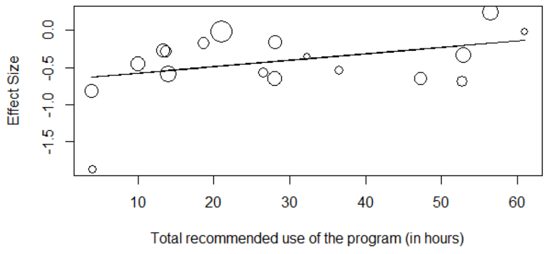
DASS-D measure-by-measure meta-regression plots for DASS-D depression at post-program compared to inactive controls for the doses total number of face-to-face sessions, duration of a face-to-face session (in hours), program length (in weeks), total amount of face-to-face facilitator contact (in hours), total amount of face-to-face facilitator contact with zero hours of contact excluded (in hours), total recommended use of the program (in hours), amount of face-to-face facilitator contact a week (in hours), amount of face-to-face facilitator contact excluding zero contact a week (in hours) and recommended use of the program a week (in hours).

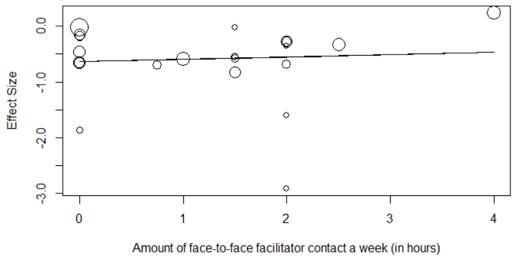
Primary dose variables

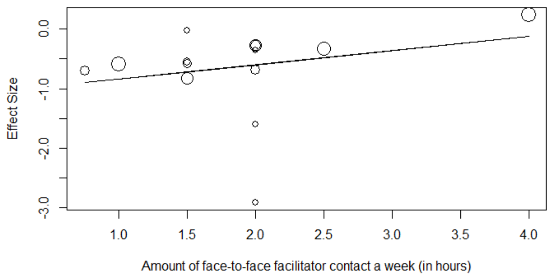


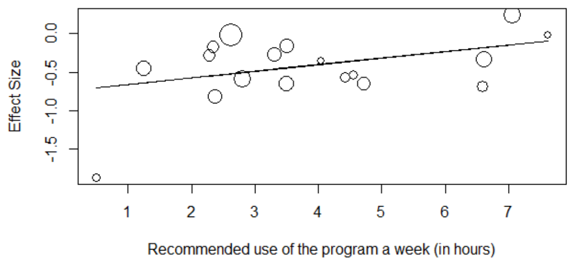
Composite dose variables











Appendix Table 4.3.1.12

Step-by-step False Discovery Rate correction (Benjamini-Hochberg procedure) for DASS-D depression at post-program compared to inactive controls

p	**.034**	.094	.112	.352	.438	.477	.651	.703	.75	.882	.94
Rank	1	2	3	4	5	6	7	8	9	10	11
$\boldsymbol{p_{adj}}$	.374	.411	.411	.875	.875	.875	.917	.917	.917	.94	.94

Significant results in bold.

Table 4.3.1.13

HADS-D measure-by-measure meta-regression analysis results for HADS-D depression by MBP dose for between group depression effect sizes at post-program compared to inactive controls

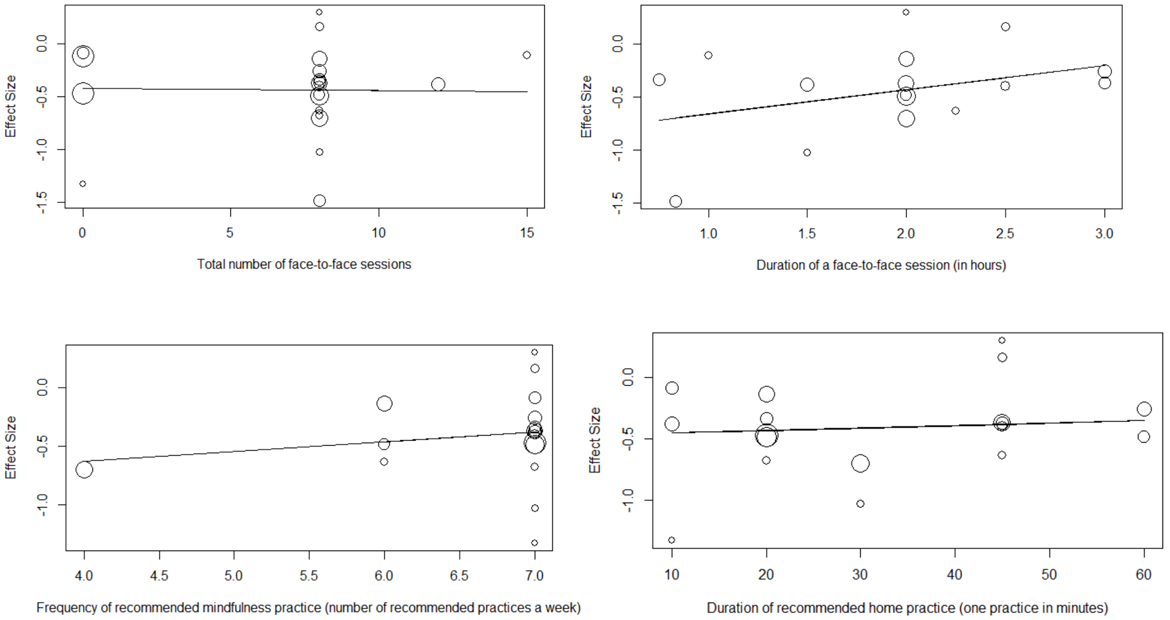
Dose	**Meta-regression model**							**Heterogeneity statistics**				
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	*SE* tau^2	Q_E	*p* (Q_E)
Total no. face-to-face sessions	-0.002	0.02	[-0.05, 0.05]	-0.1	.92	21	0.01	0.00%	0.08	0.04	45.12	<.001
Duration of a face-to-face session	0.23	0.14	[-0.07, 0.53]	1.62	.13	16	2.63	23.85%	0.06	0.04	28.11	.01
Program length**	-	-	-	-	-	-	-	-	-	-	-	-
Frequency of recommended practice	0.08	0.07	[-0.06, 0.22]	1.26	.23	18	1.58	0.00%	<.001	0.02	19.27	.255
Duration of a recommended practice	0.002	0.004	[-0.01, 0.01]	0.52	.61	18	0.27	0.00%	<.001	0.02	20.82	.19
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	*SE* tau^2	Q_E	*p* (Q_E)
Total amount of contact	0.01	0.01	[-0.01, 0.03]	0.97	.35	21	0.93	0.00%	0.08	0.05	46.16	<.001
Total amount of contact (excl. 0 hours)	0.02	0.01	[-0.003, 0.05]	1.94	.073	16	3.75	31.7%	0.05	0.04	26.71	.02
Total recommended use of program	0.001	0.003	[-0.01, 0.01]	0.29	.77	18	0.09	0.00%	<.001	0.02	21.06	.18
Total actual use of program*	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity excl. retreats**	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity incl. retreats	**0.34**	**0.18**	**[0.78, 0.02]**	**-2.23**	**.043**	**16**	**4.97**	**53.67%**	**0.04**	**0.04**	**24.36**	**.04**
Amount of contact/week	0.02	0.07	[-0.13, 0.17]	0.3	.77	21	0.09	0.00%	0.08	0.05	46.08	<.001
Amount of contact (excl. 0 hours)/week	0.1	0.13	[-0.18, 0.38]	0.79	.44	16	0.63	0.00%	0.08	0.05	32.46	.003
Recommended use of program/week	0.01	0.03	[-0.05, 0.06]	0.32	.76	18	0.1	0.00%	0.001	0.02	21.05	.18
Actual use of program/week*	-	-	-	-	-	-	-	-	-	-	-	-

significant results in bold*;* **k*<10, **all but two studies had the same score on this dose; *d*=effect size of the standardized regression coefficient, SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value= test statistic of slope, *p*-value= significance level; *k*=number of studies; *F*-distribution= test for the overall model; R^2= percentage of heterogeneity accounted for, tau^2/τ^2= variance of the underlying true effect sizes; *SE* tau^2= standard error of tau^2; Q_E= between-study heterogeneity; $p(Q_E)$=Q_E significance level.

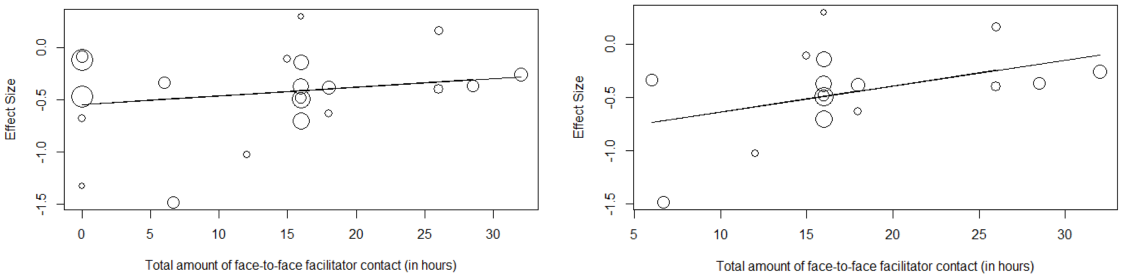
Appendix Figure 4.3.1.14

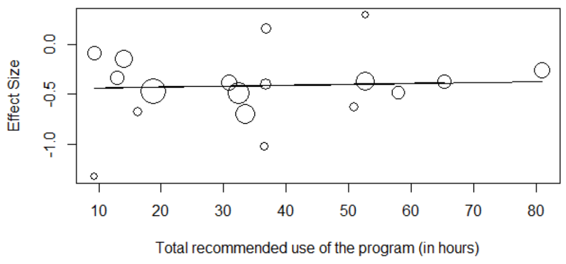
HADS-D measure-by-measure meta-regression plots for HADS-D depression at post-program compared to inactive controls for the doses total number of face-to-face sessions, duration of a face-to-face session (in hours), frequency (number) of recommended home practices a week, duration of a recommended home practice (one practice in minutes), total amount of face-to-face facilitator contact (in hours), total amount of face-to-face facilitator contact with zero hours of contact excluded (in hours), total recommended use of the program (in hours), amount of face-to-face facilitator contact a week (in hours), amount of face-to-face facilitator contact excluding zero contact a week (in hours) and recommended use of the program a week (in hours).

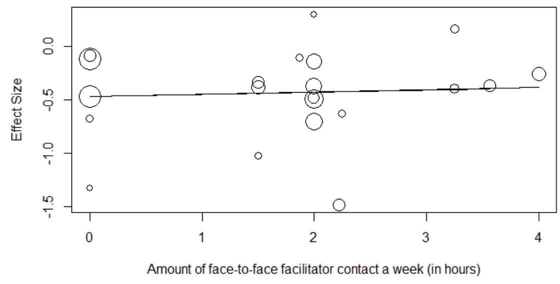
Primary dose variables

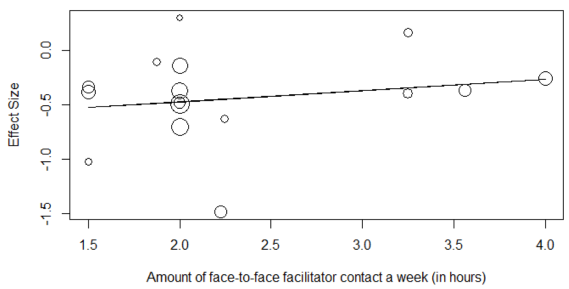


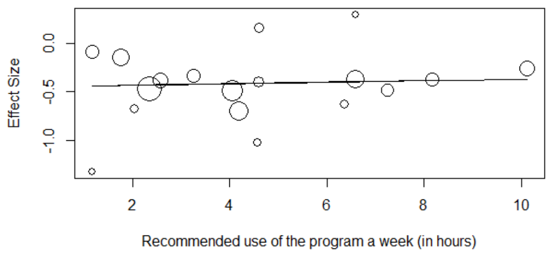
Composite dose variables











Appendix Table 4.3.1.15

Step-by-step False Discovery Rate correction (Benjamini-Hochberg procedure) for HADS-D depression at post-program compared to inactive controls

p	**.043**	.073	.127	.227	.246	.44	.611	.757	.77	.774	.921
Rank	1	2	3	4	5	6	7	8	9	10	11
$\boldsymbol{p}_{\mathbf{adj}}$	.402	.402	.466	.624	.761	.807	.851	.851	.851	.851	.921

Significant results in bold.

Appendix 4.3.2: Measure-by-Measure Meta-Regression Results: Anxiety

Appendix Table 4.3.2.1

BAI measure-by-measure meta-regression analysis results for BAI anxiety by MBP dose for between group depression effect sizes at immediately post-program compared to inactive and active controls

Compared to inactive control groups												
Dose	**Meta-regression model**						**Heterogeneity statistics**					
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R*^{2}**	***Tau*^{2}**	***SE tau*^{2}**	**Q_E**	***p* (Q_E)**
Total no. face-to-face sessions***	-	-	-	-	-	-	-	-	-	-	-	-
Duration of a face-to-face session	-0.07	.47	[-1.08, 0.94]	-0.15	.88	15	0.024	0.00%	0.65	0.3	89.76	<.001
Program length***	-	-	-	-	-	-	-	-	-	-	-	-
Frequency of recommended practice	-0.42	.0.36	[-1.21, 0.37]	-1.17	.27	14	1.37	2.34%	0.58	0.28	74.41	<.001
Duration of a recommended practice	0.03	0.02	[-0.01, 0.01]	1.89	.083	14	3.58	22.96%	0.46	0.23	56.49	<.001
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R*^{2}**	***Tau*^{2}**	***SE tau*^{2}**	**Q_E**	***p* (Q_E)**
Total amount of contact	0.01	0.04	[-0.08, 0.11]	0.28	.79	15	0.08	0.00%	0.64	0.29	88.89	<.001
Total amount of contact (excl. 0 hours)*****	-	-	-	-	-	-	-	-	-	-	-	-
Total recommended use of program	0.01	0.01	[-0.02, 0.04]	0.49	.63	14	0.24	0.00%	0.64	0.31	75.93	<.001
Total actual use of program*	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity excl. retreats**	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity incl. retreats***	-	-	-	-	-	-	-	-	-	-	-	-
Amount of contact/week	0.1	0.36	[-0.67, 0.88]	0.29	.78	15	0.08	0.00%	0.64	0.29	88.8	<.001
Amount of contact (excl. 0 hours)/week*****	-	-	-	-	-	-	-	-	-	-	-	-
Recommended use of program/week	0.11	0.15	[-0.2, 0.43]	0.79	.45	14	0.62	0.00%	0.61	0.3	73.21	<.001
Actual use of program/week*	-	-	-	-	-	-	-	-	-	-	-	-

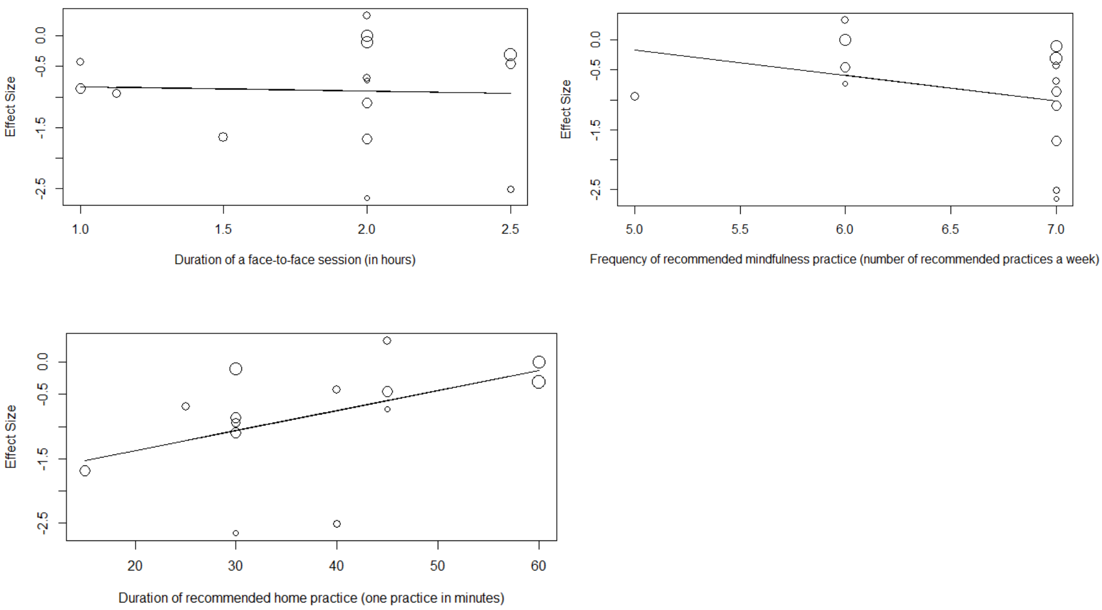
Compared to active control groups												
Dose	**Meta-regression model**							**Heterogeneity statistics**				
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	*SE* tau^2	Q_E	$p\ (Q_E)$
Total no. face-to-face sessions	-0.05	0.05	[-0.17, 0.07]	-0.88	.4	11	0.77	0.00%	0.15	0.1	25.4	.003
Duration of a face-to-face session*	-	-	-	-	-	-	-	-	-	-	-	-
Program length	-0.05	0.09	[-0.24, 0.15]	-0.54	.6	11	0.29	0.00%	0.17	0.11	26.59	.002
Frequency of recommended practice****	-	-	-	-	-	-	-	-	-	-	-	-
Duration of a recommended practice	-0.003	0.003	[-0.01, 0.004]	-1.01	.34	10	1.03	NA	0	0.03	2.94	.94
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	*SE* tau^2	Q_E	$p\ (Q_E)$
Total amount of contact	-0.01	0.02	[-0.05, 0.04]	-0.31	.77	11	0.09	0.00%	0.17	0.11	26.43	.002
Total amount of contact (excl. 0 hours)*	-	-	-	-	-	-	-	-	-	-	-	-
Total recommended use of program	0.001	0.002	[-0.004, 0.007]	0.59	0.57	10	0.35	NA	0	0.03	3.18	.92
Total actual use of program*	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity excl. retreats*	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity incl. retreats*	-	-	-	-	-	-	-	-	-	-	-	-
Amount of contact/week	-0.17	0.14	[-0.48, 0.15]	-1.22	.26	11	1.48	0.00%	0.13	0.09	24.12	.004
Amount of contact (excl. 0 hours)/week*	-	-	-	-	-	-	-	-	-	-	-	-
Recommended use of program/week	0.01	0.02	[-0.03, 0.06]	0.61	.56	10	0.37	NA	0	0.03	3.18	.92
Actual use of program/week*	-	-	-	-	-	-	-	-	-	-	-	-

*k<10, **all studies had the same score on this dose;*** all but one study had the same score on this dose; ****all but two studies had the same score on this dose; *****no studies had zero hours of contact; *d*=effect size of the standardized regression coefficient, SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value= test statistic of slope, *p*-value= significance level; *k*=number of studies; *F*-distribution= test for the overall model; R^2= percentage of heterogeneity accounted for, tau^2/τ^2= variance of the underlying true effect sizes; *SE* tau^2= standard error of tau^2; Q_E= between-study heterogeneity; $p(Q_E)$)=Q_E significance level.

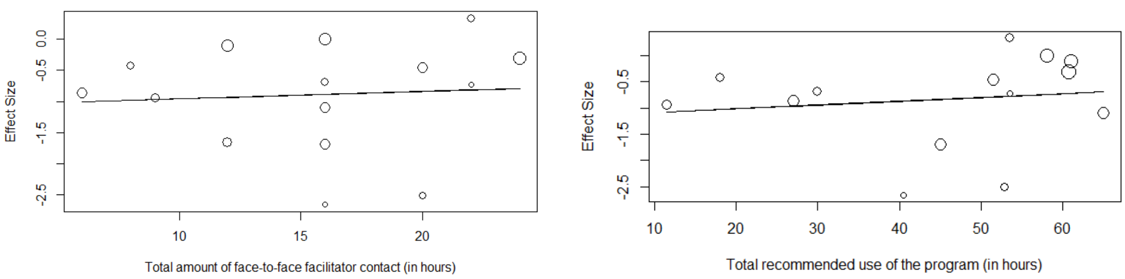
Appendix Figure 4.3.2.2

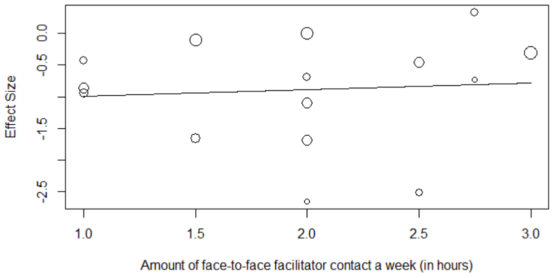
BAI measure-by-measure meta-regression plots for BAI anxiety at post-program compared to inactive and active controls for the doses total number of face-to-face sessions (compared to active controls only), duration of a face-to-face session (in hours; compared to inactive controls only), program length (in weeks; compared to active controls only), frequency (number) of recommended home practices a week (compared to inactive controls only), duration of recommended home practice (one practice in minutes), total amount of face-to-face facilitator contact (in hours), total recommended use of the program (in hours), amount of face-to-face facilitator contact a week (in hours), and recommended use of the program a week (in hours).

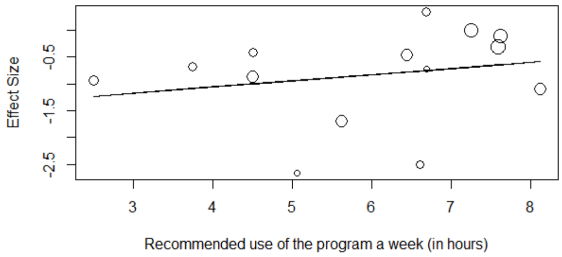
Compared to inactive controls – primary dose variables



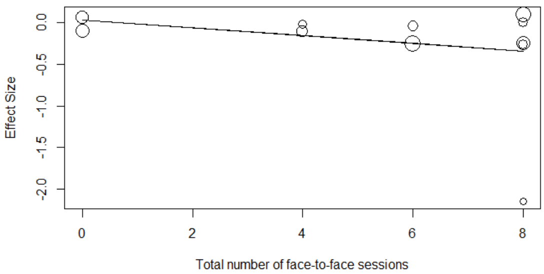
Compared to inactive controls – composite dose variables

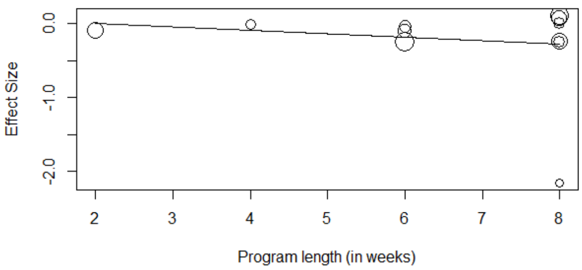


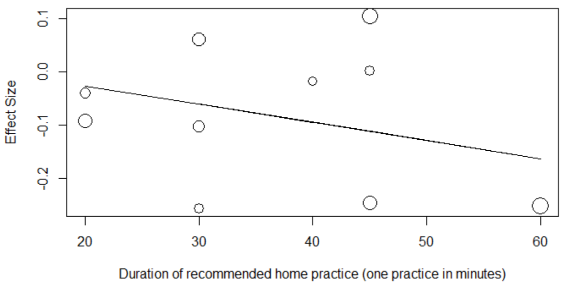




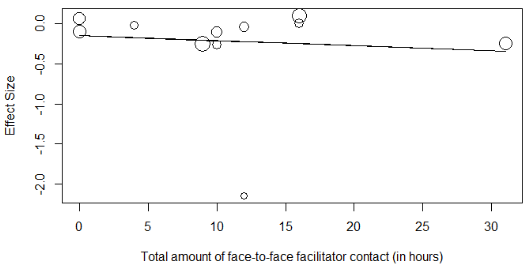
Compared to active controls – primary dose variables

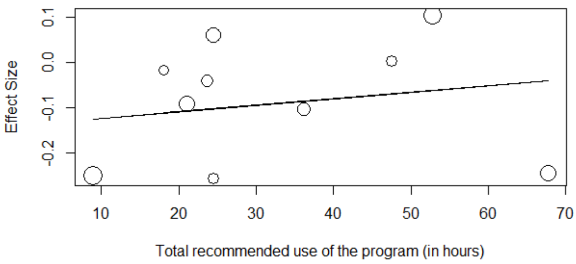


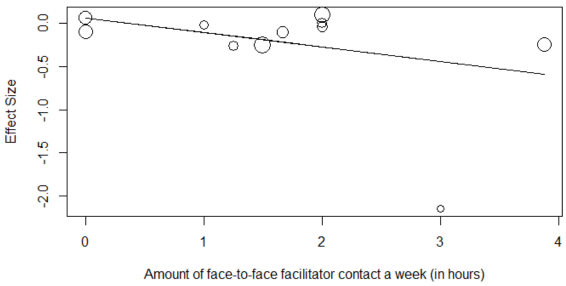




Compared to active controls – composite dose variables






Effect Size
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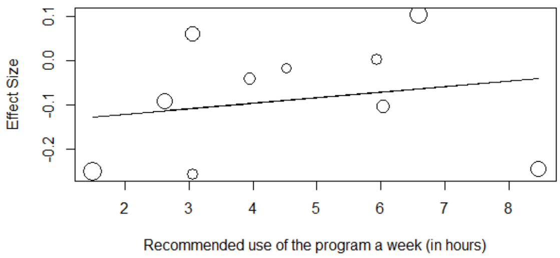
-0.5
-1.0
-1.5
-2.0
0
1
2
3
4
Amount of face-to-face facilitator contact a week (in hours)


Effect Size
0.1
0.0
-0.1
-0.2
2
3
4
5
6
7
8
Recommended use of the program a week (in hours)

Appendix Table 4.3.2.3

DASS-A measure-by-measure meta-regression analysis results for DASS-A anxiety by MBP dose for between group anxiety effect sizes at post-program compared to inactive controls

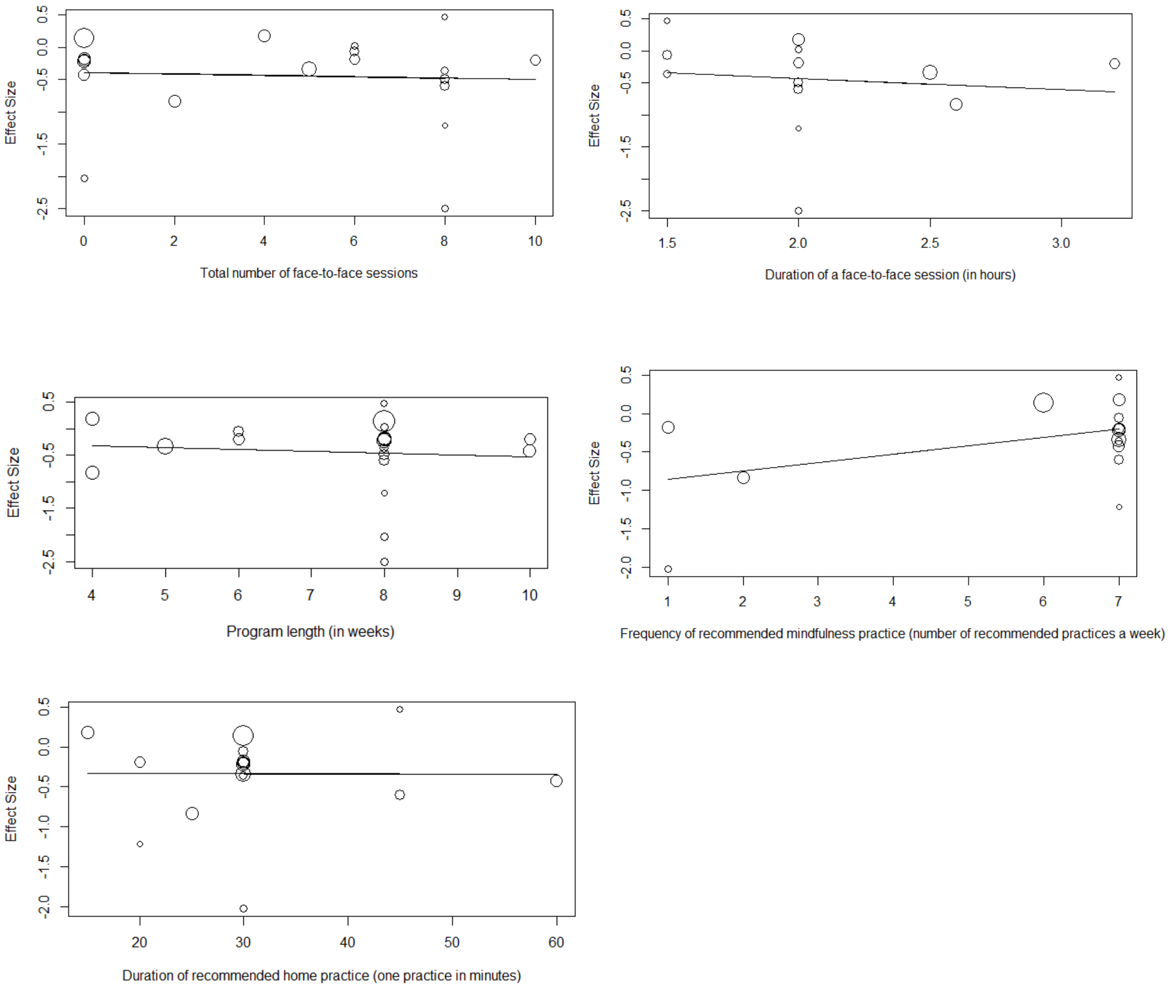
Dose	Meta-regression model							Heterogeneity statistics				
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	*SE* tau^2	Q_E	$p\ (Q_E)$
Total no. face-to-face sessions	-0.01	0.05	[-0.11, 0.09]	-0.26	.8	19	0.07	0.00%	0.35	0.15	71.41	<.001
Duration of a face-to-face session	-0.17	0.44	[-1.13, 0.79]	-0.4	.7	13	0.16	0.00%	0.38	0.21	43.98	<.001
Program length	-0.04	0.1	[-0.24, 0.17]	-0.39	.7	19	0.15	0.00%	0.35	0.15	73.63	<.001
Frequency of recommended practice	0.11	0.05	[-0.01, 0.23]	2.04	.061	16	4.17	25.15%	0.11	0.07	36.73	<.001
Duration of a recommended practice	<.001	0.01	[-0.03, 0.03]	-0.02	.99	16	<.001	0.00%	0.17	0.09	44.17	<.001
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	*SE* tau^2	Q_E	$p\ (Q_E)$
Total amount of contact	-0.01	0.02	[-0.05, 0.04]	-0.25	.81	19	0.06	0.00%	0.35	0.15	72.53	<.001
Total amount of contact (excl. 0 hours)	-0.01	0.03	[-0.07, 0.05]	-0.28	.79	13	0.08	0.00%	0.39	0.21	44.7	<.001
Total recommended use of program	0.01	0.01	[-0.01, 0.03]	1	.33	16	1	0.00%	0.16	0.09	43.71	<.001
Total actual use of program*	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity excl. retreats**	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity incl. retreats***	-	-	-	-	-	-	-	-	-	-	-	-
Amount of contact/week	-0.004	0.15	[-0.33, 0.32]	-0.03	.98	19	0.001	0.00%	0.36	0.15	72.66	<.001
Amount of contact (excl. 0 hours)/week	0.01	0.27	[-0.58, 0.6]	0.04	.97	13	0.002	0.00%	0.39	0.21	44.39	<.001
Recommended use of program/week	0.09	0.07	[-0.06, 0.24]	1.3	.21	16	1.7	0.00%	0.15	0.09	43.61	<.001
Actual use of program/week*	-	-	-	-	-	-	-	-	-	-	-	-

*$k<10$, **all but one study had the same score on this dose;***all but two studies had the same score on this dose; *d*=effect size of the standardized regression coefficient, SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value= test statistic of slope, *p*-value= significance level; *k*=number of studies; *F*-distribution= test for the overall model; R^2= percentage of heterogeneity accounted for, tau^2/τ^2= variance of the underlying true effect sizes; *SE* tau^2= standard error of tau^2; Q_E= between-study heterogeneity; $p(Q_E)$)=Q_E significance level.

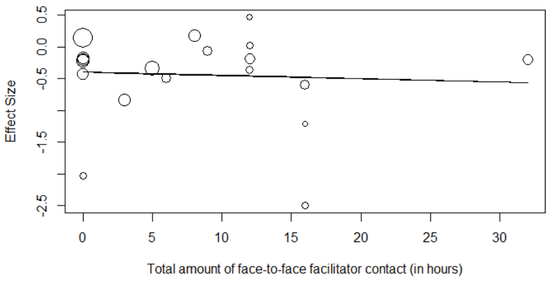
Appendix Figure 4.3.2.4

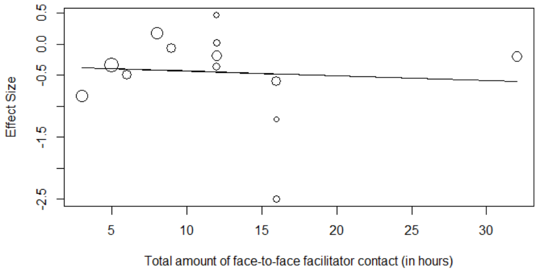
DASS-A anxiety measure-by-measure meta-regression plots for DASS-A anxiety at post-program compared to inactive controls for the doses total number of face-to-face sessions, duration of a face-to-face session (in hours), program length (in weeks), frequency (number) of recommended home practices a week, duration of recommended home practice (one practice in minutes), total amount of face-to-face facilitator contact (in hours), total amount of face-to-face facilitator contact with zero hours of contact excluded (in hours), total recommended use of the program (in hours), amount of face-to-face facilitator contact a week (in hours), amount of face-to-face facilitator contact excluding zero contact a week (in hours) and recommended use of the program a week (in hours).

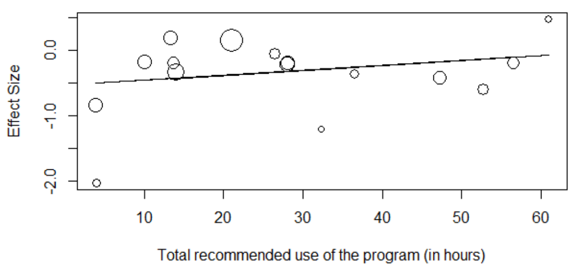
Primary dose variables

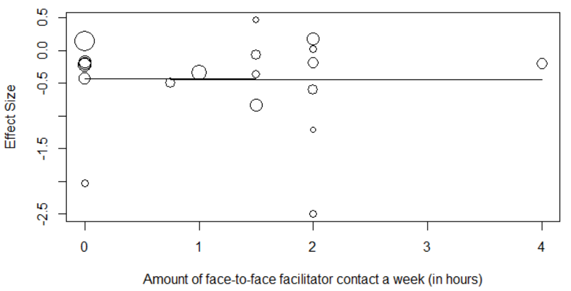


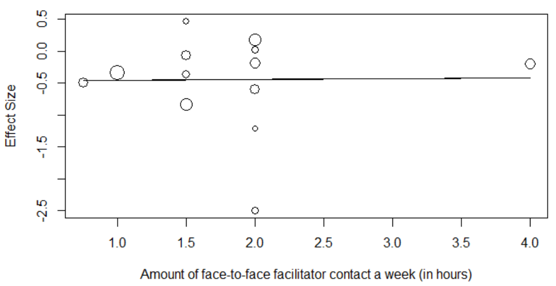
Composite dose variables

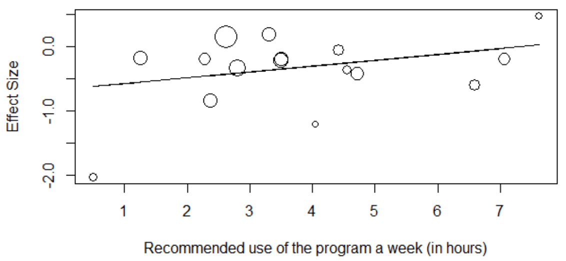












Appendix Table 4.3.2.5

HADS-A measure-by-measure meta-regression analysis results for HADS-A anxiety by MBP dose for between group anxiety effect sizes at post-program compared to inactive controls

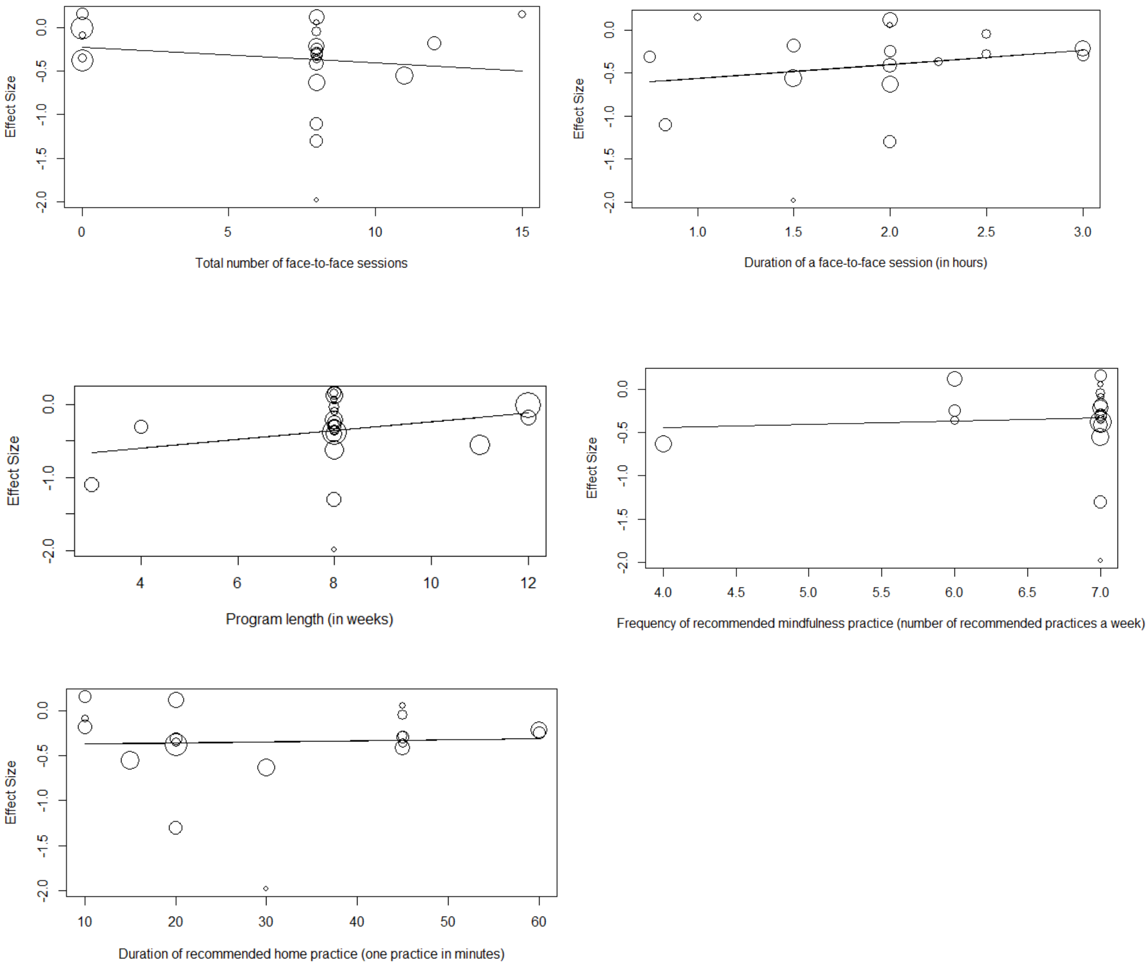
Dose	Meta-regression model							Heterogeneity statistics				
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	*SE* tau^2	Q_E	$p\ (Q_E)$
Total no. face-to-face sessions	-0.02	0.02	[-0.07, 0.03]	-0.8	.43	22	0.64	0.00%	0.1	0.05	57.99	<.001
Duration of a face-to-face session	0.16	0.17	[-0.21, 0.53]	0.94	.36	17	0.88	0.00%	0.12	0.07	45.66	<.001
Program length	0.06	0.04	[-0.03, 0.15]	1.43	.17	22	2.05	10.9%	0.09	0.05	54	<.001
Frequency of recommended practice	0.04	0.12	[-0.21, 0.28]	0.31	.76	19	0.1	0.00%	0.08	0.05	42.53	<.001
Duration of a recommended practice	0.001	0.01	[-0.01, 0.01]	0.22	.83	19	0.05	0.00%	0.08	0.05	42.87	<.001
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	*SE* tau^2	Q_E	$p\ (Q_E)$
Total amount of contact	-0.001	0.01	[-0.02, 0.02]	-0.1	.92	22	0.01	0.00%	0.11	0.05	61.75	<.001
Total amount of contact (excl. 0 hours)	0.02	0.02	[-0.01, 0.06]	1.45	.17	17	2.09	10.04%	0.11	0.06	43.04	<.001
Total recommended use of program	-0.001	0.01	[-0.01, 0.01]	-0.32	.76	19	0.1	0.00%	0.08	0.05	42.89	<.001
Total actual use of program*	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity excl. retreats**	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity incl. retreats	-0.18	0.24	[-0.69, 0.33]	-0.75	.46	17	0.57	0.00%	0.12	0.07	46	<.001
Amount of contact/week	-0.04	0.08	[-0.21, 0.12]	-0.56	.58	22	0.32	0.00%	0.1	0.05	60.26	<.001
Amount of contact (excl. 0 hours)/week	0.21	0.15	[-0.19, 0.43]	0.83	.42	17	0.69	0.00%	0.12	0.07	47.23	<.001
Recommended use of program/week	-0.01	0.04	[-0.09, 0.07]	-0.3	.77	19	0.09	0.00%	0.08	0.05	42.94	<.001
Actual use of program/week*	-	-	-	-	-	-	-	-	-	-	-	-

**k*<10, **all but two studies had the same score on this dose; *d*=effect size of the standardized regression coefficient, SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value= test statistic of slope, *p*-value= significance level; *k*=number of studies; *F*-distribution= test for the overall model; R^2= percentage of heterogeneity accounted for, tau^2/τ^2= variance of the underlying true effect sizes; *SE* tau^2= standard error of tau^2; Q_E= between-study heterogeneity; $p(Q_E)$)=Q_E significance level.

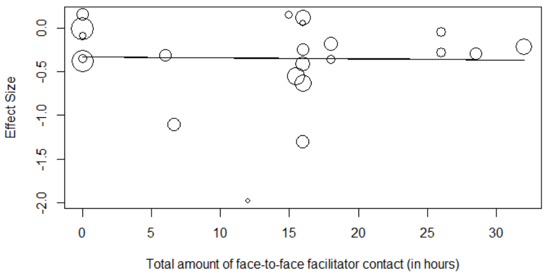
Appendix Figure 4.3.2.6

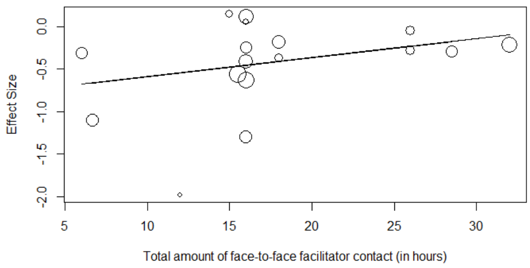
HADS-A anxiety measure-by-measure meta-regression plots for HADS-A anxiety at post-program compared to inactive controls for the doses total number of face-to-face sessions, duration of a face-to-face session (in hours), program length (in weeks), frequency (number) of recommended home practices a week, duration of recommended home practice (one practice in minutes), total amount of face-to-face facilitator contact (in hours), total amount of face-to-face facilitator contact with zero hours of contact excluded (in hours), total recommended use of the program (in hours), program intensity (number of sessions a week including all-day retreats), amount of face-to-face facilitator contact a week (in hours), amount of face-to-face facilitator contact excluding zero contact a week (in hours) and recommended use of the program a week (in hours).

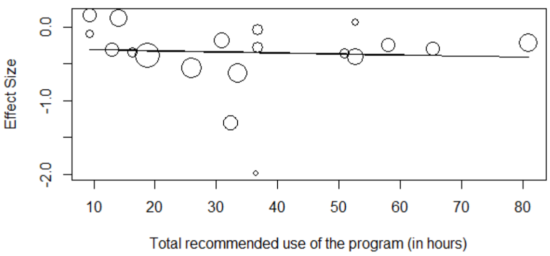
Primary dose variables

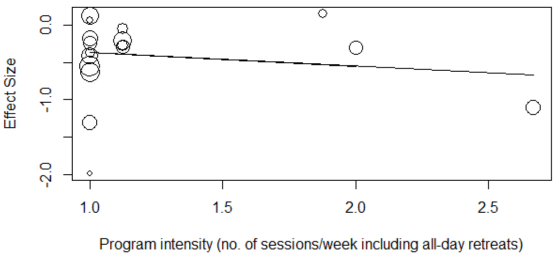


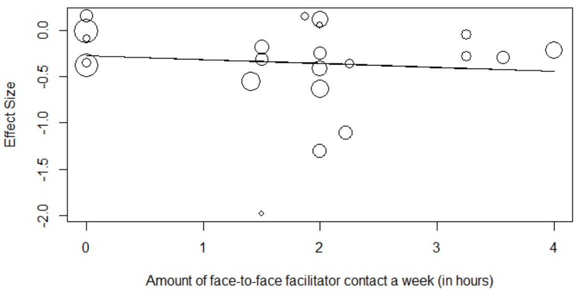
Composite dose variables

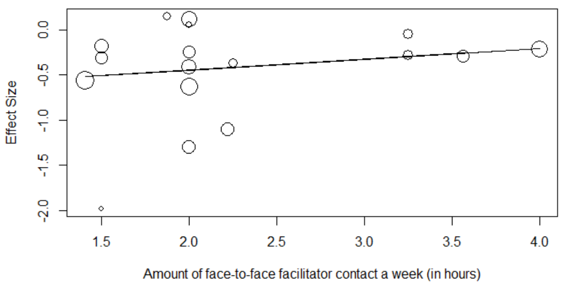


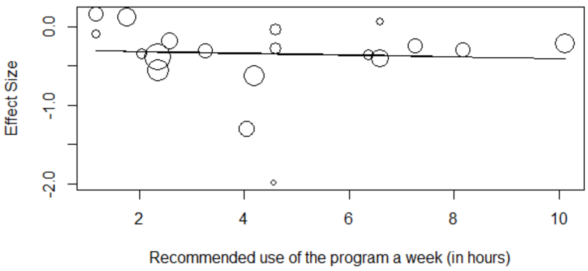












Appendix 4.3.3: Measure-by-Measure Meta-Regression Results: Stress
Appendix Table 4.3.3.1
DASS-S measure-by-measure meta-regression analysis results for DASS stress by MBP dose for between group stress effect sizes at post-program compared to inactive controls

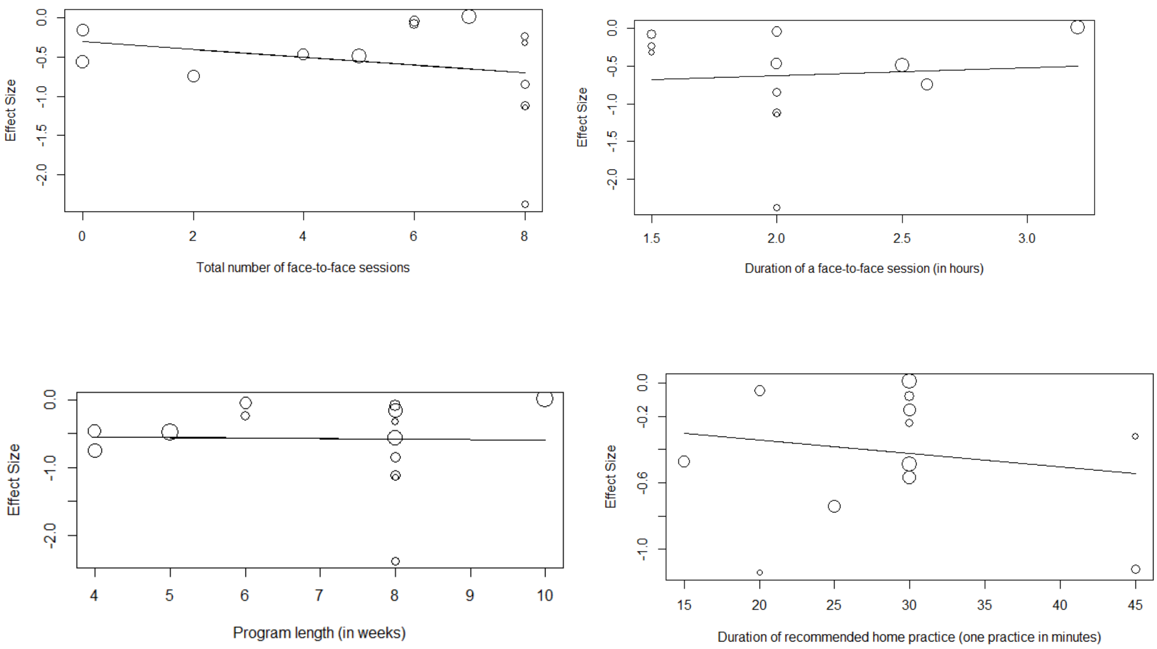
Dose	Meta-regression model							Heterogeneity statistics				
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	*SE* tau^2	Q_E	$p\ (Q_E)$
Total no. face-to-face sessions	-0.05	0.06	[-0.17, 0.07]	-0.91	.38	14	0.84	0.00%	0.23	0.13	41.81	<.001
Duration of a face-to-face session	0.11	0.39	[-0.77, 0.98]	0.27	.79	12	0.07	0.00%	0.32	0.19	38.38	<.001
Program length	-0.01	0.09	[-0.21, 0.2]	-0.07	.95	14	0.01	0.00%	0.25	0.14	41.46	<.001
Frequency of recommended practice**	-	-	-	-	-	-	-	-	-	-	-	-
Duration of a recommended practice	-0.01	0.01	[-0.04, 0.02]	-0.59	.57	12	0.35	0.00%	0.06	0.06	18.24	.051
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	*SE* tau^2	Q_E	$p\ (Q_E)$
Total amount of contact	-0.001	0.02	[-0.04, 0.04]	-0.03	.98	14	0.001	0.00%	0.25	0.14	40.83	<.001
Total amount of contact (excl. 0 hours)	0.01	0.03	[-0.05, 0.06]	0.25	.81	12	0.06	0.00%	0.32	0.19	36.98	<.001
Total recommended use of program	0.003	0.01	[-0.01, 0.02]	0.54	.6	12	0.29	0.00%	0.05	0.06	17.26	.07
Total actual use of program*	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity excl. retreats**	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity incl. retreats**	-	-	-	-	-	-	-	-	-	-	-	-
Amount of contact/week	0.07	0.15	[-0.25, 0.39]	0.48	.64	14	0.23	0.00%	0.24	0.14	39.59	<.001
Amount of contact (excl. 0 hours)/week	0.02	0.19	[-0.41, 0.45]	0.1	.92	12	0.01	0.00%	0.3	0.18	36.83	<.001
Recommended use of program/week	0.03	0.06	[-0.1, 0.15]	0.45	.66	12	0.2	0.00%	0.06	0.06	17.59	.062
Actual use of program/week*	-	-	-	-	-	-	-	-	-	-	-	-

*k<10, ** all but one study had the same score on this dose; *d*=effect size of the standardized regression coefficient, SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value= test statistic of slope, *p*-value= significance level; *k*=number of studies; *F*-distribution= test for the overall model; R^2= percentage of heterogeneity accounted for, tau^2/τ^2= variance of the underlying true effect sizes; *SE* tau^2= standard error of tau^2; Q_E= between-study heterogeneity; $p(Q_E)$)=Q_E significance level.

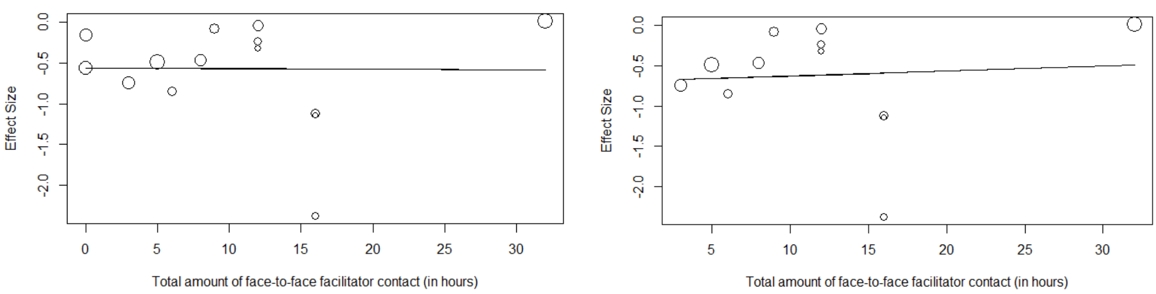
Appendix Figure 4.3.3.2

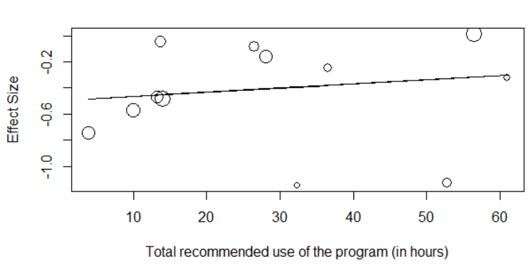
DASS stress measure-by-measure meta-regression plots for DASS stress at post-program compared to inactive controls for the doses total number of face-to-face sessions, duration of a face-to-face session (in hours), program length (in weeks), duration of recommended home practice (one practice in minutes), total amount of face-to-face facilitator contact (in hours), total amount of face-to-face facilitator contact with zero hours of contact excluded (in hours), total recommended use of the program (in hours), amount of face-to-face facilitator contact a week (in hours), amount of face-to-face facilitator contact excluding zero contact a week (in hours) and recommended use of the program a week (in hours).

