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# The effects of mindfulness meditation on attention regulation

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# The effects of mindfulness meditation on attention regulation

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# Author’s declaration

At no time during the registration for the degree of Doctor of Philosophy has the author been registered for any other University award without prior agreement of the Doctoral College Quality Sub-Committee. Work submitted for this research degree at the University of Plymouth has not formed part of any other degree either at the University of Plymouth or at another establishment.

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# Abstract


## The effects of mindfulness meditation on attention regulation

Paul Sharpe

The research in this thesis tested the claim that mindfulness meditation regulates attention. Attention regulation was operationalised as an improvement on alerting, orienting or executive attention measured using the Attention Network Test (ANT), or a reduction in the attentional blink (AB) effect, measured using a Rapid Serial Visual Presentation (RSVP) task.

Chapter 1 reviews the relevant literature. Chapters 2 and 3 report five experiments which tested the effects of Focused Attention Meditation (FAM) on the ANT. In Chapter 2, no effects of brief FAM were found on the ANT in novices (Experiment 1), or long-term meditators (Experiments 2 and 3). In Chapter 3, no effects were found on the ANT after novices completed either four weeks (Experiment 4), or eight weeks (Experiment 5) of FAM training. Motivated by the null findings in these experiments, Chapter 4 reports a systematic review and meta-analysis of studies which have tested the effects of meditation on the ANT. These meta-analyses found mindfulness meditation to have small improvements on executive attention in novices and long-term meditators, but there was limited evidence of improvements in alerting or orienting.

Chapter 5 reports a precise replication of Colzato et al. (2015), which tested the effects of brief FAM and Open Monitoring Meditation (OMM) on the AB in novices. The finding that OMM reduces the AB to a greater extent than FAM was not replicated, but overall target accuracy on the RSVP task was greater in FAM and OMM relative to a relaxation control condition. Chapter 6 reports a systematic review and meta-analysis of all studies which have tested the effects of meditation on the AB. This found small AB reductions in novices and long-term meditators.

Chapter 7 concludes that mindfulness meditation can produce small improvements to executive control, and that the RSVP task used to measure the AB may be more sensitive than the ANT at detecting these effects.

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# 1

# Mindfulness, attention regulation and wellbeing

## Contents



## 1.1 Introduction

This thesis reviews and extends evidence which is often used to support the theoretical claim that mindfulness meditation regulates attention. Improved attention regulation is one mechanism by which mindfulness meditation is thought to improve health and wellbeing. The introduction begins with a summary of the health and wellbeing benefits associated with mindfulness and meditation. Mindfulness is defined and contrasted with attention and mind wandering. Mindfulness measures are compared and contrasted. Two types of mindfulness mediation, focused attention meditation (FAM) and open monitoring meditation (OMM) are described as methods of inducing mindfulness. The effects of mindfulness on attention are discussed, and the Attention Network Test (ANT), and attentional blink (AB) are evaluated as operational measures of attention regulation.

The effects of mindfulness meditation on attention regulation were tested using experiments and meta-analyses. Chapter 2 reports three novel experiments which tested whether a single, brief period of FAM improved performance on the ANT, in novices and long-term meditators. In contrast with Chapter 2, Chapter 3 describes experiments in which novices received meditation training, and meditated regularly. One experiment tested ANT performance after four weeks of FAM training, partially replicating Tsai and Chou (2016, Experiment 2). The other was a pre-registered replication of Becerra et al. (2016), which tested ANT performance after eight weeks of FAM training. Chapter 4 is a systematic review and meta-analysis of the effects of mindfulness meditation on the ANT, including the experiments in Chapters 2 and 3. Chapter 5 shifts the focus from the ANT to the AB. It describes a direct replication of Colzato et al. (2015), which tests whether FAM and OMM have different effects on the AB in first-time meditators. Chapter 6 is a systematic review and meta-analysis of the effects of mindfulness meditation on the AB, including the experiment in Chapter 5.

To provide some initial context, this chapter begins with a summary of the role played by mindfulness in health and wellbeing. This is followed by a description of what mindfulness is, and how it contrasts with mind wandering. Psychological mindfulness measures are described, before explaining how a mindful state can be induced using different types of meditation. In preparation for the empirical research in Chapters 2–6, this chapter ends with a review of existing evidence used to support claims that mindfulness meditation regulates attention.

## 1.2 Mindfulness, health and wellbeing

Mindfulness is a term which has become associated with health and wellbeing. People might first experience mindfulness meditation in a therapeutic context, a yoga class or a meditation group. In this respect it has become somewhat detached from its origins within Buddhist monasteries, where it is necessary, but insufficient for navigating a longer path, leading to permanent liberation from suffering[1]. Buddhists consider health and wellbeing to be *side effects* of mindfulness practice (Goleman & Davidson, 2017, p. 269). Therapies which include mindfulness techniques are known collectively as Mindfulness-Based Interventions (MBIs). Monteiro et al. (2015) argue that by omitting Buddhist ethics (e.g. mindful restraint of speech and action), and emphasising mindfulness as a treatment for symptomatic relief, MBIs risk doing more harm than good. Nevertheless, some early Buddhist scriptures do describe mindfulness applied in this way, for example, as a psychological pain management strategy (Analayo, 2015). Relief from chronic pain and psychological forms of suffering are 'side effects' of interest to large numbers of people. These outcomes require less intense mindfulness training than is undertaken by monastics, which is why they are of interest to therapists and clinicians.

[1]According to Buddhist cosmology, permanent liberation occurs over many lifetimes (Gethin, 1998, pp. 112–126).

Research institutes such as the Oxford Mindfulness Centre[2] are dedicated to promoting mindfulness as a means for improving physical and mental health across the lifespan. The first MBI, Mindfulness Based Stress Reduction (MBSR, Kabat-Zinn, 2011), was developed in the early 1980s, and proved to be an effective treatment for chronic pain. MBSR set a template for teaching mindfulness in group therapy settings, led by a teacher with a regular meditation practice. MBIs often run for eight weeks and include a variety of mindfulness practices, including meditation, yoga and tai chi. Two Buddhist meditations – mindfulness of breathing and body scanning – are core techniques within many MBIs. Mindfulness Based Cognitive Therapy (MBCT) combines the eight week MBSR format with cognitive therapy, and has been shown to reduce the risk of depressive relapse (Kuyken et al., 2016)[3]. There is now evidence that mindfulness can have positive effects on many aspects of physical and mental health, such as helping people to stop smoking (Brewer et al., 2013), and modifying maladaptive eating behaviours (Brewer et al., 2018).

Mindfulness could make people happier, as well as healthier. The Oxford Mindfulness Centre define wellbeing as a state of comfort, health and/or happiness. The technical term for happiness is 'subjective wellbeing'. It has three distinct, but related components; frequent positive affect, infrequent negative affect, and cognitive evaluations such as life satisfaction (Tov & Diener, 2013). Subjective wellbeing involves interactions between psychological factors and life circumstances (Diener et al., 1999). An intriguing finding is that mindfulness can improve life satisfaction, by reducing the gap between current and desired financial states (Brown et al., 2009). Brown et al. (2009)

[2]The Oxford Mindfulness Centre (`https://www.oxfordmindfulness.org/`) was founded by the originators of Mindfulness Based Cognitive Therapy.

[3]MBCT is an approved treatment for people who are currently well, but have experienced three or more previous episodes of depression (National Institue for Health and Care Excellence, 2011).

contrast dispositional states of "wanting what one has" with "getting what one wants". A synonym for "wanting what one has" is contentment, a mental attitude which plays an important role in stilling the mind during meditation (Brahm, 2006). Mindfulness could make many people happier, if it can improve their wellbeing independent of their material circumstances. More generally, increased awareness supports hedonic and eudaimonic wellbeing (Dahl et al., 2020).

The mechanisms responsible for the beneficial effects of MBIs are less clear, but attention appears to play an important role (Chiesa et al., 2014). For example, MBCT was conceived as attentional control training (Teasdale et al., 1995). It uses mindfulness to increase awareness of dangerous mood swings, divert cognitive resources away from rumination, and 'decenter' people from automatic, habitual thoughts and moods (Segal & Teasdale, 2018, p. 49). Kuyken et al. (2010) suggest that interactions between mindfulness and attentional control could contribute to MBCT's effects on symptoms.

More research is needed to establish whether attentional control, and attention regulation more generally, mediate the beneficial effects of meditation and MBIs. A meta-analysis found that MBCT and MBSR affected cognitive and emotional reactivity, mindfulness, rumination, and worry in ways that improved mental health (Gu et al., 2015). However, Gu et al. (2015) noted that no studies tested whether attentional control and attention regulation mediate the effects of these MBIs. Attention regulation is one mechanism thought to explain how mindfulness meditation works (Hölzel et al., 2011; Isbel & Summers, 2017; Lindsay & Creswell, 2017; Malinowski, 2013; Vago & Silbersweig, 2012). More research is needed to test this claim, and to establish whether mindfulness and attention can fully explain associated effects on mental health and wellbeing.

This thesis evaluates the extent to which mindfulness meditation, a component of most MBIs, regulates attention.

## 1.3 What is mindfulness?

Mindfulness is an important technical term within Buddhist theory and practice[4]. The word 'mindfulness' is a 19th century translation of the Pali[5] word *sati* (Gethin, 2011). Buddhist scriptures (Pali: *Sutta*) emphasise the importance of mindfulness as a mental factor, and provide detailed instructions for inducing mindfulness through meditation. Buddhist psychology (Pali: *Abhidhamma*) is a comprehensive, scholastic theory of the mind, derived from the *Suttas*. In the *Abhidhamma's* ontology of mental states, mindfulness is classified as one of the 'beautiful factors' (Bodhi, 2012). Bodhi (2011, p. 22) defines the mindful state as "lucid awareness of the phenomenal field", and subjective reports of enhanced vision and other sensory experiences following meditation are common. In contrast with thinking, mindfulness is a more direct, less mediated form of experience (Gunaratana, 2011, p. 139). Gunaratana (2011, p. 131) also notes that the word 'mindfulness' describes both a mental state, and activities such as meditation, which can induce it. The word itself points towards a type of experience which is easily accessible, but is itself beyond words.

Buddhist psychology distinguishes mindfulness from related cognitive processes, and describes how they interact. Applied mindfulness is a process of detached observation which guards the mind from potentially harmful thoughts and intentions, and interacts with other mental factors to produce conscious experience (Anālayo, 2003).

[4]For a comprehensive history of mindfulness within Buddhism, see Sujato (2012b).

[5]Pali is the language of early Buddhist texts, which date back to the first century BCE.

*1. Introduction*

*Sati* is sometimes translated as 'memory', because it is associated with enhancements to encoding and recall (Anālayo, 2003, p. 47). There is some empirical evidence to support the effects of mindfulness on memory (Anālayo, 2018; Chiesa et al., 2011; Rapgay, 2019), but the process thought to be most directly affected by mindfulness is attention. In Buddhism, attention (Pali: *manasikāra*) is the process that determines which experiences enter conscious awareness (Bodhi, 2011). Buddhists and psychologists would agree that attention and mindfulness are distinct mental processes.

The psychological process that most resembles mindfulness is metacognition, the capacity to turn the mind upon itself. Metacognition is the process by which "the current contents of consciousness become the focus of attention" (Schooler & Smallwood, 2014, p. 439). Mindfulness can be thought of as meta-awareness of the content and processes of mental experience. Spontaneous, intermittent moments of meta-awareness are common, but these 'mindful moments' are distinct from the *sustained* periods of meta-awareness which are a fundamental feature of most definitions of mindfulness (Dunne et al., 2019). Dunne et al. (2019) also propose that mindfulness is non-propositional, which means it is a state in which there is no internal, verbal judgment of experience.

A widely cited operational definition of mindfulness (Bishop, 2004) has two components. The first is a metacognitive process which regulates attention. The second are attitudes of curiosity, openness, and acceptance towards present moment experience. This thesis focuses on the first of these processes, the extent to which mindfulness regulates attention. A discussion of how these components interact is postponed until Chapter 7.

## 1.4 Mindfulness and mind wandering

As a mental state and process, mindfulness is often contrasted with mind wandering. Smallwood & Schooler (2006) define mind wandering as an uncontrolled shift in executive control away from a primary task, to the processing of other personal goals. Less formally, it can be thought of as unintended thinking. For example, a student might intend to read a journal article, but soon find their attention oriented towards thoughts about socialising with friends. Smallwood & Schooler (2006) would consider this to be a failure of executive control, leading to superficial representations of the external environment, and reduced task performance. Although the student's eyes were directed towards the page, their attention was oriented towards their thoughts, degrading their perception of the words and and their meanings.

Mind-wandering is more likely to occur when task demands are low, meaning there is excess mental capacity to be captured by competing, current concerns. However, mind wandering can also occur because task demands exceed the available mental capacity (Smallwood & Schooler, 2006). In other words, the student's mind might wander if they found the article especially easy to understand, *or* especially challenging. Importantly, mind-wandering seems to be initiated and maintained due to an absence of meta-awareness that thoughts are no longer about the primary task (Smallwood & Schooler, 2006).