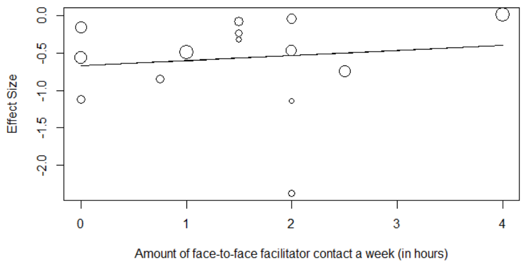
Primary dose variables

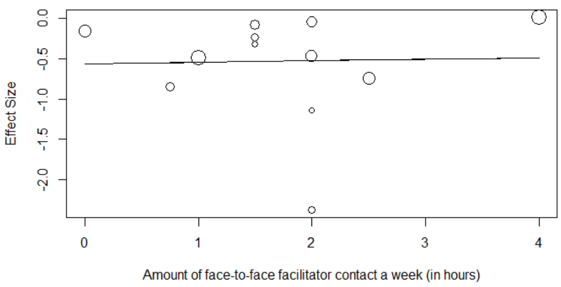


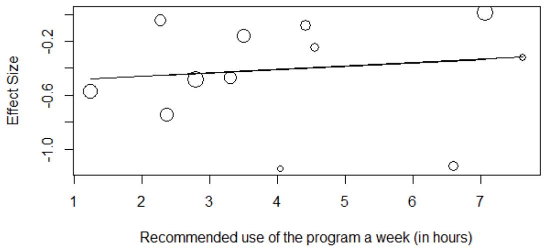
Composite dose variables




Effect Size
-0.2
-0.6
-1.0
10
20
30
40
50
60
Total recommended use of the program (in hours)


Effect Size
0.0
-0.5
-1.0
-1.5
-2.0
0
1
2
3
4
Amount of face-to-face facilitator contact a week (in hours)


Effect Size
0.0
-0.5
-1.0
-1.5
-2.0
0
1
2
3
4
Amount of face-to-face facilitator contact a week (in hours)


Effect Size
-0.2
-0.6
-1.0
1
2
3
4
5
6
7
Recommended use of the program a week (in hours)

Appendix Table 4.3.3.3

PSS measure-by-measure meta-regression analysis results for PSS stress by MBP dose for between group stress effect sizes at immediately post-program compared to inactive and active controls

Compared to inactive control groups												
Dose	**Meta-regression model**						**Heterogeneity statistics**					
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total no. face-to-face sessions	-0.05	0.05	[-0.16, 0.05]	-1.04	.31	35	1.08	0.28%	1.16	0.31	444.85	<.001
Duration of a face-to-face session	0.2	0.26	[-0.34, 0.74]	0.76	.45	24	0.58	0.00%	1.44	0.47	264.11	<.001
Program length	-0.09	0.07	[-0.23, 0.06]	-1.21	.23	35	1.47	1.49%	1.15	0.31	448.59	<.001
Frequency of recommended practice	-0.08	0.16	[-0.4, 0.25]	-0.48	.64	33	0.23	0.00%	1.22	0.33	436.99	<.001
Duration of a recommended practice	0.02	0.01	[-0.01. 0.04]	1.19	.24	33	1.42	0.86%	1.17	0.32	435.01	<.001
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total amount of contact	0.003	0.02	[-0.03, 0.04]	0.19	.85	35	0.04	0.00%	1.21	0.32	447.6	<.001
Total amount of contact (excl. 0 hours)	0.03	0.03	[-0.04, 0.1]	0.81	.43	24	0.66	0.00%	1.43	0.47	267.91	<.001
Total recommended use of program	0.002	0.01	[-0.02, 0.02]	0.21	.83	33	0.05	0.00%	1.23	0.34	437.01	<.001
Total actual use of program	0.01	0.01	[-0.01, 0.03]	1.14	.29	10	1.31	100%	0	0.03	6.95	.542
Program intensity excl. retreats	0.52	0.7	[-0.93, 1.97]	0.75	.46	24	0.56	0.00%	1.44	0.47	281.79	<.001
Program intensity incl. retreats	0.44	0.63	[-0.86, 1.75]	0.71	.49	24	0.5	0.00%	1.45	0.47	281.783	<.001
Amount of contact/week	0.06	0.13	[-0.19, 0.32]	0.49	0.63	35	0.24	0.00%	1.2	0.32	448.51	<.001
Amount of contact (excl. 0 hours)/week	0.24	0.21	[-0.19, 0.66]	1.15	.26	24	1.32	1.46%	1.39	0.45	273.08	<.001
Recommended use of program/week	0.06	0.08	[-0.11, 0.22]	0.7	.49	33	0.49	0.00%	1.21	0.33	436.05	<.001
Actual use of program/week	0.11	0.11	[-0.13, 0.36]	1.05	.33	10	1.09	1.05	0.01	0.03	7.21	.514

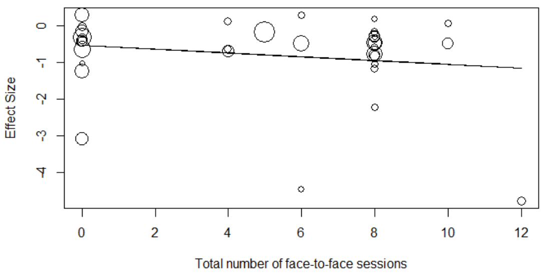
Compared to active control groups												
Dose	**Meta-regression model**						**Heterogeneity statistics**					
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	*SE* tau^2	Q_E	*p* (Q_E)
Total no. face-to-face sessions	-0.04	0.04	[-0.13, 0.06]	-0.82	.42	20	0.67	2.58%	0.19	0.09	68.4	<.001
Duration of a face-to-face session	0.16	0.18	[-0.21, 0.53]	0.91	.38	18	0.82	0.00%	0.24	0.12	67.3	<.001
Program length	-0.04	0.06	[-0.16, 0.08]	-0.7	.5	20	0.48	1.09%	0.19	0.09	66.75	<.001
Frequency of recommended practice	0.01	0.17	[-0.35, 0.37]	0.07	.95	19	0.004	0.00%	0.16	0.08	69.08	<.001
Duration of a recommended practice	0.01	0.01	[-0.01, 0.02]	0.96	.35	19	0.93	0.00%	0.15	0.08	61.36	<.001
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	*SE* tau^2	Q_E	*p* (Q_E)
Total amount of contact	0.01	0.02	[-0.03, 0.04]	0.42	.68	20	0.17	0.00%	0.21	0.1	77.77	<.001
Total amount of contact (excl. 0 hours)	0.01	0.02	[-0.04, 0.05]	0.34	.74	18	0.12	0.00%	0.26	0.12	77.6	<.001
Total recommended use of program	0.001	0.01	[-0.01, 0.01]	0.17	.87	19	0.03	0.00%	0.16	0.08	68.85	<.001
Total actual use of program*	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity excl. retreats**	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity incl. retreats**	-	-	-	-	-	-	-	-	-	-	-	-
Amount of contact/week	0.11	0.12	[-0.15, 0.37]	0.88	.39	20	0.77	0.00%	0.2	0.1	72.43	<.001
Amount of contact (excl. 0 hours)/week	0.16	0.18	[-0.22, 0.53]	0.89	.39	18	0.8	0.00%	0.24	0.12	68.9	<.001
Recommended use of program/week	0.04	0.05	[-0.07, 0.15]	0.71	.49	19	0.5	0.00%	0.16	0.08	63.77	<.001
Actual use of program/week*	-	-	-	-	-	-	-	-	-	-	-	-

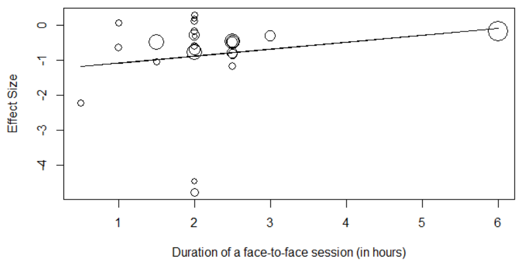
**k*<10, **all but one study had the same score on this dose; *d*=effect size of the standardized regression coefficient, SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value= test statistic of slope, *p*-value= significance level; *k*=number of studies; *F*-distribution= test for the overall model; R^2= percentage of heterogeneity accounted for, tau^2/τ^2= variance of the underlying true effect sizes; *SE* tau^2= standard error of tau^2; Q_E= between-study heterogeneity; $p(Q_E)$=Q_E significance level.

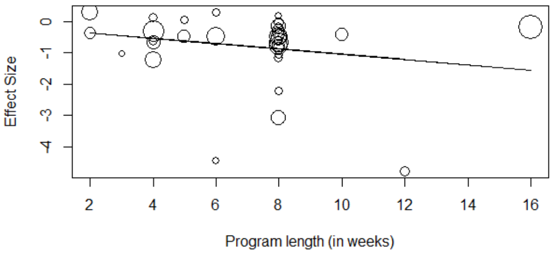
Appendix Figure 4.3.3.4

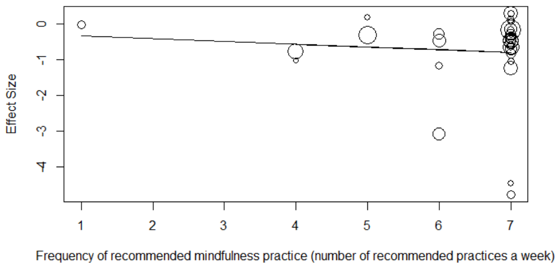
PSS stress measure-by-measure meta-regression plots for PSS stress at post-program compared to inactive and active controls for the doses total number of face-to-face sessions, duration of a face-to-face session (in hours), program length (in weeks), frequency (number) of recommended home practices a week, duration of recommended home practice (one practice in minutes), total amount of face-to-face facilitator contact (in hours), total amount of face-to-face facilitator contact with zero hours of contact excluded (in hours), total recommended use of the program (in hours), total actual use of the program (in hours; compared to inactive controls only), program intensity (number of sessions a week excluding all-day retreats; compared to inactive controls only), program intensity (number of sessions a week including all-day retreats), amount of face-to-face facilitator contact a week (in hours), amount of face-to-face facilitator contact excluding zero contact a week (in hours), recommended use of the program a week (in hours) and actual use of the program a week (in hours; compared to inactive controls only).

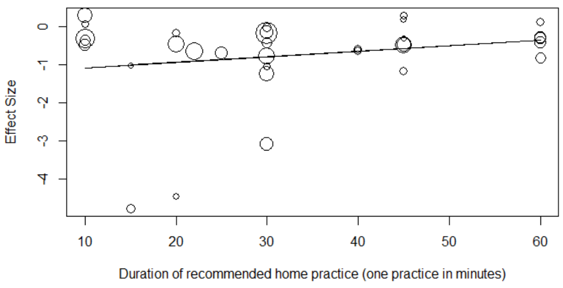
Compared to inactive controls – primary dose variables



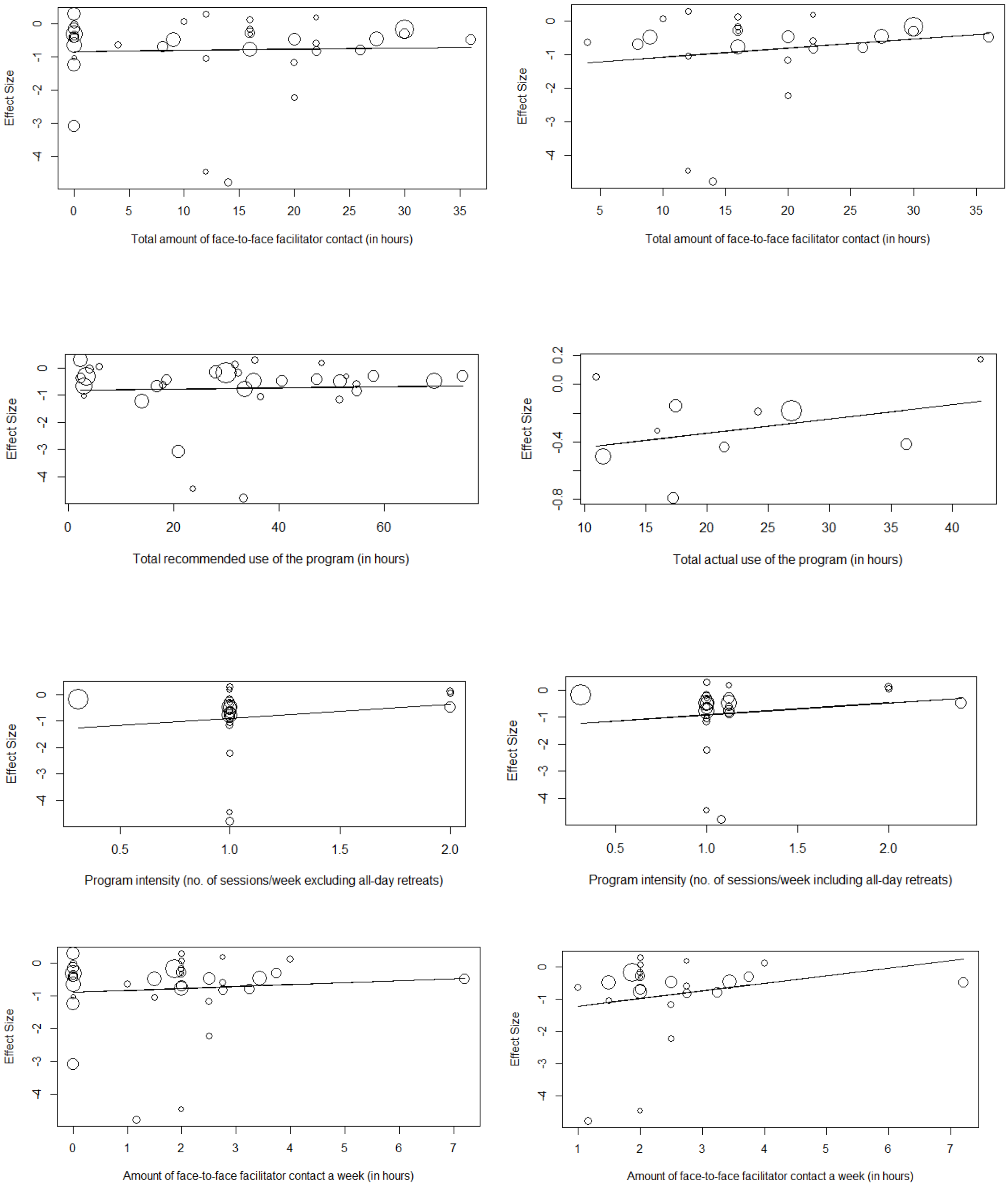


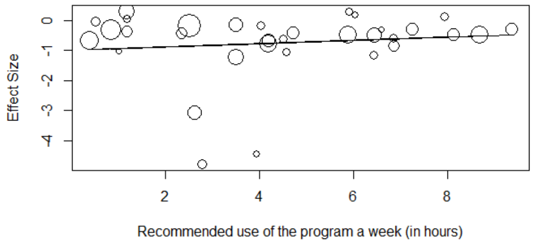


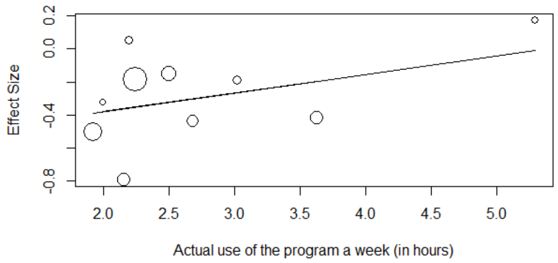




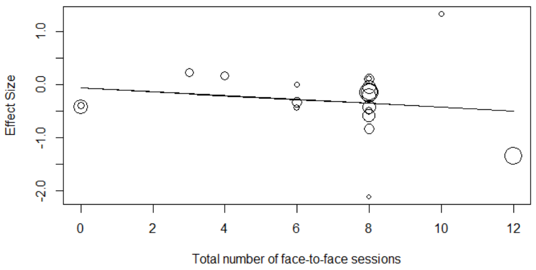
Compared to inactive controls – composite dose variables

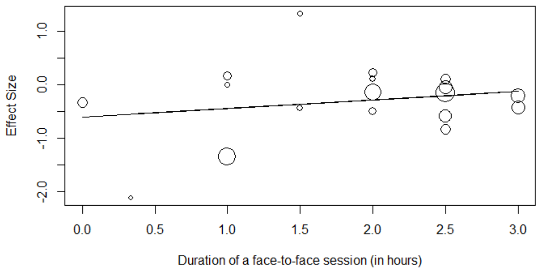


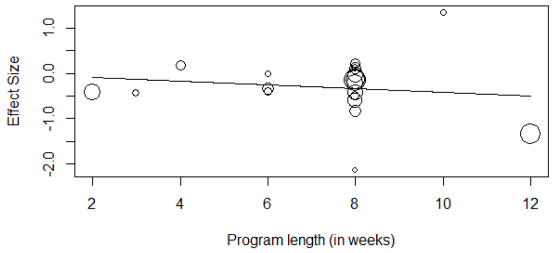


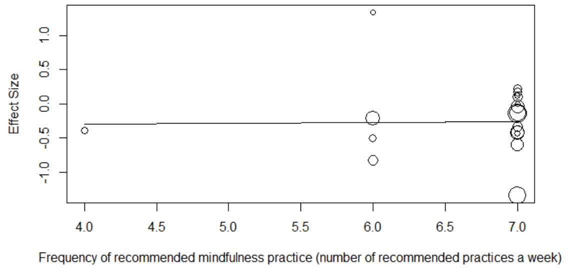


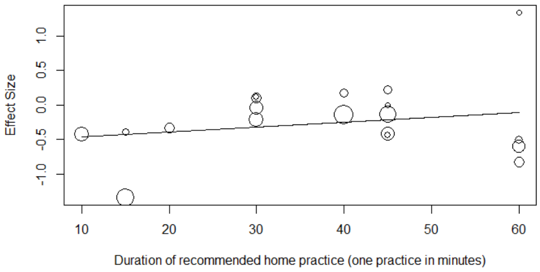
Compared to active controls – primary dose variables



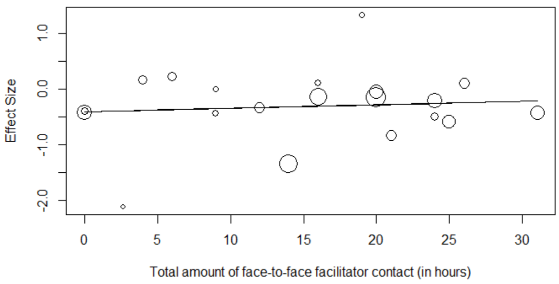


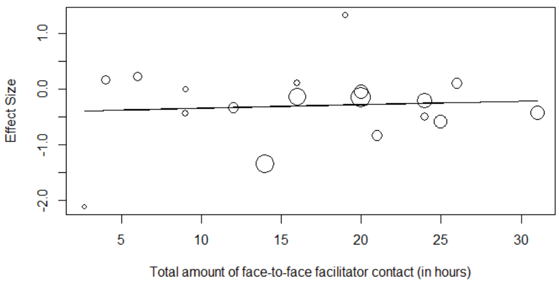


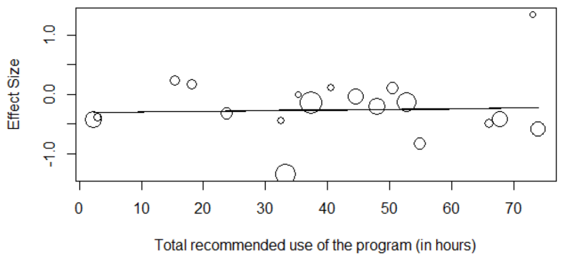




Compared to active controls – composite dose variables



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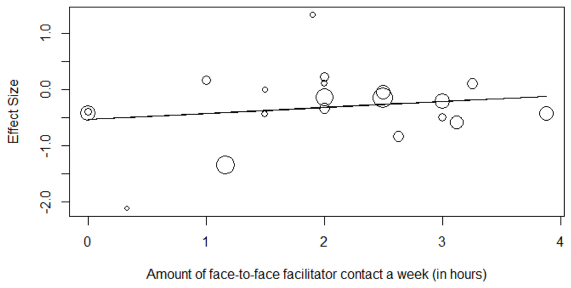
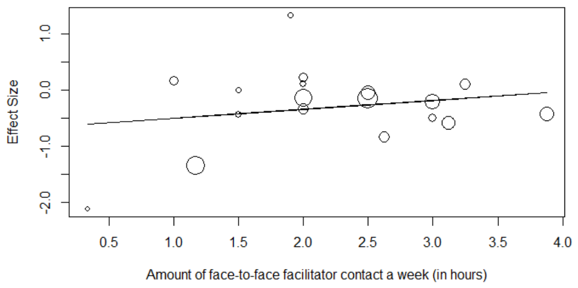
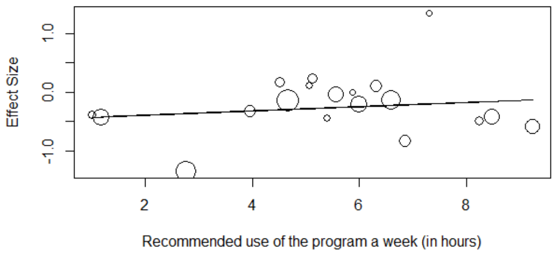
1.0
0.0
-1.0
0
10
20
30
40
50
60
70
Total recommended use of the program (in hours)


Effect Size
1.0
0.0
-1.0
-2.0
0
1
2
3
4
Amount of face-to-face facilitator contact a week (in hours)


Effect Size
1.0
0.0
-1.0
-2.0
0.5
1.0
1.5
2.0
2.5
3.0
3.5
4.0
Amount of face-to-face facilitator contact a week (in hours)


Effect Size
1.0
0.0
-1.0
2
4
6
8
Recommended use of the program a week (in hours)

Appendix Table 4.3.3.5

PSS measure-by-measure meta-regression analysis results for PSS stress by MBP dose for between group stress effect sizes at 1-4 months follow-up compared to inactive controls

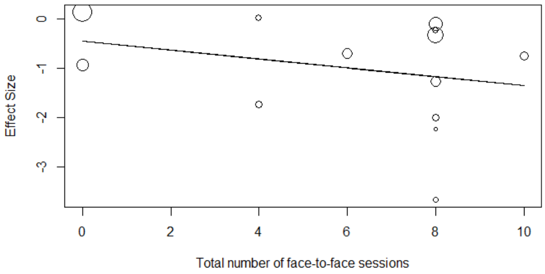
Dose	**Meta-regression model**							**Heterogeneity statistics**				
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total no. face-to-face sessions	-0.09	0.09	[-0.3, 0.12]	-0.97	.36	13	0.93	0.00%	0.95	0.46	90.18	<.001
Duration of a face-to-face session	**1.12**	**.4**	**[0.23, 2.02]**	**2.84**	**.019**	**11**	**8.08**	**49.7%**	**0.51**	**0.3**	**42.09**	**<.001**
Program length	-0.08	0.2	[-0.51, 0.36]	-0.4	.7	13	0.16	0.00%	1.03	0.49	98.87	<.001
Frequency of recommended practice	-0.32	0.38	[-1.18, 0.53]	-0.85	.42	11	0.73	0.00%	0.46	0.27	45.23	<.001
Duration of a recommended practice	0.01	0.02	[-0.03, 0.05]	0.62	.55	11	0.38	0.00%	0.5	0.29	55.97	<.001
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total amount of contact	-0.003	0.03	[-0.07, 0.06]	-0.1	.92	13	0.01	0.00%	1.04	0.5	96.78	<.001
Total amount of contact (excl. 0 hours)	0.03	0.04	[-0.06, 0.12]	0.71	.49	11	0.51	0.00%	1.08	0.58	69.8	<.001
Total recommended use of program	0.01	0.01	[-0.03, 0.04]	0.36	.73	11	0.13	0.00%	0.52	0.3	56.95	<.001
Total actual use of program*	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity excl. retreats**	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity incl. retreats	0.79	0.72	[-0.82. 2.41]	1.11	.3	11	1.23	0.22%	1.01	0.55	72.12	<.001
Amount of contact/week	0.05	0.17	[-0.33, 0.42]	0.26	.8	13	0.07	0.00%	1.04	0.5	97.87	<.001
Amount of contact (excl. 0 hours)/week	0.22	0.21	[-0.26, 0.7]	1.04	.32	11	1.09	0.00%	1.02	0.55	70.69	<.001
Recommended use of program/week	0.04	0.1	[-0.18, 0.27]	0.43	.69	11	0.18	0.00%	0.52	0.3	57.25	<.001
Actual use of program/week*	-	-	-	-	-	-	-	-	-	-	-	-

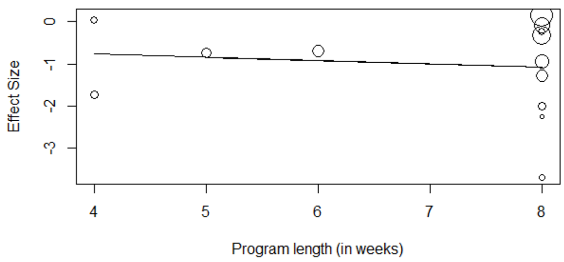
significant results in bold; *k<10, **all but two studies had the same score on this dose; *d*=effect size of the standardized regression coefficient, SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value= test statistic of slope, *p*-value= significance level; *k*=number of studies; *F*-distribution= test for the overall model; R^2= percentage of heterogeneity accounted for, tau^2/τ^2= variance of the underlying true effect sizes; *SE* tau^2= standard error of tau^2; Q_E= between-study heterogeneity; $p(Q_E)$=Q_E significance level.

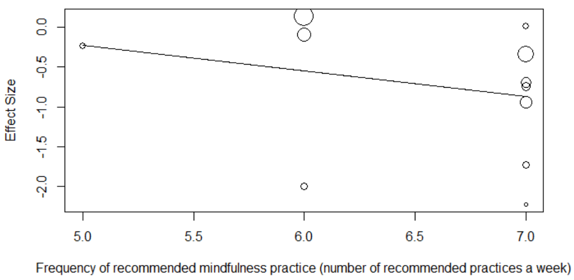
Appendix Figure 4.3.3.6

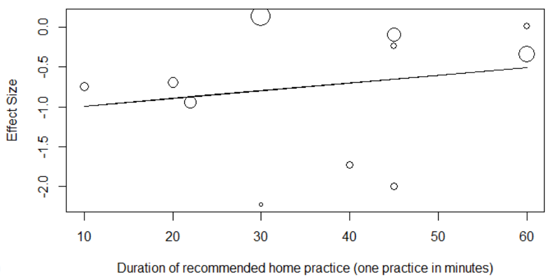
PSS stress measure-by-measure meta-regression plots for PSS stress at post-program compared to inactive controls for the doses total number of face-to-face sessions, frequency (number) of recommended home practices a week, duration of recommended home practice (one practice in minutes), total amount of face-to-face facilitator contact (in hours), total amount of face-to-face facilitator contact with zero hours of contact excluded (in hours), total recommended use of the program (in hours), program intensity (number of sessions a week including all-day retreats), amount of face-to-face facilitator contact a week (in hours), amount of face-to-face facilitator contact excluding zero contact a week (in hours) and recommended use of the program a week (in hours).

Primary dose variables

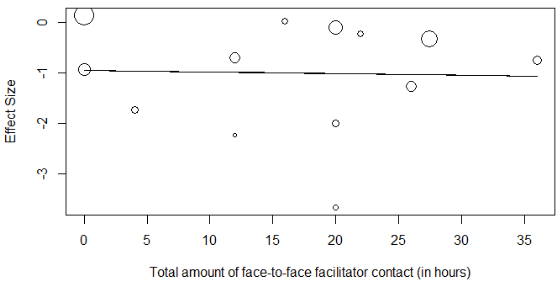


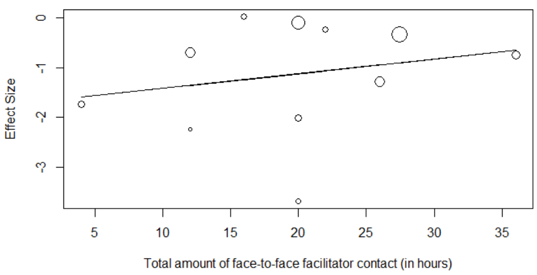


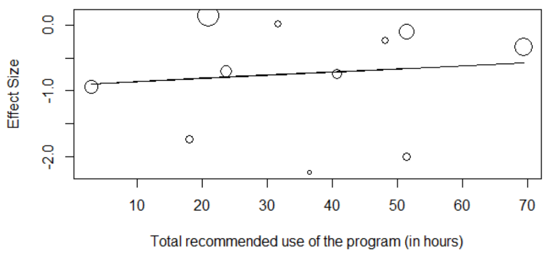


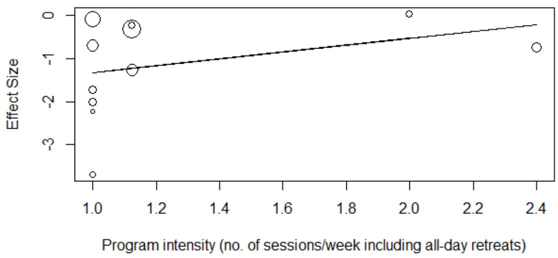


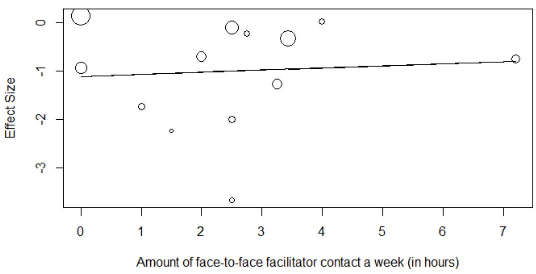
Composite dose variables

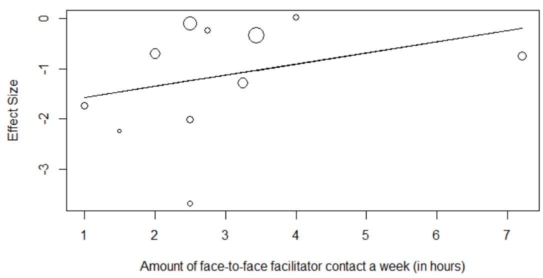


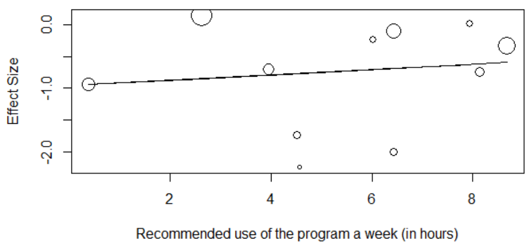












Appendix Table 4.3.3.7

Step-by-step False Discovery Rate correction (Benjamini-Hochberg procedure) for PSS stress at 1-4 months follow-up compared to inactive controls

p	**.019**	.3	.32	.36	.42	.49	.55	.69	.7	.73	.8	.92
Rank	1	2	3	4	5	6	7	8	9	10	11	12
$\boldsymbol{p}_{\mathbf{adj}}$	.23	.75	.75	.75	.75	.75	.75	.75	.88	.88	.92	.92

Significant results in bold.

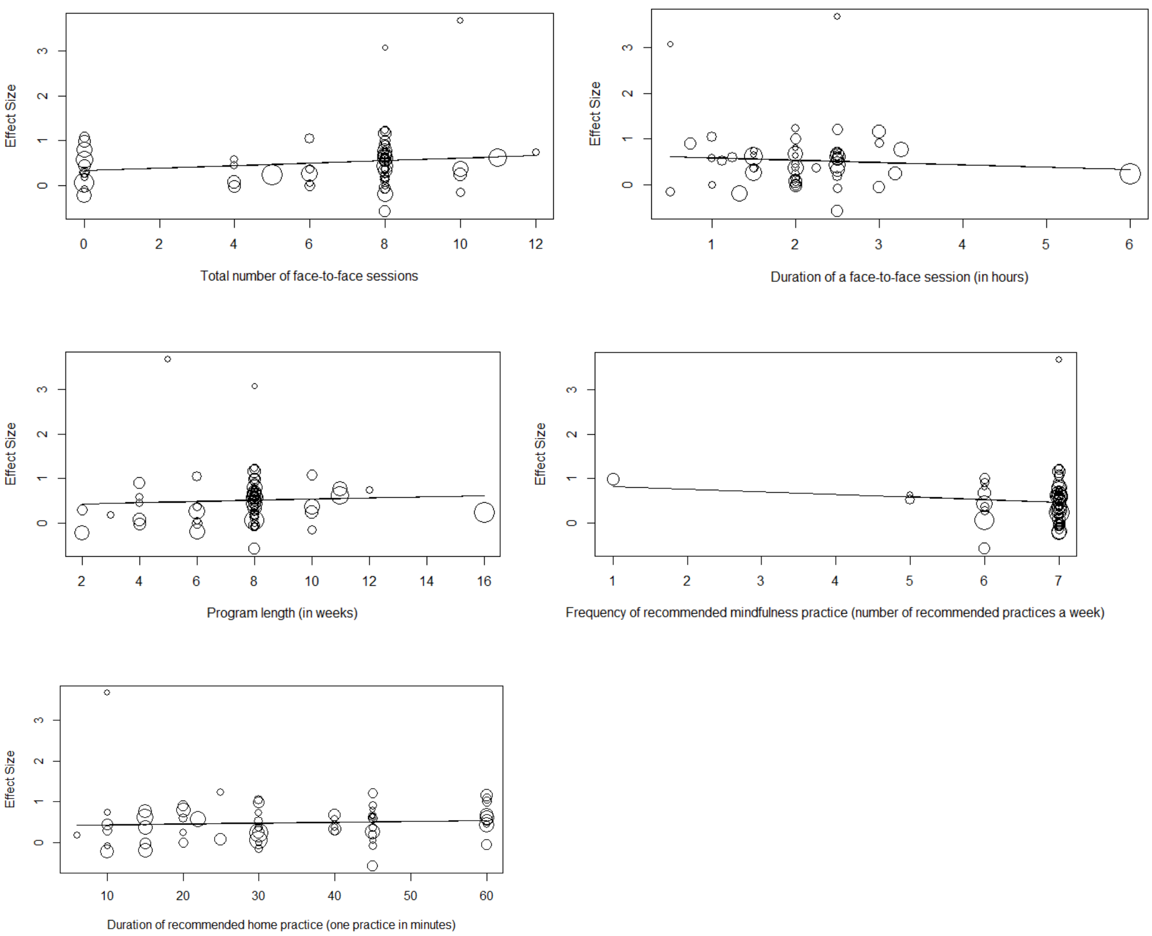
Appendices Chapter 5

Appendix 5.2: Dose-Response Meta-Regression Results: Mindfulness

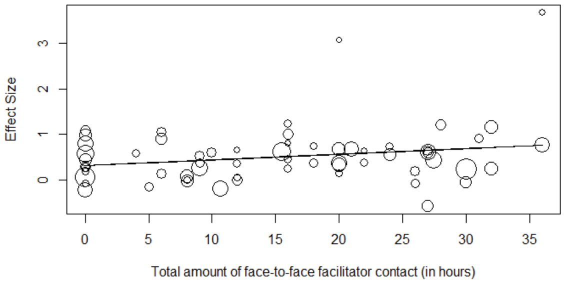
Appendix Figure 5.2.1

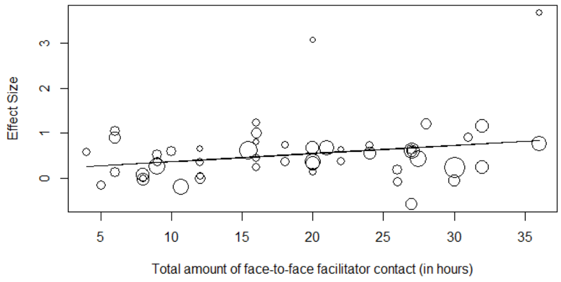
Meta-regression plots for mindfulness at post-program compared to inactive and active controls for the doses total number of face-to-face sessions, duration of a face-to-face session (in hours), program length (in weeks), frequency (number) of recommended home practices a week, duration of recommended home practice (one practice in minutes), total amount of face-to-face facilitator contact (in hours), total amount of face-to-face facilitator contact with zero hours of contact excluded (in hours), total recommended use of the program (in hours), program intensity (number of sessions a week excluding all-day retreats; compared to active controls only), program intensity (number of sessions a week including all-day retreats; compared to active controls only), amount of face-to-face facilitator contact a week (in hours; compared to active controls only), amount of face-to-face facilitator contact excluding zero contact a week (in hours; compared to active controls only), recommended use of the program a week (in hours) and actual use of the program a week (in hours).

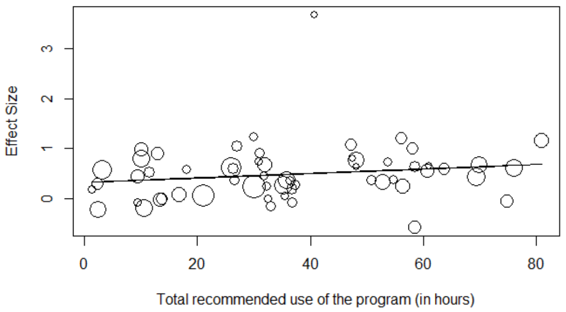
Compared to inactive controls – primary dose variables

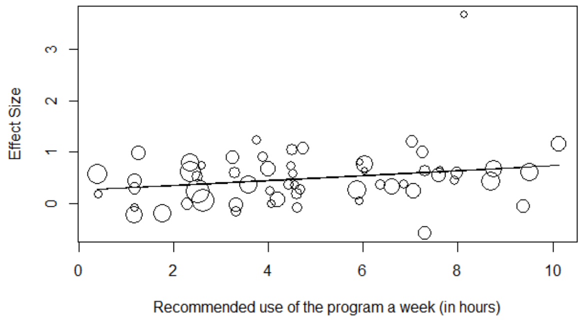


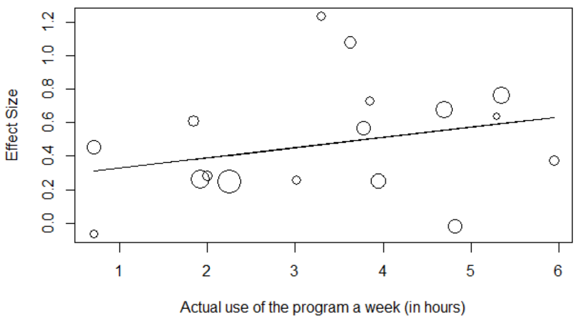
Compared to inactive controls – composite dose variables



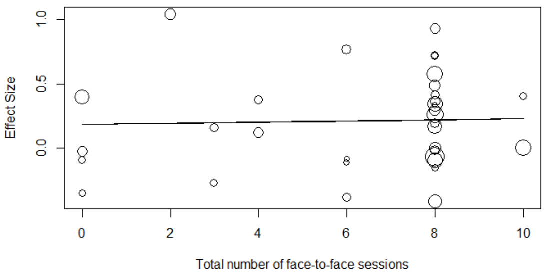


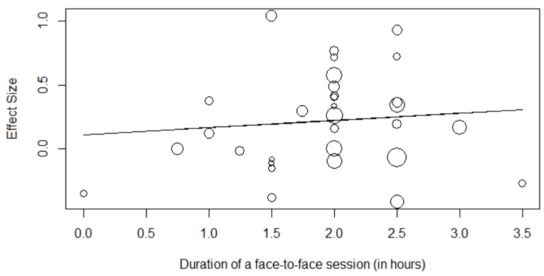


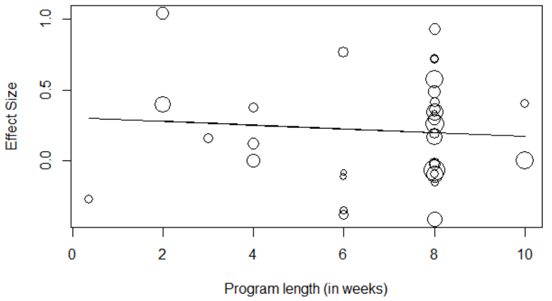


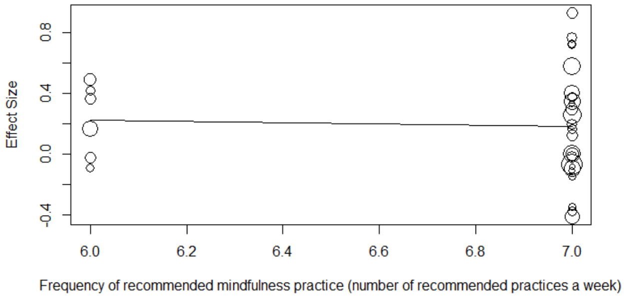


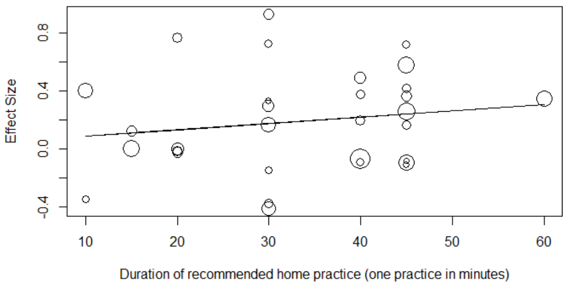
Compared to active controls – primary dose variables



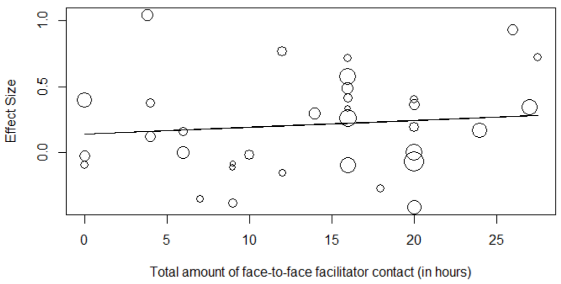


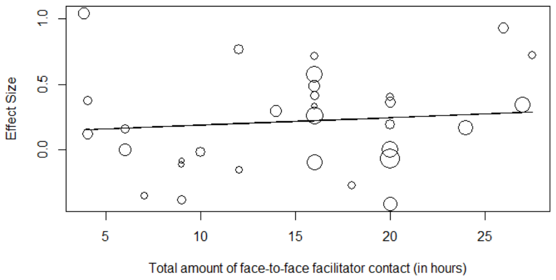


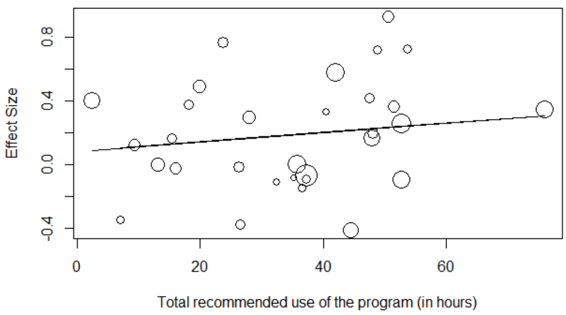


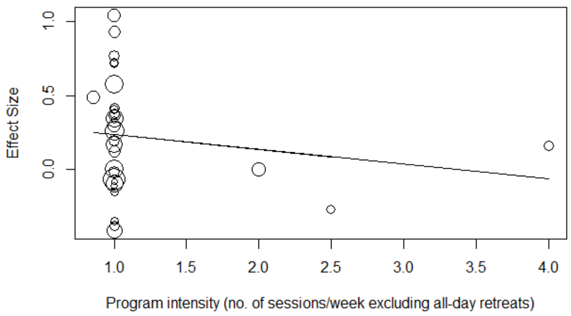


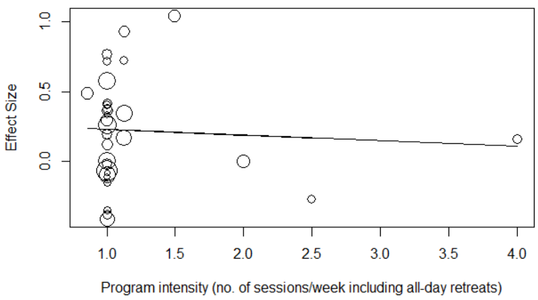
Compared to active controls – composite dose variables

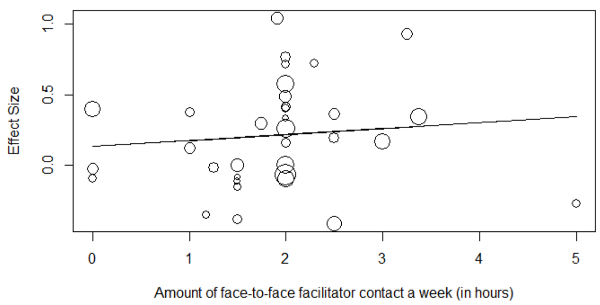


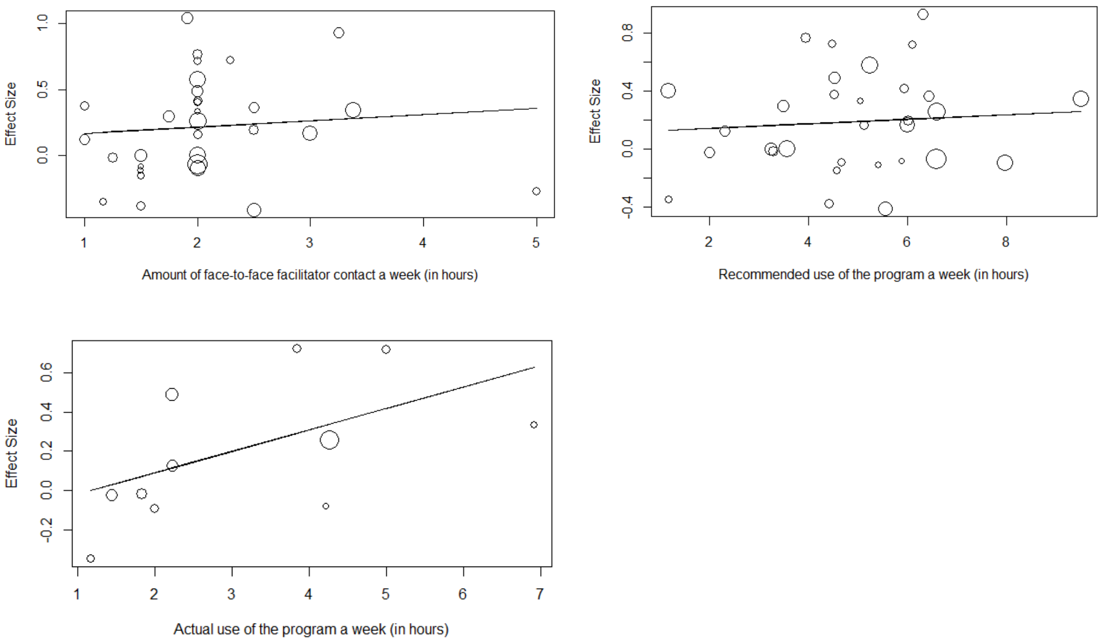











Effect Size
Amount of face-to-face facilitator contact a week (in hours)
Effect Size
Recommended use of the program a week (in hours)
Effect Size
Actual use of the program a week (in hours)

Appendix 5.2.2 Holm-Bonferroni sequential rejective test procedure for mindfulness at immediately post-program compared to inactive controls

Step 1: Rank-order significant *p*-values from smallest to largest

Rank 1: Program intensity (when including all-day retreats): H_1: p=.003

Rank 2: Program intensity (when excluding all-day retreats): H_2: p=.005

Rank 3: Amount of face-to-face facilitator contact (when excl. no contact) a week: H_3: p=.006

Rank 4: Amount of face-to-face facilitator contact a week: H_4: p=.013

Rank 5: Total actual use of the program: H_5: p=.04

Step 2: Holm-Bonferroni formula for first rank

HB=Target α / (n – rank + 1)

HB=.05 / (15 – 1 +1) = .0033

H_1<.0033

Step 3: Holm-Bonferroni formula for second rank

HB=Target α / (n – rank + 1)

HB=.05 / (15 – 2 +1) = .0036

H_2>.0036

Step 4: Holm-Bonferroni formula for third rank

HB=Target α / (n – rank + 1)

HB=.05 / (15 – 3 +1) = .0038

H_3>.0038

Step 5: Holm-Bonferroni formula for fourth rank

HB=Target α / (n – rank + 1)

HB=.05 / (15 – 4 +1) = .004

H_4>.004

Step 6: Holm-Bonferroni formula for fifth rank

HB=Target α / (n – rank + 1)

HB=.05 / (15 – 5 +1) = .0045

H_5>.0045

Appendix Table 5.2.3

Step-by-step False Discovery Rate correction (Benjamini-Hochberg procedure) for mindfulness at immediately post-program compared to inactive controls

p	**.003**	**.005**	**.006**	**.013**	**.04**	.072	.077	.078	.178	.23	.255	.437	.609	.628	.678
Rank	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
p_{adj}	**.03**	**.03**	**.03**	**.049**	.12	.146	.146	.146	.297	.345	.348	.546	.673	.673	.678

Significant results in bold.

Appendix Table 5.2.4

Step-by-step False Discovery Rate correction (Benjamini-Hochberg procedure) for mindfulness at immediately post-program compared to active controls

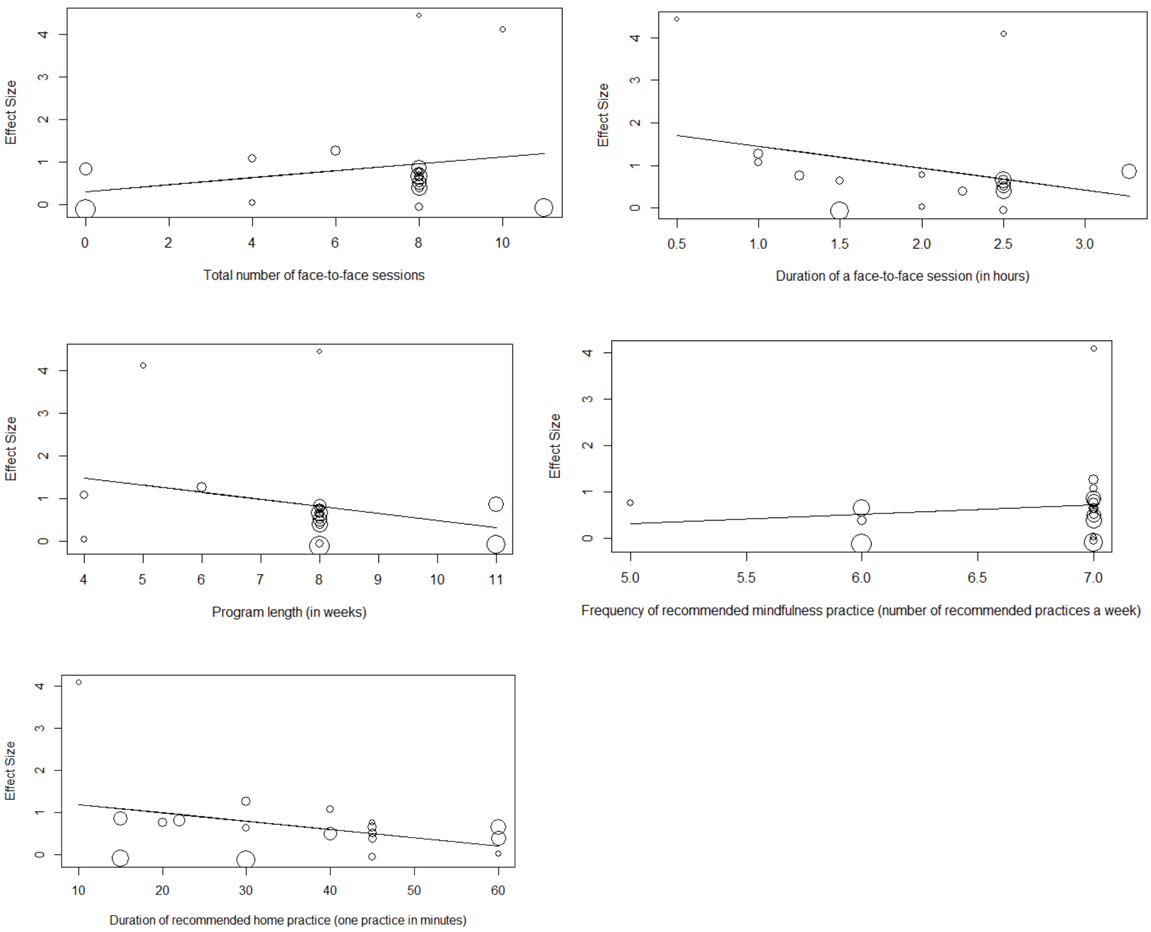
p	**.014**	.068	.356	.392	.412	.523	.538	.587	.593	.62	.623	.635	.745	.781	.847
Rank	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
$\boldsymbol{p}_{\mathbf{adj}}$	.21	.51	.794	.794	.794	.794	.794	.794	.794	.794	.794	.794	.838	.838	.847

Significant results in bold.

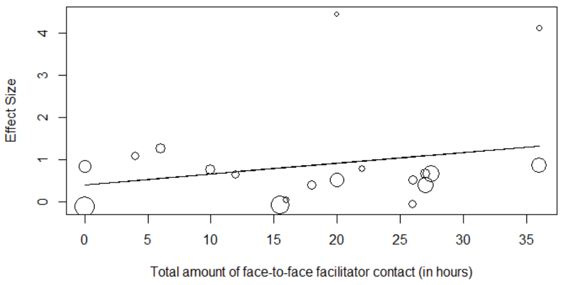
Appendix Figure 5.2.5

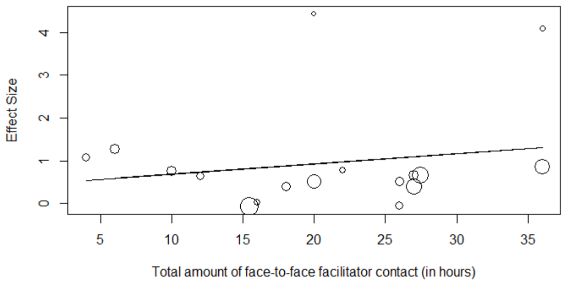
Meta-regression plots for mindfulness at 1-4 months follow-up compared to inactive and active controls for the doses total number of face-to-face sessions (compared to inactive controls only), duration of a face-to-face session (in hours), program length (in weeks; compared to inactive controls only), frequency (number) of recommended home practices a week (compared to inactive controls only), duration of recommended home practice (one practice in minutes), total amount of face-to-face facilitator contact (in hours), total amount of face-to-face facilitator contact with zero hours of contact excluded (in hours), total recommended use of the program (in hours), program intensity (number of sessions a week including all-day retreats; compared to inactive controls only), amount of face-to-face facilitator contact a week (in hours), amount of face-to-face facilitator contact excluding zero contact a week (in hours) and recommended use of the program a week (in hours).

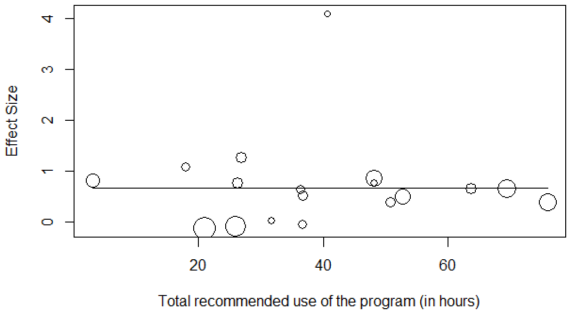
Compared to inactive controls – primary dose variables

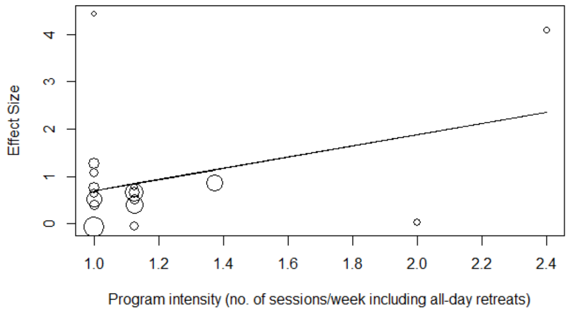


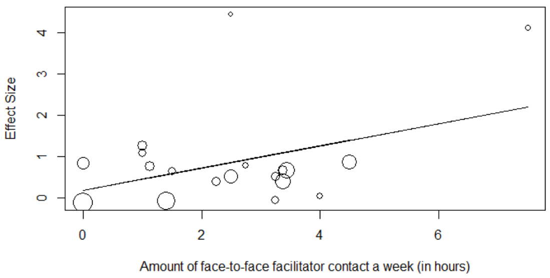
Compared to inactive controls – composite dose variables

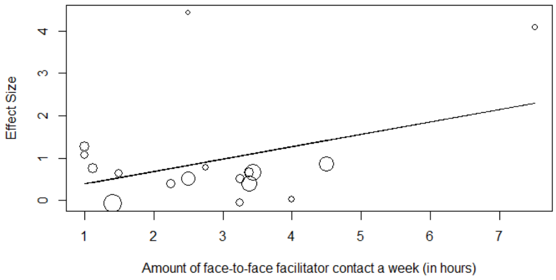


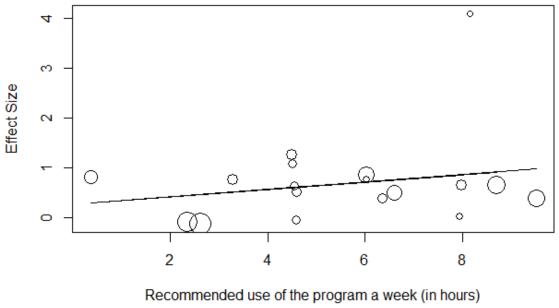




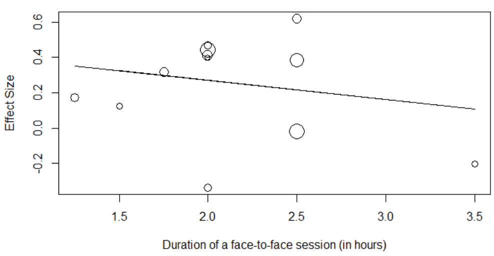


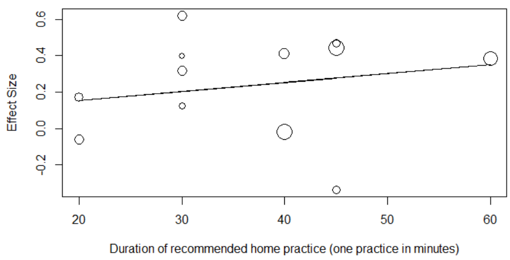




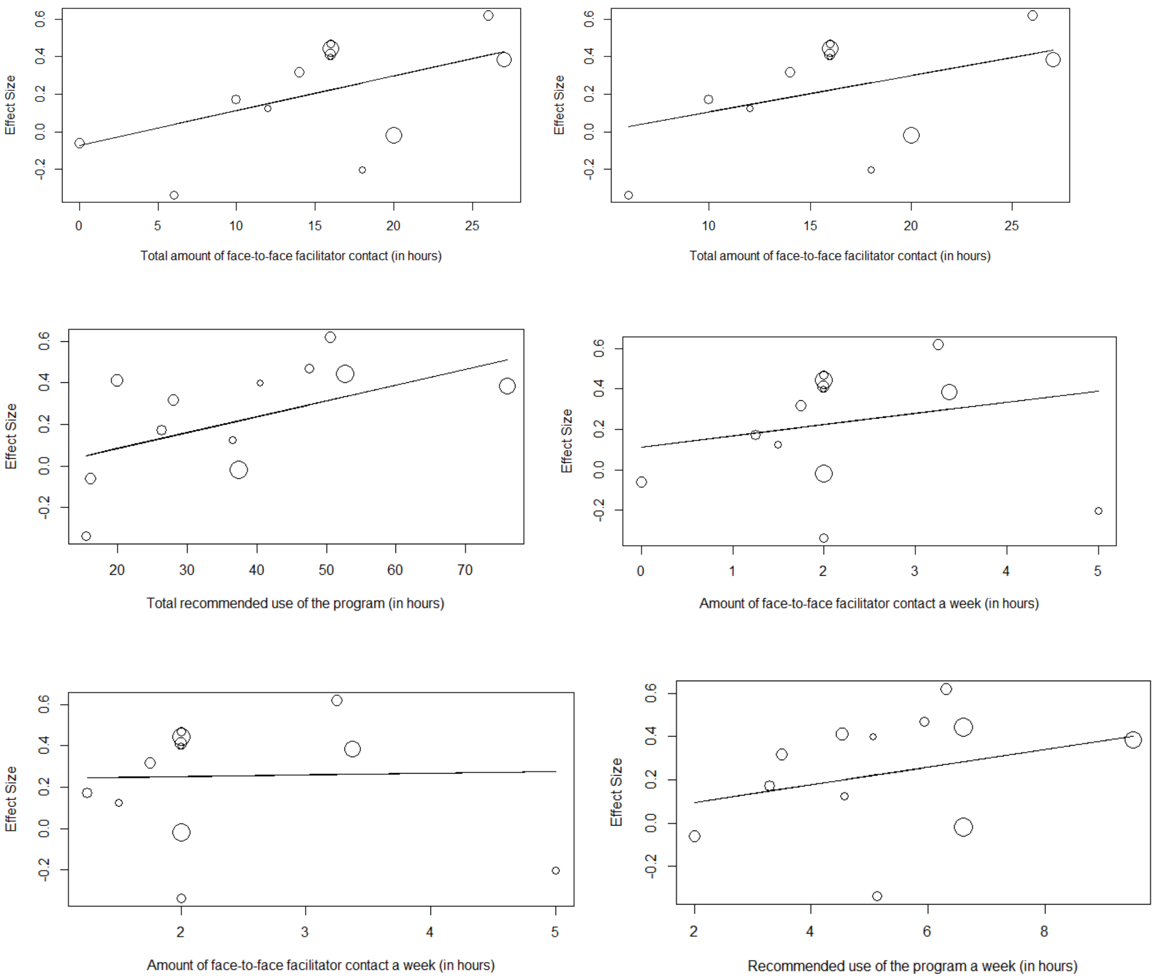


Compared to active controls – primary dose variables





Compared to active controls – composite dose variables



Appendix Table 5.2.6

Step-by-step False Discovery Rate correction (Benjamini-Hochberg procedure) for mindfulness at 1-4 months follow-up compared to active controls

p	**.029**	.055	.076	.159	.298	.481	.489	.545	.936
Rank	1	2	3	4	5	6	7	8	9
$\boldsymbol{p}_{\mathbf{adj}}$	.228	.228	.228	.358	.536	.61	.61	.61	.936

Significant results in bold.