If increased mindfulness (meta-awareness) reduces the frequency or duration of mind wandering, then it may also improve executive function. For example, Mrazek et al. (2013) found that mindfulness training reduced mind-wandering whilst increasing working memory capacity. Given that many everyday tasks require executive control, using mindfulness training to reduce mind wandering could be widely beneficial. For example, students with higher levels of mindfulness might comprehend more of what they read.

Other negative consequences of mind-wandering are more serious than reduced task performance. Self-generated thought is often negatively valenced, and is associated with unhappiness and mood disorders (Vago & Zeidan, 2016). Rumination and worry are forms of repetitive negative thought that are closely associated with depression, anxiety, and difficulties in physical health (Watkins, 2008). To reduce the risk of these negative outcomes, MBIs use meditation as a way to increase awareness of mind wandering before it develops into repetitive negative thinking (Smallwood & Schooler, 2006). This pragmatic approach to reducing negative consequences of mind wandering can also be found in Buddhism. The Pali word *papañca* is normally translated as 'conceptual proliferation', which suggests a process similar to mind wandering[6]. *Papañca* is considered to be a mental defilement which impedes mindfulness (Anālayo, 2003, p. 222; Gangodawila, 2020), leading to negative thoughts and actions. Conversely, mindfulness is used to guard against *papañca*, suggesting that mindfulness and mind wandering are mutually exclusive states. Mindfulness may improve mental health by reducing excessive mind-wandering that leads to, and sustains low mood.

Mind wandering can also have positive outcomes. Schooler et al. (2014) suggest that it can facilitate planning and lead to creative insights. In fact, they argue that that mindfulness may actively *hinder* creativity, by inhibiting spontaneous thinking. The costs and benefits of mind wandering may be mixed. Shrimpton et al. (2017) found self-focused rumination to be positively associated with both anguished fantasies, failures and aggression, and also with positive and constructive thoughts. They also found

[6]See Ñāṇananda (2012) for a comprehensive discussion of *papañca* in Buddhist literature.

that performance on a sustained attention task varied depending on whether mind wandering during the task was focused on the past, present or future. More research is needed to establish how mindfulness could be used to minimise the costs of mind wandering, and maximise its benefits.

Mindfulness and mind-wandering are contrasting mental *processes* (Schooler et al., 2014). The lucid meta-awareness associated with mindfulness is qualitatively different to the absence of awareness that the mind has wandered, and is wandering. Anālayo (2003, p. 48) describes *papañca* as the *process* by which present-mindedness becomes absent-mindedness. As mental *states*, mindfulness and mind wandering could be mutually exclusive, because mindfulness is associated with sustained meta-awareness (Dunne et al., 2019), and meta-awareness is reduced when mind wandering occurs (Smallwood & Schooler, 2006). On the other hand, it is both possible, and common to be mindful of a wandering mind. In fact, remaining aware of changing thoughts is central to 'open monitoring', one type of mindfulness meditation. Therefore, mindfulness and mind wandering may be orthogonal, rather than opposing processes and states. This could offer the best of both worlds; meta-awareness of mind wandering may be sufficient to reorient attention to a more important task, and mindful awareness of the contents of consciousness could improve the detection of creative insights.

There are good reasons to believe that meditation training might reduce mind wandering. Hasenkamp & Barsalou (2012) propose that breath meditation involves a cognitive cycle that consists of mind wandering, awareness of mind wandering, shifting of attention, and some periods of sustained attention on the breath. It is reasonable to expect that an exercise which involves repeatedly reorienting attention towards the breath would reduce the frequency and duration of mind wandering. Furthermore, the minimal cognitive demands of this task are precisely the conditions under which mind wandering is most likely. The challenge of detecting and avoiding mind wandering under low cognitive load could explain why meditation is an effective way of training meta-awareness. Empirical evidence supports this intuition. For example, Mrazek et al. (2012) found that mindfulness reduces mind-wandering to a greater extent than passive relaxation and reading. This topic is discussed in more detail in Section 1.6.

## 1.5 Measuring mindfulness

Mindfulness can be measured using psychometric scales. Table 1.1 summarises the main scales used to measure mindfulness as either a mental state, or as a trait. State measures test whether inductions, notably meditation, increase mindfulness. In contrast, trait measures test for mindfulness without deliberately inducing a mindful state. If mindfulness is high without having to induce it, then the associated benefits are, in principle, always available. High levels of trait mindfulness are normally associated with expert meditators such as Buddhist monks and nuns. The process by which mindfulness evolves from being a state, to a trait is an ongoing line of research (Goleman & Davidson, 2017).

**Table 1.1:** Self-report mindfulness measures.

| Type | Scale | Items | Factors |
|---|---|---|---|
| Trait | Mindful Attention and Awareness Scale [MAAS; Brown & Ryan (2003)] | 15 | mindfulness |
| Trait | Cognitive Affective Mindfulness Scale-Revised [CAMS-R; Hayes & Feldman (2004)] | 12 | attention, awareness of internal experiences, acceptance of internal experiences, present-focus |
| Trait | Kentucky Inventory of Mindfulness Skills [KIMS; Baer et al. (2004)] | 39 | observing, describing, acting with awareness, accepting without judgment |

| Type | Scale | Items | Factors |
| --- | --- | --- | --- |
| Trait | Five Facet Mindfulness Questionnaire [FFMQ; Baer et al. (2008); Baer et al. (2006)] | 39 | observing, describing, acting with awareness, non-judging of inner experience, non-reactivity to inner experience |
| Trait | Freiburg Mindfulness Inventory [FMI; Walach et al. (2006)] | 30 | mindfulness |
| Trait | Toronto Mindfulness Scale [Trait-TMS; K. M. Davis et al. (2009)] | 13 | curiosity, decentering |
| State | Mindful Attention and Awareness Scale [MASS-state; Brown & Ryan (2003)] | 5 | mindful attention and awareness of daily activities |
| State | Toronto Mindfulness Scale [TMS; Lau et al. (2006)] | 13 | curiosity, decentering |
| State | State Mindfulness Scales [SMS; Tanay & Bernstein (2013)] | 21 | mindfulness of mind, mindfulness of body |

Self-report mindfulness scales have reasonable psychometric properties and have contributed to an understanding of how mindfulness affects health and wellbeing (Baer, 2019). However, Baer (2019) also notes that respondents can deliberately or unintentionally misrepresent themselves on questionnaires for a variety of reasons. The validity of trait mindfulness measures is a subject of ongoing debate. Grossman (2011) is especially critical, arguing that the scales are invalid because they are constructed by researchers who lack long-term experience with mindfulness practice. This conflicts with the view held by Brown et al. (2011), that the Mindful Attention Awareness Scale is derived from established theories of attention and meta-awareness, and that its validity has been demonstrated empirically. Chiesa (2013) agree with Grossman (2011) that self-report scales could be improved if they were informed by a deeper conceptual and experiential understanding of mindfulness that is derived from Buddhism. These debates remain unresolved, but Baer (2019) argues that this should not prevent the use of self-report measures in mindfulness research.

The limitations of self-report scales have been addressed to some extent by behavioural measures of mindfulness. Behavioural tasks can measure the time-course, objects and sensitivity of mindful awareness, and attitudes toward present moment experience (Hadash & Bernstein, 2019). Hadash & Bernstein (2019) identify breath counting accuracy (Levinson et al., 2014) as an especially important measure of sustained, mindful awareness. Accurate breath counting is correlated with self-reported mindfulness and differentiates long-term meditators from age-matched controls. Improved breath counting accuracy is independent of improved performance on sustained attention and working memory tasks, suggesting that mindfulness is distinct from these cognitive processes (Levinson et al., 2014).

Other research supports breath counting as a reliable measure of mindfulness. Isbel et al. (2020) used a breath counting task similar to Levinson et al. (2014) in a randomised controlled trial which compared eight weeks of meditation training (MT) with an attention training control condition. Breath counting accuracy was higher in the MT condition than in the control condition, but there were no differences between the groups on two trait mindfulness measures - the MAAS and FFMQ. One interpretation of this finding is that breath counting accuracy is more effective at discriminating mindfulness from attention than the two self-report measures of trait mindfulness.

## 1.6 Training mindfulness with meditation

Meditation is the most common form of mindfulness training, especially in Buddhist traditions. Two aspects of meditation training appear to affect cognitive process and outcomes. One is the type of meditation practiced. The other is the duration and intensity of training, variables which are closely related to meditators' expertise.

### 1.6.1 Types of meditation

Lutz et al. (2007) claim that meditation training differs from other religious practices in that it produces well defined mental states as a meditator progresses from beginner to expert. Buddhist meditation instructions are often precise, subtle and designed to produce specific outcomes. For example, one fifth century Buddhist meditation manual goes so far as to match meditation techniques to an individual's temperament (Buddhaghosa, 1999). To some extent, Buddhism distinguishes meditation techniques designed to calm the mind (Pali: *samatha*), from those designed to develop 'insight' (Pali: *vipassanā*).

The canonical *samatha* meditation is mindfulness of breathing (Pali: *ānāpānasati*). To practice *ānāpānasati*, a meditator sits still and remains mindful of the breathing process, reorienting attention to the breath when the mind inevitably wanders. The power of these instructions is in their simplicity. By following them, even first-time meditators can experience the calming effects of *samatha*. The same technique practiced over longer periods can be used to reach progressively deeper meditative states (Yates & Immergut, 2017). The Pali word for the calm state produced by *samatha* meditation is *samādhi*. A common translation of *samādhi* is 'concentration'[7], although one expert

[7] `https://suttacentral.net/define/sam%C4%81dhi`

meditator considers ‘stillness’ to more accurately describe the experience of *samādhi* (Brahm, 2006, p. 59). Similarly, *samādhi* has been described as ‘stable attention’ (Wallace, 2010; Yates & Immergut, 2017). *Samādhi* is not mindfulness (Anālayo, 2003, pp. 61–66; Mikulas, 2011), but the *result* of sustained mindfulness.

In contrast with the calming effects of *samatha*, *vipassanā* meditation is central to the Buddhist path of spiritual awakening[8]. The canonical instructions for *vipassanā* meditation come from the *Satipaṭṭhāna Sutta* (Anālayo, 2003), and involve directing mindfulness towards four categories of experience; the body, feelings, the mind, and specific categories of mental experience (Pali: *dhammas*). Buddhists believe that focusing mindfulness in this way leads to a sequence of irreversible mental developments, culminating in the permanent liberation from suffering.

To some extent, *vipassanā* could be seen as a more advanced practice than *samatha*, as it requires conceptual knowledge of, and an ability to focus mindfulness on, these four categories of experience. However, monastics are swift to downplay the distinction between *samatha* and *vipassanā*, because in practice they are cultivated simultaneously (Sujato, 2012a). For example, the *Ānāpānasati Sutta* describes how mindfulness of breathing can be used to reach full awakening, by following a sequence of steps which satisfy the criteria set out in the *Satipaṭṭhāna Sutta*. In this sense, *samatha* and *vipassanā* are two sides of the same coin.

The scientific study of meditation has taken a different approach to classifying meditation techniques. Creating a unified taxonomy has been challenging (Nash et al., 2013), as meditation practices which appear different, are often designed to have the same outcome. A particular meditation technique may also have different outcomes

[8] Awakening is a translation of the Pali word *bodhi* (`https://suttacentral.net/define/bodhi`).

depending on the expertise of the meditator and the intensity with which it is practiced. One classification scheme which has dominated the study of mindfulness meditation, was designed to test the effects of different types of meditation on attention regulation (Lutz et al., 2008). This approach classifies meditation practices according to whether mindfulness is oriented towards a single or multiple meditation objects. Meditations using a single object are collectively known as focused attention (FA) meditation (FAM). Those where mindfulness is spread across multiple objects are known as open monitoring (OM) meditation (OMM).

Focused attention meditation includes mindfulness of breathing[9], mindfully repeated words or sounds (mantra), and some body awareness practices (Dahl et al., 2015). In contrast, OMM typically involves being mindful of any and all experiences which enter awareness, such as sensations, thoughts and emotions. Dahl et al. (2015) classify this form of OMM as 'object-oriented'[10], and distinguish it from 'subject-oriented' OMM, where the aim is to be mindful not of the objects of experience themselves, but of the awareness that is aware of them. Subject-oriented OMM is most commonly associated with Tibetan Buddhism. Dahl et al. (2015) include both FAM and OMM in a family of practices that regulate attention. The main difference between these practices is the way in which attention is oriented as objects of consciousness change. For FAM, attention is oriented away from distractions and back to the meditation object. For OMM, there are no 'distractions', just continuous mindfulness of changing experience.

Differences between FAM and OMM could produce different effects on attention. Alternatively, attention may not be affected by the objects of meditation, or even

[9] In Tibetan traditions, mindfulness of breathing (Pali: *ānāpānasati*) is also referred to as Shamata (or "calm abiding") with support, the support being the breath.