Appendix Table 5.2.7

Meta-regression analysis results by MBP dose for between-group mindfulness effect sizes at 5-10 months follow-up compared to inactive controls

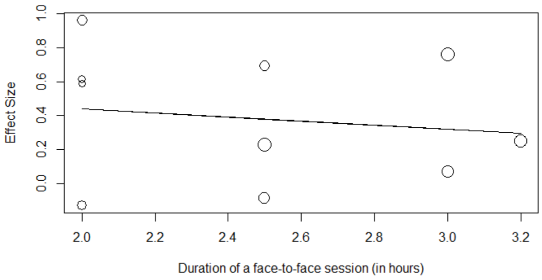
Dose	**Meta-regression model**						**Heterogeneity statistics**					
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	*SE* tau^2	Q_E	$p\ (Q_E)$
Total no. face-to-face sessions****	-	-	-	-	-	-	-	-	-	-	-	-
Duration of a face-to-face session	-0.12	0.28	[-0.78, 0.54]	-0.42	.68	10	0.18	0.00%	0.09	0.08	18.05	.02
Program length****	-	-	-	-	-	-	-	-	-	-	-	-
Freq. of recommended practice*	-	-	-	-	-	-	-	-	-	-	-	-
Duration of a recommended practice*	-	-	-	-	-	-	-	-	-	-	-	-
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	*SE* tau^2	Q_E	$p\ (Q_E)$
Total amount of contact	<.001	0.02	[-0.04, 0.04]	0.02	.98	10	<.001	0.00%	0.09	0.08	18.18	.02
Total amount of contact (excl. 0 hours)*****	-	-	-	-	-	-	-	-	-	-	-	-
Total recommended use of program*	-	-	-	-	-	-	-	-	-	-	-	-
Total actual use of program*	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity excl. retreats**	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity incl. retreats	0.82	2.03	[-3.87, 5.5]	0.4	.7	10	0.16	0.00%	0.09	0.08	17.79	.02
Amount of contact/week	-0.04	0.16	[-0.4, 0.33]	-0.22	.84	10	0.05	0.00%	0.09	0.08	18.22	.02
Amount of contact (excl. 0 hours)/week*****	-	-	-	-	-	-	-	-	-	-	-	-
Recommended use of program/week*	-	-	-	-	-	-	-	-	-	-	-	-
Actual use of program/week*	-	-	-	-	-	-	-	-	-	-	-	-

significant results in bold*;* *k<10, ** all studies had the same score on this dose; ***all but one had the same score on this dose*;* ****all but 2 studies had the same score on this dose*;* *****no studies had zero hours of face-to-face contact; d=effect size of the standardized regression coefficient, SE=standard error of the effect size, 95% CI= confidence intervals; t-value= test statistic of slope, p-value= significance level; k=number of studies; F-distribution= test for the overall model; R^2= percentage of heterogeneity accounted for, tau^2/τ^2= variance of the underlying true effect sizes; *SE* tau^2= standard error of tau^2; Q_E= between-study heterogeneity; $p(Q_E)$)=Q_E significance level.

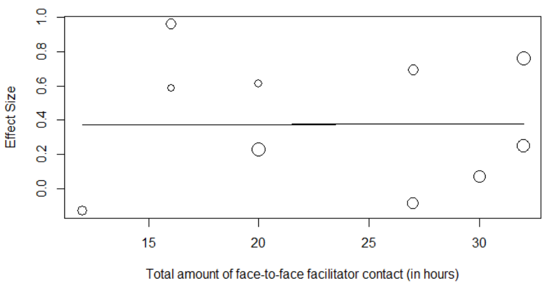
Appendix Figure 5.2.8.

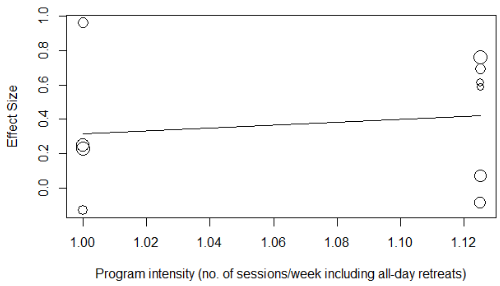
Meta-regression plots for mindfulness at 5-10 months follow-up compared to inactive controls for the doses duration of a face-to-face session (in hours), total amount of face-to-face facilitator contact (in hours), program intensity (number of sessions a week including all-day retreats) and amount of face-to-face facilitator contact a week (in hours).

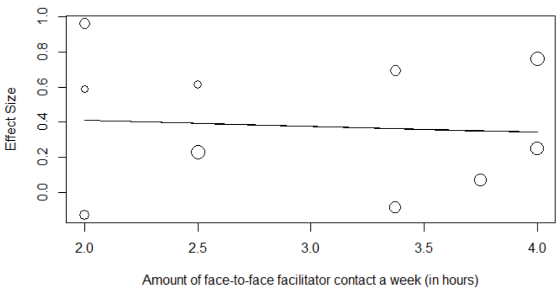
Primary dose variable



Composite dose variables







Appendix 5.3: Measure-by-Measure Dose-Response Meta-Regression Results: Mindfulness

Appendix Table 5.3.1

FFMQ measure-by-measure meta-regression analysis results for FFMQ mindfulness by MBP dose for between group mindfulness effect sizes at immediately post-program compared to inactive and active controls

Compared to inactive control groups												
Dose	**Meta-regression model**						**Heterogeneity statistics**					
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	*SE* tau^2	Q_E	$p (Q_E)$
Total no. face-to-face sessions	0.04	0.03	[-0.02, 0.09]	1.36	.18	37	1.86	2.55%	0.23	0.07	129.78	<.001
Duration of a face-to-face session	-0.04	0.23	[-0.31, 0.22]	-0.34	.74	28	0.11	0.00%	0.3	0.11	97.96	<.001
Program length	0.01	0.04	[-0.07, 0.08]	0.12	.91	37	0.02	0.00%	0.25	0.08	136.87	<.001
Frequency of recommended practice	-0.07	0.09	[-0.26, 0.13]	-0.71	.48	36	0.51	0.00%	0.25	0.08	133.46	<.001
Duration of a recommended practice	-0.001	0.01	[-0.01, 0.01]	-0.18	.86	36	0.03	0.00%	0.26	0.08	136.19	<.001
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	*SE* tau^2	Q_E	$p (Q_E)$
Total amount of contact	0.01	0.01	[-0.01, 0.03]	1.43	.16	37	2.04	1.11%	0.23	0.08	133.47	<.001
Total amount of contact (excl. 0 hours)	0.02	0.01	[-0.01, 0.05]	1.41	.17	28	1.98	1.44%	0.28	0.1	99.45	<.001
Total recommended use of program	0.01	0.01	[-0.01, 0.02]	0.98	.33	36	0.97	0.00%	0.24	0.08	131.57	<.001
Total actual use of program	0.01	0.01	[-0.01, 0.03]	1.18	.28	12	1.39	4.98%	0.06	0.06	17.09	.07
Program intensity excl. retreats	**1.18**	**0.33**	**[0.5, 1.86]**	**3.57**	**.001**	**28**	**12.74**	**39.17%**	**0.17**	**0.07**	**76.54**	**<.001**
Program intensity incl. retreats	**1.17**	**0.27**	**[0.6, 1.73]**	**4.24**	**<.001**	**28**	**18.01**	**49.6%**	**0.14**	**0.06**	**70.72**	**<.001**
Amount of contact/week	**0.15**	**0.06**	**[0.03, 0.27]**	**2.56**	**.015**	**37**	**6.56**	**13.14%**	**0.2**	**0.07**	**124.88**	**<.001**
Amount of contact (excl. 0 hours)/week	**0.28**	**0.09**	**[0.1, 0.47]**	**3.15**	**.004**	**28**	**9.94**	**28.68%**	**0.2**	**0.08**	**85.8**	**<.001**
Recommended use of program/week	0.07	0.04	[-0.02, 0.16]	1.6	.12	36	2.56	4.89%	0.23	0.08	128.63	<.001
Actual use of program/week	0.04	0.07	[-0.12, 0.2]	0.53	.61	12	0.28	0.00%	0.07	0.07	19.02	.04

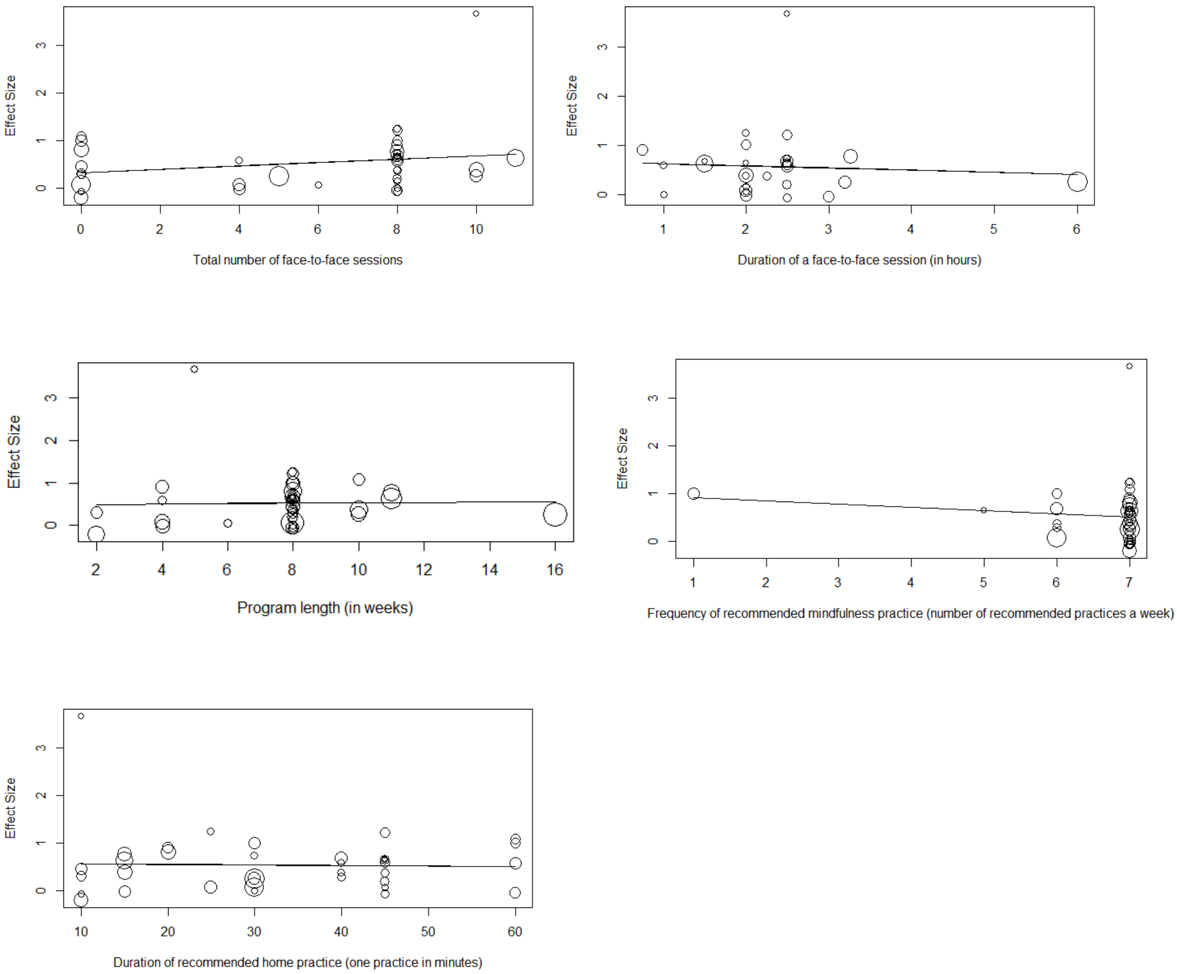
Compared to active control groups												
Dose	**Meta-regression model**						**Heterogeneity statistics**					
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	$SE\ tau^2$	Q_E	$p\ (Q_E)$
Total no. face-to-face sessions	-0.01	0.02	[-0.06, 0.04]	-0.51	.62	21	0.26	0.00%	0.04	0.03	30.77	.04
Duration of a face-to-face session	-0.1	0.13	[-0.37, 0.18]	-0.73	.48	18	0.53	0.00%	0.05	0.04	28.69	.03
Program length	-0.03	0.03	[-0.08, 0.03]	-1.01	.33	21	1.01	18.84%	0.03	0.03	29.38	.06
Frequency of recommended practice	0.03	0.16	[-0.31, 0.37]	0.18	.86	18	0.03	0.00%	0.01	0.02	19.61	.24
Duration of a recommended practice	-0.002	0.01	[-0.01, 0.01]	-0.36	.72	18	0.13	0.00%	0.01	0.02	19.38	.25
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	R^2	Tau^2	$SE\ tau^2$	Q_E	$p\ (Q_E)$
Total amount of contact	-0.004	0.01	[-0.02, 0.02]	-0.4	.69	21	0.16	0.00%	0.04	0.03	31.1	.04
Total amount of contact (excl. 0 hours)	-0.01	0.01	[-0.04, 0.02]	-0.83	.42	18	0.69	6.89%	0.05	0.04	27.57	.04
Total recommended use of program	-0.002	0.004	[-0.01, 0.01]	-0.55	.59	18	0.3	0.00%	0.01	0.02	19.1	.26
Total actual use of program*	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity excl. retreats	-0.12	0.12	[-0.37, 0.13]	-1.02	.32	18	1.03	0.00%	0.05	0.04	28.95	.02
Program intensity incl. retreats	-0.07	0.12	[-0.32, 0.18]	-0.59	.56	18	0.35	0.00%	0.06	0.04	29.83	.02
Amount of contact/week	-0.02	0.08	[-0.18, 0.14]	-0.23	.82	21	0.05	0.00%	0.04	0.04	32.08	.03
Amount of contact (excl. 0 hours)/week	-0.09	0.11	[-0.34, 0.15]	-0.82	.42	18	0.68	0.00%	0.05	0.04	29.21	.02
Recommended use of program/week	-0.03	0.03	[-0.1, 0.03]	-1.09	.29	18	1.18	37.86%	0.01	0.02	18.09	.32
Actual use of program/week*	-	-	-	-	-	-	-	-	-	-	-	-

significant results in bold; *k<10; d=effect size of the standardized regression coefficient, SE=standard error of the effect size, 95% CI= confidence intervals; t-value= test statistic of slope, p-value= significance level; k=number of studies; F-distribution= test for the overall model; R^2= percentage of heterogeneity accounted for, tau^2/τ^2= variance of the underlying true effect sizes; $SE\ tau^2$= standard error of tau^2; Q_E= between-study heterogeneity; $p(Q_E)$)=Q_E significance level.

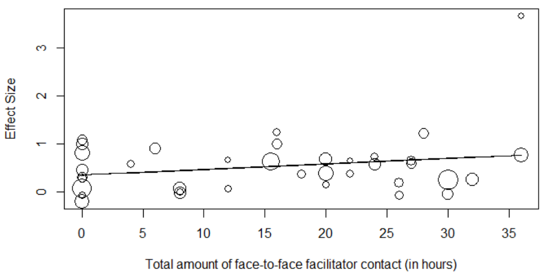
Appendix Figure 5.3.2

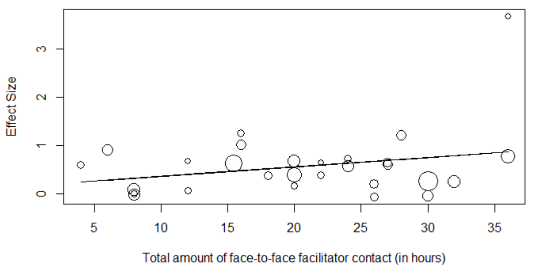
FFMQ measure-by-measure meta-regression plots for FFMQ mindfulness at post-program compared to inactive and active controls for the doses total number of face-to-face sessions, duration of a face-to-face session (in hours), program length (in weeks), frequency (number) of recommended home practices a week, duration of recommended home practice (one practice in minutes), total amount of face-to-face facilitator contact (in hours), total amount of face-to-face facilitator contact with zero hours of contact excluded (in hours), total recommended use of the program (in hours), total actual use of the program (in hours; compared to inactive controls only); program intensity (number of sessions a week excluding all-day retreats; compared to active controls only), program intensity (number of sessions a week including all-day retreats; compared to active controls only), amount of face-to-face facilitator contact a week (in hours; compared to active controls only), amount of face-to-face facilitator contact excluding zero contact a week (in hours; compared to active controls only), recommended use of the program a week (in hours) and actual use of the program a week (in hours; compared to inactive controls only).

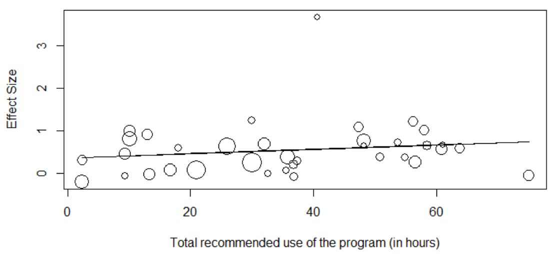
Compared to inactive controls – primary dose variables

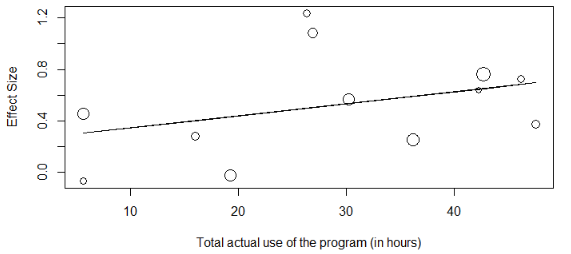


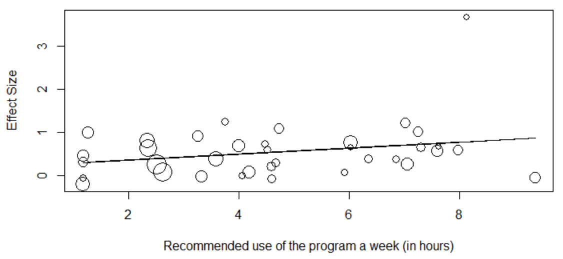
Compared to inactive controls – composite dose variables

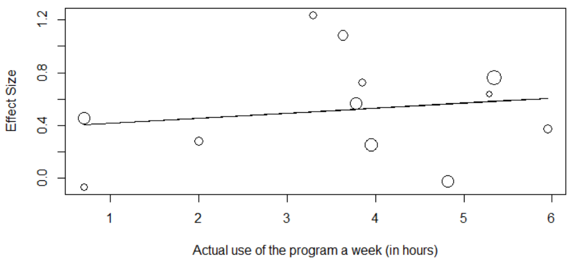




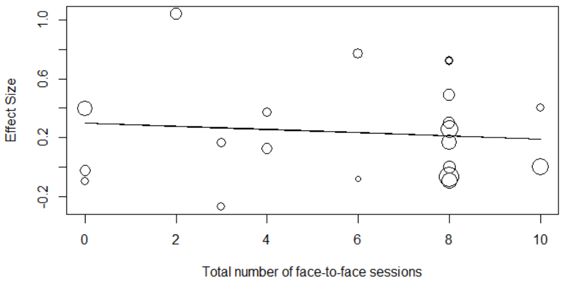


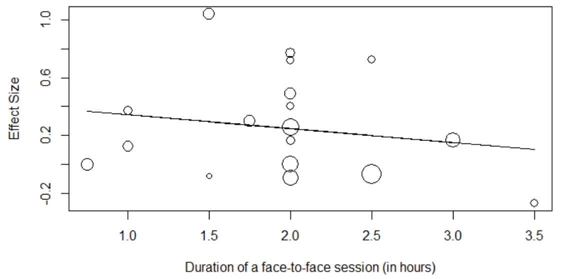


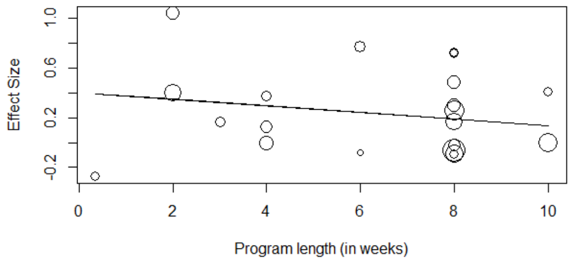


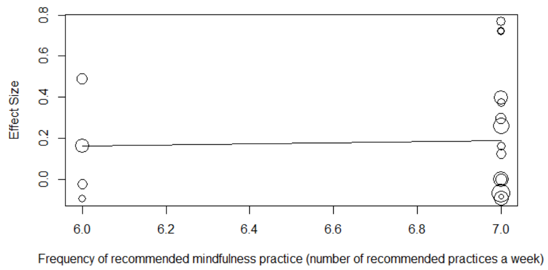


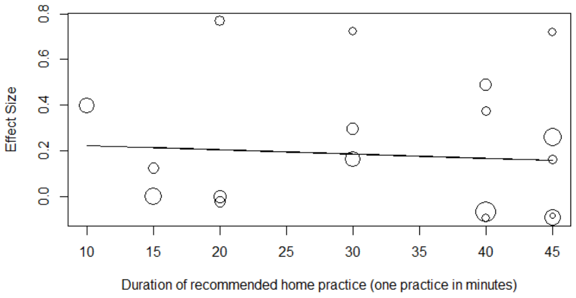
Compared to active controls – primary dose variables



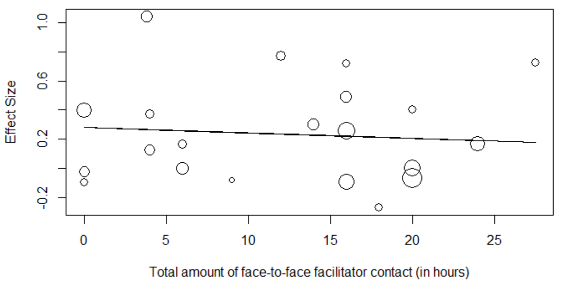


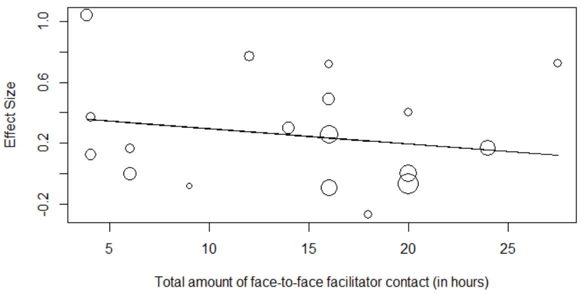


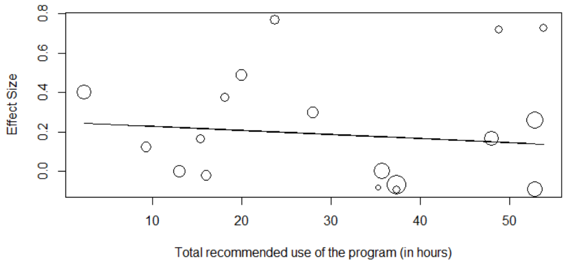


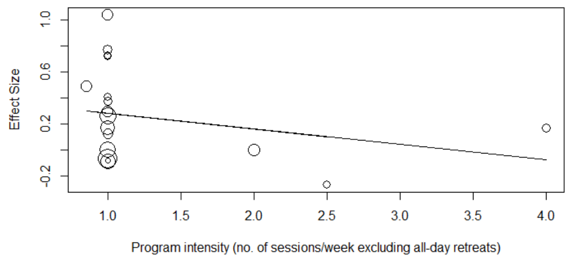


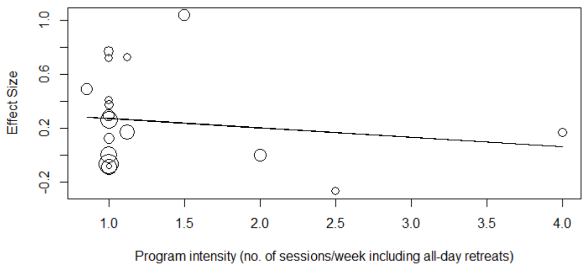
Compared to active controls – composite dose variables

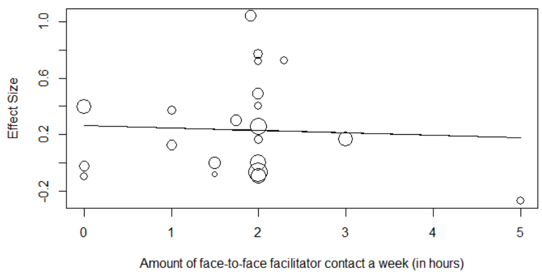


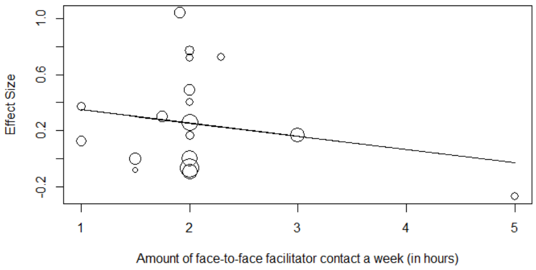


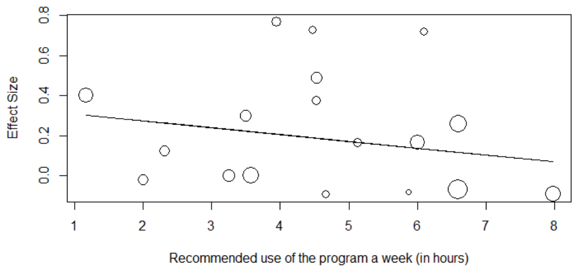









Effect Size
1.0
0.6
0.2
-0.2
1
2
3
4
5
Amount of face-to-face facilitator contact a week (in hours)


Effect Size
0.8
0.6
0.4
0.2
0.0
1
2
3
4
5
6
7
8
Recommended use of the program a week (in hours)

Appendix 5.3.3 Holm-Bonferroni sequential rejective test procedure for FFMQ mindfulness at immediately post-program compared to inactive controls

Step 1: Rank-order significant *p*-values from smallest to largest

Rank 1: Program intensity (when including all-day retreats): H_1: $p<.001$

Rank 2: Program intensity (when excluding all-day retreats): H_2: $p=.001$

Rank 3: Amount of face-to-face facilitator contact (when excl. no contact) a week: H_3: $p=.004$

Rank 4: Amount of face-to-face facilitator contact a week: H_4: $p=.015$

Step 2: Holm-Bonferroni formula for first rank

HB=Target α / (n – rank + 1)

HB=.05 / (15 – 1 +1) = .0033

$H_1<.0033$

Step 3: Holm-Bonferroni formula for second rank

HB=Target α / (n – rank + 1)

HB=.05 / (15 – 2 +1) = .0036

$H_2<.0036$

Step 4: Holm-Bonferroni formula for third rank

HB=Target α / (n – rank + 1)

HB=.05 / (15 – 3 +1) = .0038

$H_3>.0038$

Step 5: Holm-Bonferroni formula for fourth rank

HB=Target α / (n – rank + 1)

HB=.05 / (15 – 4 +1) = .004

$H_4>.004$

Appendix Table 5.3.4

Step-by-step False Discovery Rate correction (Benjamini-Hochberg procedure) for FFMQ mindfulness at immediately post-program compared to inactive controls

p	**<.001**	**.001**	**.004**	**.015**	.12	.16	.17	.18	.28	.33	.48	.61	.74	.86	.91
Rank	1	2	3	4	5	6	7	8	9	10	11	12	13	14	15
$\boldsymbol{p_{adj}}$	**.002**	**.008**	**.02**	.057	.34	.34	.34	.34	.47	.49	.65	.76	.85	.91	.91

Significant results in bold.

Appendix Table 5.3.5

MAAS measure-by-measure meta-regression analysis results for MAAS mindfulness by MBP dose for between group mindfulness effect sizes at immediately post-program compared to inactive and active controls

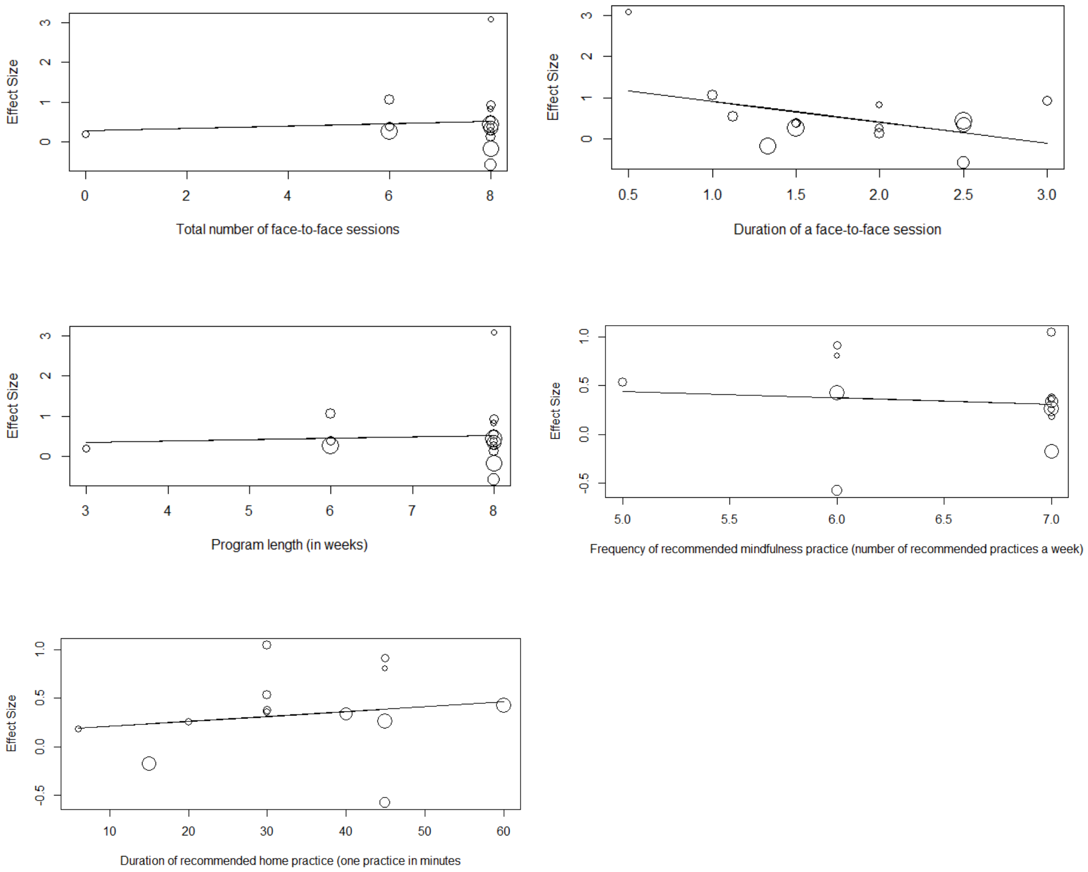
Dose	Meta-regression model							Heterogeneity statistics				
Primary	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total no. face-to-face sessions	0.03	0.1	[-0.19, 0.25]	0.29	.78	15	0.08	0.00%	0.45	0.21	64.51	<.001
Duration of a face-to-face session	-0.51	0.29	[-1.15, 0.12]	-1.76	.11	14	3.08	13.46%	0.38	0.19	60.48	<.001
Program length	0.03	0.15	[-0.29, 0.36]	0.22	.83	15	0.05	0.00%	0.45	0.21	64.34	<.001
Frequency of recommended practice	-0.07	0.2	[-0.51, 0.37]	-0.35	.74	13	0.12	0.00%	0.13	0.09	30.55	<.001
Duration of a recommended practice	0.01	0.01	[-0.01, 0.02]	0.57	.58	13	0.32	0.00%	0.13	0.09	29.29	.002
Composite	***d***	**SE**	**95% CI**	***t***	***p***	***k***	***F***	***R^2***	***Tau^2***	***SE tau^2***	***Q_E***	***p (Q_E)***
Total amount of contact	0.01	0.02	[-0.04, 0.06]	0.37	.72	15	0.13	0.00%	0.45	0.21	64.46	<.001
Total amount of contact (excl. 0 hours)	0.01	0.03	[-0.05, 0.06]	0.23	.83	14	0.05	0.00%	0.5	0.24	64.37	<.001
Total recommended use of program	-0.004	0.01	[-0.02, 0.01]	-0.53	.61	13	0.28	0.00%	0.13	0.09	30.4	.001
Total actual use of program*	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity excl. retreats**	-	-	-	-	-	-	-	-	-	-	-	-
Program intensity incl. retreats	-2.43	2.11	[-7.03, 2.18]	-1.15	.27	14	1.32	4.21%	0.42	0.21	54.66	<.001
Amount of contact/week	0.06	0.2	[-0.37, 0.48]	0.28	.78	15	0.08	0.00%	0.45	0.21	64.56	<.001
Amount of contact (excl. 0 hours)/week	0.03	0.23	[-0.47, 0.53]	0.12	.91	14	0.02	0.00%	0.5	0.24	64.38	<.001
Recommended use of program/week	-0.01	0.06	[-0.13, 0.12]	-0.11	.91	13	0.01	0.00%	0.14	0.09	30.78	.001
Actual use of program/week*	-	-	-	-	-	-	-	-	-	-	-	-

**k*<10; **all but one study had the same score on this dose; *d*=effect size of the standardized regression coefficient, SE=standard error of the effect size, 95% CI= confidence intervals; *t*-value= test statistic of slope, *p*-value= significance level; *k*=number of studies; *F*-distribution= test for the overall model; R^2= percentage of heterogeneity accounted for, tau^2/τ^2= variance of the underlying true effect sizes; *SE* tau^2= standard error of tau^2; Q_E= between-study heterogeneity; $p(Q_E)$=Q_E significance level.

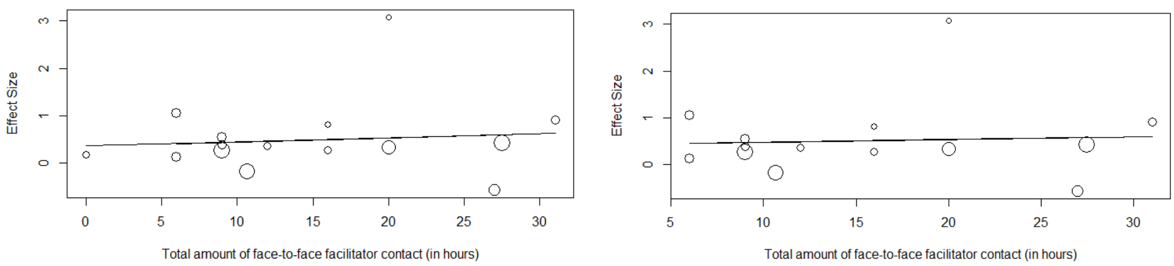
Appendix Figure 5.3.6.

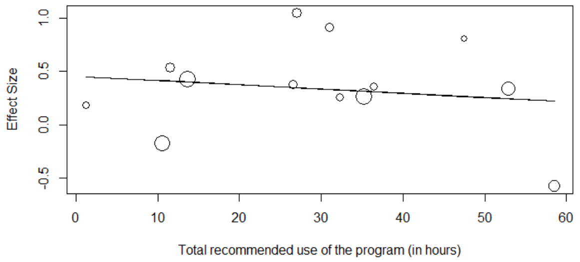
MAAS measure-by-measure meta-regression plots for MAAS mindfulness at post-program compared to inactive controls for the doses total number of face-to-face sessions, duration of a face-to-face session (in hours), program length (in weeks), frequency (number) of recommended home practices a week, duration of recommended home practice (one practice in minutes), total amount of face-to-face facilitator contact (in hours), total amount of face-to-face facilitator contact with zero hours of contact excluded (in hours), total recommended use of the program (in hours), program intensity (number of sessions a week including all-day retreats), amount of face-to-face facilitator contact a week (in hours), amount of face-to-face facilitator contact excluding zero contact a week (in hours) and recommended use of the program a week (in hours).

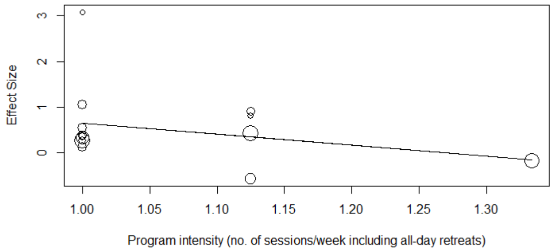
Primary dose variables

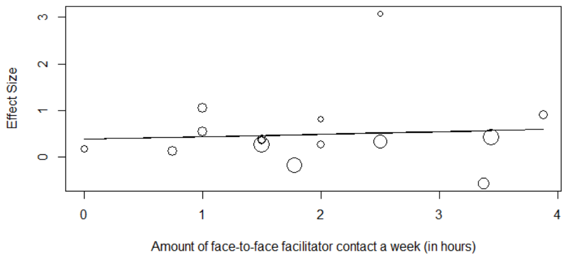


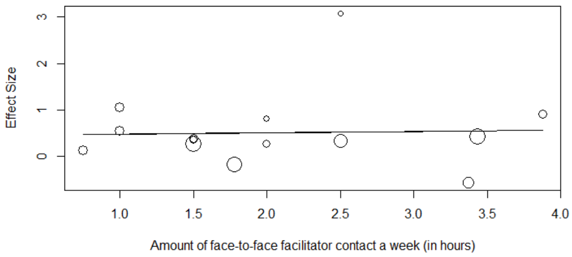
Composite dose variables

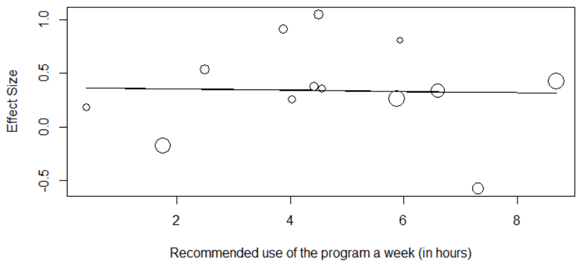



Effect Size
Total recommended use of the program (in hours)


Effect Size
Program intensity (no. of sessions/week including all-day retreats)


Effect Size
Amount of face-to-face facilitator contact a week (in hours)


Effect Size
Amount of face-to-face facilitator contact a week (in hours)


Effect Size
Recommended use of the program a week (in hours)

Appendices Chapter 6

Appendix 6.3: Chapter 6 Methods

Appendix 6.3.1 Participant information for study examining effects of mindfulness practice length

Study examining effects of mindfulness and similar audio-guided exercises

(version 4; 8th April 2019)

Hello. My name is Sarah Strohmaier and I am a Psychology PhD student at Canterbury Christ Church University. I would like to invite you to take part in a research study. Before you decide whether to take part, it is important that you understand why the research is being done and what it would involve for you.

What is the purpose of the study?

This study is part of a psychology research project which looks at the beneficial effects of listening to guided audio lessons, some of which may include mindfulness practice, in four sessions over two weeks.

Why have I been invited?

You have been invited to participate in this study because you are either currently a student or staff member at Canterbury Christ Church University or a member of the general population. Other participants in this study will either also be Canterbury Christ Church University students, both either from your course or from another course or other staff working for the University, or from the general population.

Do I have to take part?

You do not have to take part in this study if you do not want to; taking part in this research is entirely voluntary. It is up to you to decide whether to join the study. If you agree to take part, I will then ask you to sign a consent form. You are also free to withdraw your participation from the study at any time, without giving a reason.

What will happen to me if I take part?

If you decide you would like to take part, we will first of all ask you to complete a consent form to ensure that you are aware of your rights. We will then ask you to complete questionnaires including some demographic information, as well as psychological questions. This study has four sessions and each of the sessions will involve you listening to audio recordings of informational materials and depending on which group you are allocated to, part of what you may be asked to do might include some mindfulness practice. The four sessions will be delivered over two weeks (two days a week). Every session will take 25 minutes to complete followed by some questionnaires, which take 5-10 minutes to complete. Once you have signed up/let me know that you would like to take part, you will be emailed and told of the dates and times of these sessions in the next couple of weeks.

Prize draw/course credits

If you are an undergraduate student, you can have a choice of receiving course credits or entering your name into the prize draw. If you choose to receive course credits, you can receive two course credits for every session you participate in and one course credit for completing online questionnaires at the end. By participating in this study, you will therefore receive a total of **9 course credits**.

If you are not an undergraduate student, you can choose to enter your name into the prize draw only. The prize draw consists of £50 of shopping vouchers. Your name and details will be kept completely confidential. The winner of the prize draw will be chosen randomly. You will receive course credits/an entry into the prize draw for every session you attend (this includes completing questionnaires in the sessions) as well as for completing the online questionnaire that is emailed to you a week after the final session.

What are the possible disadvantages and risks of taking part?

We expect that participating in this research will not cause any distress or discomfort for the majority of people. However, occasionally when practicing mindfulness, some people can experience some discomfort. However, this is unlikely since the recordings are brief and you are welcome to stop at any point. Furthermore, for some people, completing questionnaires on their wellbeing can highlight low mood experiences. However, the measures are ones that are commonly used in psychological research and are designed for use in the general population.

In the unlikely event that participating in this research study highlights any issues you may need further support with, then we'd recommend you contact the University's wellbeing advisers on studentwellbeing@canterbury.ac.uk or 01227 922675; your GP or telephone NHS England direct on 0300 311 22 33 or 111 to speak directly to a health professional.

What are the possible benefits of taking part?

Previous research has shown that many people experience benefits from practicing mindfulness and listening to audio books or podcasts. Both can be useful tools for calming the mind and enhancing listening skills and concentration. We cannot promise the study will help you but the information we gain from this study will help improve research and practice.

When is it best not to take part?

If you are feeling particularly vulnerable at this time, for example struggling with low mood or anxiety, we would recommend that you do not take part in this study at this time. Additionally, if you currently have a regular mindfulness practice or are currently participating in a mindfulness-based intervention, we ask that you do not participate in this study.

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

If you do not want to carry on with the study, you can withdraw from the study at any time without needing to give a reason. If you withdraw from the study, we would like to use the data collected up to your withdrawal. However, if you wish, you can also ask for your questionnaire data to be removed from the study.

Complaints and feedback

If you have concerns about any aspect of this study or would like to give feedback, please either tell me in person or you can contact me by emailing me at **sarah.strohmaier@canterbury.ac.uk** and I will do my best to address your concerns and feedback. You can also contact me by leaving a message on the 24-hour voicemail phone number 01227 927070. Please leave a contact number and say that the message is for me (Sarah Strohmaier) and I will get back to you as soon as possible. If you remain unhappy and wish to complain formally, you can do this by contacting Dr Alex Hassett, Principal Lecturer, Salomons Institute for Applied Psychology – **alex.hassett@canterbury.ac.uk**, phone: 01227 927093.

Will information about me be kept confidential?

Yes. We will follow ethical and legal practice and all information about you will be handled in confidence. I (Sarah) will be collecting the questionnaires which contain your answers. Once the questionnaire answers are matched, these will be anonymised and be given a number rather than your name. These will be stored securely in a locked filing cabinet at Canterbury Christ Church University and password-protected computer for up to ten years, before being destroyed securely, as per the BPS Code of Human Research Ethics (2014). The answers to your questions will not be looked at in detail until after they have been anonymised. The only persons to have access to the questionnaires are the PhD student (Sarah) and her supervisor (Dr Fergal Jones).

All information which is collected from or about you during the course of the research will be kept strictly confidential, and any information about you will be kept securely by the researcher only. The only time when I would be obliged to pass on information from you to a third party would be if, as a result of something you told me, I were to become concerned about your safety or the safety of someone else. However, in the unlikely event where this would be the case I will do my best to notify you beforehand.

What will happen to the results of the research study?

After the data has been analysed, the project will be written up as both a PhD thesis and also be submitted for publication to an academic journal. Any data included will be anonymised and no individual participant will be identified.

Who is organising and funding the research?

This research is funded by Canterbury Christ Church University.

Who has reviewed the study?

This study has been reviewed and given favourable opinion by the PhD students' supervisors (Dr Fergal Jones and Dr James Cane) as well as The Salomons Ethics Panel, Salomons Institute for Applied Psychology, Canterbury Christ Church University.

Further information and contact details

If you would like to speak to me and find out more about the study or have questions about it, you can leave a message for me (Sarah Strohmaier) on a 24-hour voicemail phone line at 01227 927070 and leave a contact number so that I can get back to you. You can also email me at **sarah.strohmaier@canterbury.ac.uk** for any questions about this study and your participation.

Thank you very much.

Appendix 6.3.2 Participant handout for mindfulness practice groups

Handout – Study examining effects of mindfulness and similar audio-guided exercises

<u>Practicing outside of sessions</u>

We ask you not to engage in any formal mindfulness practice outside this session. By formal practice we mean listening to audio recordings or taking time to formally sit or lie down to practice mindfulness similar to how it is on the recordings.

However, if as a result of the mindfulness practice that you've done in the session you find that you are more present in everyday life and are relating to present-moment experiences with more gentleness and kindness, that's absolutely fine.

<u>**FAQs: Common experiences when practicing mindfulness**</u>

As a result of completing the Mindfulness Practice you may experience a range of emotions and feelings. This is completely normal and even very experienced mindfulness practitioners experience such emotions. The below FAQs give you some information as to how to be with any of those feelings that arise. Have a look down this list of common experiences. If there are some experiences listed that relate to you, if you could just read the paragraphs on those.

I felt tense or restless - This is very common and not a problem. We'd encourage you to bring attention to the experience of tension or restlessness during the practice and, as best you can, greet that with gentleness and patience.

I felt my mind wandering away from the present moment - It is not a failure if our mind wanders. All of our minds wander; this is what minds do. Noticing our mind wandering is a valuable skill to develop. When we notice that our mind has wandered in the mindfulness practice, as best we can we acknowledge what the mind has wandered to, let go of any judgments about the fact the mind has wandered, and then gently bring our attention back to the present moment.

I think I haven't done the practice well or can't do the practice - It can be helpful to remind ourselves that we are not aiming for a particular state of mind when we practice mindfulness. Whatever we notice is a success. If we notice thoughts such as "I am not doing it right", it is great that we have noticed them. Perhaps we can take a few moments to watch the effect these thoughts are having on our moment-by-moment experience, before returning to following the practice's guidance.

I felt I was daydreaming or getting lost in pleasant experiences - Many of us get lost in pleasant thoughts or daydreams from time to time. When we notice this has happened during practice, the invitation is to let go of these and to gently return to following the guidance in the practice.

I was feeling sleepy or falling asleep during the practice - Any of us can feel sleepy during mindfulness practice at times. Feeling sleepy during practice can be a really valuable opportunity to explore the present-moment experience of feeling sleepy, including how the body feels when this happens. If you regularly feel sleepy during practice, you may want to practice with your eyes open rather than closed. Sitting with an upright posture, with your back away from the chair if that is comfortable for you, can be helpful, as can practicing standing up.

I felt relaxed or calm - Feeling relaxed or calm can be a really interesting experience to explore and we'd encourage you to bring awareness to the body and to notice what body sensations come with feeling relaxed or calm, and whether there are any changes in these moment by moment.

I felt focused or absorbed in the present moment - Sometimes during practice we can be very absorbed with our present moment experience and our minds may wander a little, which is great to notice. Also, it is important to remind ourselves that this experience is no better or worse than when the mind wanders, from the point of view of this practice.

During the practice, I felt irritated or disturbed by an experience (e.g. a noise, people talking, an unwanted thought or feeling, etc.)

Very often there will be times in our experience during mindfulness practice when feelings of frustration or irritation will arise. These are good opportunities to explore frustration and irritation in the present moment, including noticing what is happening in our body (e.g. maybe asking ourselves "where do I notice this most intensely in my body?") and what is happening in our mind. Feelings of frustration and irritation can also be a valuable opportunity to practice inviting in the possibility of bringing patience and gentleness to our experience.

I'm doubting whether the practice will help me - If you notice thoughts doubting the practice, we invite you to continue as best you can, acknowledging this doubt and maybe suspending your judgment until the end of the study.

I'm becoming aware of bodily sensation that I haven't noticed before - It's great that you are noticing more things about your experience. When we practice, it is not unusual to start to notice things that we were not previously aware of.

I find memories arising during the practice - It is very common to notice our minds wandering to memories. When you notice this has happened, we would invite you to acknowledge where your mind has wandered to and then gently bring your attention back to the present moment, as best you can.

The practice made me feel more emotional or distressed - Because during mindfulness practice we intentionally bring our awareness to our experiences, whatever they may be, it is quite common to get in touch with feelings that can be upsetting. But if you find it at all distressing or upsetting, please let me know and remember that you can stop at any time. In the unlikely event that participating in this research study highlights any issues you may need further support with, then we'd recommend you contact the University's wellbeing advisers on studentwellbeing@canterbury.ac.uk or 01227 922675; your GP or telephone NHS England direct on 0300 311 22 33 or 111 to speak directly to a health professional. If you have any further questions about mindfulness practice, please feel free to as me in the session or email me at **sarah.strohmaier@canterbury.ac.uk**.

Appendix 6.3.3 Participant DEBRIEF email version 4; 8th April 2019

Subject: Thank you for participating in the study examining mindfulness and similar audio-guided exercises

Dear participant,

Thank you very much for participating in this study!

The aim of this study was to compare different lengths of mindfulness practice in order to determine which is most beneficial for individuals. There were three different groups in this study, group 1 practicing 20 minutes of mindfulness, group 2 practicing 5 minutes of mindfulness and group 3 practicing no mindfulness. We were interested in seeing whether engaging in longer or shorter or no mindfulness practice is most helpful for people.

Since the study is still running, please do not give this information to friends and colleagues since they may participate in this study as well.

In the unlikely event of you experiencing any discomfort or distress through participating in mindfulness practice, please remember that you can withdraw from the study at any time.

In the unlikely event that participating in this research study highlights any issues you may need further support with, then we'd recommend you contact the University's wellbeing advisers on studentwellbeing@canterbury.ac.uk or 01227 922675; your GP or telephone NHS England direct on 0300 311 22 33 or 111 to speak directly to a health professional.

If you have any concerns or feedback about any aspect of this study, please let me know by emailing me at **sarah.strohmaier@canterbury.ac.uk** and I will do my best to address your concerns and feedback. You can also contact me by leaving a message on the 24-hour voicemail phone number 01227 927070. Please leave a contact number and say that the message is for me (Sarah Strohmaier) and I will get back to you as soon as possible. If you remain unhappy and wish to complain formally, you can do this by contacting Dr Alex Hassett, Principal Lecturer, Salomons Institute for Applied Psychology – **alex.hassett@canterbury.ac.uk**, phone: 01227 927093.

If you have any further questions about the research or your participation, please contact me via **sarah.strohmaier@canterbury.ac.uk**.

Additional reading

If you are interested in learning mindfulness or would like to know more, please have a look at the below books:

Kabat-Zinn, J. (1990). *Full Catastrophe Living*. New York: Dell.

Williams, M., & Penman, D. (2011). *Mindfulness: A practice guide to finding peace in a frantic world.* London: Piatkus.

You can also complete an online mindfulness course. One option is to visit **https://www.futurelearn.com/courses/mindfulness-wellbeing-performance** which is a free online practical course titled "Mindfulness for Wellbeing and Peak Performance" run by experienced mindfulness practitioners from Monash University, Australia. This course offers flexible learning of mindfulness including practical exercises over four weeks with three hours to be completed a week.

If you are interested in attending a face-to-face course on mindfulness, you can have a look at the UK Network for Mindfulness-Based Teacher Training Organisations. This is their website: **https://www.ukmindfulnessnetwork.co.uk/uk-listing/**

Thank you very much!

Best wishes,
Sarah

Appendix 6.3.4 Transcripts for mindfulness practices

Transcript: twenty-minute mindfulness practice (Group 1)

This audio track will guide you through a mindfulness of the breath meditation practice. The practice stands the best chance of being beneficial if you follow the guidance as best you can. However, the guidance is an invitation, not a requirement. So, if at any point you do not wish to follow a specific part of the guidance, please feel free to ignore it. You're also welcome to stop the practice at any point, if you wish.

So, sitting on your chair, and if it's comfortable to do so, sitting away from the back of the chair so that your spine can be self-supporting. If possible, sitting with your back erect so that the crown of your head is pointing towards the ceiling or sky, allowing your head and neck to be balanced on your shoulders and placing your hands on your knees or in your lap in a comfortable way. And inviting your shoulders to be relaxed and dropped. As best you can, allowing your posture to embody a sense of wakefulness and alertness, and a sense of stability and dignity. In this way, our posture during practice can help embody the attitude that we can bring to our experience in each moment as it unfolds. An attitude of openness, awake-ness and dignity. So, spending a few moments now bringing awareness to your body and posture and making any adjustments that seem helpful (pause).

And knowing that during the practice our posture can change and that it is fine to readjust so that our posture continues to embody a sense of openness, awake-ness and dignity, as best it can.
And closing your eyes now if that feels comfortable; or alternatively, having a soft gaze on the floor a meter or so in front of you. And during this practice holding in mind that we're not trying to achieve any particular state, we're not even trying to relax during this practice. So, seeing if it is possible to let go of the tendency that we all have to want things to be a certain way and the tendency to judge how well we're doing. Rather, seeing if it is possible to greet your moment-to-moment experience with a sense of openness and gentleness (pause).

And now, if you're willing, bringing attention to the breath. Perhaps to the sensations in the belly or chest as they expand with the inbreath and contract with the outbreath (pause).

Or perhaps to the passage of air in and out of the mouth or nose noticing maybe the difference in temperature between the inbreath and the outbreath. Or perhaps placing attention somewhere else where the breath's sensations are particularly accessible and vivid for you right now (pause).

As best you can, not thinking about the breath, but rather being with the experiences of the body as you breathe (pause).

And if you find it a struggle to locate these experiences, then perhaps resting a hand on your chest or belly for a few moments now and feeling it move with the breath (pause).

And if you have not yet done so, returning now your hand to where it was before and continuing to experience the breath, as best you can; moment by moment, breath by breath (pause).

Throughout this sitting practice, the breath will always be present as something that you can return to if you find yourself overwhelmed by your experience. It's also fine to stop the practice at any point if that seems the best thing to do. But if it feels okay to do so now being as present as you can be with the breath, moment by moment, breath by breath (pause).

And as best you can, letting go of controlling the breath, but rather allowing it to come and go as it pleases. But if that doesn't seem possible right now, then that's fine too (pause).

So being present with the experience of breathing, moment by moment, breath by breath, as best you can (pause).

And inevitably, the mind will wander from the breath, perhaps to other sensations, perhaps to thoughts about the future or past or perhaps to some other aspect of your experience. This is completely normal and is not a problem at all. When you become aware that the mind has wandered from the breath, notice what it has wandered to and inviting the possibility that it is okay that the mind has wandered, after all, this is what our minds do. And then, letting go of whatever it is that the mind has wandered to and shifting attention back to the breath, as best you can. No need to push away experience, but rather just let it be as you return to the breath. Experiencing the breath entering the body and experiencing the breath leaving the body once again (pause).

And you may find that judgements arise, perhaps judgements about the mind wandering. If this happens, as best you can, noticing the judgements with kindness and allowing whatever arises in your experience to be just as it is. Remembering, there is no right or wrong. And now returning to the experience of breathing, as best you can. Experiencing the breath entering the body and experiencing the breath leaving the body once again (pause).

So, if you’re willing, feeling the breath’s sensations as they flux and change moment by moment, breath by breath, as best you can (pause).

Experiencing the breath entering the body and experiencing the breath leaving the body once again (pause).

And knowing that, as it is in the nature of all our minds to wander, if you find that your mind is wandering away from the breath, this is absolutely normal and to be expected. Noticing and experiencing the wandering mind is an important part of mindfulness practice and is a valuable opportunity to practice gentleness and patience. After we’ve noticed that the mind has wandered, as best we can, coming back to the experience of breathing, moment by moment, breath by breath (pause).

So being present with the breath as best you can, moment by moment, as it enters the body and as it leaves the body once again (pause).

So, noticing where your mind is right now. And remembering that wherever it is, that’s okay from the point of view of this practice. And if you find the mind has wandered away from the breath, then gently coming back to the breath now, if that feels okay to do so. And being present with the experience of breathing, moment by moment, breath by breath, as best you can (pause).

And seeing if it’s possible to be as curious as you can be towards your experience of the breath right now. Exploring the sensations connected with the breath and how they change and flux, moment by moment, breath by breath (pause).

Experiencing the breath entering the body and experiencing the breath leaving the body once again (pause).

So being present with the breath as best you can, moment by moment as it enters the body and as it leaves the body once again (pause).

And if the mind wanders away from the breath, greeting that with gentleness and patience before returning to the breath as best you can (pause).

Experiencing the breath entering the body and experiencing the breath leaving the body once again (pause).

Being present with the experience of breathing, moment by moment, breath by breath, as best you can (pause).

And in a few moments' time, I'll invite you to gently bring this period of sitting practice to a close. But for now, noticing what effect, if any, these words are having on your experience of breathing (pause).

And now, when you feel ready, opening your eyes gradually if they're closed and bringing awareness back into the room. The researcher will advise you about what to do next.

---END---

<u>Transcript: five-minute mindfulness practice (Group 2)</u>

This audio track will guide you through a mindfulness of the breath meditation practice. The practice stands the best chance of being beneficial if you follow the guidance as best you can. However, the guidance is an invitation, not a requirement. You're also welcome to stop the practice at any point if you wish.

So, sitting on your chair, and if it's comfortable to do so, sitting away from the back of the chair so that your spine can be self-supporting. If possible, sitting with your back erect and placing your hands on your knees or in your lap in a comfortable way and inviting your shoulders to be relaxed and dropped.

And closing your eyes now if that feels comfortable, or alternatively, having a soft gaze on the floor, a meter or so in front of you.

And during this practice, holding in mind that we're not trying to achieve any particular state, we're not even trying to relax during this practice. So, seeing if it is possible to let go of the tendency we all have to want things a certain way and the tendency to judge how well we're doing. Rather, seeing if it's possible to greet your moment-to-moment experience with a sense of openness and gentleness.

And now, if you're willing, bringing attention to the breath. Perhaps to the sensations in the belly or chest as they expand with the inbreath and contract with the outbreath. Or perhaps placing attention somewhere else, where the breath's sensations are particularly accessible and vivid for you right now.

As best you can, not thinking about the breath, but rather being with the experiences of the body as you breathe (pause).

And as best you can, letting go of controlling the breath, but rather allowing it to come and go as it pleases (pause).

So if you're willing, feeling the breath's sensations as they flux and change, moment by moment, breath by breath, as best you can (pause).

And inevitably, the mind will wander from the breath. This is completely normal. When you become aware that the mind has wandered from the breath, notice what it has wondered to and inviting the possibility that it is okay that the mind has wandered. And then, letting go of whatever it is that the mind has wandered to and shifting attention back to the breath (pause).

So being present with the breath as best you can moment by moment as it enters the body and as it leaves the body once again (pause).

And you may find that judgements arise, perhaps judgements about the mind wandering. If this happens, as best you can, noticing the judgements with kindness and allowing whatever arises in your experience to be, just as it is. And now returning to the experience of breathing, as best you can (pause).

And if the mind wanders away from the breath, greeting that with gentleness and patience, before returning to the breath, as best you can (pause).

Experiencing the breath entering the body, and experiencing the breath leaving the body once again (pause).

And now, when you feel ready, opening your eyes gradually if they're closed and bringing awareness back into the room. The researcher will advise you about what to do next.

---END---

Appendix 6.3.5 Study consent form

CONSENT FORM (version 4; 8th April 2019)

Name of Researcher: Sarah Strohmaier

Study examining effects of mindfulness and similar audio-guided exercises

Please initial each box:

1. I confirm that I have read and understand the information sheet (version 4; 08/04/2019 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.

3. I confirm that I am currently not experiencing significant difficulties with my mental wellbeing.

4. I confirm that I do not currently have a mindfulness practice and am not currently participating in a mindfulness-based intervention.

5. I understand that the only persons looking at the data collected during the study are the researcher (Sarah Strohmaier) and her supervisor (Dr Fergal Jones). I give permission for these individuals to look at this data.

6. I understand that my name and email address are only used to match my questionnaire answers after which it will be anonymised and there is no way for anyone to know which data are mine.

7. I agree for my anonymous data to be used for publication of this research.

8. I agree to take part in the above study.

Name of Participant____________________ Date________________

Signature ___________________

Appendix 6.3.6 Demographic questionnaire

Name: ____

Email address: _____________________________________

1. What is your age (in years)? (Please specify below).

____

2. What is your gender? (Please tick as appropriate).

- ☐ Female
- ☐ Male
- ☐ Non-binary
- ☐ Other, please specify: ____
- ☐ Prefer not to say

3. What is your ethnicity? (Please tick as appropriate).

- ☐ White British, Scottish or Welsh
- ☐ White Other
- ☐ Black British, Scottish or Welsh
- ☐ Black Other
- ☐ Asian British, Scottish or Welsh
- ☐ Asian Other
- ☐ Mixed White and Asian
- ☐ Mixed White and Black
- ☐ Mixed Asian and Black
- ☐ Mixed Other
- ☐ Other Ethnic background, please specify: _______________________________
- ☐ Prefer not to say

4. For students: What is your course of study?

   For others: What is your occupation?

(Please specify below).

____

5. In the past, have you practiced mindfulness, participated in a mindfulness-based intervention or experienced any training or teaching on mindfulness?

- ☐ No.
- ☐ Yes, please specify:

  ____

  ____

  ____

For undergraduates only

7. Which incentive would you like to choose by participating in this study:

- ☐ Course credits
- ☐ Prize draw to win £50 shopping voucher

Remember, you will receive course credits/an entry into the prize draw for every session you attend (including completing questionnaires) as well as for completing the online questionnaire that is emailed to you a week after the final session.

Appendix 6.3.7 Self-report measures in this study: DASS-21, FFMQ-15, PQ-M, TMS

Depression Anxiety and Stress Scale (DASS-21)
Instructions: Please read each statement and circle a number 0, 1, 2 or 3 which indicates how much the statement applied to you **over the past week**. There are no right or wrong answers. Do not spend too much time on any statement.
The rating scale is as follows:
0 Did not apply to me at all.
1 Applied to me to some degree, or some of the time.
2 Applied to me to a considerable degree or a good part of time.
3 Applied to me very much or most of the time.

1. I found it hard to wind down.	0	1	2	3
2. I was aware of dryness of my mouth.	0	1	2	3
3. I couldn't seem to experience any positive feeling at all.	0	1	2	3
4. I experienced breathing difficulty (e.g. excessively rapid breathing, breathlessness in the absence of physical exertion).	0	1	2	3
5. I found it difficult to work up the initiative to do things.	0	1	2	3
6. I tended to over-react to situations.	0	1	2	3
7. I experienced trembling (e.g. in the hands).	0	1	2	3
8. I felt that I was using a lot of nervous energy.	0	1	2	3
9. I was worried about situations in which I might panic and make a fool of myself.	0	1	2	3
10. I felt that I had nothing to look forward to.	0	1	2	3
11. I found myself getting agitated.	0	1	2	3
12. I found it difficult to relax.	0	1	2	3
13. I felt down-hearted and blue.	0	1	2	3
14. I was intolerant of anything that kept me from getting on with what I was doing.	0	1	2	3
15. I felt I was close to panic.	0	1	2	3
16. I was unable to become enthusiastic about anything.	0	1	2	3
17. I felt I wasn't worth much as a person.	0	1	2	3
18. I felt that I was rather touchy.	0	1	2	3
19. I was aware of the action of my heart in the absence of physical exertion (e.g. sense of heart rate increase, heart missing a beat).	0	1	2	3
20. I felt scared without any good reason.	0	1	2	3
21. I felt that life was meaningless.	0	1	2	3

Five Facet Mindfulness Questionnaire (FFMQ-15)

Instructions: Please use the 1 to 5 scale provided to indicate how true the below statements are of you. The rating scales is as follows:
1 Never or very rarely true.
2 Rarely true.
3 Sometimes true.
4 Often true.
5 Very often or always true.
Circle the number in the box to the right of each statement which represents your own opinion of what is generally true for you. For example, if you think that a statement is often true of you, circle '4' and if you think a statement is sometimes true of you, circle '3'.

1. When I take a shower or a bath, I stay alert to the sensations of water on my body.	1	2	3	4	5
2. I'm good at finding words to describe my feelings.	1	2	3	4	5
3. I don't pay attention to what I'm doing because I'm daydreaming, worrying, or otherwise distracted.	1	2	3	4	5
4. I believe some of my thoughts are abnormal or bad and I shouldn't think that way.	1	2	3	4	5
5. When I have distressing thoughts or images, I "step back" and am aware of the thought or image without getting taken over by it.	1	2	3	4	5
6. I notice how foods and drinks affect my thoughts, bodily sensations, and emotions.	1	2	3	4	5
7. I have trouble thinking of the right words to express how I feel about things.	1	2	3	4	5
8. I do jobs or tasks automatically without being aware of what I'm doing.	1	2	3	4	5
9. I think some of my emotions are bad or inappropriate and I shouldn't feel them.	1	2	3	4	5
10. When I have distressing thoughts or images I am able just to notice them without reacting.	1	2	3	4	5
11. I pay attention to sensations, such as the wind in my hair or sun on my face.	1	2	3	4	5
12. Even when I'm feeling terribly upset I can find a way to put it into words.	1	2	3	4	5
13. I find myself doing things without paying attention.	1	2	3	4	5
14. I tell myself I shouldn't be feeling the way I'm feeling.	1	2	3	4	5
15. When I have distressing thoughts or images I just notice them and let them go.	1	2	3	4	5

Practice Quality-Mindfulness (PQM-6)

With respect to today's session, please place a vertical mark on the line below each question to indicate the approximate percentage of time that your experience reflected each statement below.

1. During practice, I attempted to return to my present-moment experience, whether unpleasant, pleasant, or neutral.

0% 10% 20% 30% 40% 50% 60% 70% 80% 90% 100%

2. During practice, I attempted to return to each experience, no matter how unpleasant, with a sense that "It's OK to experience this".

0% 10% 20% 30% 40% 50% 60% 70% 80% 90% 100%

3. During practice, I attempted to feel each experience as bare sensations in the body (tension in throat, movement in belly, etc).

0% 10% 20% 30% 40% 50% 60% 70% 80% 90% 100%

4. During practice, I was struggling against having certain experiences (e.g., unpleasant thoughts, emotions, and/or bodily sensations).

0% 10% 20% 30% 40% 50% 60% 70% 80% 90% 100%

5. During practice, I was actively avoiding or "pushing away" certain experiences.

0% 10% 20% 30% 40% 50% 60% 70% 80% 90% 100%

6. During practice, I was actively trying to fix or change certain experiences, in order to get to a "better place".

0% 10% 20% 30% 40% 50% 60% 70% 80% 90% 100%

Toronto Mindfulness Scale (TMS)

Instructions:

We are interested in what you just experienced. Below is a list of things that people sometimes experience. Please read each statement. Please indicate the extent to which you agree with each statement. In other words, how well does the statement describe what you just experienced, just now?

0 Not at all
1 A Little
2 Moderately
3 Quite a bit
4 Very much

1. I experienced myself as separate from my changing thoughts and feelings.	0	1	2	3	4
2. I was more concerned with being open to my experiences than controlling or changing them.	0	1	2	3	4
3. I was curious about what I might learn about myself by taking notice of how I react to certain thoughts, feelings, or sensations.	0	1	2	3	4
4. I experienced my thoughts more as events in my mind than as a necessarily accurate reflection of the way things 'really' are.	0	1	2	3	4
5. I was curious to see what my mind was up to from moment to moment.	0	1	2	3	4
6. I was curious about each of the thoughts and feelings that I was having.	0	1	2	3	4
7. I was receptive to observing unpleasant thoughts and feelings without interfering with them.	0	1	2	3	4
8. I was more invested in just watching my experiences as they arose, than in figuring out what they could mean.	0	1	2	3	4
9. I approached each experience by trying to accept it, no matter whether it was pleasant or unpleasant.	0	1	2	3	4
10. I remained curious about the nature of each experience as it arose.	0	1	2	3	4
11. I was aware of my thoughts and feelings without overidentifying with them.	0	1	2	3	4
12. I was curious about my reactions to things.	0	1	2	3	4
13. I was curious about what I might learn about myself by just taking notice of what my attention gets drawn to.	0	1	2	3	4

Appendix 6.3.8 Question on practice since the last session question (given to all participants)

We have asked you to do the best you can not to engage in formal mindfulness practices over the course of this study. However, don't worry if you did formally practice mindfulness. If you did do that, it would really help the quality of the research if you let us know below. Either way, please can you answer the following question?

Since the last session, have you engaged in formal mindfulness practice? (By formal mindfulness practice we mean listening to audio recordings or taking time to formally sit or lie down to practice mindfulness)

- ☐ No.
- ☐ Yes, please give details of what you practiced, how you practiced (e.g. using audio recordings), how long you practiced, etc. below:

____

____

Appendix 6.3.9 Ethical approval letter

Canterbury Christ Church University

Salomons Centre for Applied Psychology

Sarah Strohmaier
Psychology PhD Student

29 November 2018
Direct line 01227 927094
E-mail margie.callanan@canterbury,ac.uk
Our Ref V:\075\Ethics\2017-18

Dear Sarah,

Effects of length of mindfulness practice on mindfulness, depression, anxiety and stress: A randomised controlled experiment.

Outcome: Full Approval

The panel would like to thank you for your submission and we are pleased to offer you approval for your proposed study. The panel had some observations for you to consider with your supervisor and to report back for our records:

1. The panel wondered whether the Control Group could access some mindfulness sessions after project is completed, perhaps even directing them to some online resources in a debrief.
2. There are typographical errors under 'Complaints and Feedback' and in the title 'Will information from or about me….' – Participant Information Sheet (PIS)
3. Under 'What will happen to me if I take part?' the last sentence refers to "in due time"; the panel's view is that this is too vague and some time frame should be offered if possible - PIS.
4. 'What are the possible benefits of taking part?' – The panel were of the view that there could be a bit more detail here, to make this rather more compelling – even just mentioning mindfulness rather more - PIS.
5. On Consent Form: point 6 should finish 'to know which data are mine'.
6. On the Debrief Sheet: when they tick box to get a summary of findings – how is this returned to the researcher? This needs to be clear. And they need to sign as well as tick the box.

Yours sincerely,

Margie Callanan

Professor Margie Callanan
Chair of the Salomons Ethics Panel

Cc Dr Fergal Jones

School of Psychology, Politics and Sociology
School of Psychology, Politics and Sociology
Faculty of Social and Applied Sciences

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www.canterbury.ac.uk

Professor Rama Thirunamachandran, Vice-Chancellor and Principal

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Appendix 6.3.10 Email correspondence with Prof Margie Callanan, Head of the Salomons Institute Ethics Panel

Sarah Strohmaier

From:	Callanan, Margie (margie.callanan@canterbury.ac.uk)
Sent:	07 March 2019 18:13
To:	Strohmaier, Sarah (sarah.strohmaier@canterbury.ac.uk)
Cc:	Shea, Jennie (jennie.shea@canterbury.ac.uk); Jones, Fergal (fergal.jones@canterbury.ac.uk); Cane, James (james.cane@canterbury.ac.uk)
Subject:	Re: Change of measure in study and participant advert

Dear Sarah,
I am happy to approve these changes, they represent a potential improvement to the project.
Good luck with the work.
Best wishes,
Margie

Professor Margie Callanan
Sent from my iPad

On 7 Mar 2019, at 18:08, Strohmaier, Sarah (sarah.strohmaier@canterbury.ac.uk) <sarah.strohmaier@canterbury.ac.uk> wrote:

Dear Margie (cc Jennie Shea, Fergal Jones, James Cane),

I hope this email finds you well.

Through correspondence at the International Mindfulness Conference I attended where I was also made aware of some recent research and validation analysis (see Medvedev et al. (2017) attached), I learned that there is a more exact scale to measure state mindfulness than the one I had previously listed in my Ethics form. This is the Toronto Mindfulness Scale (TMS), which I have attached to this email. Previous research also suggests evidence that the TMS is sensitive to briefer mindfulness practices such as those employed in my study. The TMS is also 8 items shorter than the State Mindfulness Scale (SMS) which I had originally intended on using, thus providing a lesser burden on participants.
Based on the above, I would therefore like to change the scale I am using to measure state mindfulness from the SMS to the TMS.

Additionally, I have created an advert for undergraduate participants through which they can sign up to via the Research Participation Scheme (RPS), please see attached.
After signing up on the RPS scheme with their email, participants are then sent the full information sheet which has already been submitted and approved by Ethics.

Please let me know if you are happy with these changes from the point of view of the Ethics panel.

Thank you very much!

Best wishes,
Sarah

____

Sarah Strohmaier
Research Assistant in Psychology
PhD student – Psychology

Salomons Centre for Applied Psychology

1

Canterbury Christ Church University
1 Meadow Road
Tunbridge Wells, Kent TN1 2YG
Tel: +44 (0)1227 92 7092
Email: sarah.strohmaier@canterbury.ac.uk
Website: www.canterbury.ac.uk/appliedpsychology

Please note, I work Mondays, Tuesdays, Thursdays and Fridays.

<Toronto Mindfulness Scale (TMS).docx>

<Medvedev et al. (2017) measuring mindfulness- applying G theory to distinguish between state and trait.pdf>

<Study Introduction for advert on the Research Participation Scheme (RPS).docx>

Sarah Strohmaier

From:	Margie Callanan
Sent:	29 July 2019 12:33
To:	Sarah Strohmaier
Cc:	Fergal Jones; Jennie Shea
Subject:	Re: Question to the Salomons Ethics panel

Dear Sarah,
This amendment is now noted in our records (thanks Jennie) and approved on your receipt of this email.
Good luck with the work.
Margie

Professor Margie Callanan
Sent from my iPad

On 28 Jul 2019, at 21:37, Sarah Strohmaier <sarah.strohmaier@canterbury.ac.uk> wrote:

Dear Margie (cc Fergal),

As the chair of the Salomons Ethics panel, I was wondering whether you and the panel would be fine with me employing snowball sampling by adding the sentence "please feel free to pass on the information sheet to anyone you think might be interested" to recruitment materials (such as general emails sent out, rip-off email addresses next to posters (see attached) etc.) for my PhD study examining the effects of mindfulness and similar audio-guided exercises?

Secondly, would the panel be happy for me to contact local businesses (in Tunbridge Wells and Canterbury) to ask whether they would be happy to put up a poster in their business advertising the study along with rip-off email addresses (see both attached)? Possible businesses could include hairdressers, gyms, charity shops etc.

I look forward to hearing the panel's thoughts.
Thank you very much.

Best wishes,
Sarah

Sarah Strohmaier
Research Assistant in Psychology
PhD student – Psychology

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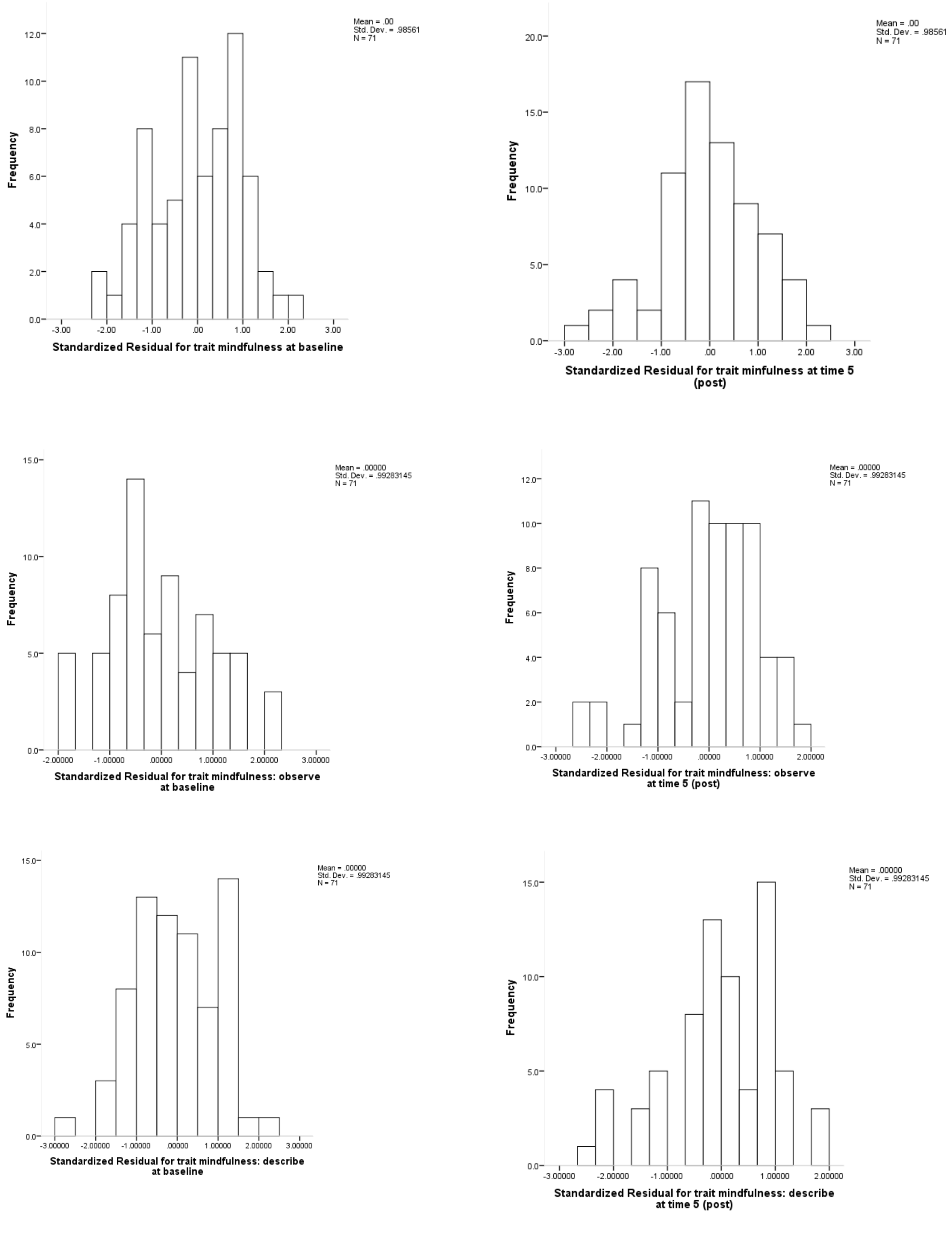
Email: sarah.strohmaier@canterbury.ac.uk
Website: www.canterbury.ac.uk/appliedpsychology

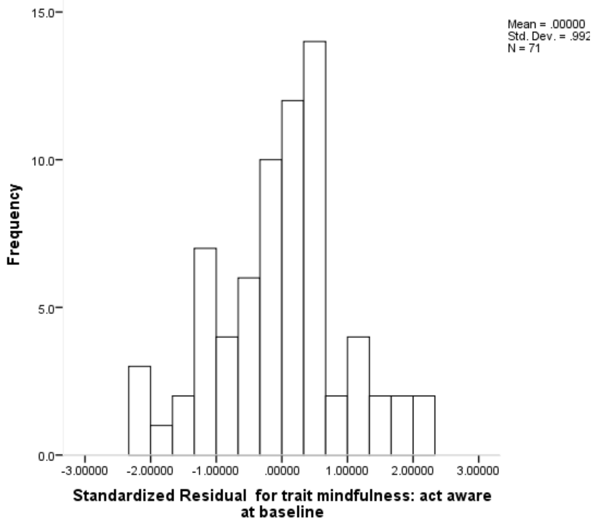
Please note, I work Mondays, Tuesdays, Thursdays and Fridays.

<Study poster .pdf>

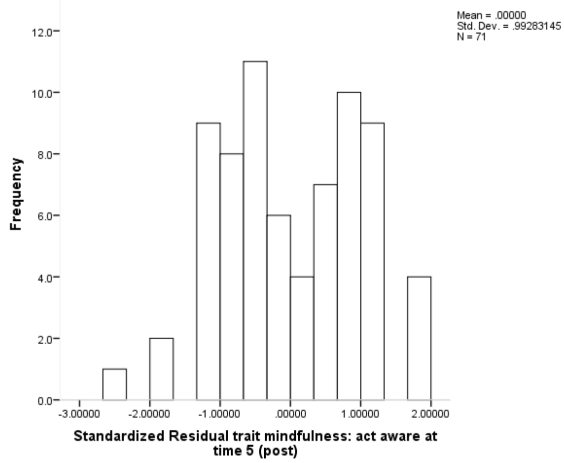
Appendix 6.4: Chapter 6 Results
Appendix Figure 6.4.1

Histograms of standardised residuals for outcomes at baseline and Time 5

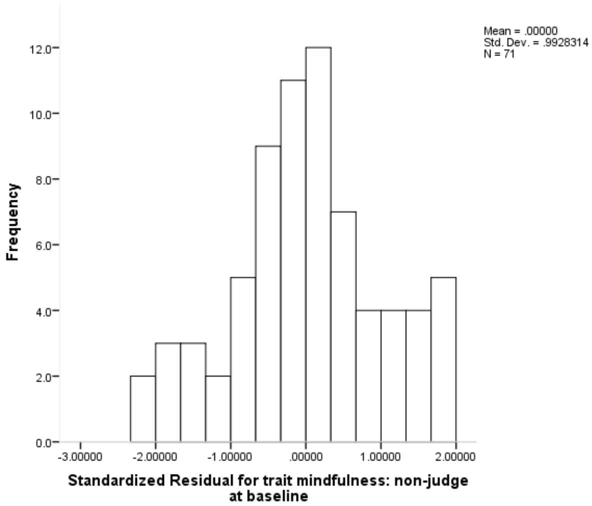




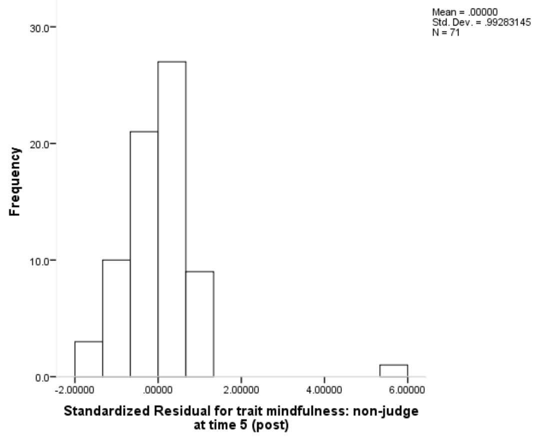
Mean = .00000
Std. Dev. = .992
N = 71
Frequency
Standardized Residual for trait mindfulness: act aware at baseline



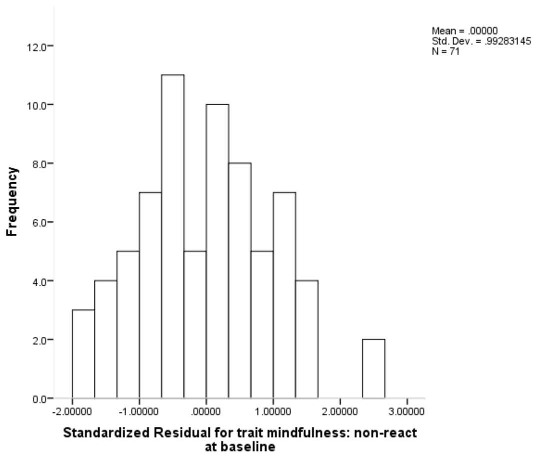
Mean = .00000
Std. Dev. = .99283145
N = 71
Frequency
Standardized Residual trait mindfulness: act aware at time 5 (post)



Mean = .00000
Std. Dev. = .9928314
N = 71
Frequency
Standardized Residual for trait mindfulness: non-judge at baseline



Mean = .00000
Std. Dev. = .99283145
N = 71
Frequency
Standardized Residual for trait mindfulness: non-judge at time 5 (post)



Mean = .00000
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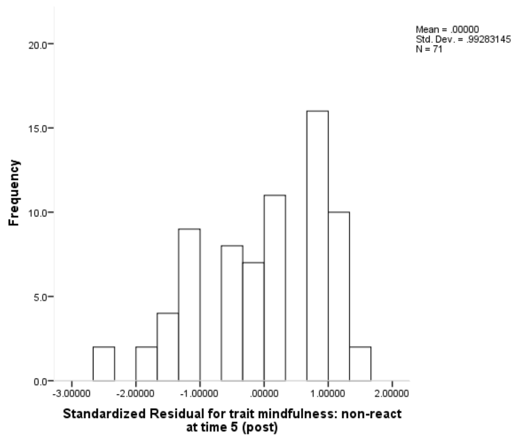
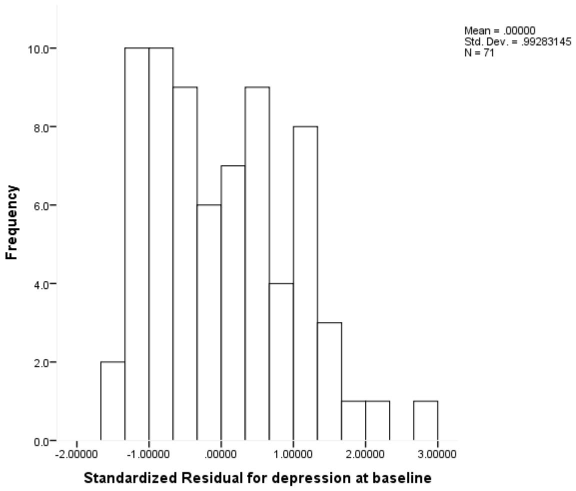
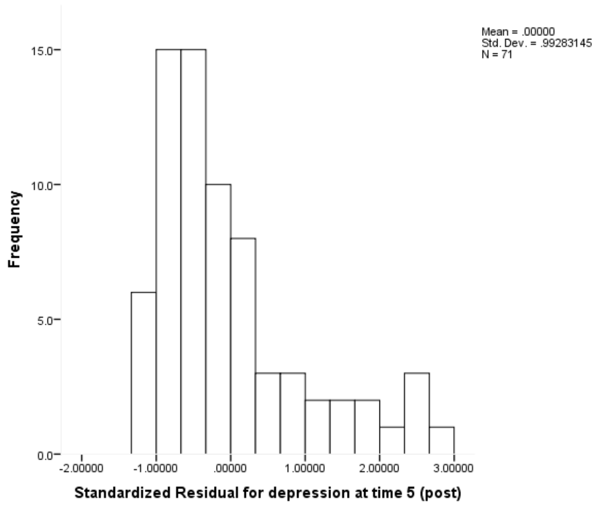
N = 71
Frequency
Standardized Residual for trait mindfulness: non-react at baseline



Mean = .00000
Std. Dev. = .99283145
N = 71
Frequency
Standardized Residual for trait mindfulness: non-react at time 5 (post)


Mean = .00000
Std. Dev. = .99283145
N = 71
Frequency
Standardized Residual for depression at baseline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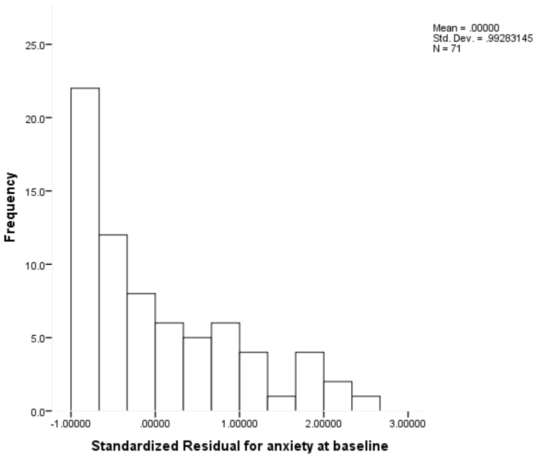
Std. Dev. = .99283145
N = 71
Frequency
Standardized Residual for depression at time 5 (post)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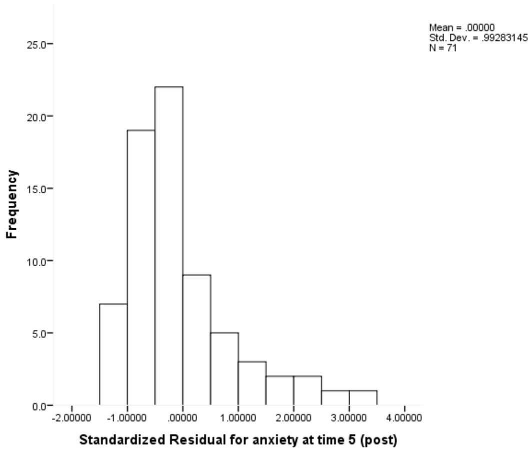
N = 71
Frequency
Standardized Residual for anxiety at baseline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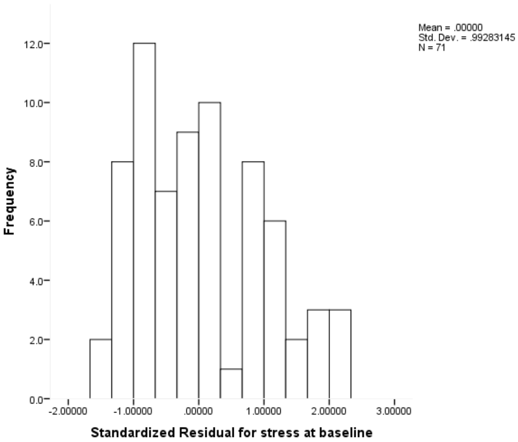
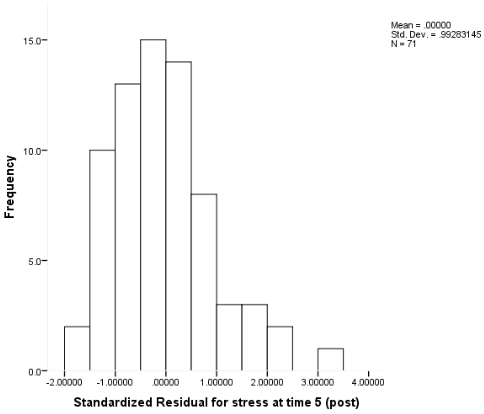
N = 71
Frequency
Standardized Residual for anxiety at time 5 (post)


Mean = .00000
Std. Dev. = .99283145
N = 71
Frequency
Standardized Residual for stress at baseline


Mean = .00000
Std. Dev. = .99283145
N = 71
Frequency
Standardized Residual for stress at time 5 (post)

Appendix 6.4.2 SPSS output for normality tests of standardised residuals for outcomes at baseline & Time 5 (post)

Tests of Normality

	Kolmogorov-Smirnov[a]			Shapiro-Wilk		
	Statistic	df	Sig.	Statistic	df	Sig.
Standardized Residual for trait mindfulness at baseline	.098	71	.086	.976	71	.187
Standardized Residual for trait mindfulness at time 5 (post)	.097	71	.095	.979	71	.292
Standardized Residual for trait mindfulness: observe at baseline	.126	71	.007	.971	71	.095
Standardized Residual for trait mindfulness: observe at time 5 (post)	.077	71	.200*	.973	71	.133
Standardized Residual for trait mindfulness: describe at baseline	.088	71	.200*	.976	71	.191
Standardized Residual for trait mindfulness: describe at time 5 (post)	.135	71	.003	.954	71	.011
Standardized Residual for trait mindfulness: act aware at baseline	.107	71	.044	.978	71	.238
Standardized Residual for trait mindfulness: act aware at time 5 (post)	.119	71	.015	.962	71	.031
Standardized Residual for trait mindfulness: non-react at baseline	.063	71	.200*	.984	71	.495
Standardized Residual for trait mindfulness: non-react at time 5 (post)	.151	71	.000	.944	71	.003
Standardized Residual for trait mindfulness: non-judge at baseline	.059	71	.200*	.974	71	.145
Standardized Residual for trait mindfulness: non-judge at time 5 (post)	.157	71	.000	.786	71	.000
Standardized Residual for depression at baseline	.120	71	.013	.951	71	.007
Standardized Residual for depression at time 5 (post)	.193	71	.000	.878	71	.000
Standardized Residual for anxiety at baseline	.186	71	.000	.863	71	.000
Standardized Residual for anxiety at time 5 (post)	.244	71	.000	.847	71	.000
Standardized Residual for stress at baseline	.115	71	.020	.940	71	.002
Standardized Residual for stress at time 5 (post)	.109	71	.036	.962	71	.031

*. This is a lower bound of the true significance.

a. Lilliefors Significance Correction

SPSS output for normality tests of standardised residuals by group

Tests of Normality

		Kolmogorov-Smirnov[a]			Shapiro-Wilk		
	Group	Statistic	df	Sig.	Statistic	df	Sig.
Standardized Residual for trait mindfulness at baseline	G1 long M	.092	24	.200*	.984	24	.951
	G2 short M	.146	24	.198	.925	24	.077
	G3 no M	.159	23	.138	.941	23	.187
Standardized Residual for trait mindfulness at time 5 (post)	G1 long M	.129	24	.200*	.970	24	.666
	G2 short M	.210	24	.007	.916	24	.049
	G3 no M	.179	23	.053	.905	23	.031
Standardized Residual for trait mindfulness: observe at baseline	G1 long M	.199	24	.015	.934	24	.118
	G2 short M	.136	24	.200*	.965	24	.557
	G3 no M	.082	23	.200*	.963	23	.523
Standardized Residual for trait mindfulness: observe at time 5 (post)	G1 long M	.151	24	.168	.913	24	.042
	G2 short M	.137	24	.200*	.935	24	.126
	G3 no M	.116	23	.200*	.972	23	.744
Standardized Residual for trait mindfulness: describe at baseline	G1 long M	.122	24	.200*	.968	24	.629
	G2 short M	.156	24	.135	.951	24	.285
	G3 no M	.147	23	.200*	.910	23	.042
Standardized Residual for trait mindfulness: describe at time 5 (post)	G1 long M	.194	24	.020	.942	24	.177
	G2 short M	.221	24	.004	.906	24	.029
	G3 no M	.132	23	.200*	.960	23	.473
Standardized Residual for trait mindfulness: act aware at baseline	G1 long M	.188	24	.028	.949	24	.259
	G2 short M	.170	24	.072	.957	24	.386
	G3 no M	.197	23	.021	.926	23	.090
Standardized Residual for trait mindfulness: act aware at time 5 (post)	G1 long M	.163	24	.096	.917	24	.050
	G2 short M	.215	24	.006	.887	24	.011
	G3 no M	.172	23	.074	.946	23	.246
Standardized Residual for trait mindfulness: non-react at baseline	G1 long M	.177	24	.051	.933	24	.113
	G2 short M	.154	24	.145	.948	24	.245
	G3 no M	.171	23	.078	.917	23	.056
Standardized Residual for trait mindfulness: non-react at time 5 (post)	G1 long M	.241	24	.001	.884	24	.010
	G2 short M	.213	24	.006	.905	24	.027
	G3 no M	.213	23	.008	.895	23	.020
Standardized Residual for trait mindfulness: non-judge at baseline	G1 long M	.115	24	.200*	.937	24	.137
	G2 short M	.137	24	.200*	.962	24	.486
	G3 no M	.116	23	.200*	.960	23	.454
Standardized Residual for trait mindfulness: non-judge at time 5 (post)	G1 long M	.194	24	.020	.858	24	.003
	G2 short M	.194	24	.020	.909	24	.034
	G3 no M	.201	23	.017	.747	23	.000
Standardized Residual for depression at baseline	G1 long M	.167	24	.083	.924	24	.071
	G2 short M	.099	24	.200*	.950	24	.274
	G3 no M	.214	23	.008	.887	23	.014
Standardized Residual for depression at time 5 (post)	G1 long M	.250	24	.000	.814	24	.000
	G2 short M	.387	24	.000	.681	24	.000
	G3 no M	.218	23	.006	.875	23	.008
Standardized Residual for anxiety at baseline	G1 long M	.203	24	.012	.869	24	.005
	G2 short M	.229	24	.002	.852	24	.002
	G3 no M	.254	23	.000	.833	23	.001
Standardized Residual for anxiety at time 5 (post)	G1 long M	.270	24	.000	.806	24	.000
	G2 short M	.405	24	.000	.659	24	.000
	G3 no M	.261	23	.000	.844	23	.002
Standardized Residual for stress at baseline	G1 long M	.143	24	.200*	.930	24	.097
	G2 short M	.199	24	.015	.874	24	.006
	G3 no M	.196	23	.023	.947	23	.257
Standardized Residual for stress at time 5 (post)	G1 long M	.277	24	.000	.885	24	.011
	G2 short M	.239	24	.001	.802	24	.000
	G3 no M	.173	23	.073	.974	23	.785

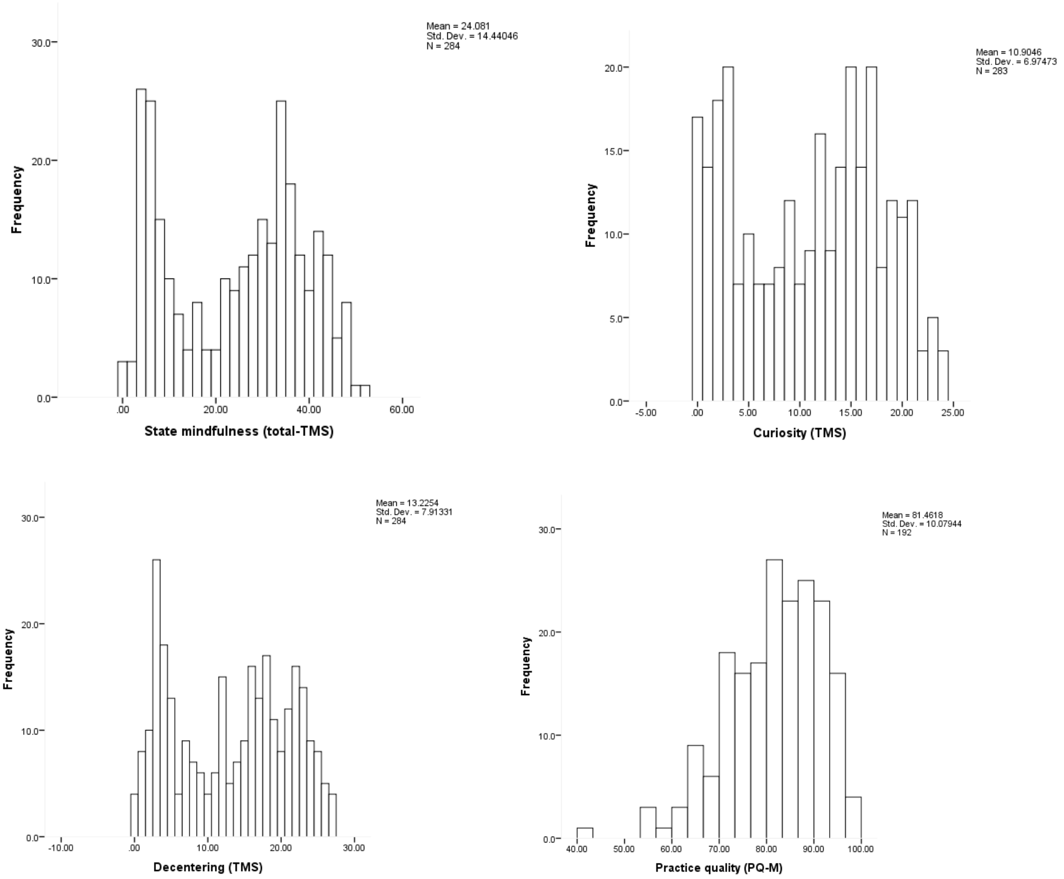
*. This is a lower bound of the true significance.

a. Lilliefors Significance Correction

Note: G1 long M=longer mindfulness practice; G2 short M= shorter mindfulness practice; G1 no M=control

Appendix Figure 6.4.3

Normality histograms for state mindfulness (total-TMS, curiosity, decentering) and practice quality



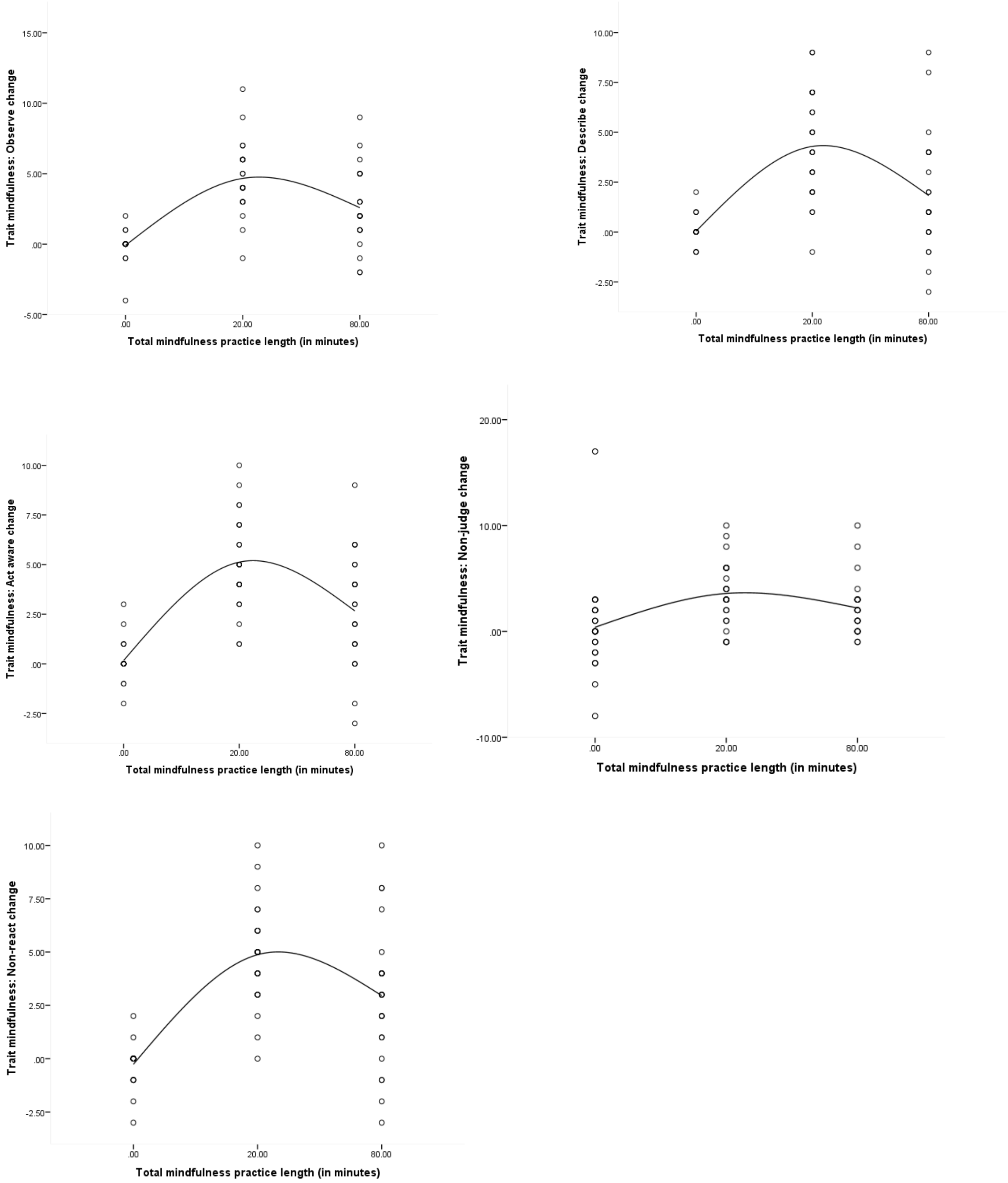
Appendix 6.4.4 SPSS output for Levene's test for homogeneity of variance

Test of Homogeneity of Variances

	Levene Statistic	df1	df2	Sig.
Trait mindfulness at baseline	.239	2	68	.788
Trait mindfulness: observe at baseline	.522	2	68	.596
Trait mindfulness: describe at baseline	1.472	2	68	.237
Trait mindfulness: act aware at baseline	.994	2	68	.376
Trait mindfulness: non-judge at baseline	2.592	2	68	.082
Trait mindfulness: non-react at baseline	1.071	2	68	.348
Depression at baseline	1.032	2	68	.362
Anxiety at baseline	1.298	2	68	.280
Stress at baseline	4.618	2	68	.013
Trait mindfulness at time 5 (post)	18.900	2	68	.000
Trait mindfulness: observe at time 5 (post)	5.224	2	68	.008
Trait mindfulness: decribe at time 5 (post)	4.344	2	68	.017
Trait mindfulness: act aware at time 5 (post)	6.465	2	68	.003
Trait mindfulness: non-judge at time 5 (post)	5.914	2	68	.004
Trait mindfulness: non-react at time 5 (post)	14.644	2	68	.000
Depression at time 5 (post)	26.550	2	68	.000
Anxiety at time 5 (post)	34.515	2	68	.000
Stress at time 5 (post)	10.352	2	68	.000

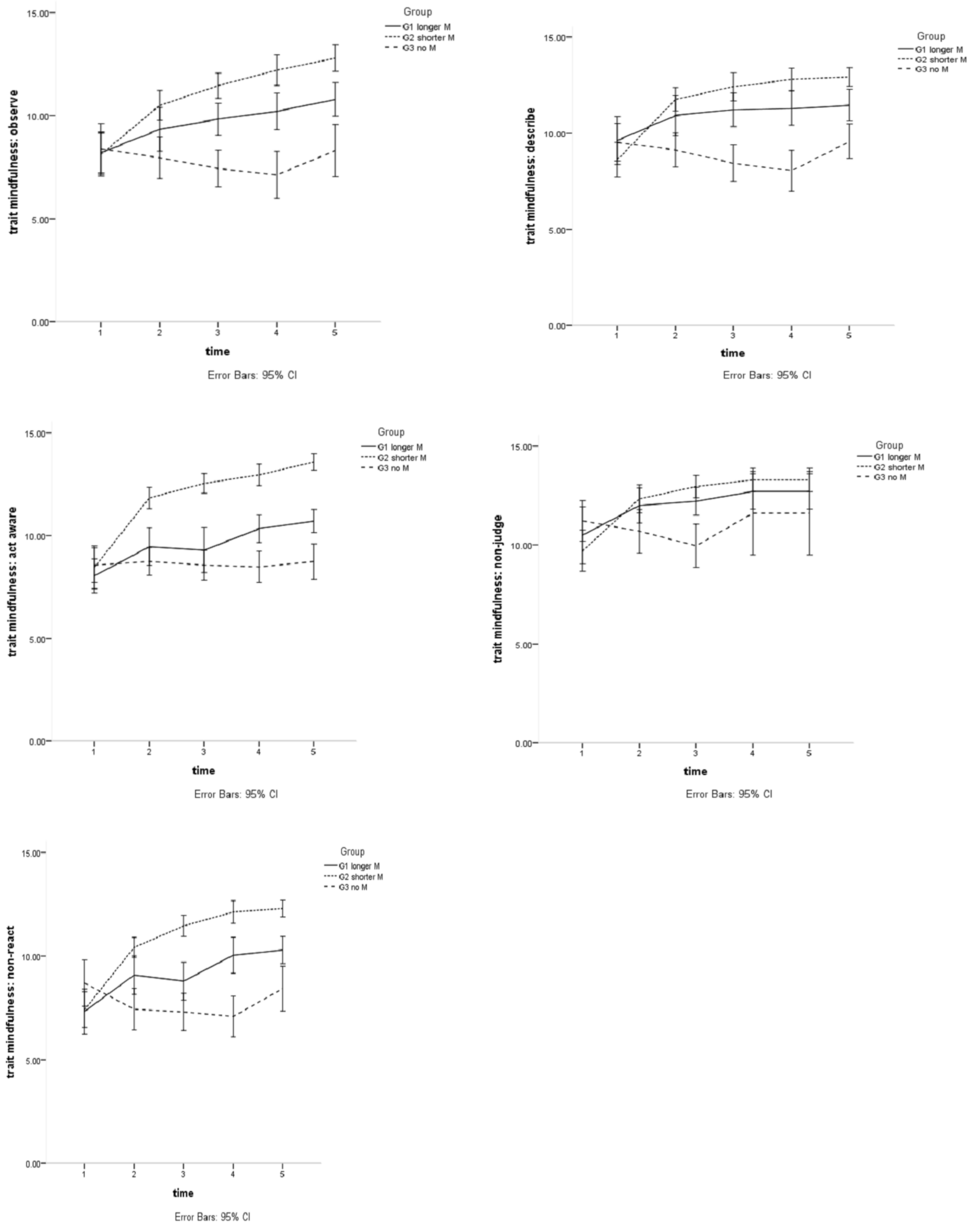
Appendix Figure 6.4.5

Inverted U-shaped effects for FFMQ subscales (observe, describe, act aware, non-judge, non-react)



Appendix Figure 6.4.6

Change in FFMQ-15 subscales across all time points, from baseline (Time 1) to end of study (Time 5), for Group 1 (longer practice), Group 2 (shorter practice) and Group 3 (control). Top left: observe. Top right: describe. Middle left: act aware. Middle right: non-judge. Bottom left: non-react.