[10] Object-oriented open monitoring practices would include "choiceless awareness" (Tibetan Buddhism) and 'letting be' (Brahm, 2006, pp. 72–74).

the way in which attention is oriented towards them, but by the period over which mindfulness is sustained. In other words, sustained mindfulness may induce *samādhi*, and it is this mental state that affects attention, rather than the means by which it is induced. This is important for experiments which explore the effects of FAM and OMM on attention. If both types of meditation induce similar depths of *samādhi* we might expect both to have similar effects on an attentional task (and to differ from a non-meditation control activity).

### 1.6.2 Expertise and intensity

The effects of meditation vary with expertise and training intensity. In this thesis, people who have never meditated before are referred to as 'novices'. Based on a broad survey of research outcomes, Goleman & Davidson (2017) classify non-novices as beginners, long-term meditators, or 'yogis'. Beginners, the largest group, are people who have meditated for 7–100 hours. They might learn to meditate as part of a training course, or using a mobile phone app. Less numerous are long-term meditators, who have 1,000–10,000 hours of mostly *vipassanā* experience. Long-term meditators have a regular practice, meaning they meditate most days, and have done so for months or years. Some practice more intensively, for example by attending meditation retreats where they might meditate for many hours each day, over a period of a week or longer. The smallest group are yogis, who have more than 10,000 hours of experience, and consist mostly of Buddhist monks and nuns. Monastic training progresses gradually, but systematically and intensively over many years. Buddhist meditators aim to maintain continuous mindfulness (Pali: *sampajañña*) during all activities (Anālayo, 2003, p. 137), and often meditate in silence for months, or even years at a time.

Interactions between expertise and practice intensity can influence cognitive, emotional and physical outcomes. It has been suggested that meditation can affect attention even when meditation experience and intensity are low. Novices who meditate for between 10–20 minutes have shown improvements on a variety of attention measures (Colzato et al., 2015; Dickenson et al., 2013; Schofield et al., 2015; Watier & Dubois, 2016). There is also evidence that brief meditation improves metacognitive ability (Baird et al., 2014), and reduces mind wandering (Mrazek et al., 2012). There are both similarities and differences between beginners and long-term meditators on emotional and physical measures. Kral et al. (2018) found that beginners and long-term meditators had less amygdala reactivity to positive pictures than active controls. In long-term meditators, reactivity to negative pictures was negatively correlated with their meditation experience. Lutz et al. (2013) found that yogis reported equal intensity, but less unpleasantness than non-meditators in response to painful thermal stimuli. They suggest that experiential openness down-regulates pain anticipation and recruits attentional resources associated with faster habituation. More generally, meta-analyses find that mindfulness interventions and long-term practice are associated with improved attentional performance on measures of inhibition, sustained attention, alerting and orienting (Verhaeghen, 2020). Meditation retreats can reduce depression, anxiety and stress and improve quality of life, with larger effects in beginners than in long-term meditators (Khoury et al., 2017). Meditation intensity can also reveal within-group differences in long-term meditators. For example, Wielgosz et al. (2016) found that respiration rate decreased as meditation experience increased, but only in meditators with experience of intensive meditation retreats.

Differentiating effects arising from expertise developed over many years, from those due to intense periods of meditation, presents challenges. Yogis are hard to recruit and tend to have both high levels of expertise and intensity of practice. This makes it hard to run controlled experiments with sufficient statistical power to test hypotheses. Although there are more long-term meditators, they are often geographically dispersed, making them hard to study under laboratory conditions. Many of the challenges of running experiments with yogis also apply to long-term meditators. The number of beginners is growing, but comparisons within this group only test effects at lower levels of expertise and training intensity. The research in this thesis focuses on beginners and long-term meditators.

## 1.7 Mindfulness and attention

This thesis explores the effects of mindfulness meditation on attention, a relationship that was introduced in Section 1.3. Attention plays a role in many cognitive processes, but it is not a unitary phenomenon (Parasuraman, 1984), and varieties of attention have been classified in different ways. One influential psychological model, derived from behavioural and neuroscientific evidence (Petersen & Posner, 2012), has identified three distinct functional and neural networks, referred to as alerting, orienting and executive attention. This model can explain most known effects of mindfulness on attention, even where different terms are used to describe the types of attention which are affected.

The alerting network is associated with vigilance tasks. For example, pilots and air traffic controllers need to remain alert over long periods in order to detect rare, but important events. Over short time periods, the alerting network is activated by warning signals which precede target events or actions (Petersen & Posner, 2012). For example, a driver waiting at a red traffic light is in a state of readiness for responding to amber, and green lights. The green light alerts the driver to pull away from the junction. The alerting network is also associated with vigilance towards a wider variety of warning signals over longer time periods. For example, in anxiety disorders, people may be hypervigilant to a range of potential threats.

The orienting network prioritises input by selecting its sensory modality or location (Petersen & Posner, 2012). For example, a passenger sitting next to our hypothetical driver might point towards a hole in the road, saying "watch out for that", causing the driver to search for the pothole[11]. This type of orienting is a top-down, goal-driven process, modulated by the detection of stimuli and associated with a dorsal neural network (Corbetta & Shulman, 2002). On perceiving the pothole, the driver would be selecting a specific visual signal in preference to others (and to stimuli from the other senses). A distinct, ventral orienting network is sensitive to detecting behaviourally relevant stimuli, especially those that are salient or unexpected (Corbetta & Shulman, 2002). Perhaps the radio is playing, and the driver becomes unaware of the pothole because their attention is briefly captured an interesting news story. Corbetta & Shulman (2002) describe the ventral system as a 'circuit breaker' for the dorsal system.

The third network, executive attention, coordinates mental processes required to resolve conflict, and initiate an appropriate response. Driving is a set of skills which, once learnt, can be carried out largely automatically, in parallel with other activities. In contrast, novel tasks, or switching between tasks require controlled processing, which tends to operate serially (Schneider & Shiffrin, 1977). Controlled processing is associated

[11] Visual attention can be oriented towards a stimulus overtly, by moving the head and eyes, or covertly, without eye movement (Posner, 1980).

with conscious awareness (Petersen & Posner, 2012), and makes demands on working memory. Executive attention is the controlling subsystem of working memory, often referred to as the 'central executive' (Baddeley, 2000). Norman & Shallice (1981) consider executive attention to be a 'supervisory mechanism', which inhibits and activates a competing set of cued responses, to influence control over behaviour. For example, as our driver approaches the pothole, auditory awareness of the news story is inhibited, so that visual attention can be reoriented towards the road, and a judgment is then made about how to turn the steering wheel in order to avoid the pothole. In summary, executive attention is required by tasks which involve planning, error detection, novelty, difficult processing, or resolving conflict (Posner & DiGirolamo, 2000).