Appendix Table 6.4.7

Regression results for practice length (longer vs. shorter practice; longer practice vs. control; shorter practice vs. control) predicting outcomes at post-study (Time 5) when controlling for baseline levels of the respective outcome

Predictor: Longer practice vs. shorter practice

Outcome	R	R^2	Adj. R^2	ΔR^2	F	B	SE_B	β	t	95% C.I.
Trait mindfulness	0.74	0.55	0.53	0.54	54.68	-0.13	0.02	-0.74	-7.4	[-0.16, -0.09]***
Observe	0.55	0.3	0.27	0.26	16.62	-0.03	0.01	-0.51	-4.08	[-0.05, -0.02]***
Describe	0.46	0.21	0.18	0.2	11.66	-0.03	0.01	-0.46	-3.42	[-0.04, 0.01]**
Act Aware	0.79	0.62	0.61	0.62	73.68	-0.05	0.01	-0.79	-8.58	[-0.06, -0.04]***
Non-Judge	0.39	0.15	0.12	0.04	2.35	-0.01	0.01	-0.21	-1.53	[-0.03, 0.004]
Non-React	0.63	0.4	0.37	0.39	29.28	-0.03	0.01	-0.63	-5.41	[-0.05, 0.02]***
Depression	0.46	0.23	0.19	0.11	6.31	0.02	0.01	0.33	2.51	[0.004, 0.04]*
Anxiety	0.66	0.44	0.42	0.03	2.21	0.01	0.01	0.17	1.49	[-0.003, 0.02]
Stress	0.55	0.3	0.27	2.03	17.18	0.04	0.01	0.52	4.14	[0.02. 0.06]***

Predictor: Longer practice vs. control

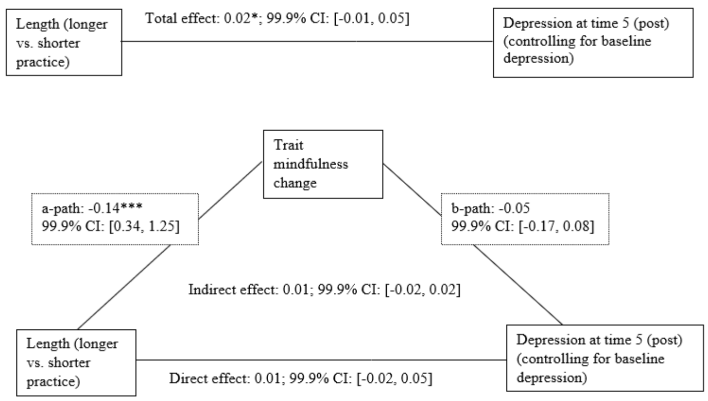
Outcome	R	R^2	Adj. R^2	ΔR^2	F	B	SE_B	β	t	95% C.I.
Trait mindfulness	0.7	0.49	0.47	0.38	33.19	0.11	0.02	0.63	5.76	[0.07, 0.14]***
Observe	0.75	0.56	0.54	0.23	22.8	0.03	0.01	0.48	4.78	[0.02, 0.05]***
Describe	0.7	0.49	0.47	0.18	15.46	0.02	0.01	0.42	3.93	[0.01, 0.04]***
Act Aware	0.59	0.35	0.32	0.3	20.16	0.03	0.01	0.55	4.49	[0.02, 0.04]***
Non-Judge	0.44	0.19	0.15	0.04	2.13	0.02	0.01	0.2	1.46	[-0.01, 0.04]
Non-React	0.61	0.37	0.34	0.27	18.72	0.03	0.01	0.54	4.33	[0.02, 0.04]***

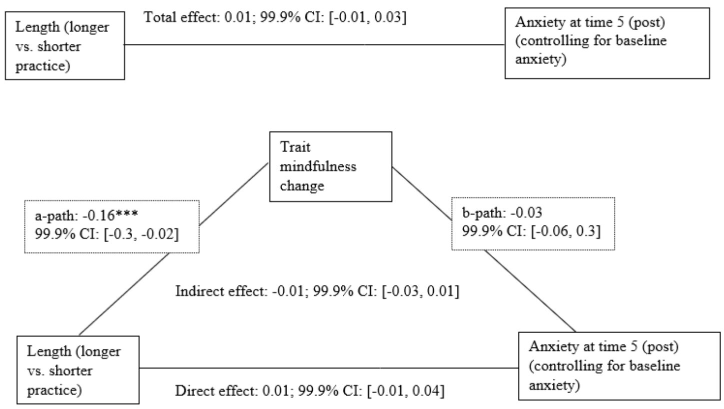
Depression	0.75	0.56	0.54	0.29	29.35	-0.06	0.01	-0.56	-5.42	[-0.08, -0.04]***
Anxiety	0.84	0.7	0.69	0.19	28.32	-0.05	0.01	-0.44	-5.32	[-0.07, -0.03]***
Stress	0.81	0.65	0.64	0.43	54.81	-0.08	0.01	-0.66	-7.4	[-0.11, -0.06]***
Predictor: Shorter practice vs. control										
Outcome	**R**	**R^2**	**Adj. R^2**	**ΔR^2**	**F**	**B**	**SE_B**	**β**	**t**	**95% C.I.**
Trait mindfulness	0.9	0.82	0.81	0.81	193.04	0.85	0.06	0.94	13.89	[0.72, 0.97]***
Observe	0.86	0.75	0.74	0.53	91.95	0.23	0.02	0.73	9.59	[0.18, 0.7]***
Describe	0.81	0.65	0.63	0.6	75.13	0.19	0.02	0.79	8.67	[0.14, 0.23]***
Act Aware	0.91	0.82	0.81	0.73	179.41	0.24	0.02	0.86	13.39	[0.21, 0.28]***
Non-Judge	0.33	0.11	0.07	0.09	4.2	0.11	0.05	0.31	2.05	[0.002, .022]*
Non-React	0.85	0.72	0.71	0.67	106.64	0.23	0.02	0.85	10.33	[0.18, 0.27]***
Depression	0.76	0.57	0.55	0.45	45.98	-0.28	0.04	-0.7	-6.78	[-0.36, -0.2]***
Anxiety	0.9	0.82	0.81	0.81	193.04	-0.2	0.04	-0.41	-5.67	[-0.27, -0.13]***
Stress	0.86	0.75	0.74	0.53	91.95	-0.46	0.05	-0.78	-9.56	[-0.55, -0.36]***

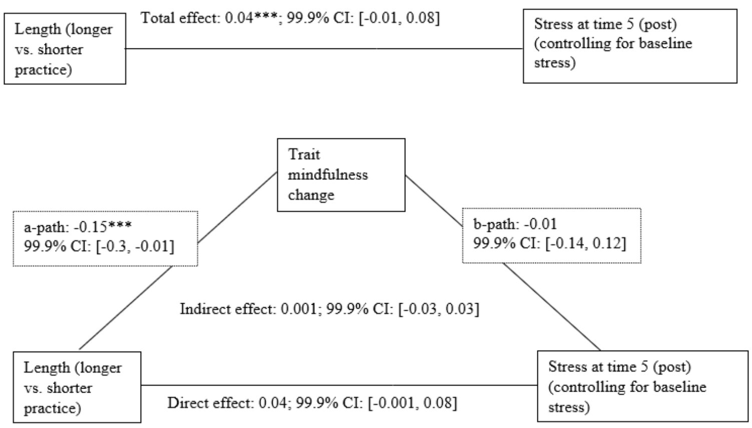
Trait mindfulness, observe, describe, act aware, non-judge, non-react measured with Five Facet Mindfulness Questionnaire (FFMQ-15); depression, anxiety, stress measured with Depression Anxiety and Stress Scale (DASS-21); R=correlation coefficient between predictors & outcome; R^2=amount of variance accounted for by predictors; Adj. R^2=adjusted R^2: generalisability of model; ΔR^2=adjusted R^2 change: improvement of R^2 when length is added as predictor after controlling for baseline levels of outcomes; F=model fit of coefficient ratio of improvement; *p*=significance of ΔR^2; B=coefficient of contribution of length predictor to model showing direction and size of effect; SE_B=Standard Error of coefficient; β= standardised beta coefficient showing standard deviation change of outcome by predictor; t=t-statistic on difference of B to 0; 95% C. I.=95% Confidence Interval; ***p<.001; **p<.01; *p<.05.

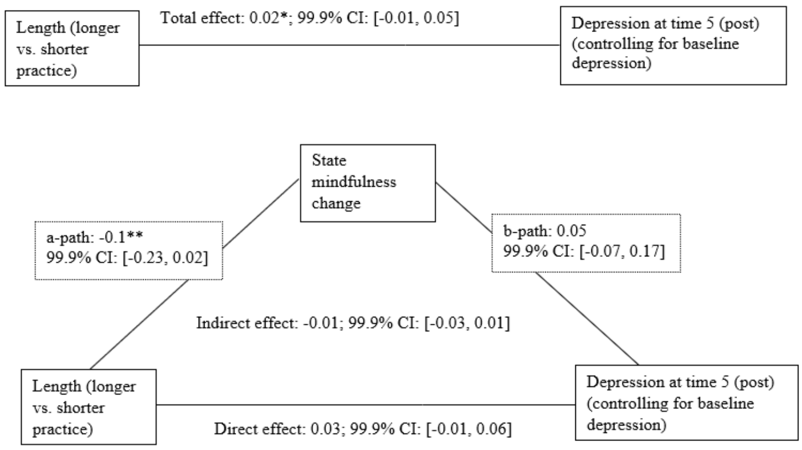
Appendix Figure 6.4.8

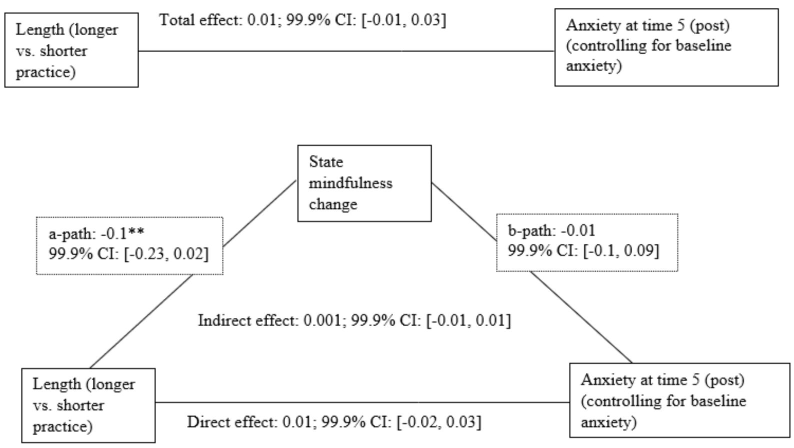
*Corrected mediation models for post-study (Time 5) outcome (depression; anxiety; stress) with length (longer vs. shorter practice; longer practice vs. control; shorter practice vs. control) as predictor, mindfulness change (trait mindfulness; state mindfulness; curiosity; decentering) as mediator and baseline outcome (depression; anxiety; stress) as covariate. Top diagram: total effect when excluding mediator, bottom diagram: indirect and direct effects when including mediator (*p<.05; **p<.01; ***p<.001; 99.9% CI=99.9% Confidence Intervals)*

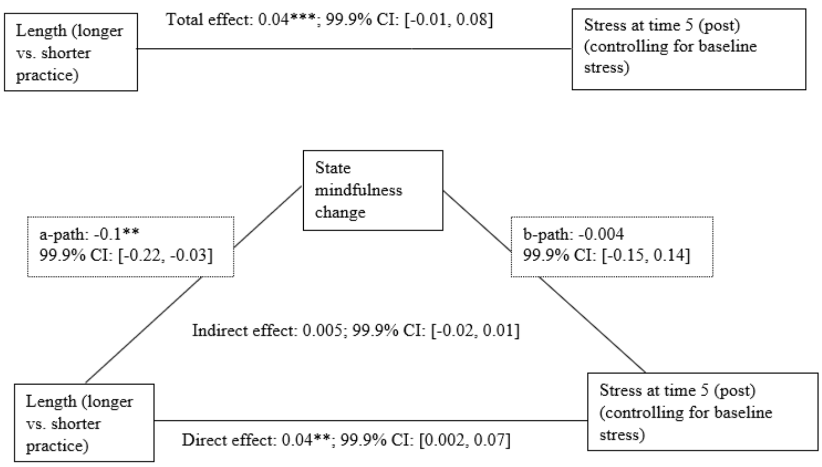


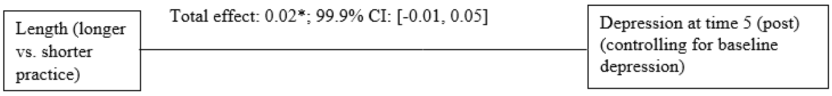


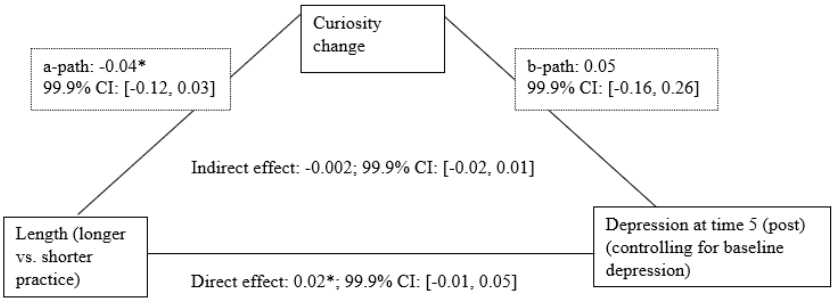


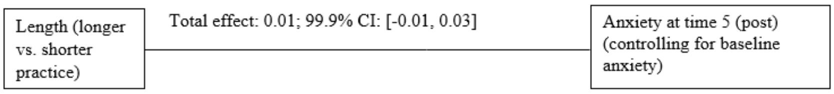

Length (longer vs. shorter practice)
Total effect: 0.02*; 99.9% CI: [-0.01, 0.05]
Depression at time 5 (post) (controlling for baseline depression)
State mindfulness change
a-path: -0.1**
99.9% CI: [-0.23, 0.02]
b-path: 0.05
99.9% CI: [-0.07, 0.17]
Indirect effect: -0.01; 99.9% CI: [-0.03, 0.01]
Length (longer vs. shorter practice)
Depression at time 5 (post) (controlling for baseline depression)
Direct effect: 0.03; 99.9% CI: [-0.01, 0.06]

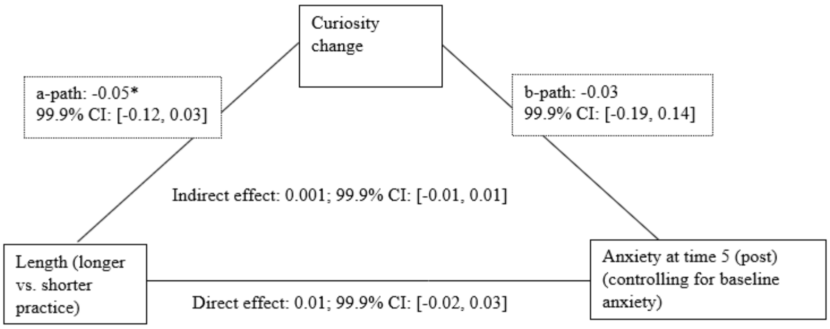

Length (longer vs. shorter practice)
Total effect: 0.01; 99.9% CI: [-0.01, 0.03]
Anxiety at time 5 (post) (controlling for baseline anxiety)
State mindfulness change
a-path: -0.1**
99.9% CI: [-0.23, 0.02]
b-path: -0.01
99.9% CI: [-0.1, 0.09]
Indirect effect: 0.001; 99.9% CI: [-0.01, 0.01]
Length (longer vs. shorter practice)
Anxiety at time 5 (post) (controlling for baseline anxiety)
Direct effect: 0.01; 99.9% CI: [-0.02, 0.03]

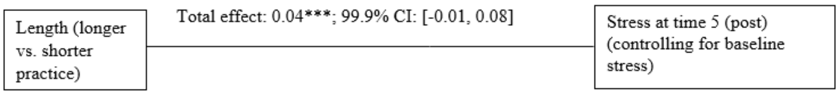

Length (longer vs. shorter practice)
Total effect: 0.04***; 99.9% CI: [-0.01, 0.08]
Stress at time 5 (post) (controlling for baseline stress)
State mindfulness change
a-path: -0.1**
99.9% CI: [-0.22, -0.03]
b-path: -0.004
99.9% CI: [-0.15, 0.14]
Indirect effect: 0.005; 99.9% CI: [-0.02, 0.01]
Length (longer vs. shorter practice)
Stress at time 5 (post) (controlling for baseline stress)
Direct effect: 0.04**; 99.9% CI: [0.002, 0.07]

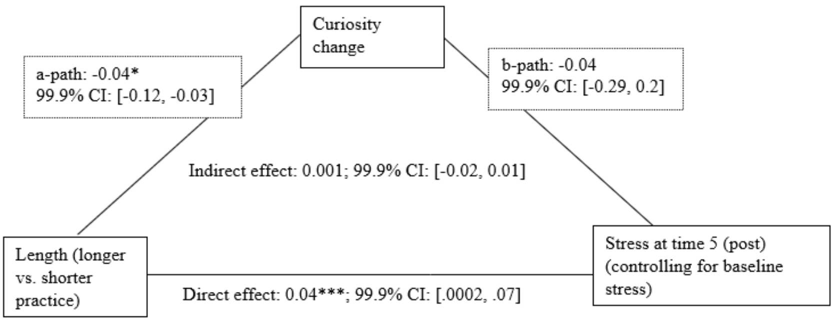

Length (longer vs. shorter practice)
Total effect: 0.02*; 99.9% CI: [-0.01, 0.05]
Depression at time 5 (post) (controlling for baseline depression)

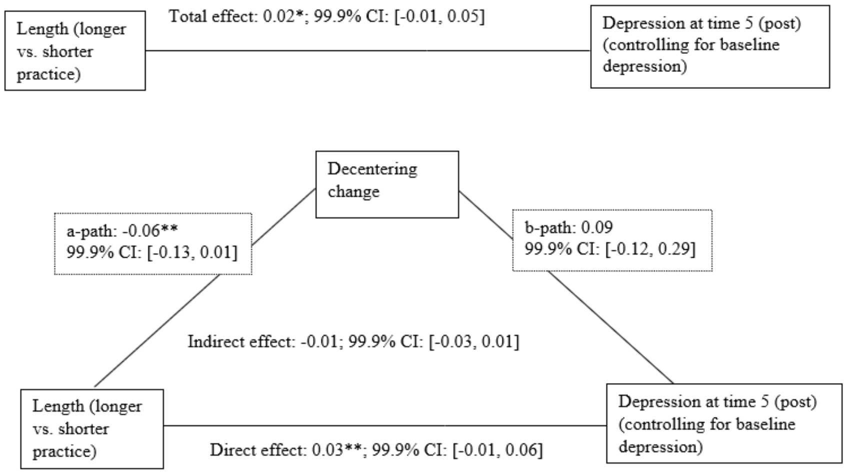

Curiosity change
a-path: -0.04*
99.9% CI: [-0.12, 0.03]
b-path: 0.05
99.9% CI: [-0.16, 0.26]
Indirect effect: -0.002; 99.9% CI: [-0.02, 0.01]
Length (longer vs. shorter practice)
Depression at time 5 (post) (controlling for baseline depression)
Direct effect: 0.02*; 99.9% CI: [-0.01, 0.05]

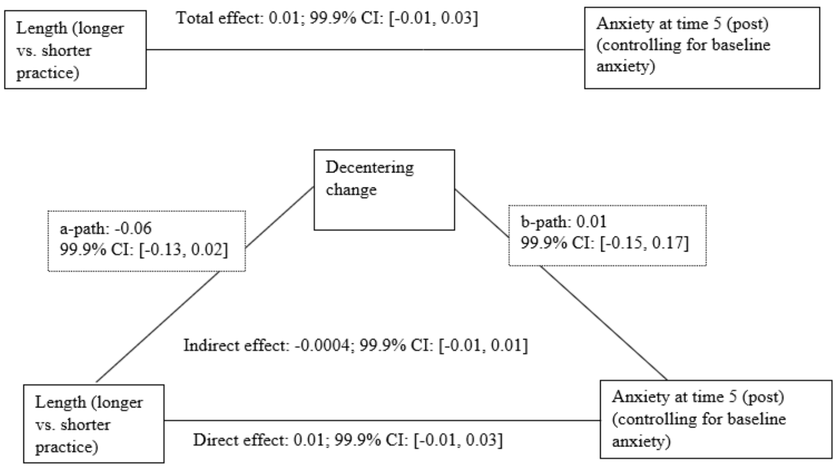

Length (longer vs. shorter practice)
Total effect: 0.01; 99.9% CI: [-0.01, 0.03]
Anxiety at time 5 (post) (controlling for baseline anxiety)

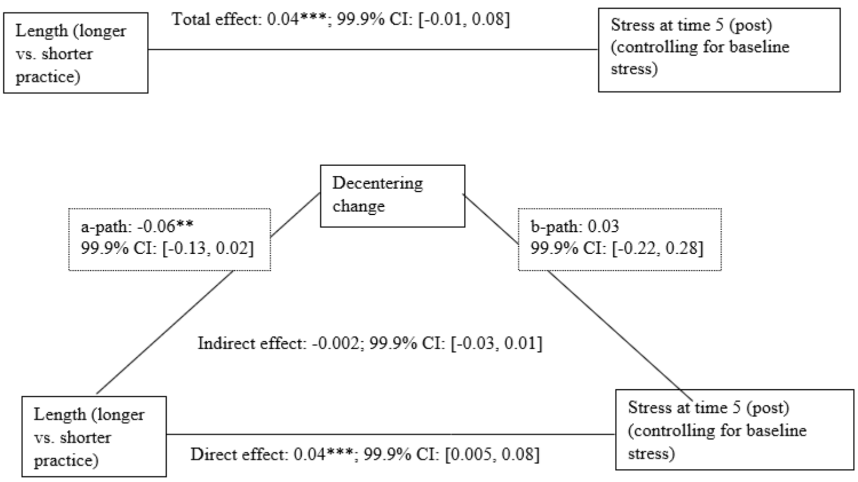

Curiosity change
a-path: -0.05*
99.9% CI: [-0.12, 0.03]
b-path: -0.03
99.9% CI: [-0.19, 0.14]
Indirect effect: 0.001; 99.9% CI: [-0.01, 0.01]
Length (longer vs. shorter practice)
Anxiety at time 5 (post) (controlling for baseline anxiety)
Direct effect: 0.01; 99.9% CI: [-0.02, 0.03]

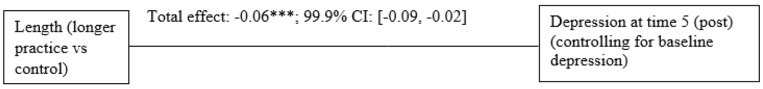

Length (longer vs. shorter practice)
Total effect: 0.04***; 99.9% CI: [-0.01, 0.08]
Stress at time 5 (post) (controlling for baseline stress)

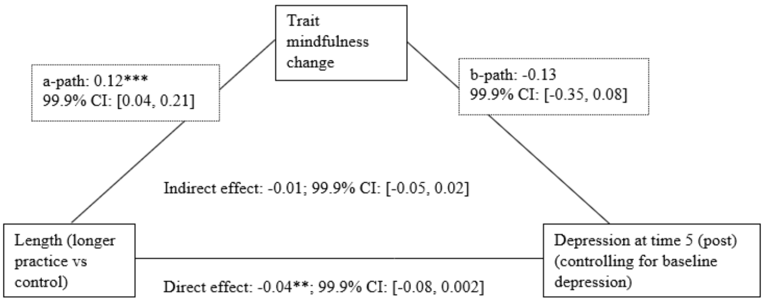

Curiosity change
a-path: -0.04*
99.9% CI: [-0.12, -0.03]
b-path: -0.04
99.9% CI: [-0.29, 0.2]
Indirect effect: 0.001; 99.9% CI: [-0.02, 0.01]
Length (longer vs. shorter practice)
Stress at time 5 (post) (controlling for baseline stress)
Direct effect: 0.04***; 99.9% CI: [.0002, .07]

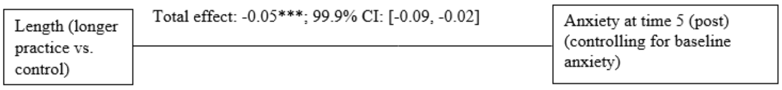

Length (longer vs. shorter practice)
Total effect: 0.02*; 99.9% CI: [-0.01, 0.05]
Depression at time 5 (post) (controlling for baseline depression)
Decentering change
a-path: -0.06**
99.9% CI: [-0.13, 0.01]
b-path: 0.09
99.9% CI: [-0.12, 0.29]
Indirect effect: -0.01; 99.9% CI: [-0.03, 0.01]
Length (longer vs. shorter practice)
Depression at time 5 (post) (controlling for baseline depression)
Direct effect: 0.03**; 99.9% CI: [-0.01, 0.06]

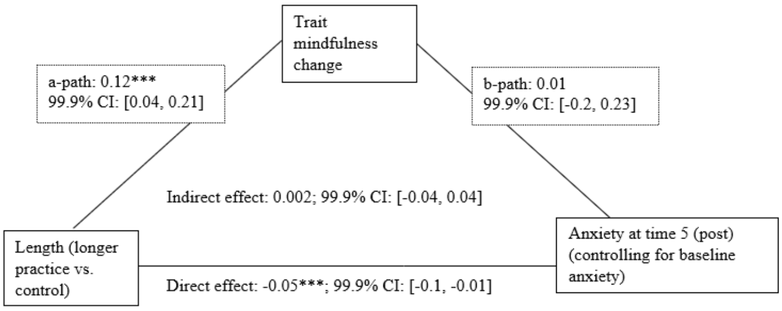

Length (longer vs. shorter practice)
Total effect: 0.01; 99.9% CI: [-0.01, 0.03]
Anxiety at time 5 (post) (controlling for baseline anxiety)
Decentering change
a-path: -0.06
99.9% CI: [-0.13, 0.02]
b-path: 0.01
99.9% CI: [-0.15, 0.17]
Indirect effect: -0.0004; 99.9% CI: [-0.01, 0.01]
Length (longer vs. shorter practice)
Anxiety at time 5 (post) (controlling for baseline anxiety)
Direct effect: 0.01; 99.9% CI: [-0.01, 0.03]

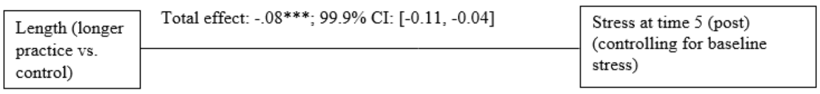

Length (longer vs. shorter practice)
Total effect: 0.04***; 99.9% CI: [-0.01, 0.08]
Stress at time 5 (post) (controlling for baseline stress)
Decentering change
a-path: -0.06**
99.9% CI: [-0.13, 0.02]
b-path: 0.03
99.9% CI: [-0.22, 0.28]
Indirect effect: -0.002; 99.9% CI: [-0.03, 0.01]
Length (longer vs. shorter practice)
Stress at time 5 (post) (controlling for baseline stress)
Direct effect: 0.04***; 99.9% CI: [0.005, 0.08]

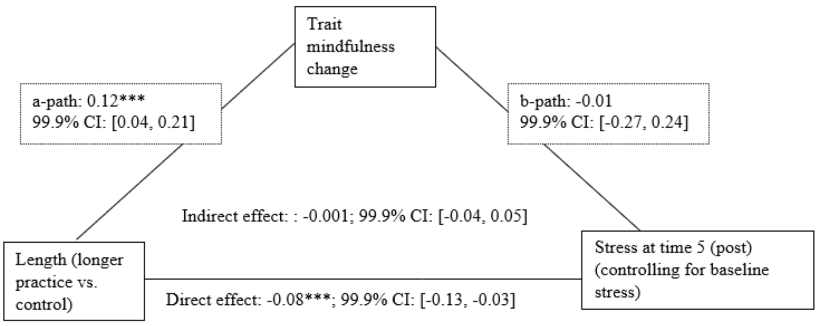

Length (longer practice vs control)
Total effect: -0.06***; 99.9% CI: [-0.09, -0.02]
Depression at time 5 (post) (controlling for baseline depression)

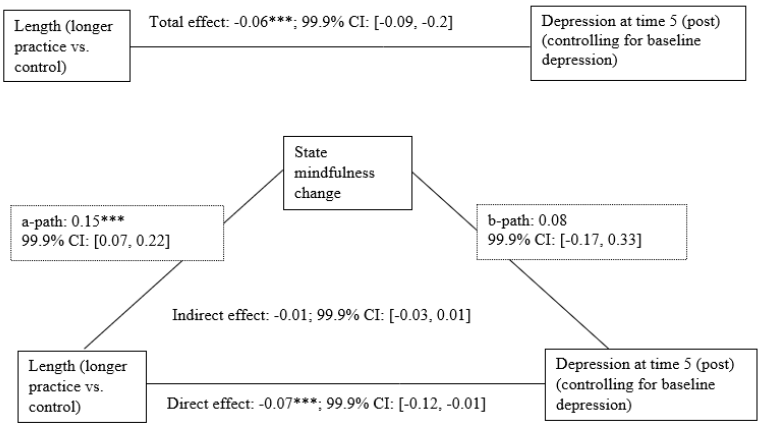

Trait mindfulness change
a-path: 0.12***
99.9% CI: [0.04, 0.21]
b-path: -0.13
99.9% CI: [-0.35, 0.08]
Indirect effect: -0.01; 99.9% CI: [-0.05, 0.02]
Length (longer practice vs control)
Depression at time 5 (post) (controlling for baseline depression)
Direct effect: -0.04**; 99.9% CI: [-0.08, 0.002]

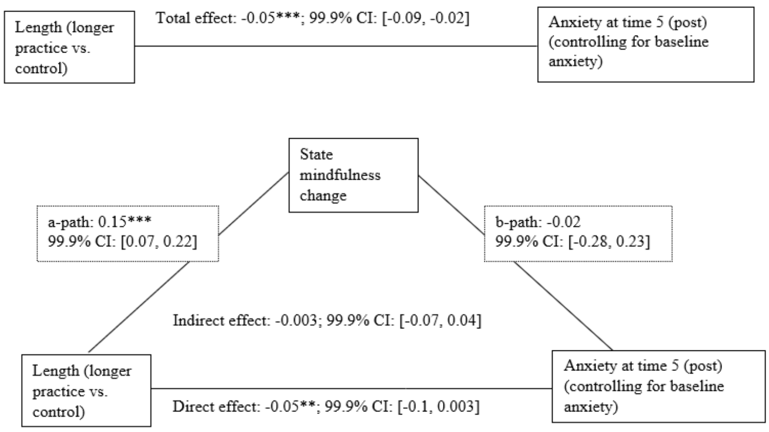

Length (longer practice vs. control)
Total effect: -0.05***; 99.9% CI: [-0.09, -0.02]
Anxiety at time 5 (post) (controlling for baseline anxiety)

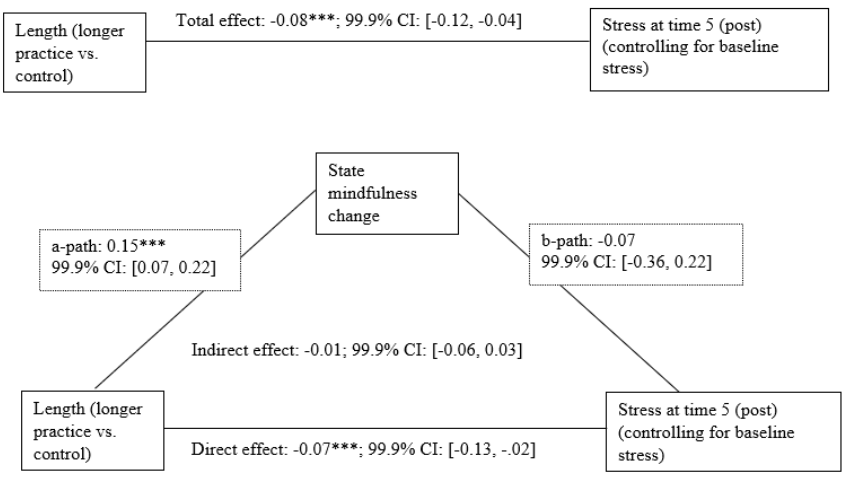

Trait mindfulness change
a-path: 0.12***
99.9% CI: [0.04, 0.21]
b-path: 0.01
99.9% CI: [-0.2, 0.23]
Indirect effect: 0.002; 99.9% CI: [-0.04, 0.04]
Length (longer practice vs. control)
Anxiety at time 5 (post) (controlling for baseline anxiety)
Direct effect: -0.05***; 99.9% CI: [-0.1, -0.01]

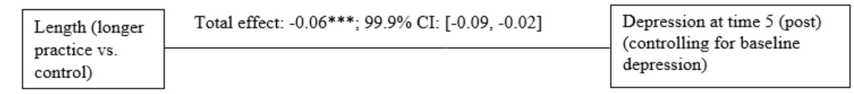

Length (longer practice vs. control)
Total effect: -.08***; 99.9% CI: [-0.11, -0.04]
Stress at time 5 (post) (controlling for baseline stress)

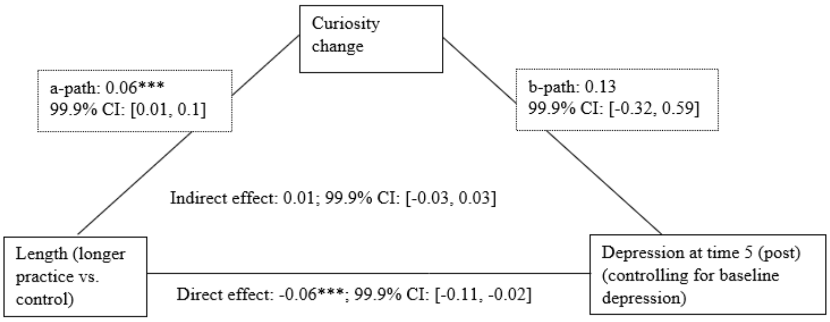

Trait mindfulness change
a-path: 0.12***
99.9% CI: [0.04, 0.21]
b-path: -0.01
99.9% CI: [-0.27, 0.24]
Indirect effect: : -0.001; 99.9% CI: [-0.04, 0.05]
Length (longer practice vs. control)
Stress at time 5 (post) (controlling for baseline stress)
Direct effect: -0.08***; 99.9% CI: [-0.13, -0.03]

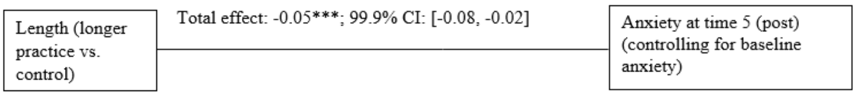

Length (longer practice vs. control)
Total effect: -0.06***; 99.9% CI: [-0.09, -0.2]
Depression at time 5 (post) (controlling for baseline depression)
State mindfulness change
a-path: 0.15***
99.9% CI: [0.07, 0.22]
b-path: 0.08
99.9% CI: [-0.17, 0.33]
Indirect effect: -0.01; 99.9% CI: [-0.03, 0.01]
Length (longer practice vs. control)
Depression at time 5 (post) (controlling for baseline depression)
Direct effect: -0.07***; 99.9% CI: [-0.12, -0.01]

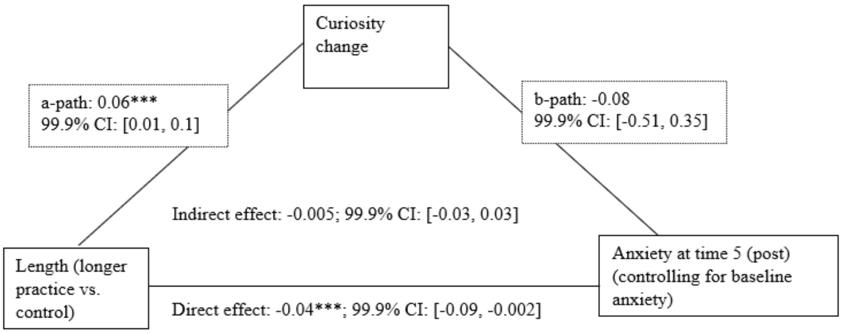

Length (longer practice vs. control)
Total effect: -0.05***; 99.9% CI: [-0.09, -0.02]
Anxiety at time 5 (post) (controlling for baseline anxiety)
State mindfulness change
a-path: 0.15***
99.9% CI: [0.07, 0.22]
b-path: -0.02
99.9% CI: [-0.28, 0.23]
Indirect effect: -0.003; 99.9% CI: [-0.07, 0.04]
Length (longer practice vs. control)
Anxiety at time 5 (post) (controlling for baseline anxiety)
Direct effect: -0.05**; 99.9% CI: [-0.1, 0.003]

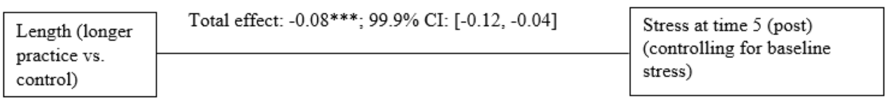

Length (longer practice vs. control)
Total effect: -0.08***; 99.9% CI: [-0.12, -0.04]
Stress at time 5 (post) (controlling for baseline stress)
State mindfulness change
a-path: 0.15***
99.9% CI: [0.07, 0.22]
b-path: -0.07
99.9% CI: [-0.36, 0.22]
Indirect effect: -0.01; 99.9% CI: [-0.06, 0.03]
Length (longer practice vs. control)
Stress at time 5 (post) (controlling for baseline stress)
Direct effect: -0.07***; 99.9% CI: [-0.13, -.02]

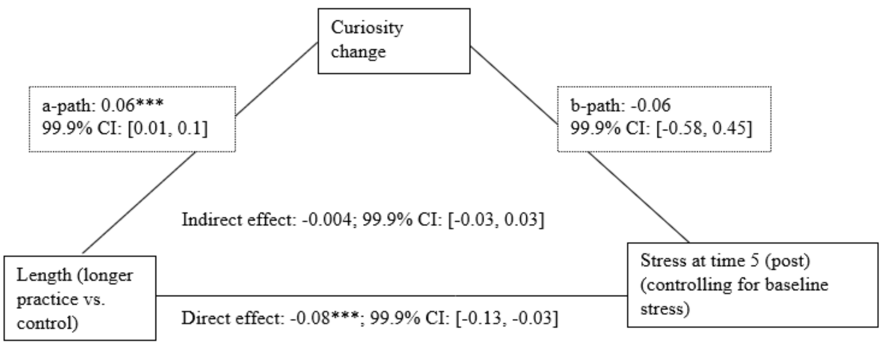

Length (longer practice vs. control)
Total effect: -0.06***; 99.9% CI: [-0.09, -0.02]
Depression at time 5 (post) (controlling for baseline depression)

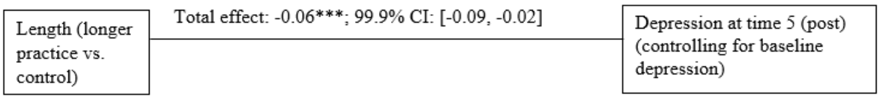

Curiosity change
a-path: 0.06***
99.9% CI: [0.01, 0.1]
b-path: 0.13
99.9% CI: [-0.32, 0.59]
Indirect effect: 0.01; 99.9% CI: [-0.03, 0.03]
Length (longer practice vs. control)
Depression at time 5 (post) (controlling for baseline depression)
Direct effect: -0.06***; 99.9% CI: [-0.11, -0.02]

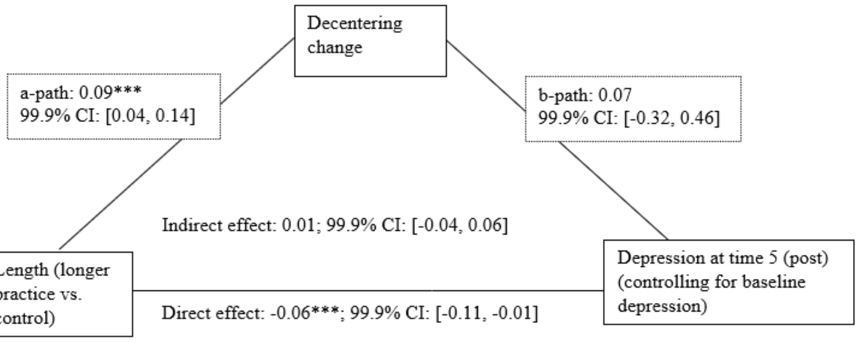

Length (longer practice vs. control)
Total effect: -0.05***; 99.9% CI: [-0.08, -0.02]
Anxiety at time 5 (post) (controlling for baseline anxiety)

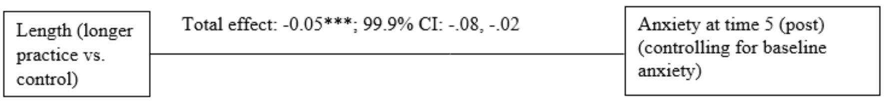

Curiosity change
a-path: 0.06***
99.9% CI: [0.01, 0.1]
b-path: -0.08
99.9% CI: [-0.51, 0.35]
Indirect effect: -0.005; 99.9% CI: [-0.03, 0.03]
Length (longer practice vs. control)
Anxiety at time 5 (post) (controlling for baseline anxiety)
Direct effect: -0.04***; 99.9% CI: [-0.09, -0.002]

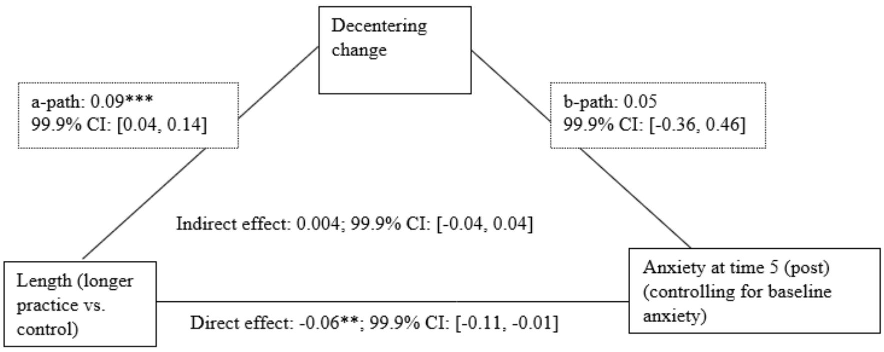

Length (longer practice vs. control)
Total effect: -0.08***; 99.9% CI: [-0.12, -0.04]
Stress at time 5 (post) (controlling for baseline stress)

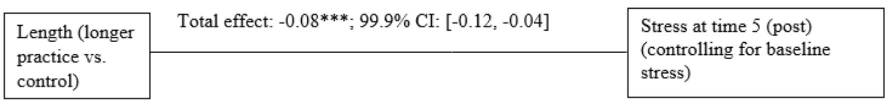

Curiosity change
a-path: 0.06***
99.9% CI: [0.01, 0.1]
b-path: -0.06
99.9% CI: [-0.58, 0.45]
Indirect effect: -0.004; 99.9% CI: [-0.03, 0.03]
Length (longer practice vs. control)
Stress at time 5 (post) (controlling for baseline stress)
Direct effect: -0.08***; 99.9% CI: [-0.13, -0.03]

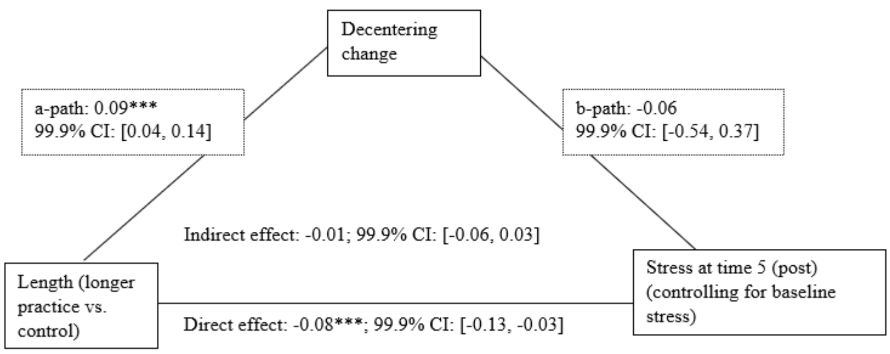

Length (longer practice vs. control)
Total effect: -0.06***; 99.9% CI: [-0.09, -0.02]
Depression at time 5 (post) (controlling for baseline depression)

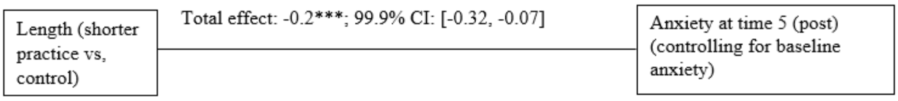

Decentering change
a-path: 0.09***
99.9% CI: [0.04, 0.14]
b-path: 0.07
99.9% CI: [-0.32, 0.46]
Indirect effect: 0.01; 99.9% CI: [-0.04, 0.06]
Length (longer practice vs. control)
Depression at time 5 (post) (controlling for baseline depression)
Direct effect: -0.06***; 99.9% CI: [-0.11, -0.01]

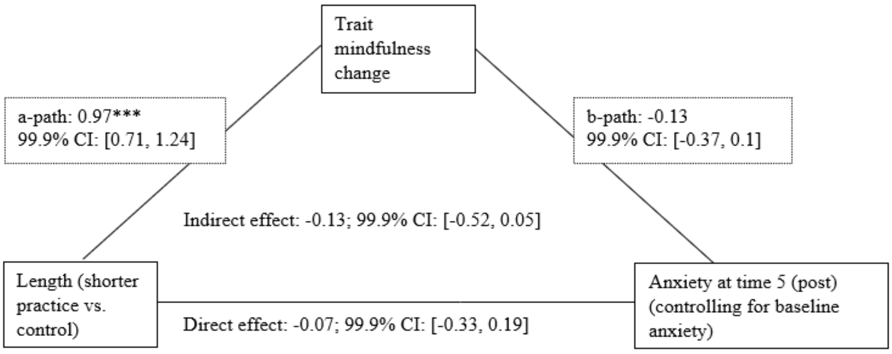

Length (longer practice vs. control)
Total effect: -0.05***; 99.9% CI: -.08, -.02
Anxiety at time 5 (post) (controlling for baseline anxiety)

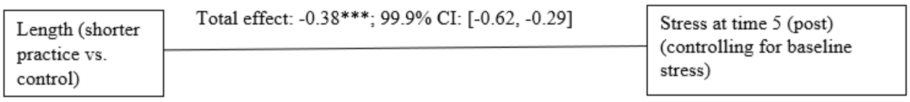

Decentering change
a-path: 0.09***
99.9% CI: [0.04, 0.14]
b-path: 0.05
99.9% CI: [-0.36, 0.46]
Indirect effect: 0.004; 99.9% CI: [-0.04, 0.04]
Length (longer practice vs. control)
Anxiety at time 5 (post) (controlling for baseline anxiety)
Direct effect: -0.06**; 99.9% CI: [-0.11, -0.01]

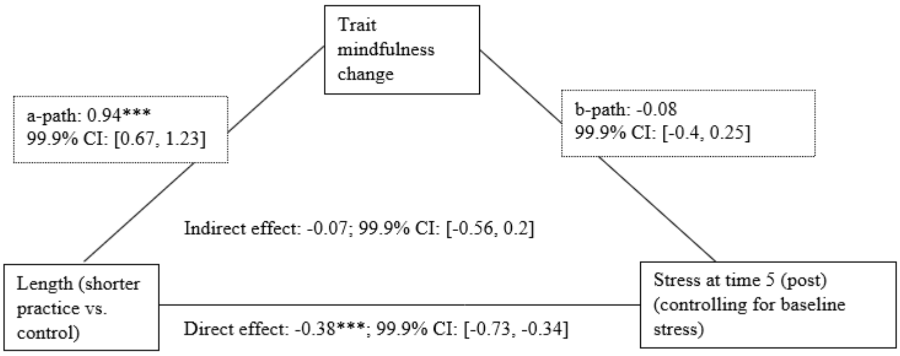

Length (longer practice vs. control)
Total effect: -0.08***; 99.9% CI: [-0.12, -0.04]
Stress at time 5 (post) (controlling for baseline stress)

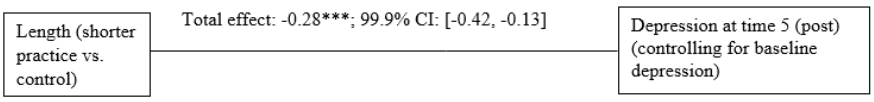

Decentering change
a-path: 0.09***
99.9% CI: [0.04, 0.14]
b-path: -0.06
99.9% CI: [-0.54, 0.37]
Indirect effect: -0.01; 99.9% CI: [-0.06, 0.03]
Length (longer practice vs. control)
Stress at time 5 (post) (controlling for baseline stress)
Direct effect: -0.08***; 99.9% CI: [-0.13, -0.03]

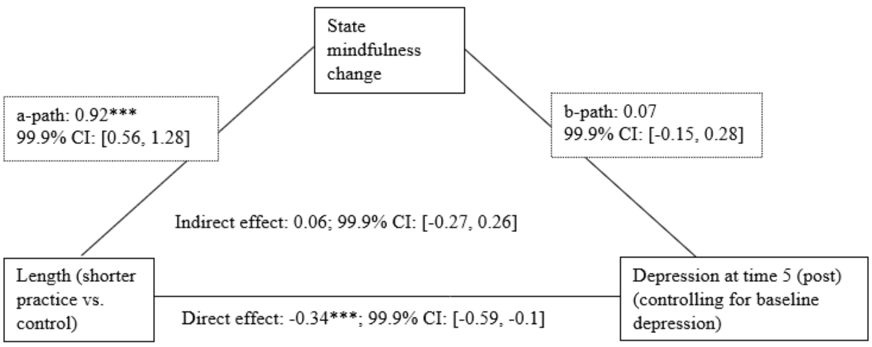

Length (shorter practice vs, control)
Total effect: -0.2***; 99.9% CI: [-0.32, -0.07]
Anxiety at time 5 (post) (controlling for baseline anxiety)

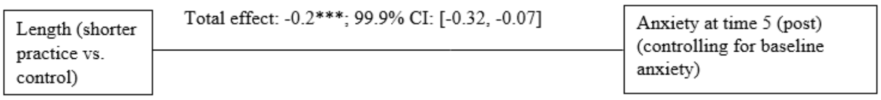

Trait mindfulness change
a-path: 0.97***
99.9% CI: [0.71, 1.24]
b-path: -0.13
99.9% CI: [-0.37, 0.1]
Indirect effect: -0.13; 99.9% CI: [-0.52, 0.05]
Length (shorter practice vs. control)
Anxiety at time 5 (post) (controlling for baseline anxiety)
Direct effect: -0.07; 99.9% CI: [-0.33, 0.19]

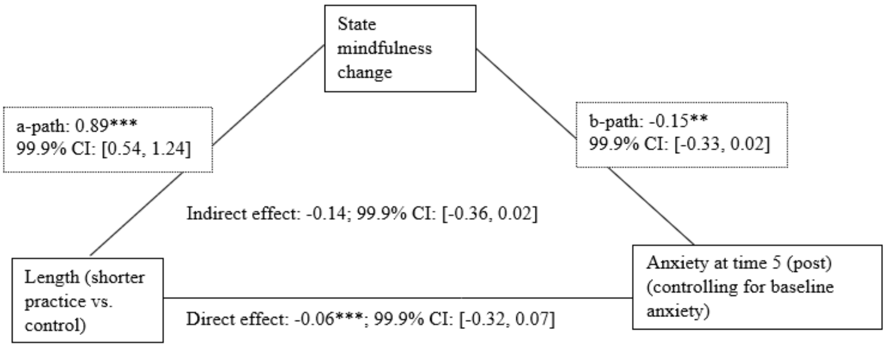

Length (shorter practice vs. control)
Total effect: -0.38***; 99.9% CI: [-0.62, -0.29]
Stress at time 5 (post) (controlling for baseline stress)

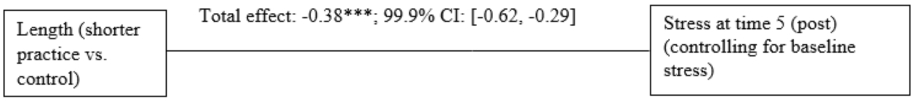

Trait mindfulness change
a-path: 0.94***
99.9% CI: [0.67, 1.23]
b-path: -0.08
99.9% CI: [-0.4, 0.25]
Indirect effect: -0.07; 99.9% CI: [-0.56, 0.2]
Length (shorter practice vs. control)
Stress at time 5 (post) (controlling for baseline stress)
Direct effect: -0.38***; 99.9% CI: [-0.73, -0.34]

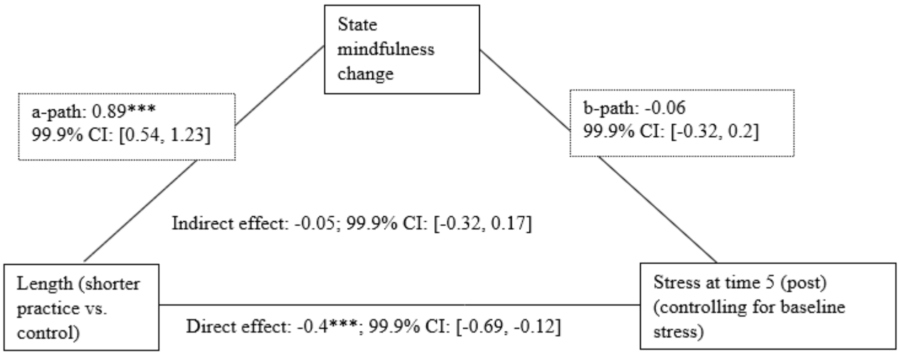

Length (shorter practice vs. control)
Total effect: -0.28***; 99.9% CI: [-0.42, -0.13]
Depression at time 5 (post) (controlling for baseline depression)

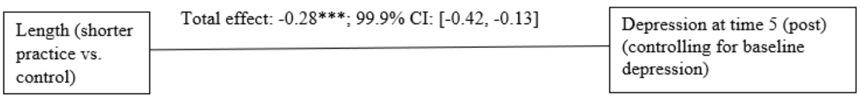

State mindfulness change
a-path: 0.92***
99.9% CI: [0.56, 1.28]
b-path: 0.07
99.9% CI: [-0.15, 0.28]
Indirect effect: 0.06; 99.9% CI: [-0.27, 0.26]
Length (shorter practice vs. control)
Depression at time 5 (post) (controlling for baseline depression)
Direct effect: -0.34***; 99.9% CI: [-0.59, -0.1]

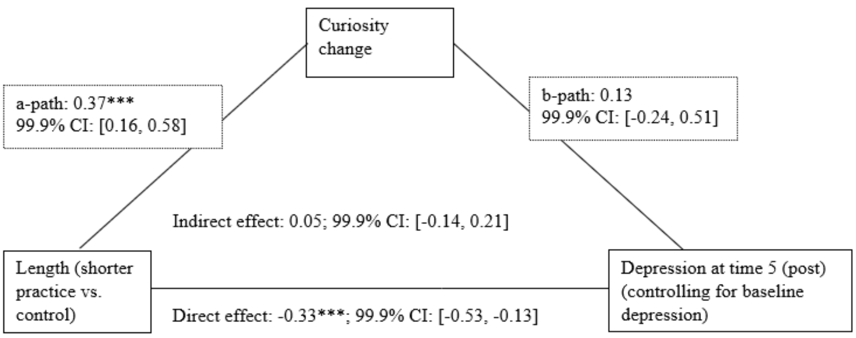

Length (shorter practice vs. control)
Total effect: -0.2***; 99.9% CI: [-0.32, -0.07]
Anxiety at time 5 (post) (controlling for baseline anxiety)

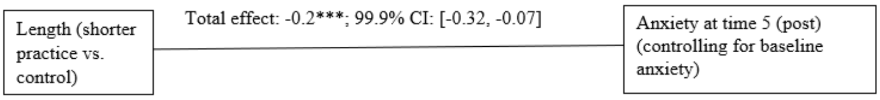

State mindfulness change
a-path: 0.89***
99.9% CI: [0.54, 1.24]
b-path: -0.15**
99.9% CI: [-0.33, 0.02]
Indirect effect: -0.14; 99.9% CI: [-0.36, 0.02]
Length (shorter practice vs. control)
Anxiety at time 5 (post) (controlling for baseline anxiety)
Direct effect: -0.06***; 99.9% CI: [-0.32, 0.07]

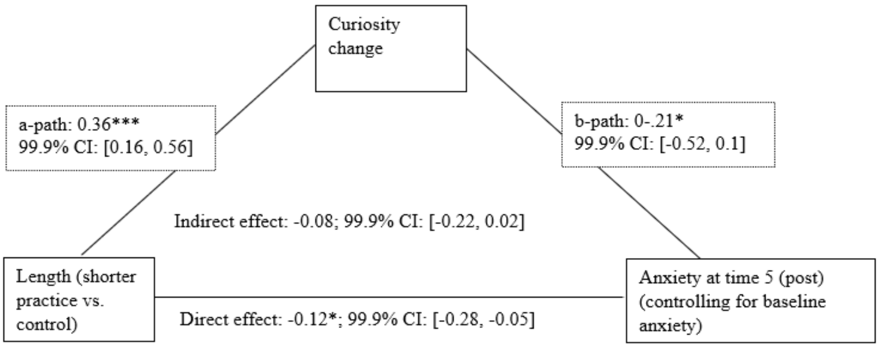

Length (shorter practice vs. control)
Total effect: -0.38***; 99.9% CI: [-0.62, -0.29]
Stress at time 5 (post) (controlling for baseline stress)

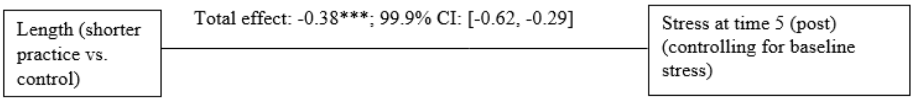

State mindfulness change
a-path: 0.89***
99.9% CI: [0.54, 1.23]
b-path: -0.06
99.9% CI: [-0.32, 0.2]
Indirect effect: -0.05; 99.9% CI: [-0.32, 0.17]
Length (shorter practice vs. control)
Stress at time 5 (post) (controlling for baseline stress)
Direct effect: -0.4***; 99.9% CI: [-0.69, -0.12]

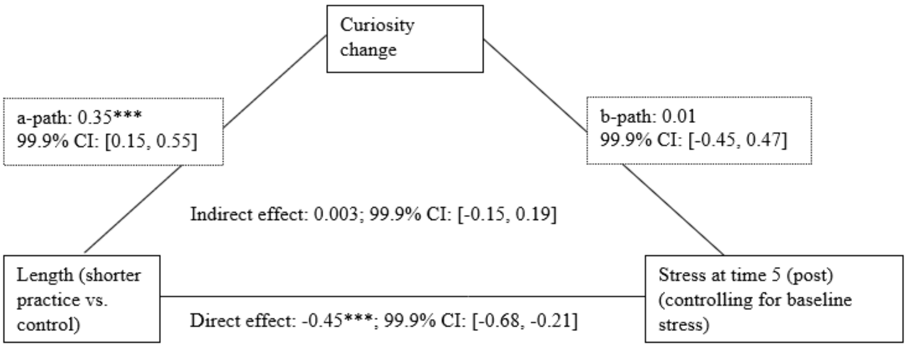

Length (shorter practice vs. control)
Total effect: -0.28***; 99.9% CI: [-0.42, -0.13]
Depression at time 5 (post) (controlling for baseline depression)

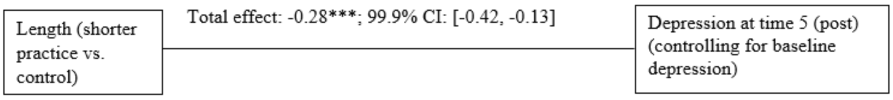

Curiosity change
a-path: 0.37***
99.9% CI: [0.16, 0.58]
b-path: 0.13
99.9% CI: [-0.24, 0.51]
Indirect effect: 0.05; 99.9% CI: [-0.14, 0.21]
Length (shorter practice vs. control)
Depression at time 5 (post) (controlling for baseline depression)
Direct effect: -0.33***; 99.9% CI: [-0.53, -0.13]

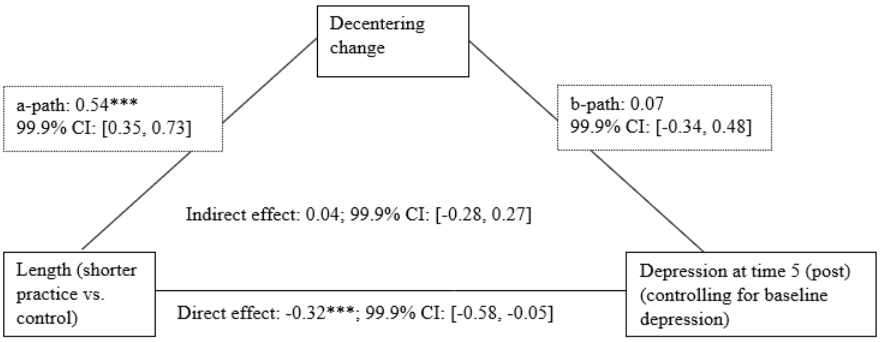

Length (shorter practice vs. control)
Total effect: -0.2***; 99.9% CI: [-0.32, -0.07]
Anxiety at time 5 (post) (controlling for baseline anxiety)