Petersen and Posner's (2012) model can explain empirical evidence that mindfulness regulates attention. Chiesa et al. (2011) reviewed 36 comparisons of the effects of meditation training on different aspects of attention. Meditation training improved attention in just under 50% (19) of the comparisons. There were five effects on sustained attention, five on selective attention, five on executive attention, and individual effects on attention switching, and miscellaneous attention measures. This taxonomy is easily mapped onto Petersen and Posner's (2012) model. The alerting network would include effects on sustained attention, the orienting network effects on selective attention, and the executive network effects on executive attention and switching.

### 1.7.1 Effects of meditation on the Attention Network Test

A more pragmatic reason for using Petersen and Posner's (2012) model is that a computer task, the Attention Network Test (ANT; Fan et al., 2002), can measure efficiency in the three networks. The ANT has been used extensively in meditation research. It is summarised here to explain how 'efficiency' is operationalised, making it a candidate for measuring attention regulation. The stimulus on each trial is a group of five horizontal arrows, which appear above or below the centre of the display. A correct response means pressing the arrow key that matches the direction of the ***central*** arrow. On congruent trials, all arrows point in the same direction, and on incongruent trials the 'flanker' arrows (two on each side) point in the opposite direction to the central arrow. On incongruent trials, pressing the key that matches the central arrow requires inhibiting the tendency to press the key matching the direction of the other four arrows. The difference in reaction time (RT) on congruent and incongruent trials operationalises executive attention efficiency. Before they appear, the arrows may be preceded by an orienting cue (an asterisk), presented at the same location (above or below fixation). Orienting is facilitated when the asterisk is presented in the same location as the five arrows, because it 'points' to the location where the arrows will appear. The difference between RTs on trials with or without orienting cues operationalises efficiency in the orienting network. Before participants orient their attention to the location of the arrows, an alerting cue (asterisk) may appear in the centre of the screen. RTs are faster when there is an alerting cue, because it prepares the nervous system for the orienting task by informing the participant that the orienting cue will soon appear.

The difference between RTs on trials with or without alerting cues operationalises efficiency in the alerting network.

The ANT is a useful experimental task for testing the effects of meditation on attention. Three studies reviewed by Chiesa et al. (2011) found that meditation improves one or more ANT scores. Jha et al. (2007) found effects on all the ANT networks, Tang et al. (2007) found improved executive attention after meditation training, and Hurk et al. (2010) reported that mindfulness meditation improved reaction times for orienting and executive networks. All three studies found an improvement in executive attention. However, these studies have some limitations which weaken accounts which draw on them as evidence that mindfulness regulates attention (Hölzel et al., 2011; Isbel & Summers, 2017; Malinowski, 2013; Vago & Silbersweig, 2012). Chiesa et al. (2011) conclude that the effects of meditation on attention should be treated with caution, due to an overemphasis on correlational evidence, a limited number of randomised controlled trials and a lack of replications. For example, three of the positive effects on the ANT reported by Chiesa et al. (2011) came from the same non-randomised study (Jha et al., 2007), findings which have not been replicated. Replications are needed to validate previous experimental designs, and a meta-analysis is needed to quantify effect sizes of meditation on the ANT in beginners and long-term meditators.

Improved ANT performance as a result of meditation could also help to explain how increased mindfulness reduces mind wandering. Improved alerting scores might be associated with an ability to detect the onset of mind wandering episodes. Better executive attention scores could indicate an ability to inhibit mind wandering that might otherwise be automatically triggered. Finally, improved orienting scores might indicate an ability to orient attention away from the content of mind wandering towards other aspects of experience.

### 1.7.2 Effects of meditation on the attentional blink

Mindfulness has also been show to reduce a phenomenon called the 'attentional blink' (AB; Shapiro et al., 1997). The AB measures people's ability to detect two targets amongst distractors, on a task in which stimuli are presented in rapid succession. The phenomenon takes its name from trials where target stimuli appear close together in the sequence. On these trials attention tends to be captured by the first target, preventing detection of the second target. Detecting *both* targets requires an ability to flexibly allocate attentional resources. Two studies reviewed by Chiesa et al. (2011) found that meditation reduced the AB (increased probability of detecting the second target having detected the first). Slagter et al. (2007) found that the AB was reduced after long-term meditators completed a 3-month Vipassana retreat, relative to beginners who meditated daily for two weeks. Meditation may also reduce natural, age-related declines in attentional abilities. Leeuwen et al. (2009) found that the AB in older, long-term meditators was smaller than non-meditators of a similar age, and similar to much younger people. Both findings suggest that mindfulness training may improve the ability to flexibly allocate attentional resources.

Reductions in AB are often cited as evidence that meditation training regulates attention (Hölzel et al., 2011; Malinowski, 2013; Vago & Silbersweig, 2012). These claims would be more persuasive if they could be replicated. Chiesa et al. (2011) refer to Leeuwen et al. (2009) as replicating the reduction in AB found by Slagter et al. (2007), but this is relatively weak evidence of attention regulation, as the individual findings have not been replicated. As with the ANT, meta-analytic estimates are needed for the effects of meditation on the AB, in both beginners and long-term meditators.

## 1.8 Does mindfulness meditation regulate attention?

This thesis tests the claim that mindfulness meditation regulates attention. A concise definition of attention regulation is hard to find in the mindfulness literature, which is surprising given its theoretical importance as an effect of mindfulness. According to Bishop (2004, p. 232), one aspect of mindfulness is "the self-regulation of attention so that it is maintained on immediate experience, thereby allowing for increased recognition of mental events in the present moment". In this operational definition of mindfulness, 'self-regulation of attention' means improvements on sustained attention and switching tasks, and an ability to inhibit secondary elaborative processing i.e. the implications, meanings, or need to act on thoughts, feelings and sensations (Bishop, 2004). Mindfulness meditation is an activity which is likely to improve attention regulation, because it involves sustaining, shifting, and disengaging attention, monitoring experience and detecting distractions from an ongoing meditation task (Malinowski, 2013, Figure 2).

An unstated assumption in the literature discussed so far is that meditation increases mindfulness (sustained meta-awareness), and that this mediates improved attention regulation. To demonstrate mediation, we need evidence that mindfulness meditation increases mindfulness, *and* that higher amounts of mindfulness correspond with improvements in attentional performance. Experiments using the SMS and breath counting tasks (Section 1.5) have shown that mindfulness meditation increases mindfulness. First-time meditators show improved mindfulness on the SMS relative to controls after 10 minutes or less of mindfulness meditation (Bravo et al., 2018; Lueke & Gibson, 2016; Paz et al., 2017). Breath counting accuracy, a more objective mindfulness measure, has been shown to improve after longer periods of training. Levinson et al. (2014, Study 4) found that breath counting accuracy improved in a group who did four weeks of breath counting training, compared with control groups who did no training, or four weeks of working memory training. The breath counting group also showed reduced mind wandering relative to the control groups.