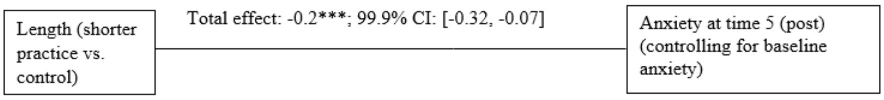

Curiosity change
a-path: 0.36***
99.9% CI: [0.16, 0.56]
b-path: 0-.21*
99.9% CI: [-0.52, 0.1]
Indirect effect: -0.08; 99.9% CI: [-0.22, 0.02]
Length (shorter practice vs. control)
Direct effect: -0.12*; 99.9% CI: [-0.28, -0.05]
Anxiety at time 5 (post) (controlling for baseline anxiety)

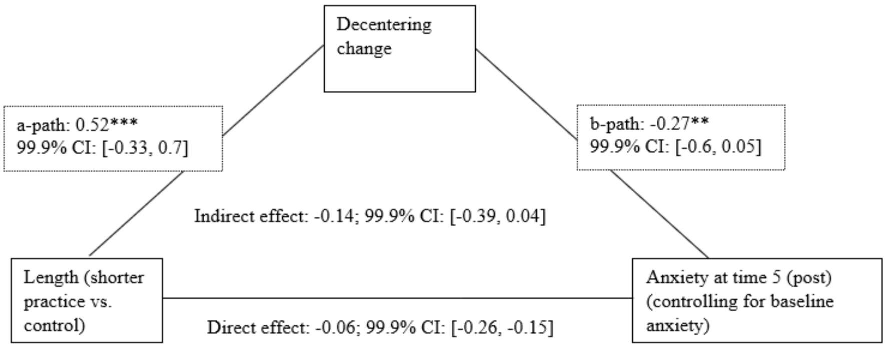

Length (shorter practice vs. control)
Total effect: -0.38***; 99.9% CI: [-0.62, -0.29]
Stress at time 5 (post) (controlling for baseline stress)

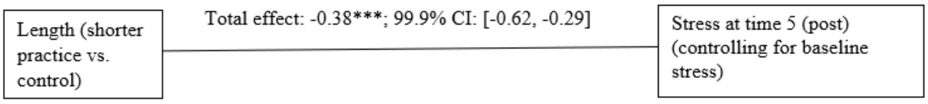

Curiosity change
a-path: 0.35***
99.9% CI: [0.15, 0.55]
b-path: 0.01
99.9% CI: [-0.45, 0.47]
Indirect effect: 0.003; 99.9% CI: [-0.15, 0.19]
Length (shorter practice vs. control)
Direct effect: -0.45***; 99.9% CI: [-0.68, -0.21]
Stress at time 5 (post) (controlling for baseline stress)

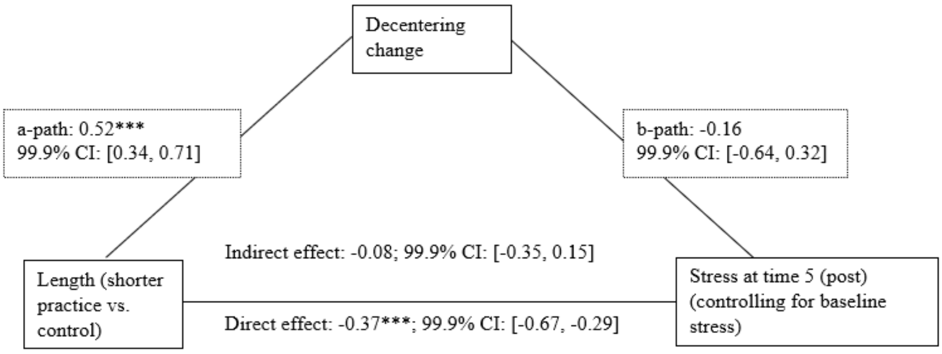

Length (shorter practice vs. control)
Total effect: -0.28***; 99.9% CI: [-0.42, -0.13]
Depression at time 5 (post) (controlling for baseline depression)


Decentering change
a-path: 0.54***
99.9% CI: [0.35, 0.73]
b-path: 0.07
99.9% CI: [-0.34, 0.48]
Indirect effect: 0.04; 99.9% CI: [-0.28, 0.27]
Length (shorter practice vs. control)
Direct effect: -0.32***; 99.9% CI: [-0.58, -0.05]
Depression at time 5 (post) (controlling for baseline depression)


Length (shorter practice vs. control)
Total effect: -0.2***; 99.9% CI: [-0.32, -0.07]
Anxiety at time 5 (post) (controlling for baseline anxiety)

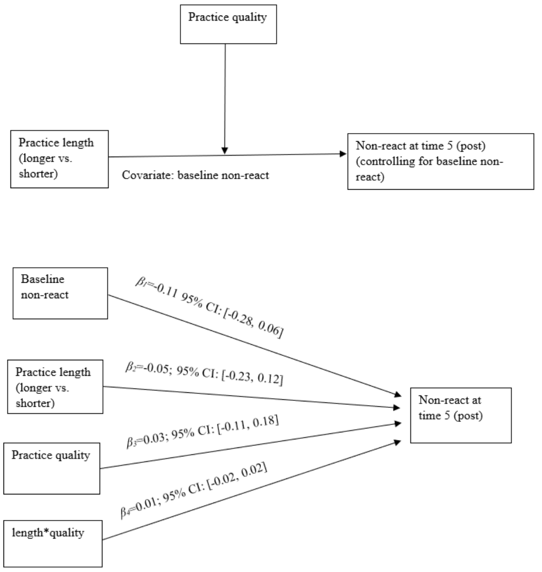

Decentering change
a-path: 0.52***
99.9% CI: [-0.33, 0.7]
b-path: -0.27**
99.9% CI: [-0.6, 0.05]
Indirect effect: -0.14; 99.9% CI: [-0.39, 0.04]
Length (shorter practice vs. control)
Anxiety at time 5 (post) (controlling for baseline anxiety)
Direct effect: -0.06; 99.9% CI: [-0.26, -0.15]


Length (shorter practice vs. control)
Total effect: -0.38***; 99.9% CI: [-0.62, -0.29]
Stress at time 5 (post) (controlling for baseline stress)

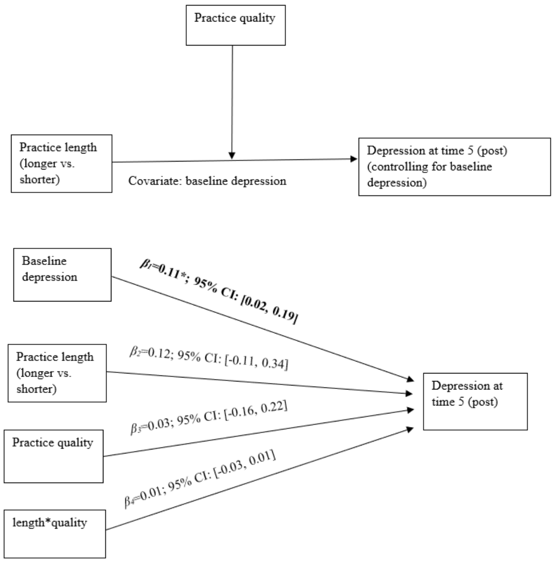

Decentering change
a-path: 0.52***
99.9% CI: [0.34, 0.71]
b-path: -0.16
99.9% CI: [-0.64, 0.32]
Indirect effect: -0.08; 99.9% CI: [-0.35, 0.15]
Length (shorter practice vs. control)
Stress at time 5 (post) (controlling for baseline stress)
Direct effect: -0.37***; 99.9% CI: [-0.67, -0.29]

Appendix Figure 6.4.9

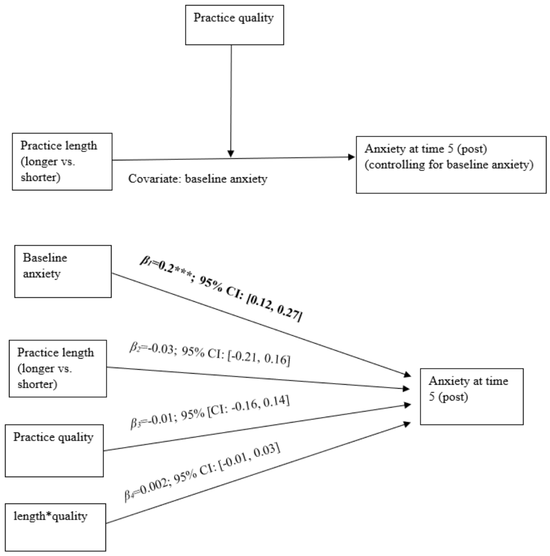
*Moderation models for post-study (Time 5) outcomes FFMQ-15 subscales observe, describe, act aware, non-judge, and non-react, depression, anxiety, and stress and moderator practice quality change (length*quality=interaction of practice length and practice quality; β_1 = coefficient of outcomes at baseline on post-study outcomes; β_2 = coefficient on the effect of practice length on post-study outcomes, β_3 = coefficient on the effect of practice quality on post-study outcomes, and β_4 = coefficient on the effect of the interaction of practice length and practice quality on post-study outcomes.*



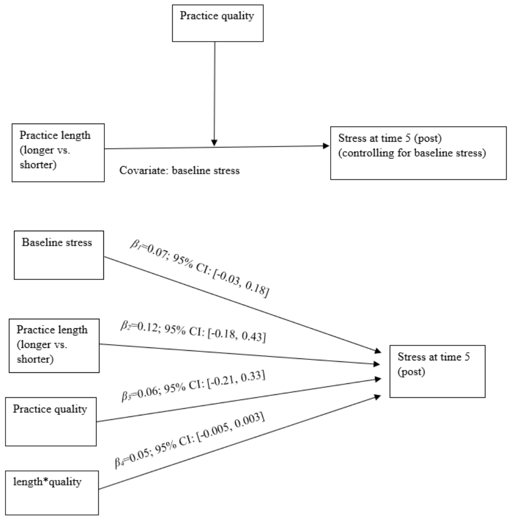
Practice quality
Practice length (longer vs. shorter)
Covariate: baseline non-react
Non-react at time 5 (post) (controlling for baseline non-react)
Baseline non-react
β1=-0.11 95% CI: [-0.28, 0.06]
Practice length (longer vs. shorter)
β2=-0.05; 95% CI: [-0.23, 0.12]
Practice quality
β3=0.03; 95% CI: [-0.11, 0.18]
length*quality
β4=0.01; 95% CI: [-0.02, 0.02]
Non-react at time 5 (post)



Practice quality
Practice length (longer vs. shorter)
Covariate: baseline depression
Depression at time 5 (post) (controlling for baseline depression)
Baseline depression
β1=0.11*; 95% CI: [0.02, 0.19]
Practice length (longer vs. shorter)
β2=0.12; 95% CI: [-0.11, 0.34]
Practice quality
β3=0.03; 95% CI: [-0.16, 0.22]
length*quality
β4=0.01; 95% CI: [-0.03, 0.01]
Depression at time 5 (post)



Practice quality
Practice length (longer vs. shorter)
Covariate: baseline anxiety
Anxiety at time 5 (post) (controlling for baseline anxiety)
Baseline anxiety
β1=0.2***; 95% CI: [0.12, 0.27]
Practice length (longer vs. shorter)
β2=-0.03; 95% CI: [-0.21, 0.16]
Practice quality
β3=-0.01; 95% [CI: -0.16, 0.14]
length*quality
β4=0.002; 95% CI: [-0.01, 0.03]
Anxiety at time 5 (post)



Practice quality
Practice length (longer vs. shorter)
Covariate: baseline stress
Stress at time 5 (post) (controlling for baseline stress)
Baseline stress
β1=0.07; 95% CI: [-0.03, 0.18]
Practice length (longer vs. shorter)
β2=0.12; 95% CI: [-0.18, 0.43]
Practice quality
β3=0.06; 95% CI: [-0.21, 0.33]
length*quality
β4=0.05; 95% CI: [-0.005, 0.003]
Stress at time 5 (post)

Appendix Table 6.4.10

Coding frame (including participant quotes) for content analysis by group

Group 1: longer mindfulness practice		
Category	**Code/subcategory**	**Quote**
Positive experiences of practice	Feeling calm during/after	"I do remember leaving sessions feeling calm" "I think I felt calmer after the sessions" "Definitely had a calming effect, particularly due to taking part during my working day" "On the days where I completed the mindfulness, I felt calmer and more able to think objectively about stressful things rather than allow them to take over." "I think I felt calmer afterwards" "Confirmed my belief that mindfulness has a calming effect on the body and mind" "I did find the voice quite calming […]"
	Learning something new (positive, helpful)	"[…] this is the first time I did this!" "I've never heard of mindfulness before, this is interesting" "It influenced my feelings in a good way, I found myself to be more in piece with who I am, and I have learnt how to approach my feelings, no matter how they are, in a non-judgemental way" "I do not often, if ever, simply sit still and be aware of my breathe" "[…]and learn a bit about mindfulness."
	Future plans for practicing mindfulness (incl. asking for materials)	"I did enjoy the sessions and felt this would be something I could do more off once the study was finished" "motivation to re-engage with mindfulness practice regularly" "I really enjoyed it and I'll have a look at the additional materials you will sent!"
	Welcome/enjoyable experience (incl. looking forward to it)	"mindfulness practice, thank you for giving me the opportunity to take part and experience this. It felt like a precious gift." "reminded me of the positive impact on my mood that day when I take part in mindful practice" "[…] and it was on the whole a pleasant experience." "a welcome and interesting bit of mindfulness training" "[…] I just enjoyed it." "[…] I just enjoyed it." "An enjoyable experience"

	Increased awareness (physical, emotional, openness, incl. lack of awareness) /decentering	"Perhaps it has reinforced to me that possibly I find it more relaxing not to have any specific focus sometimes." "I was more able to notice my thoughts and not respond to them" "It has made me more aware of the importance of taking a bit of time to stop and reflect." "a reminder of how beneficial it appears to be" "I am more aware of how to recognise thoughts I have and acknowledge them, without trying to suppress them. I feel more confident about centring myself when I feel my emotions are taking over." "connecting a bit more with my breath via formal mindfulness practices" "I think haven't realised just how busy and active my mind is and during the mindfulness" "It brought my attention to negative feelings and prompted me to step back from them." "I liked that we participated at various times throughout the day as it made me aware of how varied my mindset and concentration is at different points in the day."
	Relief from busy day/taking time to be mindful	"It gave me some time out from a busy day so short-term relief from my to do list" "The fact that there was an external reason to carry out the mindfulness practice - otherwise it can end up being something I mean to do but don't get around to!" "having something else to do" "Actually taking time out of a busy work schedule to try something new […]" "It's like jogging, I always feel better after, but it takes a lot of work during"
	Increased focus	
	Mindfulness helpful in daily life	
	Permission to focus on self	"I can remember the phrases and calls to action on the audio played during the sessions. It also reminded me of the value of just sitting alone with your thoughts without reaching for something to do"
	Positive comments on guided practice	
	Positive comments on practice length (esp. short practice benefits)	
	Mindfulness becomes easier with time	
Difficulty of practice	Practices too long	"[…] but I do remember that I found the mindfulness quite long sometimes" "the practices were quite long though" "I liked the practices but think they could have been shorter"

		“it was hard to do for so long”
		“but took a while to get used to sitting for long”
		“I feel that if it was shorter mindfulness over a longer period of time it might have been helpful.”
		“I think a shorter one would be better for me to learn mindfulness”
	Practice difficult/like work	“I'm not sure, it felt a bit like a lot of work sometimes”
		“the recordings had definitely a huge impact, the rest was “my job“.”
		“I have a rather strong work ethic - not easy to give self-permission to 'do nothing'.“
		“I sometimes found it quite difficult to stop my mind from wandering”
		“I was sometimes waiting for it to end”
	Dislike of practice/irritation	“retrospect it was interesting to note that I got slightly irritated with keeping going back to the breath. As if slightly constraining”
		“Noticing my slight feeling of irritation occasionally at constantly being reminded to come back to the breath and asked to be curious about it. Possibly my definition of curiosity is active enquiry rather than a passive open frame of mind, although I think to discover things you need both. “
		“Difficult to engage fully in the work situation, I think. Also, I don't like mindfulness of the breath!”
		“I feel like I was quite fidgety during the mindfulness”
	Uncertainty about practice and length	“not sure If I did it right? this is my first time”
		“Sometimes I wasn’t sure if the practice was over yet, and I opened my eyes and realised that it wasn’t”
	Doubt on whether longer practice would have been helpful	
No effect of study	No effect	“I don't think the study has had any effect on me”
		“I don't think it had a massively positive effect on me [....]”
		“Honestly, I'm not sure that it has. I think the study has come at quite a stressful time for me.”
		“none” (7)
		“No effect”
		“None that I'm aware of.”
		“I'm not aware of an obvious effect.”
		“I don't think it had much effect on me”
	Study during stressful time/life circumstances	
	Not feeling mindful from study participation	

Positive comments about study (general)	Interesting/enjoyable audiobook	“I really enjoyed the 5 minutes recordings in the beginning of the sessions too, as I truly like the topics about the universe. Very interesting! Thank you !!!” “I thought to start with that I wasn't going to be very interested in the audio clip but actually found it fascinating. “ “I really enjoyed the audio book that we listened to at the start of the session.” “The book was very interesting”
	Audiobook in relation to practice	
	Positive comments about researcher	“The study was conducted very professionally, and the researcher was lovely. “ “[…]and well run by Sarah”
	Gratitude (incl. invitation and opportunity to take part)	“Thank you for inviting me to take part.” “thank you!” “Thank you for the opportunity.”
	Generic comment about study/study manageable	“It was interesting”
	Interest in results	
Negative comments about study (general)	Confusing questionnaires	“I found the questionnaire questions sometimes a little confusing - eg when you have distressing thoughts are you able to let go of them (or words similar). Well I don’t often have the thoughts so should I answer rarely or frequently because when I do, I can get rid of them quickly !”
	Boring audiobook	
	Study mysterious	“Is there a way I could decide beforehand how long the practices are?”

Group 2: shorter mindfulness practice

Category	Code/subcategory	Quote
Positive experiences of practice	Feeling calm during/after	“I felt calmer, less stressed and able to tell myself "everything is okay"” “I really liked it, thank you! I felt very calm every time” “It made me feel calm and relaxed afterwards” “thank you, mindfulness made me feel calm” “I find my job very stressful at the moment. I felt calmer and better equipped to deal with stress after the exercises.” “[…] I felt more relaxed” “Relaxing effect […]” “[…] felt more relaxed my days felt more manageable” “Doing the mindfulness practice in the study put my mind in a positive mindset to the rest of the day.”

		"felt more calm, "mostly it made me feel calm" "It was a relaxing experience" felt like I have done something good for me"
	Learning something new (positive, helpful)	"Made me think about if mindfulness might be useful to me." "[…] (something new to me)" "Realised I didn't really know how to do it before i took part in this […]" "it definitely had an effect! I've always wanted to try mindfulness but thought I'd have to spend a lot of time sitting which seemed daunting" "when I'm feeling negative or scared, I now go to my breathing which I never did before"
	Future plans for practicing mindfulness (incl. asking for materials)	"[…] made me wonder if it would be worth investigating further" "Mindfulness was something I knew nothing about but will use this now" "[…] I am planning on using this again" "Would it be possible to have a recording of the mindfulness?" "I would like to continue to practise mindfulness as I enjoyed the feeling of deep focus and would be interested to see if this has any effects in the longer term" "Could I have the recording?" "I plan to continue with mindfulness exercises." "Could you send me more short mindfulness recordings?" "I'm looking to getting more involved in mindfulness practices, I think i can do 5 minutes more often"
	Welcome/joyful experience (incl. looking forward to it)	"after the audiobook, i always looked forward to the meditation"
	Increased awareness (physical, emotional, openness, incl. lack of awareness)	"Made me aware of mindfulness and how aware I was of what was going on inside myself." "More awareness of physical sensation without over analysing, just acceptance." "participation has made me think more about my day to day surroundings" "mindfulness made me feel […] more open to the world around me" "Made me more aware of my feelings" "recognising emotions and being more aware of how they make me feel" "Slightly improved awareness of some sensations e.g. water in the shower. An awareness of how often I am acting not in a mindful way." "More relaxed approach to understanding my body's reactions to thoughts." "I was open to feeling whatever it made me feel […]" "The meditation helped me observe the business of my mind." "[…] turn made me think about how I manage myself, my stresses and anxieties, and the importance of making the most of the time we have."

		"After listening to something so specific and then taking the time to concentrate on yourself and your breathing and the sensations in your body gives you the opportunity to reflect on yourself and for me feel positive." "I think the questionnaires I answered differently after the mindfulness" "Drawing my attention to my responses via the questionnaires […]"
	Relief of busy day/taking time to be mindful	"Able to take time away from the office and work and take a step back from life" "Committing to the time slot of the study forced me to take a break. […]"
	Increased focus	"better focus […]" as I enjoyed the feeling of deep focus and would be interested to see if this has any effects in the longer term"
	Mindfulness helpful (incl. in daily life)	"The mindfulness practice I felt like it really helped!" "I think the mindfulness practice has really helped […]" "the practice, even if it wasn't long, it's strange how much better I feel about my busy day!" felt like I have done something good for me" "The exercises helped me put things that stress me out into perspective." "been very helpful whilst writing my last essay, i have used the breathing technique"
	Permission to focus on self	"in a way, being told to concentrate on me!" "Maybe just a reminder that it's okay to 'just be' rather than constantly do" When i find things get a little stressful, i stop myself and allow time to concrete on me, my breathing and feelings i am having, like the mindfulness, even just briefly" "it was good to be able to have some time to spend on myself […]"
	Positive comments on guided practice	"I am a highly strung, do not relax kind of person who needed guidance on how to do this"
	Positive comments on practice length (esp. short practice benefits)	"I really liked the length of mindfulness." "I like the mindfulness practice, especially that it was short!" "It's so nice to be able to just sit and focus on the breath for a minute" "I really liked the short length of the mindfulness practice, i think it was a good length for me as someone who has never done this before." "It reminded me that I can forge out time in a busy day to make space for myself. It reminded me how much I enjoy and can manage a 5-minute meditation." "It was a nice break from the day to just be" "[…] taking a break/time out, trying to focus on something and realising the business of my mind"

		"I think that the 5-minute meditation felt manageable following a 20-minute section of audio. I would normally be doing something like driving when listening to an audiobook so being still helped show that 5 minutes is very manageable for a practice." "it felt like a manageable amount of time to do this" "[…] I think I can do 5 minutes more often" "the study has taught me the benefits of short mindfulness" "i really like that 5 minutes can be helpful!" "feeling more in control, being able to stop myself and relax even just for 5 mins makes all the difference" "I thought that doing mindfulness you need to spend a lot of time doing it but I feel better with only 5 minutes" "I liked it a lot! who knew focusing on the breath for 5 minutes helped so much!" "[…] but I've learned that 5 minutes can help!"
	Mindfulness becomes easier with time	"I found the subsequent sessions easier than the first one." "[…] Also repeatedly doing the mindfulness of the breath practice means I struggled less with it each time. […]"
Difficulty of practice	Practices too long Practice difficult/like work Dislike of practice/irritation Uncertainty about practice and length	
	Doubt on whether longer practice would have been helpful	"[…] but I'm not sure if I would have been good at doing that for longer" "[…] I have tried to avoid this type of practice in the past because I am concerned it could lead me to panic. but it has actually helped me! not sure how I'd feel if the mindfulness was longer though" "I always thought that to do mindfulness, I'd need a long time to dedicate to this which seemed too challenging to even start. […]" "thought I'd have to spend a lot of time sitting which seemed daunting"
No effect of study	No effect	
	Study during stressful time/life circumstances	"I found some of my responses effected by life circumstances that were unusual and very sad. This led to answers that were outside my norm in terms of mood and distraction. I hope this does not interfere with your findings"
	Not feeling mindful from study participation	
Positive comments about study (general)	Interesting/enjoyable audiobook	"[…]The first part of the sessions gave me the opportunity to try to focus on something and the more interesting the topics were the easier it was to focus and put my busy mind at rest.[…]"

Category	Code/subcategory	Quote
	Audiobook in relation to practice	"The first session where we listened to a 20-minute talk on the Solar System, and the expanse of the Universe vs a 5 minute mindfulness exercise, where we bring all our thoughts to ourselves and the internal workings of the breath. It made me realise how little and insignificant we really are in the grand scheme of things, which in turn made me think about how I manage myself, my stresses and anxieties, and the importance of making the most of the time we have."
	Positive comments about researcher	
	Gratitude (incl. invitation and opportunity to take part)	"thank you" (10?) thank you very much! […]" "Many thanks for the opportunity to take part in your study. […]" "I was happy to help, and I hope my participation contributed" "I enjoyed it, thank you."
	Generic comment about study/study manageable	"it felt like a nice study and manageable for me to take part in"
	Interest in results	"I'd been keen to see final group results once data has been analysed." "I would like to know the title of the study and the purpose, I've clicked to receive summary report"
Negative comments about study (general)	Confusing questionnaires	
	Boring audiobook	
	Study mysterious	

Group 3: control (no mindfulness practice)

Category	Code/subcategory	Quote
Positive experiences of practice	Feeling calm during/after	
	Learning something new (positive, helpful)	
	Future plans for practicing mindfulness (incl. asking for materials)	
	Welcome/joyful experience (incl. looking forward to it)	
	Increased awareness (physical, emotional, openness, incl. lack of awareness)	

	Relief of busy day/taking time to be mindful Increased focus Mindfulness helpful in daily life Permission to focus on self Positive comments on guided practice Positive comments on practice length (esp. short practice benefits) Mindfulness becomes easier with time	
Difficulty of practice	Practices too long Practice difficult/like work Dislike of practice/irritation Uncertainty about practice and length Doubt on whether longer practice would have been helpful	
No effect of study	No effect	None (4 no None None really nothing N/A not really "I don't think it's had any particular effects." "Not really much of an effect." "I don't think participating in the study had any effect on me" "I don't feel it has had any effect on me" "no effect" nothing Not much effect

	Study during stressful time/life circumstances	
	Not feeling mindful from study participation	"I did not feel more mindful after the sessions." "When attempting to listen to the audiobook I didn't pay attention to my body sensations or thoughts."
Positive comments about study (general)	Interesting/enjoyable audiobook	"None, although I did find listening to the audiobook between lectures relaxing." "Thinking about the universe and the phrases in the audiobook" "I enjoyed the audio tape"
	Audiobook in relation to practice	
	Positive comments about researcher	
	Gratitude (incl. invitation and opportunity to take part)	
	Generic comment about study/study manageable/taking part	"Taking a break away from technology and stress" "Spending time not doing anything and just being in the moment not thinking about assignments or the past" "The audio at first made me feel some perspective of time, what a short time I'm on the planet, that everything I've felt has certainly happened in the history of time and people before. It's good to think what a pause in your day could mean." "The time spent quietly listening to something completely unrelated to my normal thoughts and issues" "It was relaxing to take time out from the day and not have to think about anything or interact with anyone." "No except it was an enjoyable pause in the day." "Enjoyed it"
	Interest in results	I want to get the result"
Negative comments about study (general)	Confusing questionnaires	"Some of the questions were ambiguous and 'weighted'"
	Boring audiobook	"I guess time to sit and not do anything was somewhat valuable although I try to do that anyway and wouldn't choose to do that with an audiobook that I found to be fairly boring (incredibly boring in the first session). So i guess the positive was balanced out by the negative making me feel quite neutral about its effect"
	Study mysterious	"all a bit mysterious overall, but I suspect this is deliberate"

Appendices Chapter 7

Appendix 7.3: Chapter 7 Methods

Appendix 7.3.1 Transcript: Ten-minute mindfulness practice

This audio track will guide you through a mindfulness of the breath meditation practice. The practice stands the best chance of being beneficial if you follow the guidance as best you can. However, the guidance is an invitation, not a requirement. You're also welcome to stop the practice at any point if you wish.

So, sitting on your chair, and if it's comfortable to do so, sitting away from the back of the chair so that your spine can be self-supporting. If possible, sitting with your back erect and placing your hands on your knees or in your lap in a comfortable way and inviting your shoulders to be relaxed and dropped.

And closing your eyes now if that feels comfortable, or alternatively, having a soft gaze on the floor, a meter or so in front of you.

And during this practice, holding in mind that we're not trying to achieve any particular state, we're not even trying to relax during this practice. So, seeing if it is possible to let go of the tendency that we all have to want things a certain way and the tendency to judge how well we're doing. Rather, seeing if it's possible to greet your moment-by-moment experience with a sense of openness and gentleness.

And now, if you're willing, bringing attention to the breath. Perhaps to the sensations in the belly or chest as they expand with the inbreath and contract with the outbreath. Or perhaps to the passage of air in and out of the mouth or nose. Noticing maybe the difference in temperature between the inbreath and the outbreath.

Or perhaps placing attention somewhere else, where the breath's sensations are particularly accessible and vivid for you right now.

As best you can, not thinking about the breath, but rather being with the experiences of the body as you breathe (pause).

And if you find it a struggle to locate these experiences, the perhaps resting a hand on your chest or belly for a few moments now and feeling it move with the breath (pause).

An if you have not yet done so, returning now your hand to where it was before and continuing to experience your breath, as best you can.

Moment by moment, breath by breath.

And throughout this sitting practice, the breath will always be present as something that you can return to.

Being as present as you can be with the breath, moment by moment, breath by breath.

And as best you can, letting go of controlling the breath, but rather allowing it to come and go as it pleases (pause).

So being present with the experience of breathing moment by moment, breath by breath, as best you can (pause).

And inevitably, the mind will wander from the breath. Perhaps to other sensations, perhaps to thoughts about the future or past, or perhaps to some other aspect of your experience. This is completely normal and isn't a problem at all. When you become aware that the mind has wandered from the breath, notice what it has wondered to and inviting the possibility that it is okay that the mind has wandered. Afterall, this is what our minds do. And then, letting go of whatever it is that the mind has wandered to and shifting attention back to the breath as best you can.

No need to push away experience but rather just let it be as you return to the breath.

Experiencing the breath entering the body, and experiencing the breath leaving the body once again (pause).

And you may find that judgements arise, perhaps judgements about the mind wandering. If this happens, as best you can, noticing the judgements with kindness and allowing whatever arises in your experience to be, just as it is. Remembering there is no right or wrong. And now returning to the experience of breathing, as best you can (pause).

Experiencing the breath entering the body, and experiencing the breath leaving the body once again (pause).

So if you're willing, feeling the breath's sensations as they flux and change, moment by moment, breath by breath, as best you can (pause).

Experiencing the breath entering the body, and experiencing the breath leaving the body once again (pause).

And knowing that, as it is in the nature of all our minds to wander, if you find that your mind is wandering away from your breath, this is absolutely normal and to be expected. Noticing and experiencing the wandering mind is an important part of mindfulness practice and is a valuable opportunity to practice gentleness and patience. After we have noticed that the mind has wandered, as best we can, coming back to the experience of breathing, moment by moment, breath by breath (pause).

So being present with the breath as best you can moment by moment as it enters the body and as it leaves the body once again (pause).

So noticing where your mind is right now and remembering that wherever it is, that's okay from the point of view of this practice. And if you find that the mind has wandered away from the breath then gently coming back to the breath now, if that feels okay to do so (pause).

And seeing if it is possible to be as curious as you can be towards your experience of the breath right now. Exploring the sensations connected with the breath and how they change and flux moment by moment, breath by breath (pause).

Being present with the experience of breathing moment by moment, breath by breath as best you can (pause).

And now, when you feel ready, opening your eyes gradually if they're closed and bringing awareness back into the room.

---END---

Appendix 7.3.2 Self-report measures: Demographic questionnaire, FFMQ-15, SHS, GAC TMS, compliance check

Demographic questionnaire

1. What is your age (in years)? (Please specify below).

   ____

2. What is your gender? (Please tick as appropriate).

- ☐ Female
- ☐ Male
- ☐ Non-binary
- ☐ Other, please specify: ____
- ☐ Prefer not to say

3. What is your ethnicity? (Please tick as appropriate).

- ☐ Asian
- ☐ Black
- ☐ White
- ☐ Mixed Black and Asian
- ☐ Mixed Black and White
- ☐ Mixed Asian and White
- ☐ Mixed Other
- ☐ Other Ethnic background, please specify: _______________________________
- ☐ Prefer not to say

4. What is your nationality?

____

5. What is your main occupation?

(Please specify below). *Please note, this is a drop-down list from which people can select.*

- ☐ Architecture and Engineering
- ☐ Arts, Design, Entertainment, Sports, and Media
- ☐ Building and Grounds Cleaning and Maintenance
- ☐ Business and Financial Operations
- ☐ Carer for dependant(s)
- ☐ Community and Social Service
- ☐ Computer and Mathematical

- ☐ Construction and Extraction
- ☐ Educational Instruction and Library
- ☐ Farming, Fishing, and Forestry
- ☐ Food Preparation and Serving Related
- ☐ Healthcare Practitioners and Technical
- ☐ Healthcare Support
- ☐ Installation, Maintenance, and Repair
- ☐ Legal
- ☐ Life, Physical, and Social Science
- ☐ Management
- ☐ Military Specific
- ☐ Office and Administrative Support
- ☐ Personal Care and Service
- ☐ Production
- ☐ Protective Service
- ☐ Sales and Related
- ☐ Transportation and Material Moving
- ☐ Unemployed
- ☐ University student
- ☐ Other, please specify: ____
- ☐ Prefer not to say

6. In the past, have you practiced mindfulness, participated in a mindfulness-based intervention or experienced any training or teaching on mindfulness? Don't worry if you have not! We are interested in including participants who both do and don't have previous experiences of mindfulness.

- ☐ No.
- ☐ Yes, please specify:

____

7. Do you currently regularly practice mindfulness?

- ☐ No.
- ☐ Yes, please specify.

____

Five Facet Mindfulness Questionnaire (FFMQ-15)

<u>Instructions:</u> Please use the 1 to 5 scale provided to indicate how true the below statements are of you. The rating scales is as follows:
1 Never or very rarely true.
2 Rarely true.
3 Sometimes true.
4 Often true.
5 Very often or always true.
Circle the number in the box to the right of each statement which represents your own opinion of what is generally true for you. For example, if you think that a statement is often true of you, circle '4' and if you think a statement is sometimes true of you, circle '3'.

1. When I take a shower or a bath, I stay alert to the sensations of water on my body.	1	2	3	4	5
2. I'm good at finding words to describe my feelings.	1	2	3	4	5
3. I don't pay attention to what I'm doing because I'm daydreaming, worrying, or otherwise distracted.	1	2	3	4	5
4. I believe some of my thoughts are abnormal or bad and I shouldn't think that way.	1	2	3	4	5
5. When I have distressing thoughts or images, I "step back" and am aware of the thought or image without getting taken over by it.	1	2	3	4	5
6. I notice how foods and drinks affect my thoughts, bodily sensations, and emotions.	1	2	3	4	5
7. I have trouble thinking of the right words to express how I feel about things.	1	2	3	4	5
8. I do jobs or tasks automatically without being aware of what I'm doing.	1	2	3	4	5
9. I think some of my emotions are bad or inappropriate and I shouldn't feel them.	1	2	3	4	5
10. When I have distressing thoughts or images I am able just to notice them without reacting.	1	2	3	4	5
11. I pay attention to sensations, such as the wind in my hair or sun on my face.	1	2	3	4	5
12. Even when I'm feeling terribly upset I can find a way to put it into words.	1	2	3	4	5
13. I find myself doing things without paying attention.	1	2	3	4	5
14. I tell myself I shouldn't be feeling the way I'm feeling.	1	2	3	4	5
15. When I have distressing thoughts or images I just notice them and let them go.	1	2	3	4	5

State Hope Scale (SHS)

Directions: Read each item carefully. Using the scale shown below, please select the number that best describes how you think about yourself right now by clicking in the circle underneath it. Please take a few moments to focus on yourself and what is going on in your life at this moment. Once you have this "here and now" set, go ahead and answer each item according to the following scale:

1 = Definitely False, 2 = Mostly False, 3 = Somewhat False, 4 = Slightly False, 5 = Slightly True, 6 = Somewhat True, 7 = Mostly True, and 8 = Definitely True

1. If I should find myself in a jam, I could think of many ways to get out of it.	1	2	3	4	5	6	7	8
2. At the present time, I am energetically pursuing my goals	1	2	3	4	5	6	7	8
3. There are lots of ways around any problem that I am facing now.	1	2	3	4	5	6	7	8
4. Right now I see myself as being pretty successful.	1	2	3	4	5	6	7	8
5. I can think of many ways to reach my current goals.	1	2	3	4	5	6	7	8
6. At this time, I am meeting the goals that I have set for myself.	1	2	3	4	5	6	7	8

Gratitude Adjective Checklist (GAC)

Instructions: Think about how you feel right now. Using a scale from 1 (not at all), 2 (a little), 3 (moderately), 4 (quite a bit), to 5 (extremely), please click below a number to indicate your level of feeling the following:

1. Grateful	1	2	3	4	5
2. Thankful	1	2	3	4	5
3. Appreciative	1	2	3	4	5

Toronto Mindfulness Scale (TMS)

Instructions:

We are interested in what you just experienced. Below is a list of things that people sometimes experience. Please read each statement. Please indicate the extent to which you agree with each statement. In other words, how well does the statement describe what you just experienced, just now?

0 Not at all
1 A Little
2 Moderately
3 Quite a bit
4 Very much

1. I experienced myself as separate from my changing thoughts and feelings.	0	1	2	3	4
2. I was more concerned with being open to my experiences than controlling or changing them.	0	1	2	3	4
3. I was curious about what I might learn about myself by taking notice of how I react to certain thoughts, feelings, or sensations.	0	1	2	3	4
4. I experienced my thoughts more as events in my mind than as a necessarily accurate reflection of the way things 'really' are.	0	1	2	3	4
5. I was curious to see what my mind was up to from moment to moment.	0	1	2	3	4
6. I was curious about each of the thoughts and feelings that I was having.	0	1	2	3	4
7. I was receptive to observing unpleasant thoughts and feelings without interfering with them.	0	1	2	3	4
8. I was more invested in just watching my experiences as they arose, than in figuring out what they could mean.	0	1	2	3	4
9. I approached each experience by trying to accept it, no matter whether it was pleasant or unpleasant.	0	1	2	3	4
10. I remained curious about the nature of each experience as it arose.	0	1	2	3	4
11. I was aware of my thoughts and feelings without overidentifying with them.	0	1	2	3	4
12. I was curious about my reactions to things.	0	1	2	3	4
13. I was curious about what I might learn about myself by just taking notice of what my attention gets drawn to.	0	1	2	3	4

Compliance Check Questions

Please note these questions were added below the audio recordings on Qualtrics.

Compliance check questions for the mindfulness practice group:

1. On a scale from 1 (not at all) to 10 (completely), please indicate how well you paid attention to the recording. Please be as honest as possible.
   1, 2, 3, 4, 5, 6, 7, 8, 9, 10
2. On a scale from 1 (not at all) to 10 (completely), please indicate how much you felt you were following the guidance during this practice? Please be as honest as possible.
   1, 2, 3, 4, 5, 6, 7, 8, 9, 10

Compliance check question for the control group:

1. On a scale from 1 (not at all) to 10 (completely), please indicate how well you paid attention to the recording. Please be as honest as possible.
   1, 2, 3, 4, 5, 6, 7, 8, 9, 10

Appendix 7.3.3 Ethical approval outcome letter, addressal of observations and full acceptance

Canterbury Christ Church University

Salomons Institute for Applied Psychology

Sarah Strohmaier
Psychology PhD Student

22 July 2019

Direct line 01227 927094
E-mail margie.callanan@canterbury.ac.uk
Our Ref V:\075\Ethics\2018-19

Dear Sarah,

One-session mindfulness of the breath meditation practice: A randomized controlled study of the effects on state hope and state gratitude.

Outcome: Full Approval

The panel would like to thank you for your submission and we are pleased to offer you approval for your proposed study. The panel had some observations for you to consider with your supervisor and to report back for our records:

1) With regard to the unique ID, please do not ask for first letter of mother's (or primary caregiver's) first name; this reference to mother or primary caregiver is frequently experienced as difficult or painful by many (for reasons of loss, unhappy relationship and so on). Please replace this with first letter of street name where you live, or something similarly neutral.
2) Appendix 2: information section, under 'What is the purpose of the study?': please delete the word 'beneficial' as this is not the only possible outcome of impartial research.
3) Appendix 5: Demographic questionnaire, question 7: 'mindfulness' is spelled incorrectly and needs to be corrected. Also, is this question interested in people who have a mindfulness practice? Or is it interested to know whether an individual currently practices mindfulness regularly? If the latter is what is intended, this question needs rephrasing to reflect that.

Yours sincerely,

Margie Callanan

Professor Margie Callanan
Chair of the Salomons Ethics Panel

Cc Dr Fergal Jones

School of Psychology, Politics and Sociology
Faculty of Social and Applied Sciences

Canterbury Christ Church University
1 Meadow Road Tunbridge Wells Kent TN1 2YG (UK)
Tel +44 (0)1227 927166
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Professor Rama Thirunamachandran, Vice-Chancellor and Principal

Canterbury Christ Church University

Salomons Institute for Applied Psychology

Sarah Strohmaier
Psychology PhD student
Salomons Institute for Applied Psychology
Canterbury Christ Church University
Tel: +44 (0)1227 92 7092
Email: sarah.strohmaier@canterbury.ac.uk

24th July 2019

Dear Professor Margie Callanan,

Thank you very much for the very helpful observations from the Salomons Ethics panel on my study titled "One-session mindfulness of the breath meditation practice: A randomised controlled study of the effects on state hope and state gratitude". I have considered these observations with my supervisor Dr Fergal Jones and have addressed them as follows (please find attached the amended Ethics form including appendices with changes highlighted):

1. With regards to participants creating a unique ID, reference to the mother's (or primary caregiver's) first name has been removed and replaced with "the first letter of the street name where you live".

2. The word 'beneficial' has been removed from Appendix 2: Information section, under the heading 'What is the purpose of this study'.

3. Question 7 of the demographic questionnaire in Appendix 5 has been reworded, now stating 'Do you currently regularly practice mindfulness?' The typographical error of the spelling of the word 'mindfulness' has been corrected.

Please let me know if the panel approves of these changes or whether there are further changes or clarifications you wish for me to make.

Yours sincerely,

Sarah Strohmaier

Sarah Strohmaier
Psychology PhD student

CC Dr Fergal Jones (first supervisor), Dr James Cane (second supervisor)

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Professor Rama Thirunamachandran, Vice-Chancellor and Principal

Sarah Strohmaier

From:	Margie Callanan
Sent:	24 July 2019 13:18
To:	Sarah Strohmaier
Cc:	Jennie Shea; Fergal Jones; James Cane
Subject:	RE: Response to Ethics outcome

Dear Sarah,
Thank you for addressing the panel's points so thoroughly. Full approval is now granted for your study. We wish you every success with the work and look forward to receiving a summary report when the work is complete.
With best wishes,
Margie

Professor Margie Callanan
Programme Director of Clinical Psychology Doctorate
Director of Salomons Institute for Applied Psychology

SALOMONS INSTITUTE FOR APPLIED PSYCHOLOGY,
Lucy Fildes Building
Canterbury Christ Church University,
1 Meadow Road,
Tunbridge Wells TN1 2YG
01227 927094

margie.callanan@canterbury.ac.uk

Appendix 7.3.4 Study Information

Study examining a brief online mindfulness and listening exercise (version 3, 29th September 2019)

Hello. My name is Sarah Strohmaier, and I am a Psychology PhD student at Canterbury Christ Church University. I would like to invite you to take part in a research study. Before you decide whether to take part, it is important that you understand why the research is being done and what it would involve for you.

What is the purpose of the study?

This study is part of a psychology research project which looks at the effects of listening to a ten-minute guided audio recording, some of which may include mindfulness practice.

Do I have to take part?

You do <u>not</u> have to take part in this study if you do not want to; taking part in this research is entirely voluntary. If you agree to take part, you will need to consent (see below). You are also free to withdraw your participation from the study at any time up until two weeks after you participated, without giving a reason.

When is it best not to take part?

If you are currently experiencing very severe problems with your mental health, we would recommend that you do not take part in this study at this time.

What will happen to me if I take part?

If you decide you would like to participate, you will firstly be asked to consent below to ensure that you are aware of your rights. We will then ask you to complete some brief questionnaires followed by listening to an audio recording for **10 minutes.** For this **please make sure you use headphones** if you are in public and make sure that you are not disturbed for 10 minutes. After listening to the audio recording, we ask you to complete some more brief questionnaires. The whole study should take **no longer than 20 minutes.**

Prize draw/course credits

You can choose to enter your name in the prize draw to win a £50 Amazon voucher (please note, this is transferable to different countries). The winner of the prize draw will be chosen randomly. If you are an undergraduate student of Canterbury Christ Church University, you can choose to receive course credits for your participation instead.

What are the possible disadvantages and risks of taking part?

It is very unlikely that participating in this research will cause any distress for the majority of people, although occasionally some people can experience some discomfort when practicing mindfulness. However, this is unlikely since this recording is brief and you are welcome to stop at any point. Furthermore, for some people, completing questions on their wellbeing can highlight low mood experiences. However, the measures are designed for and commonly used in the general population. In the unlikely event that participating in this research study highlights any issues you may need further support with, then we'd recommend you contact your GP or dial 111 to speak directly to a health professional in the NHS.

What are the possible benefits of taking part?

Previous research has shown that people can experience benefits from practicing mindfulness and listening to audio books/podcasts. Both can be useful tools for calming the mind and enhancing listening skills and concentration. We cannot promise the study will help you, but the information we gain from this study will help improve research and practice.

Will information from or about me be kept confidential?

Yes. We will follow ethical and legal practice and all information about you will be handled in confidence. Your data will be anonymised and stored on a password-protected computer. Only anonymised data will be used for data analysis.

What will happen if I do not want to carry on with the study?

You can withdraw your data from the study at any time up until two weeks after you participated without having to give a reason. To do this, please email **sarah.strohmaier@canterbury.ac.uk** or phone Sarah Strohmaier on 01227 927092 with the unique code we will ask you to create. We will then remove your data from the study.

Contact and complaints

If you have questions about any aspect of this study or would like to give feedback, please email me at **sarah.strohmaier@canterbury.ac.uk**. If you remain unhappy and wish to complain formally, you can do this by contacting Prof Alex Hassett, Principal Lecturer, Salomons Institute for Applied Psychology **– alex.hassett@canterbury.ac.uk**, phone: 01227 927093.

What will happen to the results of the research study?

This study will be written up as a PhD thesis and submitted for publication to an academic journal. Any data included will be anonymised and no individual participant will be identified.

Who is sponsoring and funding the research?

This research is funded by Canterbury Christ Church University.

Who has reviewed the study?

This study has been reviewed and given favourable opinion by the PhD students' supervisors and the Salomons Ethics Panel, Salomons Institute for Applied Psychology, Canterbury Christ Church University.

If you have any questions before deciding to take part, please email me at **sarah.strohmaier@canterbury.ac.uk**.

Appendix 7.3.5 Study Consent Form

CONSENT FORM

Title of Project: Study examining a brief online mindfulness and listening exercise (version 3; 29th September 2019)

Name of Researcher: Sarah Strohmaier

Please tick each:

1. I confirm that I have read and understand the above information about the study. I have had the opportunity to consider the information, ask questions and have had these answered satisfactorily, where needed. ☐

2. I understand that my participation is voluntary and that I am free to withdraw my responses at any time up until two weeks after my participation without giving any reason. ☐

3. I confirm that I am not currently experiencing very severe problems with my mental health. ☐

4. I agree for my anonymous data to be used for publication of this research. ☐

5. I agree to take part in this study. ☐

Please create a unique ID which will be assigned and stored with your answers. This should consist of the first two letters of your favourite colour, the first two numbers of your date of birth and the first letter of the street name where you live, for instance YE15M. Please make a note of your unique ID for future reference in case you wish to withdraw your data from the dataset.

Appendix 7.3.6 Study Debrief Form

DEBRIEF version 3; 29th September 2019

Thank you very much for participating in this study!

The aim of this study was to examine whether participating in a 10-minute mindfulness meditation practice is helpful for individuals compared to a not-practicing group.

Since this study is still running, please do not share the content of this study with your friends/colleagues since they may participate in this study as well.

If you would like to be sent a written summary report of the study's findings, please enter your email address below and the report will be sent to you once the study is complete.

……………………………………………………………………

Please select and enter your email address below if

- ☐ You would like to be entered in the prize draw to a £50 Amazon voucher (please note, this is transferable to different countries). …………………………………………………
- ☐ You would like to receive course credits for participating in this study ……………………………………………………………………

Please note, your email address will be kept confidential and securely by the researcher and is used only for the purposes of notifying you if you are the winner of the prize draw, alert RPS about receiving course credits you earned (CCCU undergraduates only) or to send you the summary report if you would like this.

In the unlikely event that participating in this research study highlights any issues you may need further support with, then we'd recommend you contact your GP or dial 111 to speak directly to a health professional in the NHS.

If you wish to withdraw your data from the study, please email **sarah.strohmaier@canterbury.ac.uk** or phone Sarah Strohmaier on 01227 927092 up until 2 weeks after your participation with the unique code you have created, and we will remove your data.

If you have any concerns, feedback or questions about any aspect of this study, please email me at **sarah.strohmaier@canterbury.ac.uk** and I will do my best to address these. You can also contact me by leaving a message on the 24-hour voicemail phone number 01227 927070. Please leave a contact number and say that the message is for me (Sarah Strohmaier) and I will get back to you as soon as possible. If you remain unhappy and wish to complain formally, you can do this by contacting Prof Alex Hassett, Principal Lecturer at the Salomons Institute for Applied Psychology, **alex.hassett@canterbury.ac.uk**, tel: +44(0)1227 92 7093.

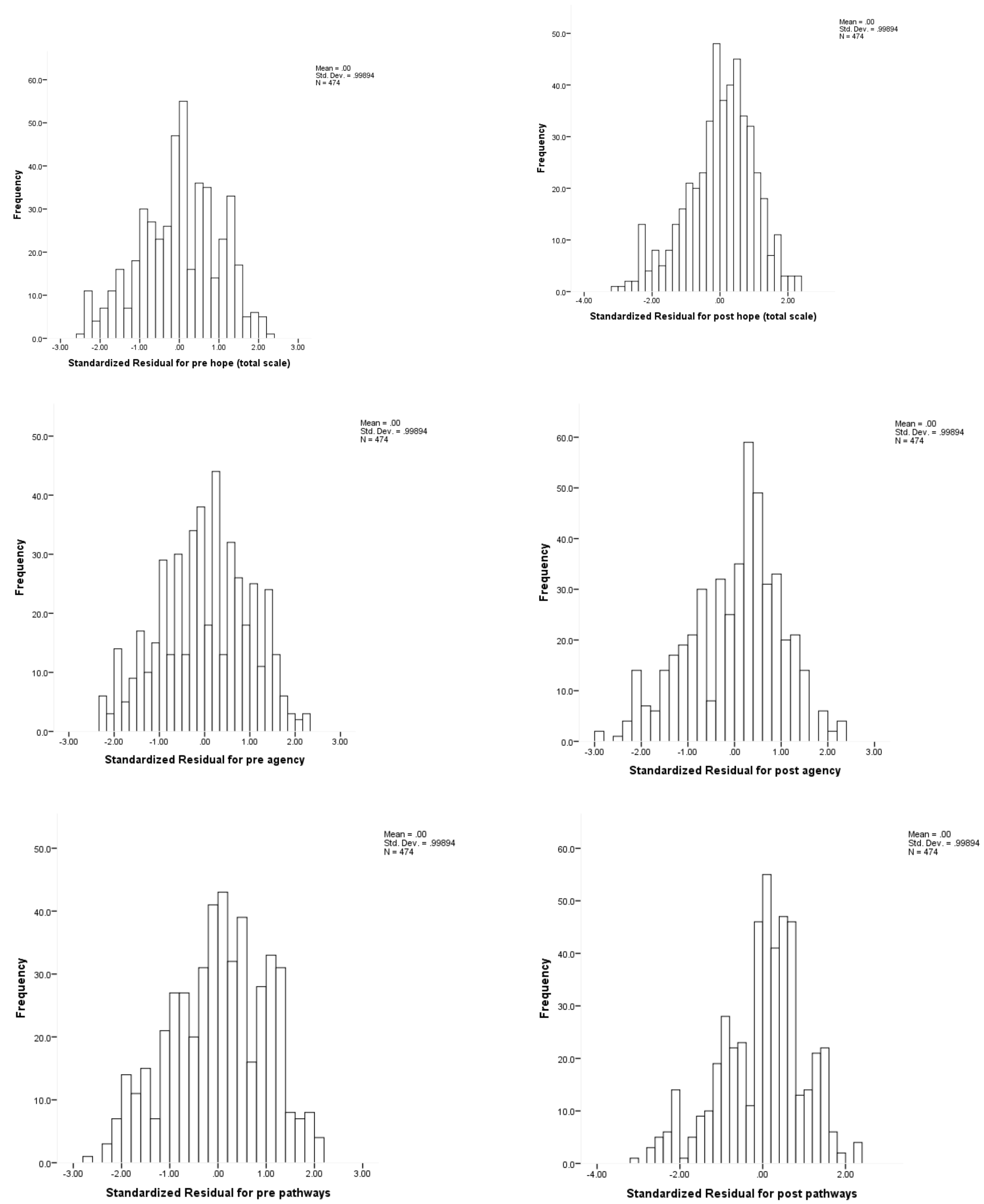
Please click the **Submit** button on the right/below to submit your answers.

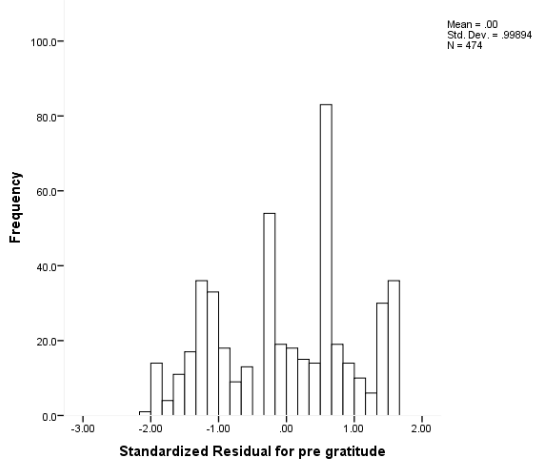
Thank you very much!