This thesis uses the ANT and AB as operational measures of attention regulation. They fit within an established model of attention (Petersen & Posner, 2012), and there is preliminary evidence that mindfulness meditation improves performance on these tasks (Chiesa et al., 2011). If meditation increases mindfulness, measured using the SMS or breath counting accuracy, and improves ANT performance or reduces the AB, then we can say that mindfulness mediates attention regulation.

Both FAM and OMM are thought to regulate attention (Dahl et al., 2015), but their effects may differ. Because of the way in which attention is allowed to move across multiple objects of awareness, OMM is thought to train 'receptive' attention, a state which is 'ambient' or 'diffuse' (Vago & Silbersweig, 2012). Receptive attention is contrasted with focused attention, a state thought to arise as a result of sustaining mindfulness on a single meditation object (FAM). J. Davis & Thompson (2017) suggest that these differences occur because top-down orienting of attention is reduced in OMM relative to FAM. There is some empirical evidence to support this view. Individual studies have found that OMM reduces the AB more than FAM after brief meditation in beginners (Colzato et al., 2015) and long-term meditators (Vugt & Slagter, 2014).

Alternatively, we might expect FAM and OMM to have similar effects. Traditionally, both OMM and FAM are *samatha* practices, meaning they are designed to induce *samādhi*. The objects of mindfulness differ for OMM and FAM, but their effects could be the same. This seems especially likely in beginners if OMM is a more advanced technique than FAM (Lutz et al., 2008). It is also to be expected based the most detailed models of how FAM and OMM work (Vago & Silbersweig, 2012, Figure 3 and Figure 4). Apart from the differences in focused and receptive attention in this model, FAM and OMM appear similar in terms of orienting, disengagement, and executive functioning processes.

If meditation improves ANT performance, this could partially explain how the monitoring aspect of mindfulness meditation regulates attention. Lindsay & Creswell (2017) claim that monitoring is responsible for improvements in selective attention, sustained attention, task switching and working memory. Hölzel et al. (2011) emphasise the role of conflict monitoring in improving attention regulation. Isbel & Summers (2017) propose a model in which metacognitive monitoring develops metacognitive skills, which are responsible for improvements in attentional control. Malinowski (2013) suggests that the ability to monitor and regulate attentional states may be distinct from the ability to monitor and control responses. The common feature in these accounts is regulation of attentional control, which is measured by the ANT executive attention score.

If meditation reduces the AB, then it can be considered to improve the ability to flexibly allocate attentional resources. According to Malinowski (2013), where mindfulness meditation improves attentional selection and control, this is primarily mediated by flexibility in allocating attentional resources. Evidence from EEG studies suggest that this flexibility enhances perceptual discrimination and conflict resolution

(Malinowski, 2013). This claim is especially relevant to reductions in AB, where attention must be flexibly allocated to process two stimuli which appear in quick succession. Practicing meditation may also increase the availability of cognitive resources. Vago & Silbersweig (2012) suggest that efficient, effortless attention allocation frees up cognitive resources, because with practice, less effort is required to apply meditation techniques. Tasks which show improved attention allocation or increased cognitive resources following meditation, could signify attention regulation.

Meditation is likely to regulate attention gradually, so effects on the ANT and AB may vary with experience. Harris et al. (2007, p. 26) define children's attention regulation as "the ability to self-monitor one's deployment of attention, which includes maintaining attention, ignoring distracting or irrelevant stimuli, staying alert to task goals, and coordinating one's attention during a task". Attention regulation improves as part of normal development, but before this process matures, some aspects of regulation may be more developed than others. For example, children may be able to monitor attention, but find it harder to orient attention away from distracting stimuli towards a task. Harris et al.'s (2007) definition also seems relevant to attention regulation in adults, who may exhibit patterns of attention *disregulation* similar to children. A common and sometimes surprising early meditation experience is that meditation consists most of thinking, mind wandering or falling asleep, with very little awareness of the breath. If meditation regulates attention in adults, then the degree of regulation may vary between novices and long-term meditators, and with varying amounts of meditation training.

## 1.9 Research aims

There is widespread consensus linking mindfulness with improved meta-cognitive monitoring and attention regulation. However, the empirical evidence used to support this claim has not been systematically evaluated, and is based on a relatively small evidence base. The link between mindfulness mediation and attention regulation would be strengthened if existing effects could be replicated and extended. Further research is also needed to establish whether different types of meditation have differing affects on attention, and the extent to which meditation expertise affects outcomes. This thesis tests whether mindfulness meditation regulates attention in novices and long-term meditators, and if it does, whether the effects are mediated by mindfulness.

# 2

# The effects of brief focused attention meditation on the Attention Network Test

## Contents

## 2.1 Introduction

The claim that mindfulness mediates attention regulation was first tested by exploring whether a brief period of mindfulness of breathing increased mindfulness to the extent that performance improved on the Attention Network Test.

### 2.1.1 Focused attention meditation

Mindfulness of breathing is a type of focused attention meditation (FAM). It is a canonical Buddhist meditation practice which can easily be explained to novices. Maintaining awareness of breathing sensations requires three things: continuous conflict monitoring to detect when attention has become oriented towards something other than the breath, the ability to disengage attention from distractions, and the ability to reorient attention towards the breath. Wallace (2010) suggests that this has a stabilising effect on attention. Therefore mindfulness of breathing could regulate attention in ways which are reflected in performance on a subsequent attentional task.

### 2.1.2 The Attention Network Test

The Attention Network Test (ANT; Fan et al., 2002) was chosen to operationalise attention regulation, because it measures attention networks which are likely to be affected by meditation. The ANT is an established test of the efficiency and independence of the alerting, orienting and executive attention networks. It combines a flanker task (Eriksen & Eriksen, 1974), with a cued response time (RT) task (Posner et al., 1980). In the flanker task, participants see five, horizontally arranged arrows, and must press the arrow key which points in the same direction of the central arrow (see Figure 2.1, panel B). The central arrow, may point in the same direction (congruent) or the opposite direction (incongruent) as the flanking arrows. The flanker task tests executive functions, in that participants must perceive the central arrow, inhibit perceptions of incongruent flankers, decide which direction the arrow points, and initiate a motor response in order to press the corresponding arrow key[1]. Executive attention is one of three networks measured by the ANT. The other two networks, alerting and orienting, will be described below, after an explanation of the cuing aspect of the ANT shown in Figure 2.1, panel A.