Appendix 7.4: Chapter 7 Results

Appendix Figure 7.4.1

Histograms of standardised residuals for outcomes at pre and post

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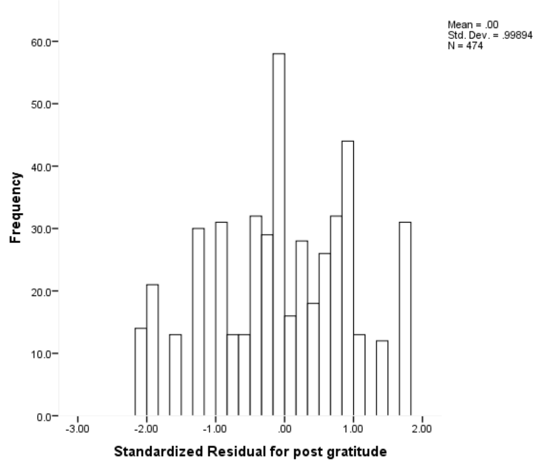
Std. Dev. = .99894
N = 474
Frequency
Standardized Residual for pre gratitude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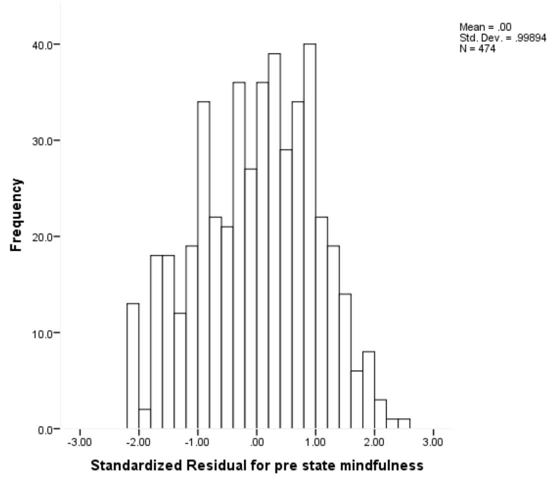
Std. Dev. = .99894
N = 474
Frequency
Standardized Residual for post gratitude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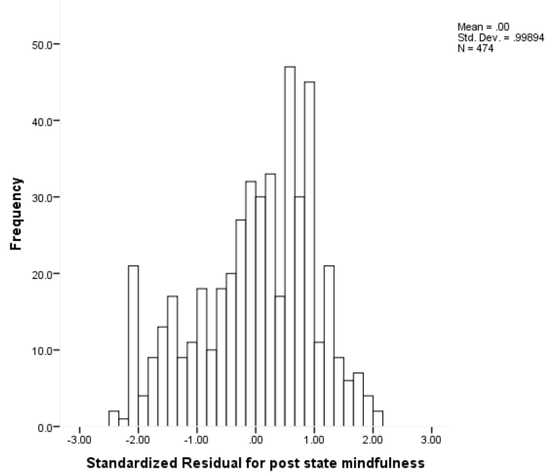
Std. Dev. = .99894
N = 474
Frequency
Standardized Residual for pre state mindfulness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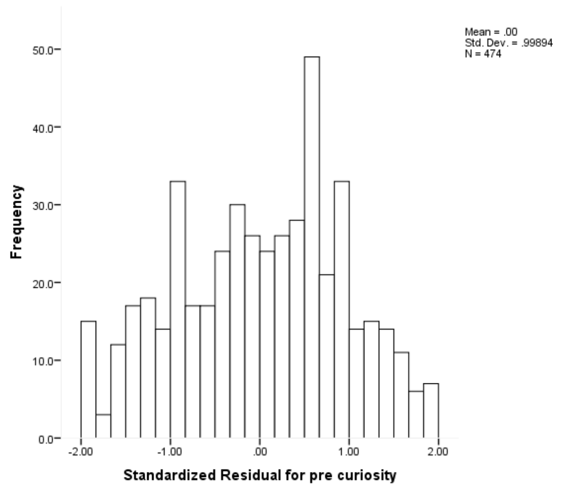
Std. Dev. = .99894
N = 474
Frequency
Standardized Residual for post state mindfulness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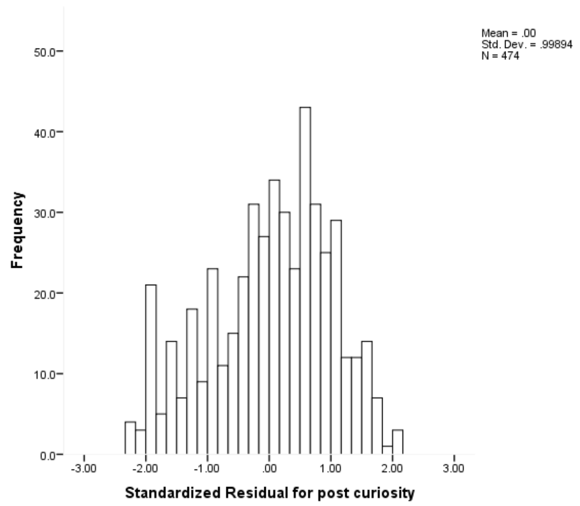
Std. Dev. = .99894
N = 474
Frequency
Standardized Residual for pre curiosity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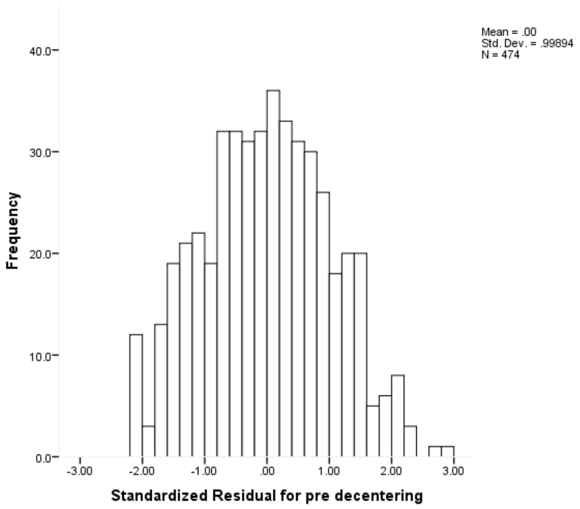
Std. Dev. = .99894
N = 474
Frequency
Standardized Residual for post curiosity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Std. Dev. = .99894
N = 474
Frequency
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30.0
20.0
10.0
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-2.00
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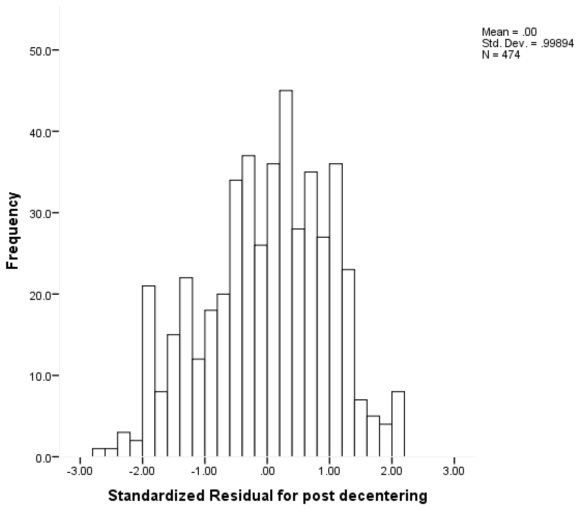
2.00
3.00
Standardized Residual for pre decentering


Mean = .00
Std. Dev. = .99894
N = 474
Frequency
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40.0
30.0
20.0
10.0
0.0
-3.00
-2.00
-1.00
.00
1.00
2.00
3.00
Standardized Residual for post decentering

Appendix 7.4.2 Normality test outputs

SPSS output for normality tests of standardised residuals for outcomes at pre and post timepoints

Tests of Normality

	Kolmogorov-Smirnov[a]			Shapiro-Wilk		
	Statistic	df	Sig.	Statistic	df	Sig.
Standardized Residual for preHOPE	.050	474	.006	.988	474	.001
Standardized Residual for postHOPE	.072	474	.000	.980	474	.000
Standardized Residual for preGRATITUDE	.122	474	.000	.950	474	.000
Standardized Residual for postGRATITUDE	.082	474	.000	.970	474	.000
Standardized Residual for PREagency	.046	474	.019	.987	474	.000
Standardized Residual for POSTagency	.103	474	.000	.982	474	.000
Standardized Residual for PREpathways	.058	474	.001	.985	474	.000
Standardized Residual for POSTpathways	.095	474	.000	.975	474	.000
Standardized Residual for pre state mindfulness	.058	474	.001	.987	474	.000
Standardized Residual for post state mindfulness	.079	474	.000	.968	474	.000
Standardized Residual for pre curiosity	.077	474	.000	.976	474	.000
Standardized Residual for post curiosity	.084	474	.000	.974	474	.000
Standardized Residual for pre decentering	.037	474	.139	.992	474	.008
Standardized Residual for post decentering	.058	474	.001	.985	474	.000

a. Lilliefors Significance Correction

SPSS output for normality tests of standardised residuals for outcomes by group at pre and post timepoints

Tests of Normality

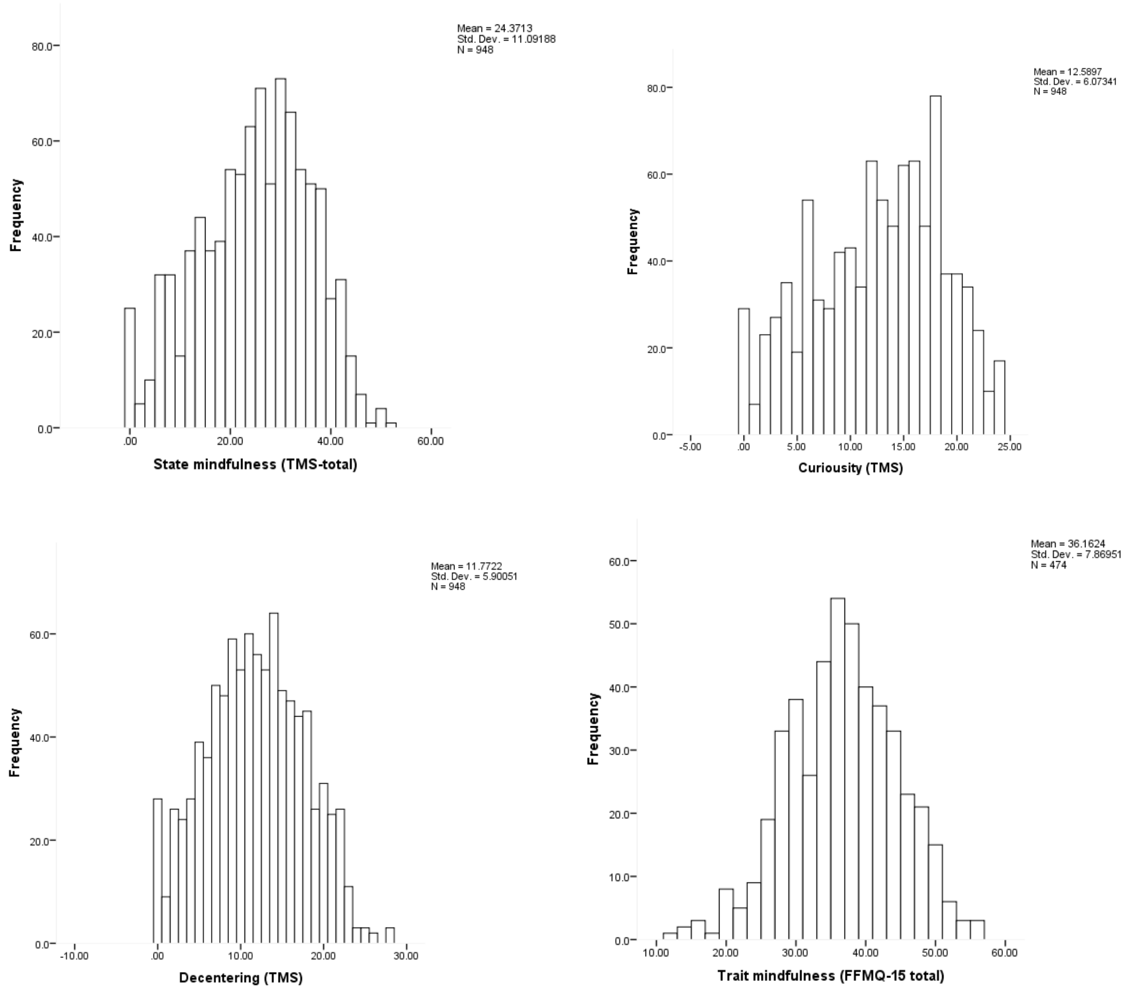
	Group	Kolmogorov-Smirnov[a] Statistic	df	Sig.	Shapiro-Wilk Statistic	df	Sig.
Standardized Residual for preHOPE	Mindfulness practice group	.053	237	.200*	.987	237	.032
	Control group	.092	237	.000	.978	237	.001
Standardized Residual for postHOPE	Mindfulness practice group	.074	237	.003	.957	237	.000
	Control group	.088	237	.000	.979	237	.001
Standardized Residual for preGRATITUDE	Mindfulness practice group	.151	237	.000	.935	237	.000
	Control group	.131	237	.000	.936	237	.000
Standardized Residual for postGRATITUDE	Mindfulness practice group	.151	237	.000	.923	237	.000
	Control group	.113	237	.000	.943	237	.000
Standardized Residual for PREagency	Mindfulness practice group	.065	237	.016	.984	237	.009
	Control group	.085	237	.000	.978	237	.001
Standardized Residual for POSTagency	Mindfulness practice group	.144	237	.000	.956	237	.000
	Control group	.091	237	.000	.978	237	.001
Standardized Residual for PREpathways	Mindfulness practice group	.072	237	.004	.981	237	.003
	Control group	.103	237	.000	.975	237	.000
Standardized Residual for POSTpathways	Mindfulness practice group	.117	237	.000	.946	237	.000
	Control group	.105	237	.000	.974	237	.000
Standardized Residual for pre state mindfulness	Mindfulness practice group	.066	237	.014	.988	237	.054
	Control group	.090	237	.000	.967	237	.000
Standardized Residual for post state mindfulness	Mindfulness practice group	.106	237	.000	.958	237	.000
	Control group	.077	237	.002	.967	237	.000
Standardized Residual for pre curiosity	Mindfulness practice group	.082	237	.001	.972	237	.000
	Control group	.083	237	.000	.969	237	.000
Standardized Residual for post curiosity	Mindfulness practice group	.101	237	.000	.960	237	.000
	Control group	.079	237	.001	.969	237	.000
Standardized Residual for pre decentering	Mindfulness practice group	.074	237	.003	.985	237	.012
	Control group	.063	237	.025	.980	237	.002
Standardized Residual for post decentering	Mindfulness practice group	.089	237	.000	.973	237	.000
	Control group	.058	237	.051	.977	237	.001

*. This is a lower bound of the true significance.

a. Lilliefors Significance Correction

Appendix Figure 7.4.3

Normality histograms for state mindfulness (total-TMS, curiosity, decentering) and trait mindfulness



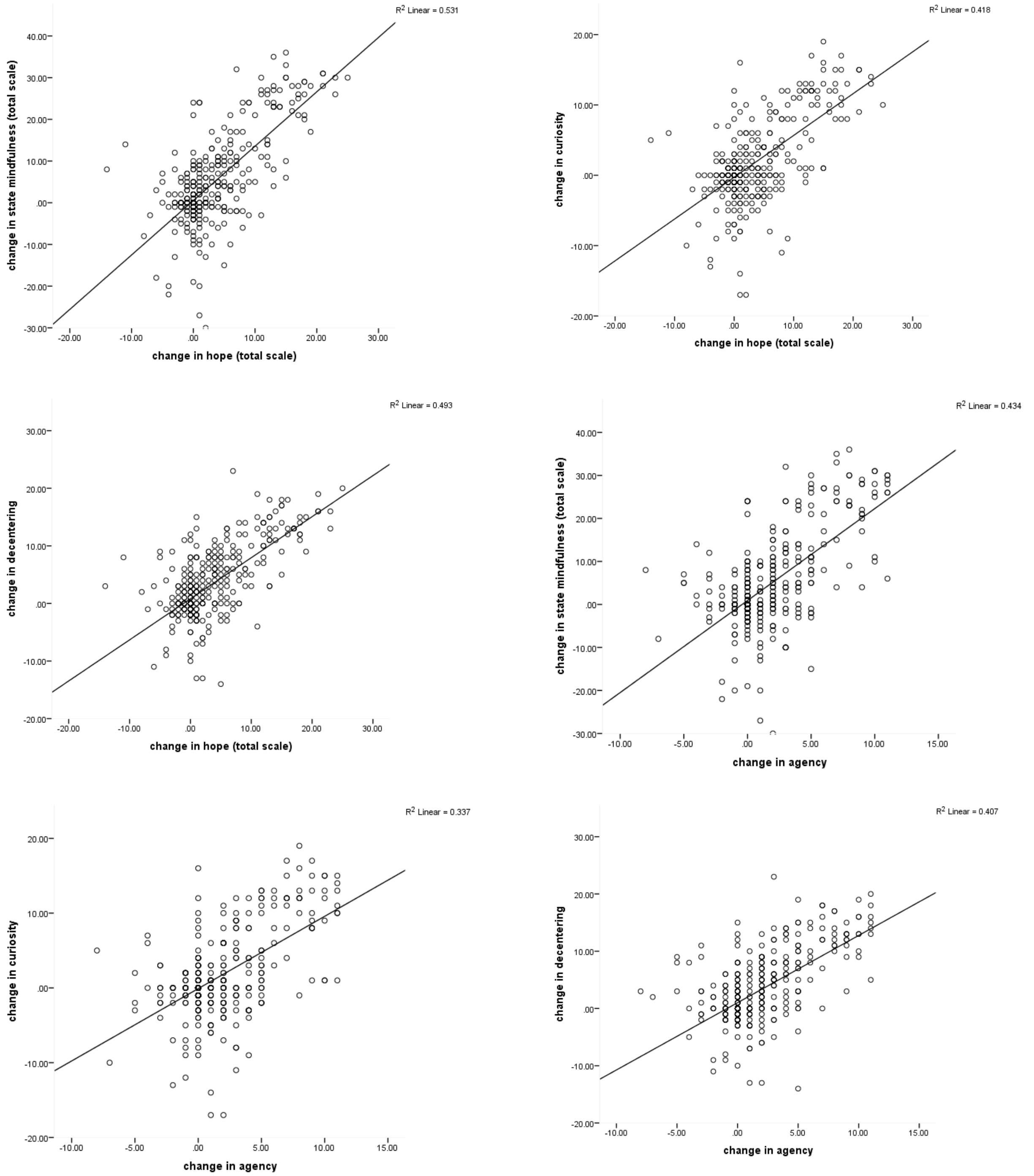
Appendix 7.4.4 SPSS output for Levene's test for homogeneity of variance between groups and pre and post

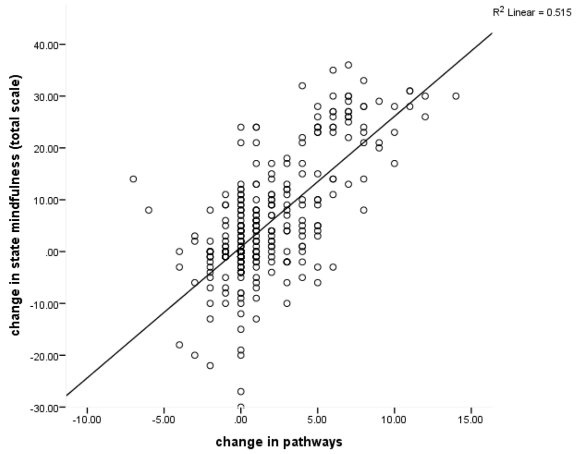
Test of Homogeneity of Variances

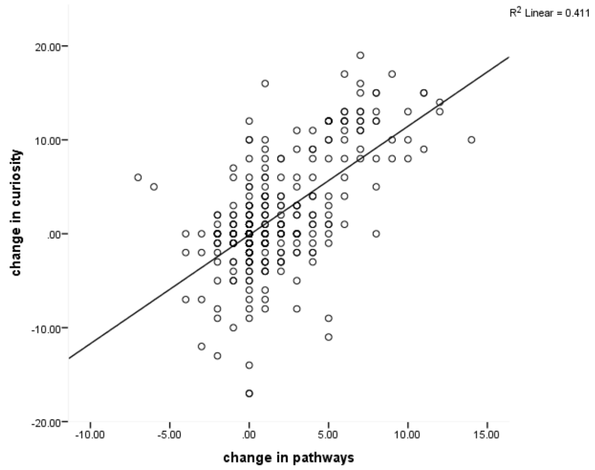
	Levene Statistic	df1	df2	Sig.
Pre Hope (total)	.542	1	472	.462
Post Hope (total)	9.807	1	472	.002
Pre Agency	2.057	1	472	.152
Post Agency	8.423	1	472	.004
Pre Pathways	.324	1	472	.569
Post Pathways	14.027	1	472	.000
Pre Gratitude (total)	3.784	1	472	.052
Post Gratitude (total)	75.829	1	472	.000
Pre State Mindfulness (total)	3.795	1	472	.052
Post State Mindfulness (total)	3.062	1	472	.081
Pre Curiousity	.569	1	472	.451
Post Curiosity	10.909	1	472	.001
Pre Decentering	3.690	1	472	.055
Post Decentering	.222	1	472	.638

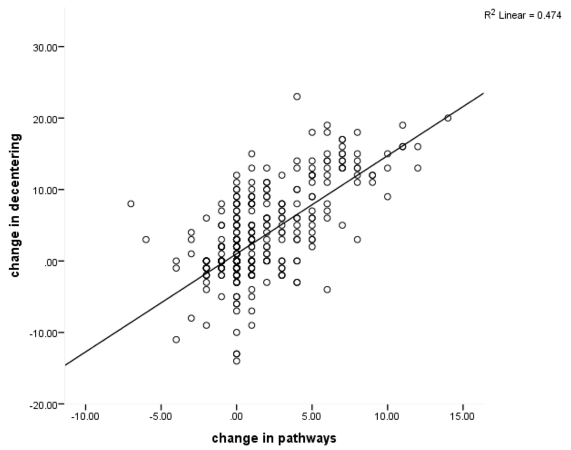
Appendix Figure 7.4.5

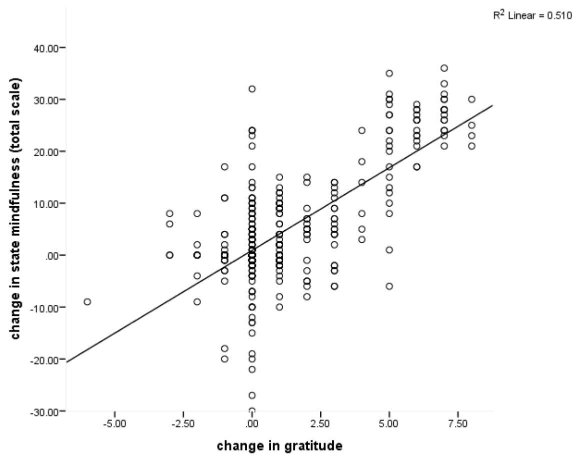
Linearity scatterplots showing linear relationships between state mindfulness (total scale and subscales curiosity and decentering) and outcomes state hope (total scale and subscales agency and pathways) and state gratitude as well as linear relationships between baseline trait mindfulness and state mindfulness (total scale and subscales curiosity and decentering)

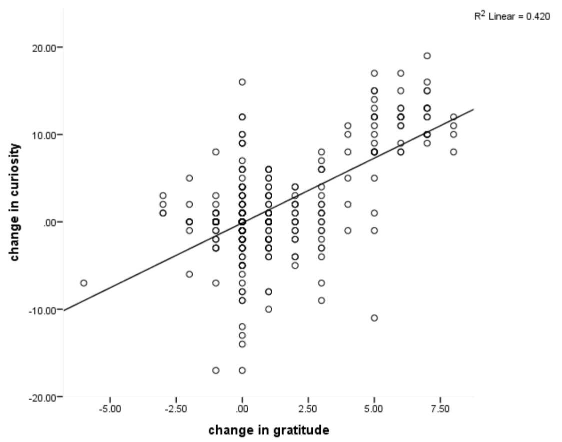


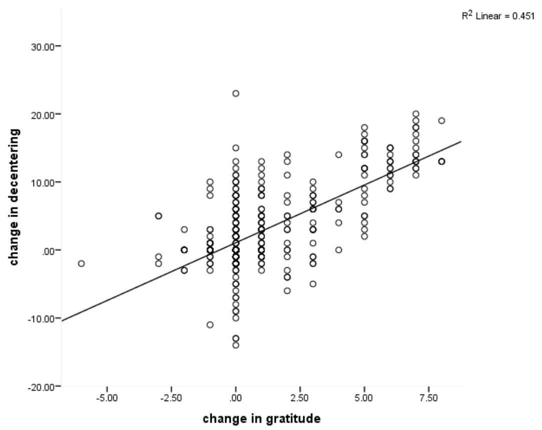

R2 Linear = 0.515
change in state mindfulness (total scale)
change in pathways

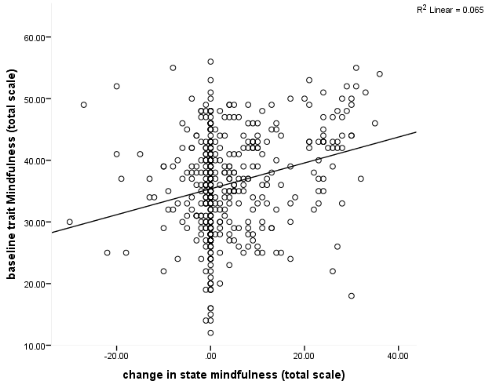

R2 Linear = 0.411
change in curiosity
change in pathways


R2 Linear = 0.474
change in decentering
change in pathways

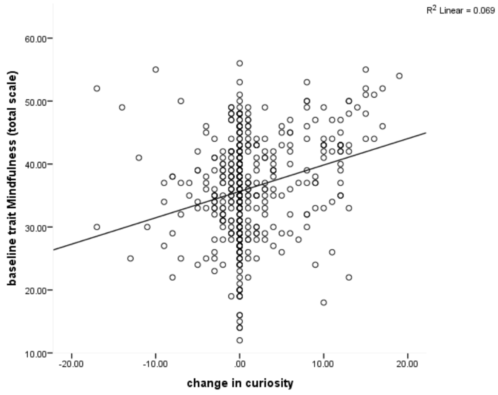

R2 Linear = 0.510
change in state mindfulness (total scale)
change in gratitude


R2 Linear = 0.420
change in curiosity
change in gratitude

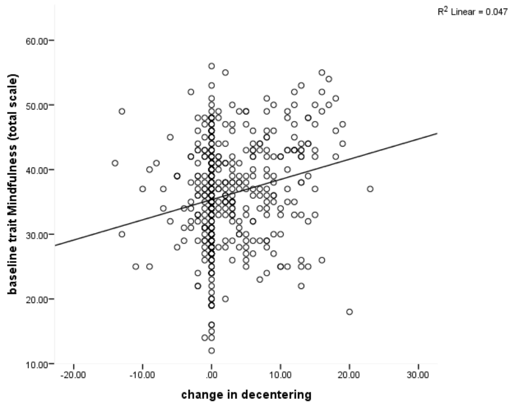

R2 Linear = 0.451
change in decentering
change in gratitude



R2 Linear = 0.065
baseline trait Mindfulness (total scale)
change in state mindfulness (total scale)



R2 Linear = 0.069
baseline trait Mindfulness (total scale)
change in curiosity



R2 Linear = 0.047
baseline trait Mindfulness (total scale)
change in decentering

Appendix 7.4.6 SPSS output multicollinearity assumption check (trait and state mindfulness)

Correlations

		Trait Mindfulness (total)	Trait Mindfulness: Observe	Trait Mindfulness: Describe	Trait Mindfulness: Act Aware	Trait Mindfulness: Non-Judge	Trait Mindfulness: Non-React	Pre State Mindfulness (total)	Post State Mindfulness (total)	Pre Decentering	Post Decentering	Pre Curiousity	Post Curiosity	TMSchange	DecenteringChange	CuriosityChange
Trait Mindfulness (total)	Pearson Correlation	1	.461**	.748**	.726**	.806**	.704**	.196**	.391**	.243**	.391**	.124**	.338**	.256**	.216**	.262**
	Sig. (2-tailed)		.000	.000	.000	.000	.000	.000	.000	.000	.000	.007	.000	.000	.000	.000
	Sum of Squares and Cross-products	29292.492	4488.603	7440.070	6464.143	8866.734	6521.544	7573.312	16695.506	4733.962	9098.127	2804.812	7597.380	9122.194	4364.165	4792.568
	Covariance	61.929	9.490	15.730	13.666	18.746	13.788	16.011	35.297	10.008	19.235	5.930	16.062	19.286	9.227	10.132
	N	474	474	474	474	474	474	474	474	474	474	474	474	474	474	474
Trait Mindfulness: Observe	Pearson Correlation	.461**	1	.430**	.266**	.234**	.463**	.420**	.492**	.383**	.455**	.384**	.466**	.136**	.154**	.099*
	Sig. (2-tailed)	.000		.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	.003	.001	.032
	Sum of Squares and Cross-products	4488.603	3233.359	1419.772	787.743	855.506	1425.582	5385.675	6992.797	2479.215	3512.949	2880.675	3479.848	1607.122	1033.734	599.173
	Covariance	9.490	6.836	3.002	1.665	1.809	3.014	11.386	14.784	5.241	7.427	6.090	7.357	3.398	2.185	1.267
	N	474	474	474	474	474	474	474	474	474	474	474	474	474	474	474
Trait Mindfulness: Describe	Pearson Correlation	.748**	.430**	1	.374**	.434**	.418**	.271**	.358**	.254**	.335**	.241**	.335**	.136**	.140**	.113*
	Sig. (2-tailed)	.000	.000		.000	.000	.000	.000	.000	.000	.000	.000	.000	.003	.002	.014
	Sum of Squares and Cross-products	7440.070	1419.772	3374.551	1128.316	1622.443	1314.759	3541.924	5188.823	1677.063	2639.456	1849.424	2549.367	1646.899	962.392	699.943
	Covariance	15.730	3.002	7.134	2.385	3.430	2.780	7.488	10.970	3.546	5.580	3.910	5.390	3.482	2.035	1.480
	N	474	474	474	474	474	474	474	474	474	474	474	474	474	474	474
Trait Mindfulness: Act Aware	Pearson Correlation	.726**	.266**	.374**	1	.518**	.319**	-.028	.222**	-.010	.218**	-.040	.197**	.296**	.260**	.290**
	Sig. (2-tailed)	.000	.000	.000		.000	.000	.541	.000	.825	.000	.390	.000	.000	.000	.000
	Sum of Squares and Cross-products	6464.143	787.743	1128.316	2703.561	1733.519	898.747	-329.308	2882.392	-60.354	1539.848	-271.308	1342.544	3211.700	1600.203	1613.852
	Covariance	13.666	1.665	2.385	5.716	3.665	1.900	-.696	6.094	-.128	3.255	-.574	2.838	6.790	3.383	3.412
	N	474	474	474	474	474	474	474	474	474	474	474	474	474	474	474
Trait Mindfulness: Non-Judge	Pearson Correlation	.806**	.234**	.434**	.518**	1	.395**	.044	.212**	.087	.217**	.001	.179**	.206**	.166**	.219**
	Sig. (2-tailed)	.000	.000	.000	.000		.000	.337	.000	.058	.000	.988	.000	.000	.000	.000
	Sum of Squares and Cross-products	8866.734	855.506	1622.443	1733.519	4136.127	1374.646	640.835	3407.949	638.304	1896.987	5.835	1510.962	2767.114	1258.684	1505.127
	Covariance	18.746	1.809	3.430	3.665	8.744	2.906	1.355	7.205	1.349	4.011	.012	3.194	5.850	2.661	3.182
	N	474	474	474	474	474	474	474	474	474	474	474	474	474	474	474
Trait Mindfulness: Non-React	Pearson Correlation	.704**	.463**	.418**	.319**	.395**	1	.305**	.386**	.402**	.411**	.171**	.309**	.133**	.085	.168**
	Sig. (2-tailed)	.000	.000	.000	.000	.000		.000	.000	.000	.000	.000	.000	.004	.065	.000
	Sum of Squares and Cross-products	6521.544	1425.582	1314.759	898.747	1374.646	2933.392	3719.861	5216.342	2478.949	3021.835	1220.861	2194.506	1496.481	542.886	973.646
	Covariance	13.788	3.014	2.780	1.900	2.906	6.202	7.864	11.028	5.241	6.389	2.581	4.640	3.164	1.148	2.058
	N	474	474	474	474	474	474	474	474	474	474	474	474	474	474	474
Pre State Mindfulness (total)	Pearson Correlation	.196**	.420**	.271**	-.028	.044	.305**	1	.619**	.901**	.520**	.928**	.641**	-.339**	-.270**	-.358**
	Sig. (2-tailed)	.000	.000	.000	.541	.337	.000		.000	.000	.000	.000	.000	.000	.000	.000
	Sum of Squares and Cross-products	7573.312	5385.675	3541.924	-329.308	640.835	3719.861	50754.447	34852.266	23100.405	15921.316	27559.447	18930.949	-15902.181	-7179.089	-8628.498
	Covariance	16.011	11.386	7.488	-.696	1.355	7.864	107.303	73.683	48.838	33.660	58.265	40.023	-33.620	-15.178	-18.242
	N	474	474	474	474	474	474	474	474	474	474	474	474	474	474	474
Post State Mindfulness (total)	Pearson Correlation	.391**	.492**	.358**	.222**	.212**	.386**	.619**	1	.564**	.938**	.570**	.933**	.529**	.535**	.442**
	Sig. (2-tailed)	.000	.000	.000	.000	.000	.000	.000		.000	.000	.000	.000	.000	.000	.000
	Sum of Squares and Cross-products	16695.506	6992.797	5188.823	2882.392	3407.949	5216.342	34852.266	62397.620	16021.278	31817.405	18761.266	30580.215	27545.354	15796.127	11818.949
	Covariance	35.297	14.784	10.970	6.094	7.205	11.028	73.683	131.919	33.872	67.267	39.664	64.652	58.235	33.396	24.987
	N	474	474	474	474	474	474	474	474	474	474	474	474	474	474	474
Pre Decentering	Pearson Correlation	.243**	.383**	.254**	-.010	.087	.402**	.901**	.564**	1	.564**	.678**	.490**	-.299**	-.315**	-.234**
	Sig. (2-tailed)	.000	.000	.000	.825	.058	.000	.000	.000		.000	.000	.000	.000	.000	.000
	Sum of Squares and Cross-products	4733.962	2479.215	1677.063	-60.354	638.304	2478.949	23100.405	16021.278	12942.329	8708.570	10162.405	7312.709	-7079.127	-4233.759	-2849.696
	Covariance	10.008	5.241	3.546	-.128	1.349	5.241	48.838	33.872	27.362	18.411	21.485	15.460	-14.966	-8.951	-6.025
	N	474	474	474	474	474	474	474	474	474	474	474	474	474	474	474
Post Decentering	Pearson Correlation	.391**	.455**	.335**	.218**	.217**	.411**	.520**	.938**	.564**	1	.401**	.750**	.561**	.607**	.426**
	Sig. (2-tailed)	.000	.000	.000	.000	.000	.000	.000	.000	.000		.000	.000	.000	.000	.000
	Sum of Squares and Cross-products	9098.127	3512.949	2639.456	1539.848	1896.987	3021.835	15921.316	31817.405	8708.570	18448.101	7186.316	13369.304	15896.089	9739.532	6182.987
	Covariance	19.235	7.427	5.580	3.255	4.011	6.389	33.660	67.267	18.411	39.002	15.193	28.265	33.607	20.591	13.072
	N	474	474	474	474	474	474	474	474	474	474	474	474	474	474	474
Pre Curiousity	Pearson Correlation	.124**	.384**	.241**	-.040	.001	.171**	.928**	.570**	.678**	.401**	1	.669**	-.320**	-.191**	-.412**
	Sig. (2-tailed)	.007	.000	.000	.390	.988	.000	.000	.000	.000	.000		.000	.000	.000	.000
	Sum of Squares and Cross-products	2804.812	2880.675	1849.424	-271.308	5.835	1220.861	27559.447	18761.266	10162.405	7186.316	17378.947	11574.949	-8798.181	-2976.089	-5803.998
	Covariance	5.930	6.090	3.910	-.574	.012	2.581	58.265	39.664	21.485	15.193	36.742	24.471	-18.601	-6.292	-12.271
	N	474	474	474	474	474	474	474	474	474	474	474	474	474	474	474
Post Curiosity	Pearson Correlation	.338**	.466**	.335**	.197**	.179**	.309**	.641**	.933**	.490**	.750**	.669**	1	.426**	.391**	.402**
	Sig. (2-tailed)	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000		.000	.000	.000
	Sum of Squares and Cross-products	7597.380	3479.848	2549.367	1342.544	1510.962	2194.506	18930.949	30580.215	7312.709	13369.304	11574.949	17210.911	11649.266	6056.595	5635.962
	Covariance	16.062	7.357	5.390	2.838	3.194	4.640	40.023	64.652	15.460	28.265	24.471	36.387	24.628	12.805	11.915
	N	474	474	474	474	474	474	474	474	474	474	474	474	474	474	474
TMSchange	Pearson Correlation	.256**	.136**	.136**	.296**	.206**	.133**	-.339**	.529**	-.299**	.561**	-.320**	.426**	1	.932**	.917**
	Sig. (2-tailed)	.000	.003	.003	.000	.000	.004	.000	.000	.000	.000	.000	.000		.000	.000
	Sum of Squares and Cross-products	9122.194	1607.122	1646.899	3211.700	2767.114	1496.481	-15902.181	27545.354	-7079.127	15896.089	-8798.181	11649.266	43447.536	22975.215	20447.447
	Covariance	19.286	3.398	3.482	6.790	5.850	3.164	-33.620	58.235	-14.966	33.607	-18.601	24.628	91.855	48.573	43.229
	N	474	474	474	474	474	474	474	474	474	474	474	474	474	474	474
DecenteringChange	Pearson Correlation	.216**	.154**	.140**	.260**	.166**	.085	-.270**	.535**	-.315**	.607**	-.191**	.391**	.932**	1	.714**
	Sig. (2-tailed)	.000	.001	.002	.000	.000	.065	.000	.000	.000	.000	.000	.000	.000		.000
	Sum of Squares and Cross-products	4364.165	1033.734	962.392	1600.203	1258.684	542.886	-7179.089	15796.127	-4233.759	9739.532	-2976.089	6056.595	22975.215	13973.291	9032.684
	Covariance	9.227	2.185	2.035	3.383	2.661	1.148	-15.178	33.396	-8.951	20.591	-6.292	12.805	48.573	29.542	19.097
	N	474	474	474	474	474	474	474	474	474	474	474	474	474	474	474
CuriosityChange	Pearson Correlation	.262**	.099*	.113*	.290**	.219**	.168**	-.358**	.442**	-.234**	.426**	-.412**	.402**	.917**	.714**	1
	Sig. (2-tailed)	.000	.032	.014	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	.000	
	Sum of Squares and Cross-products	4792.568	599.173	699.943	1613.852	1505.127	973.646	-8628.498	11818.949	-2849.696	6182.987	-5803.998	5635.962	20447.447	9032.684	11439.960
	Covariance	10.132	1.267	1.480	3.412	3.182	2.058	-18.242	24.987	-6.025	13.072	-12.271	11.915	43.229	19.097	24.186
	N	474	474	474	474	474	474	474	474	474	474	474	474	474	474	474

**. Correlation is significant at the 0.01 level (2-tailed).

*. Correlation is significant at the 0.05 level (2-tailed).

Appendix 7.4.7 SPSS outputs of group comparison for participants who withdrew and completed the study for demographic variables (outputs 1-6: gender, ethnicity, occupation, nationality, practice experience, age) and outcome measures (output 7)

Chi-Square Tests

	Value	df	Asymptotic Significance (2-sided)
Pearson Chi-Square	1.367[a]	3	.713
Likelihood Ratio	1.789	3	.617
Linear-by-Linear Association	.678	1	.410
N of Valid Cases	559		

a. 4 cells (50.0%) have expected count less than 5. The minimum expected count is .46.

Chi-Square Tests

	Value	df	Asymptotic Significance (2-sided)
Pearson Chi-Square	8.599[a]	8	.377
Likelihood Ratio	9.864	8	.275
Linear-by-Linear Association	1.308	1	.253
N of Valid Cases	559		

a. 7 cells (38.9%) have expected count less than 5. The minimum expected count is .30.

Chi-Square Tests

	Value	df	Asymptotic Significance (2-sided)
Pearson Chi-Square	172.716[a]	163	.286
Likelihood Ratio	162.135	163	.504
N of Valid Cases	580		

a. 326 cells (99.4%) have expected count less than 5. The minimum expected count is .18.

Chi-Square Tests

	Value	df	Asymptotic Significance (2-sided)
Pearson Chi-Square	56.898[a]	50	.234
Likelihood Ratio	52.671	50	.371
N of Valid Cases	580		

a. 100 cells (98.0%) have expected count less than 5. The minimum expected count is .18.

Chi-Square Tests

	Value	df	Asymptotic Significance (2-sided)
Pearson Chi-Square	60.812[a]	51	.163
Likelihood Ratio	54.975	51	.327
N of Valid Cases	580		

a. 102 cells (98.1%) have expected count less than 5. The minimum expected count is .18.

Independent Samples Test

		Levene's Test for Equality of Variances		t-test for Equality of Means						
									95% Confidence Interval of the Difference	
		F	Sig.	t	df	Sig. (2-tailed)	Mean Difference	Std. Error Difference	Lower	Upper
Age	Equal variances assumed	4.326	.038	1.477	555	.140	1.431	.969	-.472	3.335
	Equal variances not assumed			1.476	548.754	.141	1.431	.970	-.474	3.336

Independent Samples Test

		Levene's Test for Equality of Variances		t-test for Equality of Means						
									95% Confidence Interval of the Difference	
		F	Sig.	t	df	Sig. (2-tailed)	Mean Difference	Std. Error Difference	Lower	Upper
Trait Mindfulness: Observe	Equal variances assumed	11.008	.001	-.806	542	.421	-.26106	.32395	-.89742	.37530
	Equal variances not assumed			-.696	83.294	.488	-.26106	.37491	-1.00670	.48457
Trait Mindfulness: Describe	Equal variances assumed	4.363	.037	-.583	541	.560	-.16933	.29040	-.73977	.40112
	Equal variances not assumed			-.639	95.172	.524	-.16933	.26506	-.69552	.35686
Trait Mindfulness: Act Aware	Equal variances assumed	5.282	.022	-.019	541	.985	-.00422	.22393	-.44410	.43566
	Equal variances not assumed			-.024	112.743	.981	-.00422	.17270	-.34637	.33793
Trait Mindfulness: Non-Judge	Equal variances assumed	.242	.623	1.281	541	.201	.36214	.28264	-.19307	.91734
	Equal variances not assumed			1.313	90.490	.192	.36214	.27571	-.18556	.90983
Trait Mindfulness: Non-React	Equal variances assumed	1.228	.268	-.864	541	.388	-.26867	.31108	-.87975	.34241
	Equal variances not assumed			-.817	85.947	.416	-.26867	.32878	-.92227	.38494
Trait Mindfulness (total)	Equal variances assumed	2.724	.099	-.829	543	.408	-.43858	.52933	-1.47837	.60121
	Equal variances not assumed			-.722	85.028	.472	-.43858	.60769	-1.64682	.76966
Pre Decentering	Equal variances assumed	3.440	.064	-.375	535	.708	-.26502	.70756	-1.65495	1.12491
	Equal variances not assumed			-.355	77.017	.724	-.26502	.74663	-1.75174	1.22170
Pre Curiousity	Equal variances assumed	2.785	.096	.691	536	.490	.56758	.82138	-1.04594	2.18110
	Equal variances not assumed			.625	76.645	.534	.56758	.90799	-1.24059	2.37574
Pre State Mindfulness (total)	Equal variances assumed	2.567	.110	.658	535	.511	.90215	1.37062	-1.79031	3.59460
	Equal variances not assumed			.727	84.860	.469	.90215	1.24141	-1.56616	3.37046
Pre Agency	Equal variances assumed	.576	.448	.373	535	.710	.23970	.64344	-1.02427	1.50367
	Equal variances not assumed			.384	80.923	.702	.23970	.62396	-1.00180	1.48120
Pre Pathways	Equal variances assumed	.314	.576	-1.467	535	.143	-.88487	.60336	-2.07012	.30038
	Equal variances not assumed			-1.495	80.328	.139	-.88487	.59194	-2.06279	.29305
Pre Hope (total)	Equal variances assumed	.620	.431	-.558	535	.577	-.64517	1.15721	-2.91840	1.62807
	Equal variances not assumed			-.582	81.543	.562	-.64517	1.10911	-2.85172	1.56139
Pre Gratitude (total)	Equal variances assumed	4.563	.033	-.168	535	.867	-.07705	.45842	-.97759	.82348
	Equal variances not assumed			-.191	86.733	.849	-.07705	.40369	-.87947	.72536

Appendix Table 7.4.8

Group by time ANOVAs using standard and robust (mixed ANOVA on trimmed means) methods for state mindfulness, curiosity, and decentering

Outcomes		**Standard**	**Robust**
State Mindfulness (TMS-total)	group	$F(1, 472) = 21.94$*** part. $\eta^2 = .04$	$F(1, 251.22) = 14.45$***
	time	$F(1, 472) = 93.16$*** part. $\eta^2 = .17$	$F(1, 165.35) = 99.33$***
	group*time	$F(1, 472) = 130.81$*** part. $\eta^2 = .22$	$F(1, 165.35) = 136.51$***
TMS Curiosity	group	$F(1, 472) = 9.35$** part. $\eta^2 = .84$	$F(1, 252.33) = 6.33$*
	time	$F(1, 472) = 31.53$*** part. $\eta^2 = .06$	$F(1, 160.86) = 33.3$***
	group*time	$F(1, 472) = 56.11$*** part. $\eta^2 = .11$	$F(1, 160.86) = 56.07$***
TMS Decentering	group	$F(1, 472) = 32.68$*** part. $\eta^2 = .07$	$F(1, 250.45) = 26.56$***
	time	$F(1, 472) = 148.47$*** part. $\eta^2 = .24$	$F(1, 166.93) = 133.62$***
	group*time	$F(1, 472) = 184.38$*** part. $\eta^2 = .28$	$F(1, 166.93) = 171.19$***

TMS=Toronto Mindfulness Scale; *$p<.05$; **$p<.01$; ***$p<.001$; part. η^2= partial eta squared.

Appendix Table 7.4.9

The main effect of group in between-group, one-way, standard, and robust ANOVAs conducted separately for pre and post timepoints for state mindfulness, curiosity, and decentering

	PRE	
Outcomes	**Standard**	**Robust**
State Mindfulness (TMS total)	F(1, 472)=0.11	F(1, 282.21) = 1.72
TMS Curiosity	F(1, 472)=0.01	F(1, 283.02) = 0.41
TMS Decentering	F(1, 472)=0.38	F(1, 273.62) = 1.55
	POST	
Outcomes	**Standard**	**Robust**
State Mindfulness (TMS total)	F(1, 472)=77.11*** *d*=0.78	F(1, 268.77) = 67.75***
TMS Curiosity	F(1, 472)=34.36*** *d*=0.54	F(1, 263.09) = 30.94***
TMS Decentering	F(1, 472)=111.79*** *d*=0.97	F(1, 282.16) = 96.28***

SHS=State Hope Scale; GAC=Gratitude Adjective Checklist. ****p*<.001; *d*=Cohen's *d*.

Appendix Table 7.4.10

Main effect of time from within group, standard and robust ANOVAs conducted separately on each group for state mindfulness, curiosity, and decentering

Outcome	**Standard**		**Robust**	
	Mindfulness group	Control group	Mindfulness group	Control group
State Mindfulness (TMS total)	F(1, 236)=117.89*** *d*=0.7	F(1, 236)=14	F(1, 281.71) =93.22 ***	F(1, 282.53) = 0.43
TMS Curiosity	F(1, 236)=45.56*** *d*=0.44	F(1, 236)=15.26	F(1, 269.98) = 33.13***	F(1, 284) = 0.4
TMS Decentering	F(1, 236)=177.21*** *d*=0.87	F(1, 236)=7.63	F(1, 274.1) = 136.65***	F(1, 282.38) = 0.4

SHS=State Hope Scale; GAC=Gratitude Adjective Checklist; *d*=Cohen's *d; ***p*<.001.

Appendix 7.4.11 SPSS ANCOVA output when controlling for previous and current practice experience

State hope (SHS-total)

Tests of Between-Subjects Effects

Dependent Variable: HopeCHANGE

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Corrected Model	3562.146[a]	3	1187.382	55.777	.000	.263
Intercept	506.646	1	506.646	23.800	.000	.048
previousP	188.414	1	188.414	8.851	.003	.019
currentP	2.777	1	2.777	.130	.718	.000
group	3355.257	1	3355.257	157.613	.000	.252
Error	9984.061	469	21.288			
Total	16480.000	473				
Corrected Total	13546.207	472				

a. R Squared = .263 (Adjusted R Squared = .258)

Agency (SHS)

Tests of Between-Subjects Effects

Dependent Variable: AgencyCHANGE

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Corrected Model	953.981[a]	3	317.994	47.098	.000	.232
Intercept	113.038	1	113.038	16.742	.000	.034
previousP	30.144	1	30.144	4.465	.035	.009
currentP	.779	1	.779	.115	.734	.000
group	922.406	1	922.406	136.619	.000	.226
Error	3166.535	469	6.752			
Total	4973.000	473				
Corrected Total	4120.516	472				

a. R Squared = .232 (Adjusted R Squared = .227)

Pathways (SHS)

Tests of Between-Subjects Effects

Dependent Variable: PathwaysCHANGE

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Corrected Model	836.174[a]	3	278.725	48.859	.000	.238
Intercept	141.060	1	141.060	24.727	.000	.050
previousP	67.832	1	67.832	11.891	.001	.025
currentP	.614	1	.614	.108	.743	.000
group	759.192	1	759.192	133.084	.000	.221
Error	2675.467	469	5.705			
Total	4135.000	473				
Corrected Total	3511.641	472				

a. R Squared = .238 (Adjusted R Squared = .233)

State gratitude (GAC)

Tests of Between-Subjects Effects

Dependent Variable: GratitudeCHANGE

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Corrected Model	525.848[a]	3	175.283	49.664	.000	.241
Intercept	68.399	1	68.399	19.380	.000	.040
previousP	44.015	1	44.015	12.471	.000	.026
currentP	3.006	1	3.006	.852	.357	.002
group	484.325	1	484.325	137.227	.000	.226
Error	1655.281	469	3.529			
Total	2563.000	473				
Corrected Total	2181.129	472				

a. R Squared = .241 (Adjusted R Squared = .236)

State mindfulness (TMS-total)

Tests of Between-Subjects Effects

Dependent Variable: TMSchange

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Corrected Model	10384.876[a]	3	3461.625	49.125	.000	.239
Intercept	1729.928	1	1729.928	24.550	.000	.050
previousP	766.102	1	766.102	10.872	.001	.023
currentP	.347	1	.347	.005	.944	.000
group	9431.550	1	9431.550	133.846	.000	.222
Error	33048.464	469	70.466			
Total	50162.000	473				
Corrected Total	43433.340	472				

a. R Squared = .239 (Adjusted R Squared = .234)

Curiosity (TMS)

Tests of Between-Subjects Effects

Dependent Variable: CuriosityChange

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Corrected Model	1785.512[a]	3	595.171	28.917	.000	.156
Intercept	499.872	1	499.872	24.287	.000	.049
previousP	464.935	1	464.935	22.589	.000	.046
currentP	1.379	1	1.379	.067	.796	.000
group	1227.359	1	1227.359	59.632	.000	.113
Error	9653.004	469	20.582			
Total	12123.000	473				
Corrected Total	11438.516	472				

a. R Squared = .156 (Adjusted R Squared = .151)

Decentering (TMS)

Tests of Between-Subjects Effects

Dependent Variable: DecenteringChange

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Corrected Model	3962.984[a]	3	1320.995	61.932	.000	.284
Intercept	328.936	1	328.936	15.421	.000	.032
previousP	37.344	1	37.344	1.751	.186	.004
currentP	.308	1	.308	.014	.904	.000
group	3913.852	1	3913.852	183.493	.000	.281
Error	10003.625	469	21.330			
Total	17134.000	473				
Corrected Total	13966.609	472				

a. R Squared = .284 (Adjusted R Squared = .279)

Appendix 7.4.12 SPSS outputs of ANCOVA results controlling for amount of attention paid (compliance to listening to recording)

State hope (SHS)

Tests of Between-Subjects Effects

Dependent Variable: HopeCHANGE

Source	Type III Sum of Squares	df	Mean Square	F	Sig.
Corrected Model	4003.497[a]	2	2001.749	98.736	.000
Intercept	95.099	1	95.099	4.691	.031
group	2856.839	1	2856.839	140.914	.000
PaidAttention	654.130	1	654.130	32.265	.000
Error	9548.899	471	20.274		
Total	16480.000	474			
Corrected Total	13552.397	473			

a. R Squared = .295 (Adjusted R Squared = .292)

Agency (SHS)

Tests of Between-Subjects Effects

Dependent Variable: AgencyCHANGE

Source	Type III Sum of Squares	df	Mean Square	F	Sig.
Corrected Model	1112.920[a]	2	556.460	87.091	.000
Intercept	28.027	1	28.027	4.387	.037
group	782.840	1	782.840	122.522	.000
PaidAttention	191.146	1	191.146	29.916	.000
Error	3009.395	471	6.389		
Total	4973.000	474			
Corrected Total	4122.314	473			

a. R Squared = .270 (Adjusted R Squared = .267)

Pathways (SHS)

Tests of Between-Subjects Effects

Dependent Variable: PathwaysCHANGE

Source	Type III Sum of Squares	df	Mean Square	F	Sig.
Corrected Model	895.037[a]	2	447.518	80.515	.000
Intercept	19.872	1	19.872	3.575	.059
group	648.727	1	648.727	116.715	.000
PaidAttention	138.073	1	138.073	24.841	.000
Error	2617.919	471	5.558		
Total	4135.000	474			
Corrected Total	3512.956	473			

a. R Squared = .255 (Adjusted R Squared = .252)

State gratitude (GAC)

Tests of Between-Subjects Effects

Dependent Variable: GratitudeCHANGE

Source	Type III Sum of Squares	df	Mean Square	F	Sig.
Corrected Model	637.266[a]	2	318.633	96.982	.000
Intercept	44.074	1	44.074	13.415	.000
group	394.766	1	394.766	120.155	.000
PaidAttention	155.232	1	155.232	47.248	.000
Error	1547.460	471	3.285		
Total	2564.000	474			
Corrected Total	2184.726	473			

a. R Squared = .292 (Adjusted R Squared = .289)

State mindfulness (TMS)

Tests of Between-Subjects Effects

Dependent Variable: TMSchange

Source	Type III Sum of Squares	df	Mean Square	F	Sig.
Corrected Model	13405.903[a]	2	6702.951	105.090	.000
Intercept	1456.982	1	1456.982	22.843	.000
PaidAttention	3977.641	1	3977.641	62.362	.000
group	7515.479	1	7515.479	117.829	.000
Error	30041.633	471	63.783		
Total	50162.000	474			
Corrected Total	43447.536	473			

a. R Squared = .309 (Adjusted R Squared = .306)

Curiosity (TMS)

Tests of Between-Subjects Effects

Dependent Variable: CuriosityChange

Source	Type III Sum of Squares	df	Mean Square	F	Sig.
Corrected Model	2174.866[a]	2	1087.433	55.281	.000
Intercept	511.578	1	511.578	26.007	.000
PaidAttention	959.506	1	959.506	48.777	.000
group	893.522	1	893.522	45.423	.000
Error	9265.094	471	19.671		
Total	12123.000	474			
Corrected Total	11439.960	473			

a. R Squared = .190 (Adjusted R Squared = .187)

Decentering (TMS)

Tests of Between-Subjects Effects

Dependent Variable: DecenteringChange

Source	Type III Sum of Squares	df	Mean Square	F	Sig.
Corrected Model	4962.663[a]	2	2481.331	129.703	.000
Intercept	241.896	1	241.896	12.644	.000
PaidAttention	1037.566	1	1037.566	54.235	.000
group	3270.843	1	3270.843	170.972	.000
Error	9010.628	471	19.131		
Total	17134.000	474			
Corrected Total	13973.291	473			

a. R Squared = .355 (Adjusted R Squared = .352)

Appendix 7.4.13 SPSS output of ANOVA with individuals with low compliance (<5) removed

State hope (SHS-total)

Tests of Within-Subjects Effects

Measure: MEASURE_1

Source		Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
prepost	Sphericity Assumed	1400.749	1	1400.749	124.949	.000	.221
	Greenhouse-Geisser	1400.749	1.000	1400.749	124.949	.000	.221
	Huynh-Feldt	1400.749	1.000	1400.749	124.949	.000	.221
	Lower-bound	1400.749	1.000	1400.749	124.949	.000	.221
prepost * group	Sphericity Assumed	1595.962	1	1595.962	142.363	.000	.244
	Greenhouse-Geisser	1595.962	1.000	1595.962	142.363	.000	.244
	Huynh-Feldt	1595.962	1.000	1595.962	142.363	.000	.244
	Lower-bound	1595.962	1.000	1595.962	142.363	.000	.244
Error(prepost)	Sphericity Assumed	4932.637	440	11.211			
	Greenhouse-Geisser	4932.637	440.000	11.211			
	Huynh-Feldt	4932.637	440.000	11.211			
	Lower-bound	4932.637	440.000	11.211			

Tests of Between-Subjects Effects

Measure: MEASURE_1

Transformed Variable: Average

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Intercept	799724.814	1	799724.814	6081.061	.000	.933
group	3503.565	1	3503.565	26.641	.000	.057
Error	57864.726	440	131.511			

Agency (SHS)

Tests of Within-Subjects Effects

Measure: MEASURE_1

Source		Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
prepost	Sphericity Assumed	409.283	1	409.283	118.810	.000	.213
	Greenhouse-Geisser	409.283	1.000	409.283	118.810	.000	.213
	Huynh-Feldt	409.283	1.000	409.283	118.810	.000	.213
	Lower-bound	409.283	1.000	409.283	118.810	.000	.213
prepost * group	Sphericity Assumed	437.337	1	437.337	126.954	.000	.224
	Greenhouse-Geisser	437.337	1.000	437.337	126.954	.000	.224
	Huynh-Feldt	437.337	1.000	437.337	126.954	.000	.224
	Lower-bound	437.337	1.000	437.337	126.954	.000	.224
Error(prepost)	Sphericity Assumed	1515.735	440	3.445			
	Greenhouse-Geisser	1515.735	440.000	3.445			
	Huynh-Feldt	1515.735	440.000	3.445			
	Lower-bound	1515.735	440.000	3.445			

Tests of Between-Subjects Effects

Measure: MEASURE_1

Transformed Variable: Average

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Intercept	186196.981	1	186196.981	4581.191	.000	.912
group	775.080	1	775.080	19.070	.000	.042
Error	17883.268	440	40.644			

Pathways (SHS)

Tests of Within-Subjects Effects

Measure: MEASURE_1

Source		Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
prepost	Sphericity Assumed	295.697	1	295.697	96.634	.000	.180
	Greenhouse-Geisser	295.697	1.000	295.697	96.634	.000	.180
	Huynh-Feldt	295.697	1.000	295.697	96.634	.000	.180
	Lower-bound	295.697	1.000	295.697	96.634	.000	.180
prepost * group	Sphericity Assumed	362.403	1	362.403	118.433	.000	.212
	Greenhouse-Geisser	362.403	1.000	362.403	118.433	.000	.212
	Huynh-Feldt	362.403	1.000	362.403	118.433	.000	.212
	Lower-bound	362.403	1.000	362.403	118.433	.000	.212
Error(prepost)	Sphericity Assumed	1346.390	440	3.060			
	Greenhouse-Geisser	1346.390	440.000	3.060			
	Huynh-Feldt	1346.390	440.000	3.060			
	Lower-bound	1346.390	440.000	3.060			

Tests of Between-Subjects Effects

Measure: MEASURE_1
Transformed Variable: Average

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Intercept	214154.095	1	214154.095	6144.650	.000	.933
group	982.864	1	982.864	28.201	.000	.060
Error	15334.934	440	34.852			

State gratitude (GAC)

Tests of Within-Subjects Effects

Measure: MEASURE_1

Source		Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
prepost	Sphericity Assumed	196.346	1	196.346	105.994	.000	.194
	Greenhouse-Geisser	196.346	1.000	196.346	105.994	.000	.194
	Huynh-Feldt	196.346	1.000	196.346	105.994	.000	.194
	Lower-bound	196.346	1.000	196.346	105.994	.000	.194
prepost * group	Sphericity Assumed	247.025	1	247.025	133.352	.000	.233
	Greenhouse-Geisser	247.025	1.000	247.025	133.352	.000	.233
	Huynh-Feldt	247.025	1.000	247.025	133.352	.000	.233
	Lower-bound	247.025	1.000	247.025	133.352	.000	.233
Error(prepost)	Sphericity Assumed	815.067	440	1.852			
	Greenhouse-Geisser	815.067	440.000	1.852			
	Huynh-Feldt	815.067	440.000	1.852			
	Lower-bound	815.067	440.000	1.852			

Tests of Between-Subjects Effects

Measure: MEASURE_1
Transformed Variable: Average

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Intercept	93578.977	1	93578.977	4912.838	.000	.918
group	486.009	1	486.009	25.515	.000	.055
Error	8381.051	440	19.048			

State mindfulness (TMS)

Tests of Within-Subjects Effects

Measure: MEASURE_1

Source		Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
prepost	Sphericity Assumed	3737.098	1	3737.098	110.620	.000	.201
	Greenhouse-Geisser	3737.098	1.000	3737.098	110.620	.000	.201
	Huynh-Feldt	3737.098	1.000	3737.098	110.620	.000	.201
	Lower-bound	3737.098	1.000	3737.098	110.620	.000	.201
prepost * group	Sphericity Assumed	4899.451	1	4899.451	145.026	.000	.248
	Greenhouse-Geisser	4899.451	1.000	4899.451	145.026	.000	.248
	Huynh-Feldt	4899.451	1.000	4899.451	145.026	.000	.248
	Lower-bound	4899.451	1.000	4899.451	145.026	.000	.248
Error(prepost)	Sphericity Assumed	14864.667	440	33.783			
	Greenhouse-Geisser	14864.667	440.000	33.783			
	Huynh-Feldt	14864.667	440.000	33.783			
	Lower-bound	14864.667	440.000	33.783			

Tests of Between-Subjects Effects

Measure: MEASURE_1

Transformed Variable: Average

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Intercept	524538.692	1	524538.692	2811.175	.000	.865
group	4986.167	1	4986.167	26.723	.000	.057
Error	82099.837	440	186.591			

Curiosity (TMS)

Tests of Within-Subjects Effects

Measure: MEASURE_1

Source		Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
prepost	Sphericity Assumed	409.640	1	409.640	39.935	.000	.083
	Greenhouse-Geisser	409.640	1.000	409.640	39.935	.000	.083
	Huynh-Feldt	409.640	1.000	409.640	39.935	.000	.083
	Lower-bound	409.640	1.000	409.640	39.935	.000	.083
prepost * group	Sphericity Assumed	635.907	1	635.907	61.993	.000	.123
	Greenhouse-Geisser	635.907	1.000	635.907	61.993	.000	.123
	Huynh-Feldt	635.907	1.000	635.907	61.993	.000	.123
	Lower-bound	635.907	1.000	635.907	61.993	.000	.123
Error(prepost)	Sphericity Assumed	4513.400	440	10.258			
	Greenhouse-Geisser	4513.400	440.000	10.258			
	Huynh-Feldt	4513.400	440.000	10.258			
	Lower-bound	4513.400	440.000	10.258			

Tests of Between-Subjects Effects

Measure: MEASURE_1
Transformed Variable: Average

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Intercept	140185.875	1	140185.875	2351.736	.000	.842
group	721.807	1	721.807	12.109	.001	.027
Error	26228.197	440	59.610			

Decentering (TMS)

Tests of Within-Subjects Effects

Measure: MEASURE_1

Source		Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
prepost	Sphericity Assumed	1696.686	1	1696.686	169.294	.000	.278
	Greenhouse-Geisser	1696.686	1.000	1696.686	169.294	.000	.278
	Huynh-Feldt	1696.686	1.000	1696.686	169.294	.000	.278
	Lower-bound	1696.686	1.000	1696.686	169.294	.000	.278
prepost * group	Sphericity Assumed	2031.980	1	2031.980	202.750	.000	.315
	Greenhouse-Geisser	2031.980	1.000	2031.980	202.750	.000	.315
	Huynh-Feldt	2031.980	1.000	2031.980	202.750	.000	.315
	Lower-bound	2031.980	1.000	2031.980	202.750	.000	.315
Error(prepost)	Sphericity Assumed	4409.727	440	10.022			
	Greenhouse-Geisser	4409.727	440.000	10.022			
	Huynh-Feldt	4409.727	440.000	10.022			
	Lower-bound	4409.727	440.000	10.022			

Tests of Between-Subjects Effects

Measure: MEASURE_1
Transformed Variable: Average

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Intercept	122176.654	1	122176.654	2519.355	.000	.851
group	1887.703	1	1887.703	38.926	.000	.081
Error	21337.895	440	48.495			

Appendix Table 7.4.14

Corrected (99.9% C.I.) interaction effects between group (mindfulness vs. control) and compliance to listening to recordings for the outcomes state mindfulness and subscales curiosity and decentering

	Group x Compliance						
	F(1,469)	**ΔR^2**	***p***	***b***	**SE(boot)**	**t**	**99.9%C.I.**
State Mindfulness (TMS total)	96.04	0.07	<.001	5.78	0.5	11.48	[4.11, 7.44]
TMS Curiosity	70.52	0.06	<.001	2.69	0.28	9.7	[1.77, 3.61]
TMS Decentering	92.18	0.07	<.001	3.11	0.28	11.23	[2.2, 4.03]

ΔR^2=adjusted R^2 change; *b*=effect size of moderation; SE(boot)=bootstrapped Standard Error; 95% C.I.= 95% Confidence Intervals.

Appendix 7.4.15 SPSS output of ANOVA with individuals with low levels of following guidance (<5) removed

State hope (SHS-total)

Tests of Within-Subjects Effects

Measure: MEASURE_1

Source		Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
prepost	Sphericity Assumed	1496.862	1	1496.862	139.055	.000	.234
	Greenhouse-Geisser	1496.862	1.000	1496.862	139.055	.000	.234
	Huynh-Feldt	1496.862	1.000	1496.862	139.055	.000	.234
	Lower-bound	1496.862	1.000	1496.862	139.055	.000	.234
prepost * group	Sphericity Assumed	1705.899	1	1705.899	158.474	.000	.258
	Greenhouse-Geisser	1705.899	1.000	1705.899	158.474	.000	.258
	Huynh-Feldt	1705.899	1.000	1705.899	158.474	.000	.258
	Lower-bound	1705.899	1.000	1705.899	158.474	.000	.258
Error(prepost)	Sphericity Assumed	4897.851	455	10.765			
	Greenhouse-Geisser	4897.851	455.000	10.765			
	Huynh-Feldt	4897.851	455.000	10.765			
	Lower-bound	4897.851	455.000	10.765			

Tests of Between-Subjects Effects

Measure: MEASURE_1
Transformed Variable: Average

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Intercept	832847.321	1	832847.321	6310.060	.000	.933
group	3583.526	1	3583.526	27.151	.000	.056
Error	60054.189	455	131.987			

Agency (SHS)

Tests of Within-Subjects Effects

Measure: MEASURE_1

Source		Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
prepost	Sphericity Assumed	434.170	1	434.170	132.243	.000	.225
	Greenhouse-Geisser	434.170	1.000	434.170	132.243	.000	.225
	Huynh-Feldt	434.170	1.000	434.170	132.243	.000	.225
	Lower-bound	434.170	1.000	434.170	132.243	.000	.225
prepost * group	Sphericity Assumed	469.387	1	469.387	142.969	.000	.239
	Greenhouse-Geisser	469.387	1.000	469.387	142.969	.000	.239
	Huynh-Feldt	469.387	1.000	469.387	142.969	.000	.239
	Lower-bound	469.387	1.000	469.387	142.969	.000	.239
Error(prepost)	Sphericity Assumed	1493.823	455	3.283			
	Greenhouse-Geisser	1493.823	455.000	3.283			
	Huynh-Feldt	1493.823	455.000	3.283			
	Lower-bound	1493.823	455.000	3.283			

Tests of Between-Subjects Effects

Measure: MEASURE_1

Transformed Variable: Average

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Intercept	192500.180	1	192500.180	4626.699	.000	.910
group	892.412	1	892.412	21.449	.000	.045
Error	18930.903	455	41.606			

Pathways (SHS)

Tests of Within-Subjects Effects

Measure: MEASURE_1

Source		Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
prepost	Sphericity Assumed	318.713	1	318.713	108.074	.000	.192
	Greenhouse-Geisser	318.713	1.000	318.713	108.074	.000	.192
	Huynh-Feldt	318.713	1.000	318.713	108.074	.000	.192
	Lower-bound	318.713	1.000	318.713	108.074	.000	.192
prepost * group	Sphericity Assumed	385.619	1	385.619	130.761	.000	.223
	Greenhouse-Geisser	385.619	1.000	385.619	130.761	.000	.223
	Huynh-Feldt	385.619	1.000	385.619	130.761	.000	.223
	Lower-bound	385.619	1.000	385.619	130.761	.000	.223
Error(prepost)	Sphericity Assumed	1341.810	455	2.949			
	Greenhouse-Geisser	1341.810	455.000	2.949			
	Huynh-Feldt	1341.810	455.000	2.949			
	Lower-bound	1341.810	455.000	2.949			

Tests of Between-Subjects Effects

Measure: MEASURE_1
Transformed Variable: Average

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Intercept	224539.761	1	224539.761	6414.132	.000	.934
group	899.358	1	899.358	25.691	.000	.053
Error	15928.202	455	35.007			

State Gratitude (GAC)

Tests of Within-Subjects Effects

Measure: MEASURE_1

Source		Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
prepost	Sphericity Assumed	203.908	1	203.908	114.054	.000	.200
	Greenhouse-Geisser	203.908	1.000	203.908	114.054	.000	.200
	Huynh-Feldt	203.908	1.000	203.908	114.054	.000	.200
	Lower-bound	203.908	1.000	203.908	114.054	.000	.200
prepost * group	Sphericity Assumed	256.017	1	256.017	143.201	.000	.239
	Greenhouse-Geisser	256.017	1.000	256.017	143.201	.000	.239
	Huynh-Feldt	256.017	1.000	256.017	143.201	.000	.239
	Lower-bound	256.017	1.000	256.017	143.201	.000	.239
Error(prepost)	Sphericity Assumed	813.460	455	1.788			
	Greenhouse-Geisser	813.460	455.000	1.788			
	Huynh-Feldt	813.460	455.000	1.788			
	Lower-bound	813.460	455.000	1.788			

Tests of Between-Subjects Effects

Measure: MEASURE_1
Transformed Variable: Average

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Intercept	98394.466	1	98394.466	4987.807	.000	.916
group	448.939	1	448.939	22.758	.000	.048
Error	8975.785	455	19.727			

State mindfulness (TMS-total)

Tests of Within-Subjects Effects

Measure: MEASURE_1

Source		Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
prepost	Sphericity Assumed	3860.625	1	3860.625	115.878	.000	.203
	Greenhouse-Geisser	3860.625	1.000	3860.625	115.878	.000	.203
	Huynh-Feldt	3860.625	1.000	3860.625	115.878	.000	.203
	Lower-bound	3860.625	1.000	3860.625	115.878	.000	.203
prepost * group	Sphericity Assumed	5278.109	1	5278.109	158.424	.000	.258
	Greenhouse-Geisser	5278.109	1.000	5278.109	158.424	.000	.258
	Huynh-Feldt	5278.109	1.000	5278.109	158.424	.000	.258
	Lower-bound	5278.109	1.000	5278.109	158.424	.000	.258
Error(prepost)	Sphericity Assumed	15158.906	455	33.316			
	Greenhouse-Geisser	15158.906	455.000	33.316			
	Huynh-Feldt	15158.906	455.000	33.316			
	Lower-bound	15158.906	455.000	33.316			

Tests of Between-Subjects Effects

Measure: MEASURE_1

Transformed Variable: Average

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Intercept	550502.180	1	550502.180	2947.293	.000	.866
group	4649.974	1	4649.974	24.895	.000	.052
Error	84985.947	455	186.782			

Curiosity (TMS)

Tests of Within-Subjects Effects

Measure: MEASURE_1

Source		Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
prepost	Sphericity Assumed	431.145	1	431.145	42.762	.000	.086
	Greenhouse-Geisser	431.145	1.000	431.145	42.762	.000	.086
	Huynh-Feldt	431.145	1.000	431.145	42.762	.000	.086
	Lower-bound	431.145	1.000	431.145	42.762	.000	.086
prepost * group	Sphericity Assumed	719.263	1	719.263	71.339	.000	.136
	Greenhouse-Geisser	719.263	1.000	719.263	71.339	.000	.136
	Huynh-Feldt	719.263	1.000	719.263	71.339	.000	.136
	Lower-bound	719.263	1.000	719.263	71.339	.000	.136
Error(prepost)	Sphericity Assumed	4587.485	455	10.082			
	Greenhouse-Geisser	4587.485	455.000	10.082			
	Huynh-Feldt	4587.485	455.000	10.082			
	Lower-bound	4587.485	455.000	10.082			

Tests of Between-Subjects Effects

Measure: MEASURE_1
Transformed Variable: Average

Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Intercept	146533.446	1	146533.446	2407.168	.000	.841
group	659.280	1	659.280	10.830	.001	.023
Error	27697.573	455	60.874			

Decentering (TMS)

Tests of Within-Subjects Effects

Measure: MEASURE_1

Source		Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
prepost	Sphericity Assumed	1737.128	1	1737.128	178.405	.000	.282
	Greenhouse-Geisser	1737.128	1.000	1737.128	178.405	.000	.282
	Huynh-Feldt	1737.128	1.000	1737.128	178.405	.000	.282
	Lower-bound	1737.128	1.000	1737.128	178.405	.000	.282
prepost * group	Sphericity Assumed	2128.945	1	2128.945	218.644	.000	.325
	Greenhouse-Geisser	2128.945	1.000	2128.945	218.644	.000	.325
	Huynh-Feldt	2128.945	1.000	2128.945	218.644	.000	.325
	Lower-bound	2128.945	1.000	2128.945	218.644	.000	.325
Error(prepost)	Sphericity Assumed	4430.342	455	9.737			
	Greenhouse-Geisser	4430.342	455.000	9.737			
	Huynh-Feldt	4430.342	455.000	9.737			
	Lower-bound	4430.342	455.000	9.737			

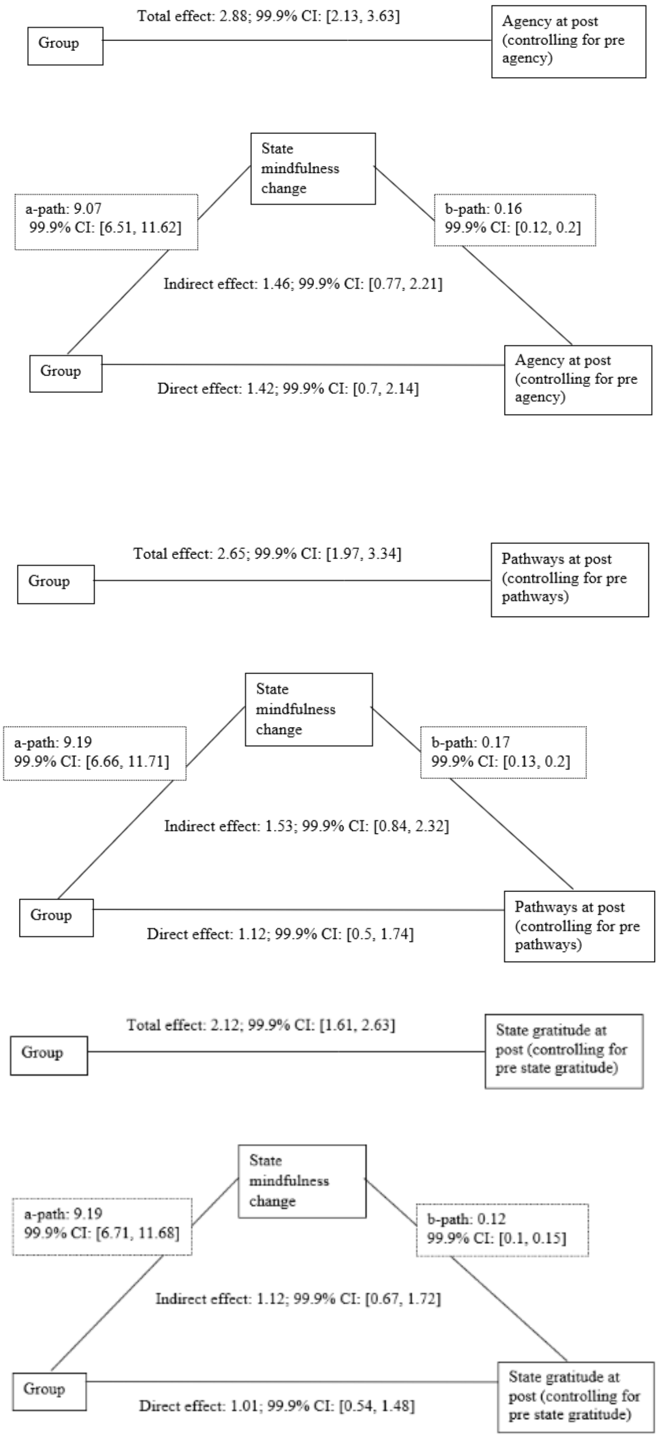
Tests of Between-Subjects Effects

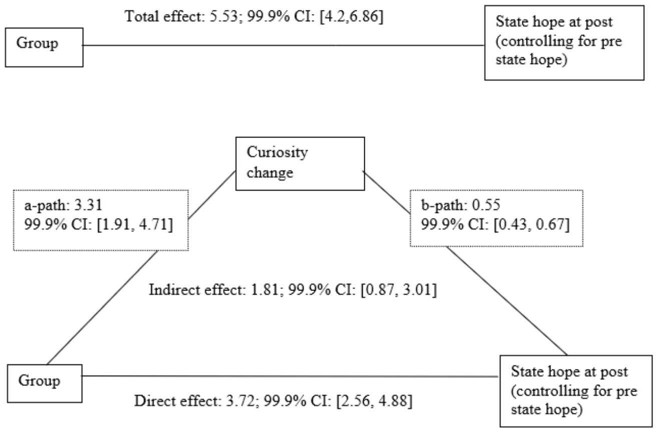
Measure: MEASURE_1
Transformed Variable: Average

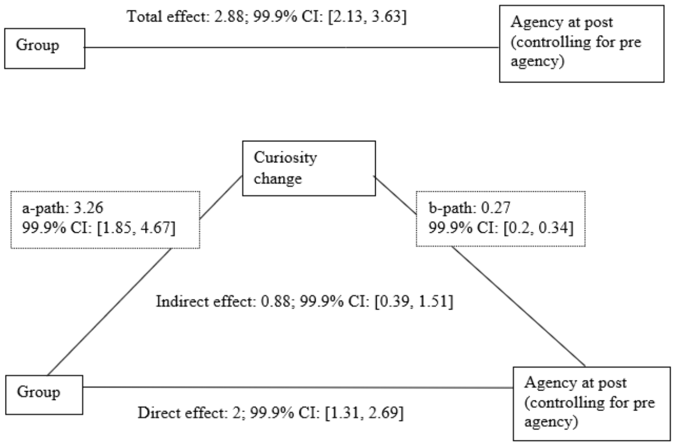
Source	Type III Sum of Squares	df	Mean Square	F	Sig.	Partial Eta Squared
Intercept	128775.107	1	128775.107	2652.746	.000	.854
group	1781.286	1	1781.286	36.694	.000	.075
Error	22087.554	455	48.544			

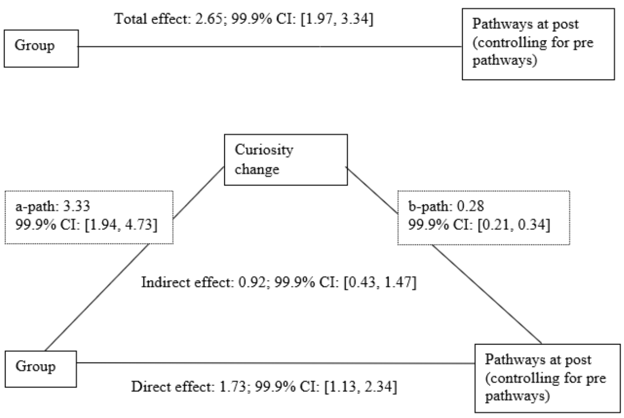
Appendix Figure 7.4.16

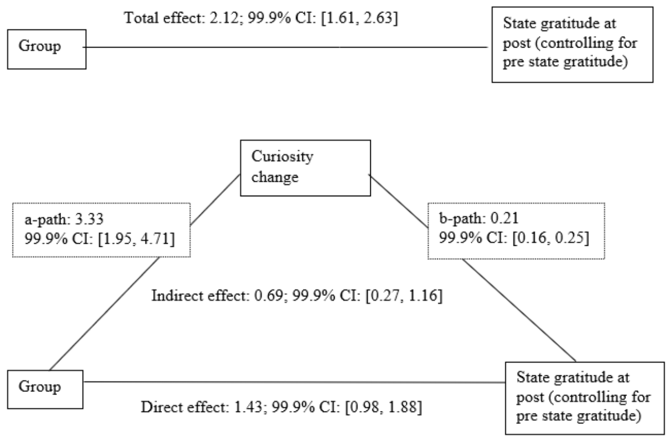
*Corrected mediation models for post-study outcome (state hope (SHS-total); agency (SHS); pathways (SHS); gratitude (GAC)) with group allocation as predictor, state mindfulness change (total-scale TMS; curiosity (TMS); decentering (TMS)) as mediator and baseline outcome (state hope; agency; pathways; gratitude) as covariate. Top diagram: total effect when excluding mediator, bottom diagram: indirect and direct effects when including mediator (*p<.05; **p<.01; ***p<.001; 99.9% CI=99.9% Confidence Intervals)*

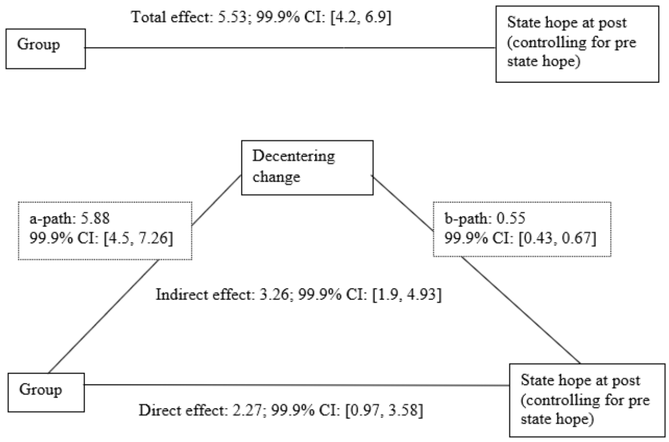


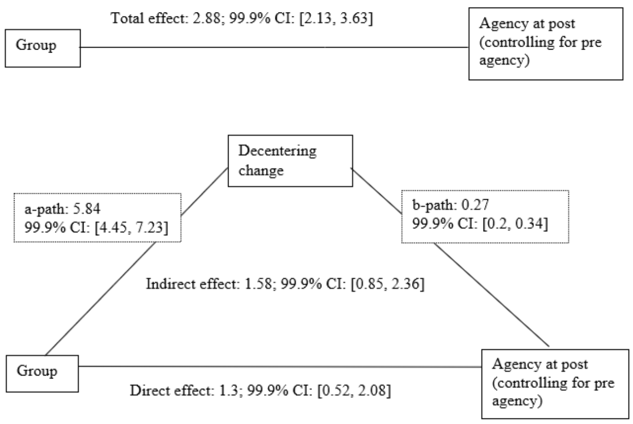

Total effect: 5.53; 99.9% CI: [4.2,6.86]
Group
State hope at post (controlling for pre state hope)
Curiosity change
a-path: 3.31
99.9% CI: [1.91, 4.71]
b-path: 0.55
99.9% CI: [0.43, 0.67]
Indirect effect: 1.81; 99.9% CI: [0.87, 3.01]
Group
State hope at post (controlling for pre state hope)
Direct effect: 3.72; 99.9% CI: [2.56, 4.88]

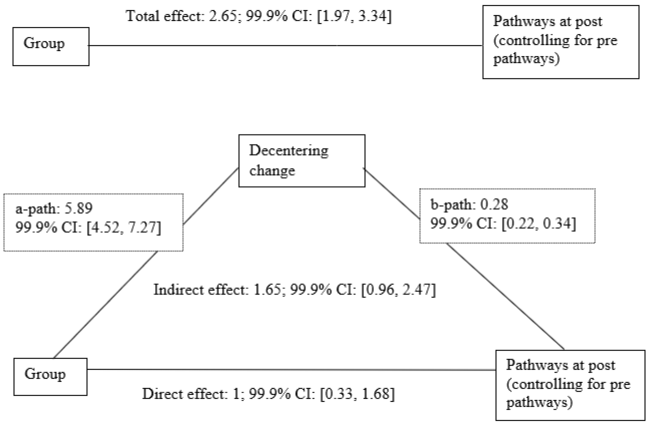

Total effect: 2.88; 99.9% CI: [2.13, 3.63]
Group
Agency at post (controlling for pre agency)
Curiosity change
a-path: 3.26
99.9% CI: [1.85, 4.67]
b-path: 0.27
99.9% CI: [0.2, 0.34]
Indirect effect: 0.88; 99.9% CI: [0.39, 1.51]
Group
Agency at post (controlling for pre agency)
Direct effect: 2; 99.9% CI: [1.31, 2.69]

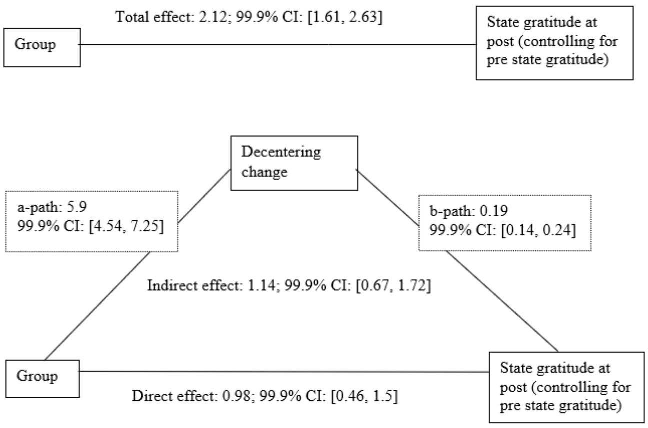

Total effect: 2.65; 99.9% CI: [1.97, 3.34]
Group
Pathways at post (controlling for pre pathways)
Curiosity change
a-path: 3.33
99.9% CI: [1.94, 4.73]
b-path: 0.28
99.9% CI: [0.21, 0.34]
Indirect effect: 0.92; 99.9% CI: [0.43, 1.47]
Group
Pathways at post (controlling for pre pathways)
Direct effect: 1.73; 99.9% CI: [1.13, 2.34]


Total effect: 2.12; 99.9% CI: [1.61, 2.63]
Group
State gratitude at post (controlling for pre state gratitude)
Curiosity change
a-path: 3.33
99.9% CI: [1.95, 4.71]
b-path: 0.21
99.9% CI: [0.16, 0.25]
Indirect effect: 0.69; 99.9% CI: [0.27, 1.16]
Group
State gratitude at post (controlling for pre state gratitude)
Direct effect: 1.43; 99.9% CI: [0.98, 1.88]


Total effect: 5.53; 99.9% CI: [4.2, 6.9]
Group
State hope at post (controlling for pre state hope)
Decentering change
a-path: 5.88
99.9% CI: [4.5, 7.26]
b-path: 0.55
99.9% CI: [0.43, 0.67]
Indirect effect: 3.26; 99.9% CI: [1.9, 4.93]
Group
State hope at post (controlling for pre state hope)
Direct effect: 2.27; 99.9% CI: [0.97, 3.58]

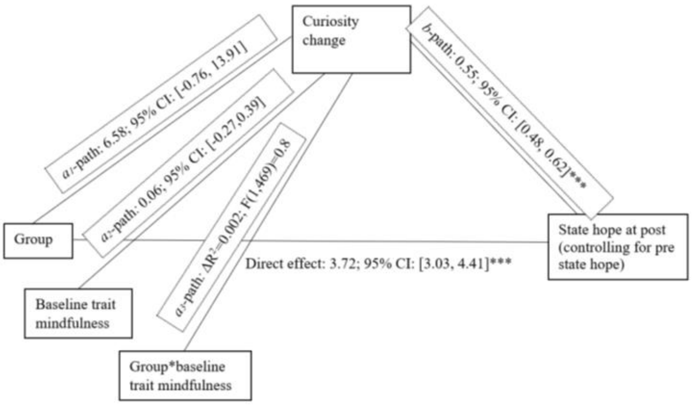

Total effect: 2.88; 99.9% CI: [2.13, 3.63]
Group
Agency at post (controlling for pre agency)
Decentering change
a-path: 5.84
99.9% CI: [4.45, 7.23]
b-path: 0.27
99.9% CI: [0.2, 0.34]
Indirect effect: 1.58; 99.9% CI: [0.85, 2.36]
Group
Agency at post (controlling for pre agency)
Direct effect: 1.3; 99.9% CI: [0.52, 2.08]

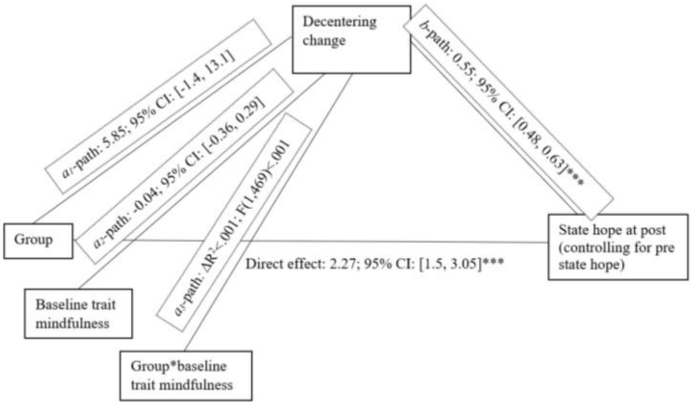

Total effect: 2.65; 99.9% CI: [1.97, 3.34]
Group
Pathways at post (controlling for pre pathways)
Decentering change
a-path: 5.89
99.9% CI: [4.52, 7.27]
b-path: 0.28
99.9% CI: [0.22, 0.34]
Indirect effect: 1.65; 99.9% CI: [0.96, 2.47]
Group
Direct effect: 1; 99.9% CI: [0.33, 1.68]
Pathways at post (controlling for pre pathways)

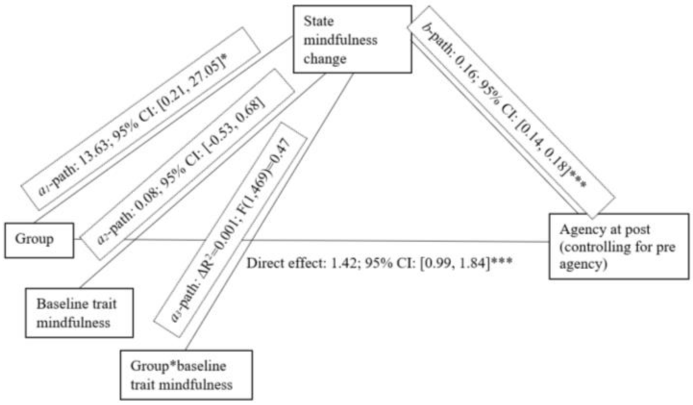

Total effect: 2.12; 99.9% CI: [1.61, 2.63]
Group
State gratitude at post (controlling for pre state gratitude)
Decentering change
a-path: 5.9
99.9% CI: [4.54, 7.25]
b-path: 0.19
99.9% CI: [0.14, 0.24]
Indirect effect: 1.14; 99.9% CI: [0.67, 1.72]
Group
Direct effect: 0.98; 99.9% CI: [0.46, 1.5]
State gratitude at post (controlling for pre state gratitude)

Appendix Figure 7.4.17

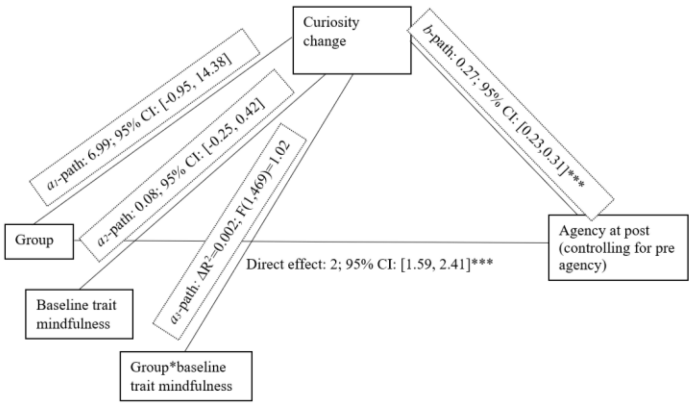
*Moderated mediation models for post post-study outcome (state hope (SHS-total); agency (SHS); pathways (SHS); gratitude (GAC)) with group allocation as predictor, state mindfulness change (total-scale TMS; curiosity (TMS); decentering (TMS)) as mediator, baseline trait mindfulness (FFMQ-15 total) as moderator and baseline outcome (state hope; agency; pathways; gratitude) as covariate (95% CI=95% Confidence Intervals; *p<.05; ***p<.001; ΔR^2= adjusted R^2 change)*



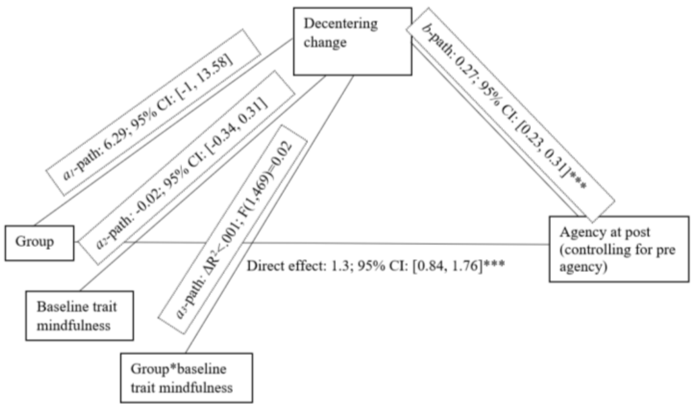




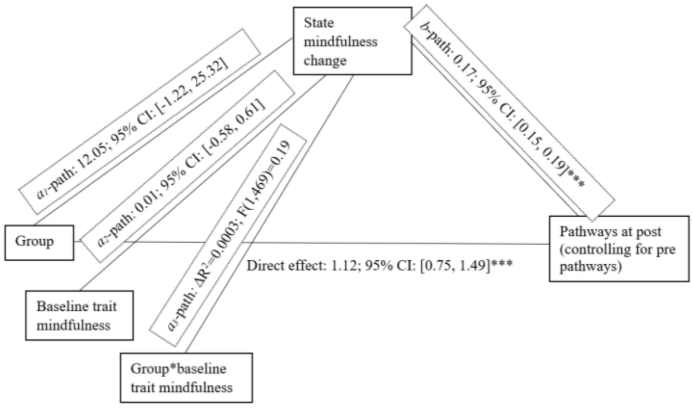
State mindfulness change
a1-path: 13.63; 95% CI: [0.21, 27.05]*
a2-path: 0.08; 95% CI: [-0.53, 0.68]
a3-path: ΔR=0.001; F(1,469)=0.47
b-path: 0.16; 95% CI: [0.14, 0.18]***
Group
Agency at post (controlling for pre agency)
Direct effect: 1.42; 95% CI: [0.99, 1.84]***
Baseline trait mindfulness
Group*baseline trait mindfulness

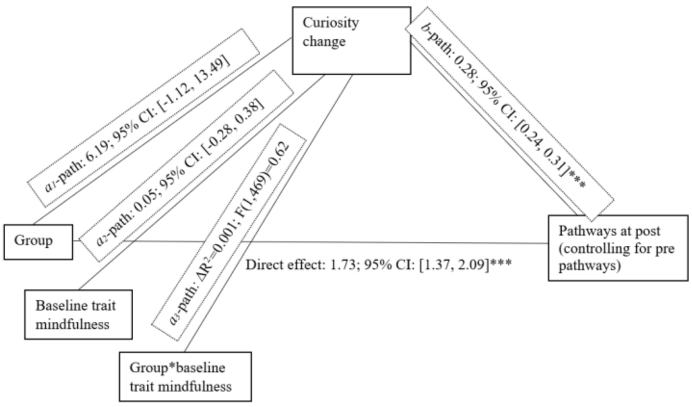


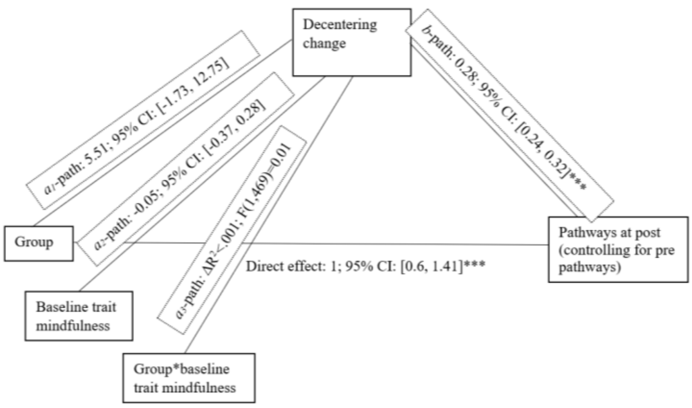
Curiosity change
a1-path: 6.99; 95% CI: [-0.95, 14.38]
a2-path: 0.08; 95% CI: [-0.25, 0.42]
a3-path: ΔR2=0.002; F(1,469)=1.02
b-path: 0.27; 95% CI: [0.23,0.31]***
Group
Agency at post (controlling for pre agency)
Direct effect: 2; 95% CI: [1.59, 2.41]***
Baseline trait mindfulness
Group*baseline trait mindfulness

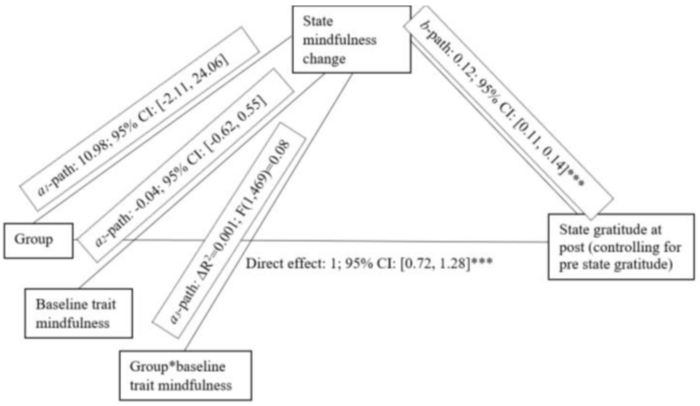


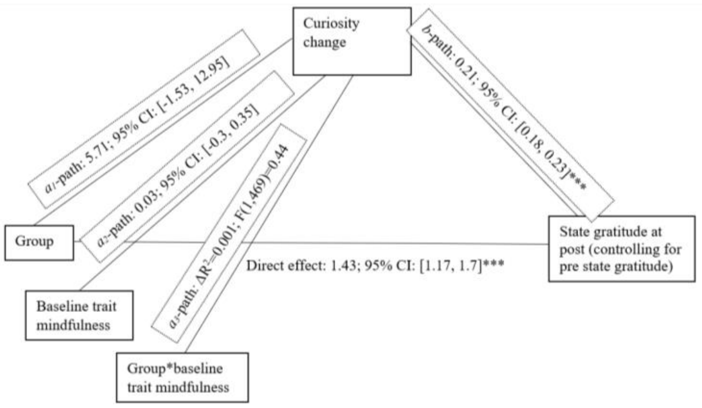
Decentering change
a1-path: 6.29; 95% CI: [-1, 13.58]
a2-path: -0.02; 95% CI: [-0.34, 0.31]
a3-path: ΔR2<.001; F(1,469)=0.02
b-path: 0.27; 95% CI: [0.23, 0.31]***
Group
Agency at post (controlling for pre agency)
Direct effect: 1.3; 95% CI: [0.84, 1.76]***
Baseline trait mindfulness
Group*baseline trait mindfulness

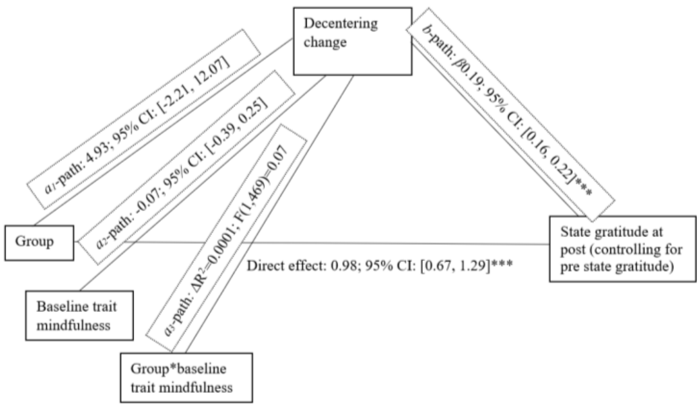

State mindfulness change
a_1-path: 12.05; 95% CI: [-1.22, 25.32]
a_2-path: 0.01; 95% CI: [-0.58, 0.61]
a_3-path: ΔR^2=0.0003; F(1,469)=0.19
b-path: 0.17; 95% CI: [0.15, 0.19]***
Group
Baseline trait mindfulness
Group*baseline trait mindfulness
Direct effect: 1.12; 95% CI: [0.75, 1.49]***
Pathways at post (controlling for pre pathways)


Curiosity change
a_1-path: 6.19; 95% CI: [-1.12, 13.49]
a_2-path: 0.05; 95% CI: [-0.28, 0.38]
a_3-path: ΔR^2=0.001; F(1,469)=0.62
b-path: 0.28; 95% CI: [0.24, 0.31]***
Group
Baseline trait mindfulness
Group*baseline trait mindfulness
Direct effect: 1.73; 95% CI: [1.37, 2.09]***
Pathways at post (controlling for pre pathways)


Decentering change
a_1-path: 5.51; 95% CI: [-1.73, 12.75]
a_2-path: -0.05; 95% CI: [-0.37, 0.28]
a_3-path: ΔR^2<.001; F(1,469)=0.01
b-path: 0.28; 95% CI: [0.24, 0.32]***
Group
Baseline trait mindfulness
Group*baseline trait mindfulness
Direct effect: 1; 95% CI: [0.6, 1.41]***
Pathways at post (controlling for pre pathways)

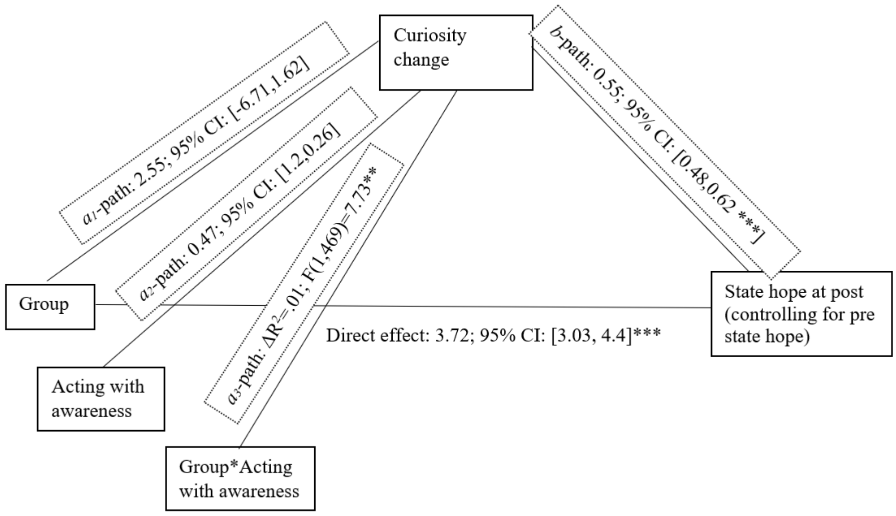

State mindfulness change
a_1-path: 10.98; 95% CI: [-2.11, 24.06]
a_2-path: -0.04; 95% CI: [-0.62, 0.55]
a_3-path: ΔR^2=0.001; F(1,469)=0.08
b-path: 0.12; 95% CI: [0.11, 0.14]***
Group
Baseline trait mindfulness
Group*baseline trait mindfulness
Direct effect: 1; 95% CI: [0.72, 1.28]***
State gratitude at post (controlling for pre state gratitude)

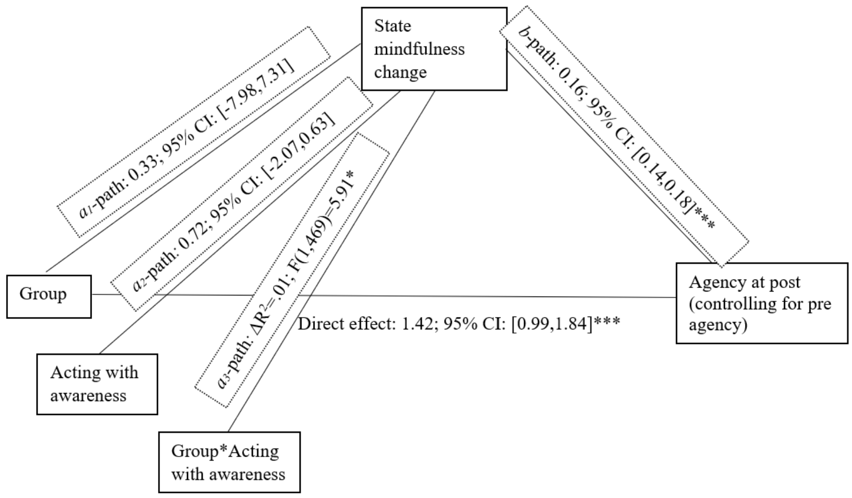

Curiosity change
a_1-path: 5.71; 95% CI: [-1.53, 12.95]
a_2-path: 0.03; 95% CI: [-0.3, 0.35]
a_3-path: ΔR^2=0.001; F(1,469)=0.44
b-path: 0.21; 95% CI: [0.18, 0.23]***
Group
Baseline trait mindfulness
Group*baseline trait mindfulness
Direct effect: 1.43; 95% CI: [1.17, 1.7]***
State gratitude at post (controlling for pre state gratitude)

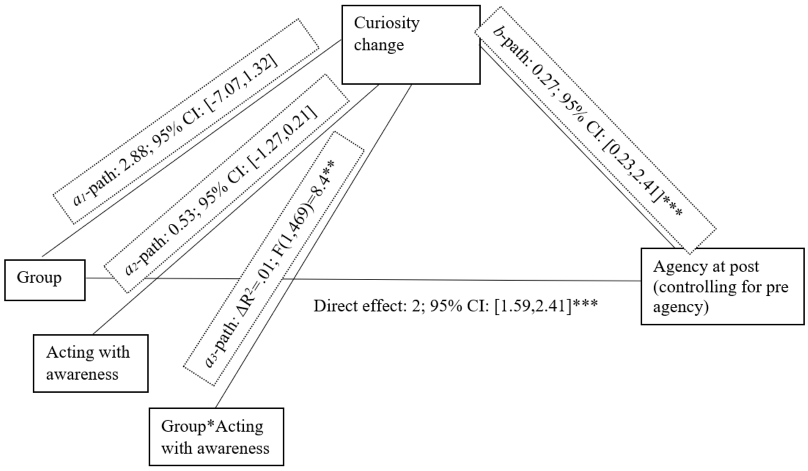

Decentering change
a_1-path: 4.93; 95% CI: [-2.21, 12.07]
a_2-path: -0.07; 95% CI: [-0.39, 0.25]
a_3-path: ΔR^2=0.0001; F(1,469)=0.07
b-path: β0.19; 95% CI: [0.16, 0.22]***
Group
Baseline trait mindfulness
Group*baseline trait mindfulness
Direct effect: 0.98; 95% CI: [0.67, 1.29]***
State gratitude at post (controlling for pre state gratitude)

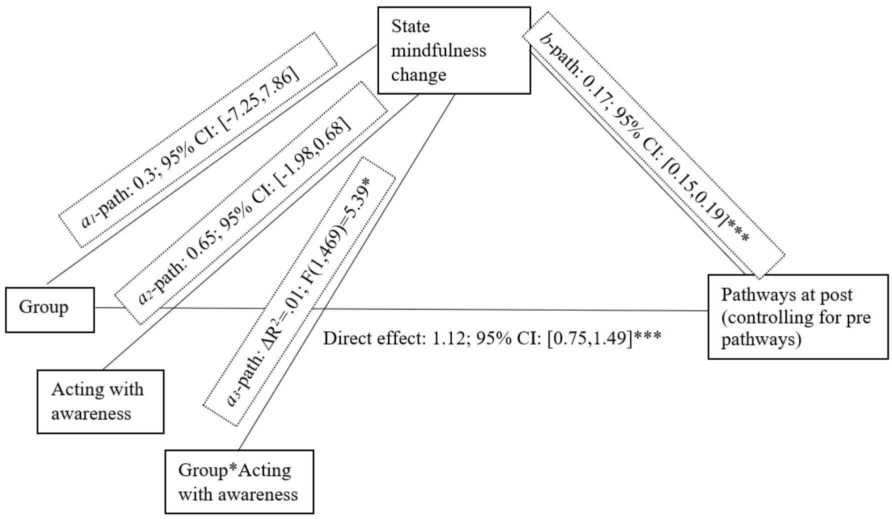
Appendix Figure 7.4.18

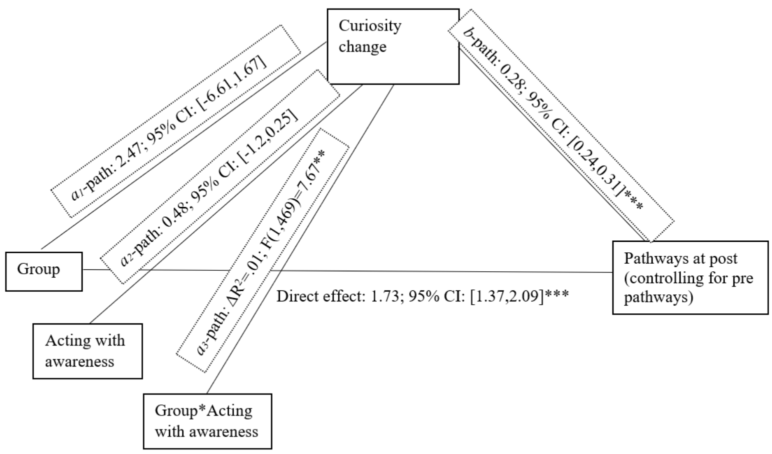
*Significant moderated mediation models for post post-study outcome (state hope (SHS-total); agency (SHS); pathways (SHS); gratitude (GAC)) with group allocation as predictor, state mindfulness change (total-scale TMS; curiosity (TMS); decentering (TMS)) as mediator, baseline acting with awareness (FFMQ-15) as moderator and baseline outcome (state hope; agency; pathways; gratitude) as covariate (95% CI=95% Confidence Intervals; *p<.05; ***p<.001; ΔR^2= adjusted R^2 change)*

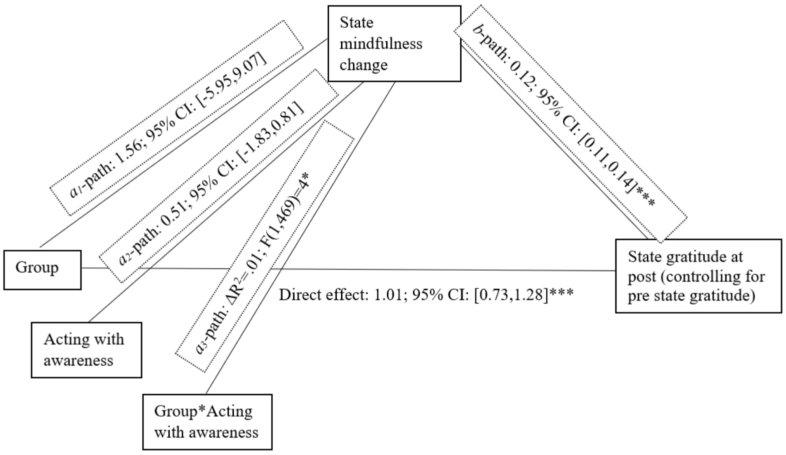


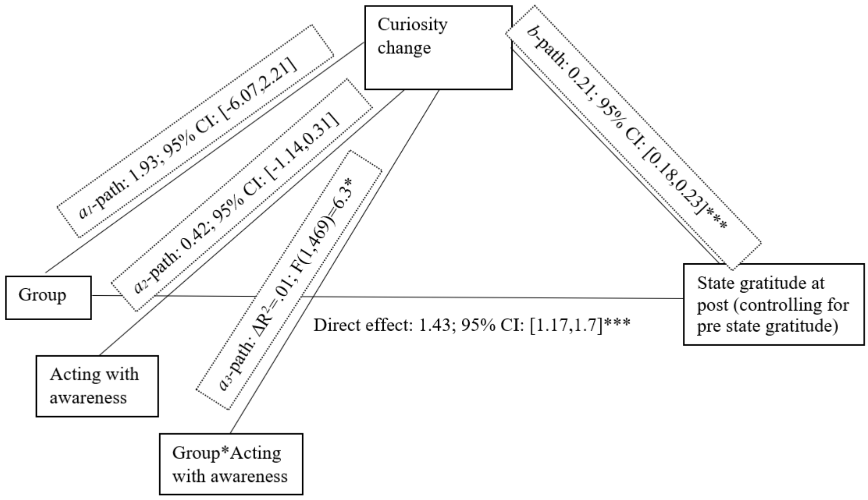



Curiosity change
a_1-path: 2.88; 95% CI: [-7.07,1.32]
a_2-path: 0.53; 95% CI: [-1.27,0.21]
a_3-path: ΔR^2=.01; F(1,469)=8.4**
b-path: 0.27; 95% CI: [0.23,2.41]***
Group
Acting with awareness
Group*Acting with awareness
Agency at post (controlling for pre agency)
Direct effect: 2; 95% CI: [1.59,2.41]***


State mindfulness change
a_1-path: 0.3; 95% CI: [-7.25,7.86]
a_2-path: 0.65; 95% CI: [-1.98,0.68]
a_3-path: ΔR^2=.01; F(1,469)=5.39*
b-path: 0.17; 95% CI: [0.15,0.19]***
Group
Acting with awareness
Group*Acting with awareness
Pathways at post (controlling for pre pathways)
Direct effect: 1.12; 95% CI: [0.75,1.49]***


Curiosity change
a_1-path: 2.47; 95% CI: [-6.61,1.67]
a_2-path: 0.48; 95% CI: [-1.2,0.25]
a_3-path: ΔR^2=.01; F(1,469)=7.67**
b-path: 0.28; 95% CI: [0.24,0.31]***
Group
Acting with awareness
Group*Acting with awareness
Pathways at post (controlling for pre pathways)
Direct effect: 1.73; 95% CI: [1.37,2.09]***


State mindfulness change
a1-path: 1.56; 95% CI: [-5.95,9.07]
a2-path: 0.51; 95% CI: [-1.83,0.81]
a3-path: ΔR²=.01; F(1,469)=4*
b-path: 0.12; 95% CI: [0.11,0.14]***
Group
Acting with awareness
Group*Acting with awareness
Direct effect: 1.01; 95% CI: [0.73,1.28]***
State gratitude at post (controlling for pre state gratitude)


Curiosity change
a1-path: 1.93; 95% CI: [-6.07,2.21]
a2-path: 0.42; 95% CI: [-1.14,0.31]
a3-path: ΔR²=.01; F(1,469)=6.3*
b-path: 0.21; 95% CI: [0.18,0.23]***
Group
Acting with awareness
Group*Acting with awareness
Direct effect: 1.43; 95% CI: [1.17,1.7]***
State gratitude at post (controlling for pre state gratitude)