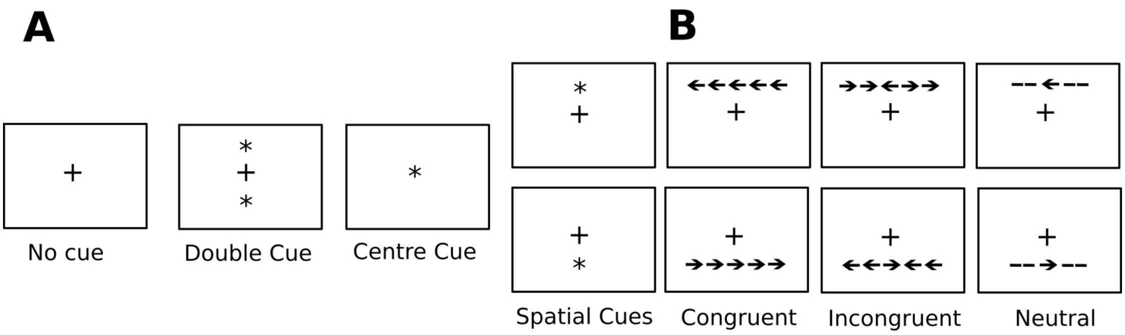


**Figure 2.1:** Attention Network Test. (A) cue types (B) target types.

The arrow stimulus in the flanker task may be preceded by a cue which provides information about when, and optionally where, the stimulus will be presented (see Figure 2.1, panel A). A central cue is a single asterisk displayed at the centre of the display. A double cue is a pair of asterisks, displayed simultaneously above and below fixation. Central and double cues provide temporal, but not spatial information about stimulus onset, i.e. they draw attention to the display, but do not provide information about where the arrows will subsequently appear. Spatial cues are single asterisks presented

[1]The executive attention network is often referred to as the "conflict" network, because it reflects the ability to resolve the conflicts in arrow direction on incongruent trials.

either above or below fixation. These provide both temporal *and* spatial information, as the asterisk always predicts the location of the subsequent arrow stimulus. No-cue trials provide no temporal or spatial cueing information, because no asterisk is presented before the arrow stimulus.

In addition to the executive attention network, the ANT measures alerting and orienting networks. The alerting network is associated with vigilance and is activated by warning signals which precede target events or actions. Each trial in a reaction time task can be thought of as "mini vigilance situation" (Posner & Boies, 1971). *Any* cue preceding the arrow flanker task should alert the participant that a trial has begun, thereby reducing RTs. This is why alerting is operationalised as the difference in RTs between no-cue and double cue trials. One aspect of the orienting network is to prioritise input by selecting its location. Spatial cues should reduce RTs to a greater extent than non-spatial cues, by helping participants orient towards the location where the arrows will appear. Therefore orienting efficiency is operationalised as the difference between trials preceded by either central or spatial cues.

**Rationale underlying Attention Network Test scores**

Fan et al. (2002) consider ANT scores to be a measure of network "efficiency". Each score is calculated as the difference in mean RTs between specific pairs of trial types[2]. The subtractions are ordered to produce a positive number. In each calculation, one of the trial types can be thought of as a "control", or baseline condition, and the other as the condition where RTs affect the score to reflect greater or lesser efficiency. The rationale for each score is described below.

[2]Similar calculations for scoring ANT error rates are not described here.

Executive attention scores are the mean difference in RTs between congruent and incongruent target trials on the flanker task, collapsed across all cue types. On average, RTs for congruent trials should be faster than those for incongruent trials, so subtracting congruent trial RTs from incongruent trial RTs produces a positive number. Congruent trials can be thought of as the control condition, and fast, accurate responses on incongruent trials, as the indicator of network efficiency. As incongruent trial RTs reduce, the congruent trial RTs are subtracted from a smaller number, thereby *reducing* the score. Therefore, a lower executive attention score means a more efficient network.

Alerting scores are calculated by subtracting RTs on double cue trials from RTs on no-cue trials. Because neither of these trial types cue the target location, attention tends to be diffused across the two locations where the arrows could appear. On average, responses on double cue trials should be faster than on no-cue trials, because they provide temporal information about stimulus onset. Therefore subtracting double cue trial RTs from no-cue trial RTs should produce a positive number. The no-cue trials can be thought of as the control condition, because they provide no alerting signal to draw attention towards the display. Double cue trials should prepare the participant to orient towards the subsequent arrow stimulus. The benefit provided by the double cue should be reflected in faster RTs on these trials. This will *increase* the alerting score, as a faster RT is being subtracted from the slower, no-cue RT. Therefore a higher alerting score is considered to be evidence of a more efficient network, in the sense that more benefit is derived from the double cues.

Orienting scores are calculated by subtracting RTs on spatial cue trials from RTs on centre cue trials. In this calculation, targets preceded by a central cue are considered the control condition because, like a double cue, they alert the participant that a stimulus will appear, but do not cue its location. The central cue focuses attention on a smaller region, in contrast with the diffused attention on double cue trials. On average, RTs for spatial cues should be faster than for centre cues, as they provide information about the onset *and* location of the subsequent arrows. Therefore subtracting RTs on spatial cue trials from those on centre cue trials should produce a positive number. The orienting score reflects the extent to which spatial cues facilitate the flanker task, by orientating attention towards the area where the arrows will appear. Faster RTs on spatial cue trials should *increase* the orienting score, as smaller RTs are being subtracted from the larger RTs on centre cue trials. Therefore a higher orienting score indicates a more efficient network.

The rationale behind ANT scores as indexes of network efficiency is important for interpreting the effects of mindfulness on attention. To summarise, incongruent flankers *hinder* fast, correct responses. The speed at which this conflict can be resolved is reflected in lower executive attention scores. In contrast, cues which appear before the flanker task *facilitate* faster responses. Any cue which prepares the participant for the next flanker task trial should reduce RTs. The extent to which they do so is a measure of alerting efficiency. By additionally providing information about stimulus location, spatial cues should facilitate responses over and above non-spatial cues. The extent of this facilitation is a measure of orienting efficiency. Facilitation for both alerting and orienting is represented by higher ANT scores.

**Appraising the Attention Network Test**

The independence of the attention networks measured by the ANT has been questioned, as has the validity of the difference scores. Contrary to the original claim that it provides independent measures of alerting, orienting and executive attention (Fan et al., 2002), subsequent studies often find interactions between networks (Callejas et al., 2004; Fan et al., 2009; Galvao-Carmona et al., 2014; Ishigami & Klein, 2010; MacLeod et al., 2010; McConnell & Shore, 2011). For example, Fan et al. (2009) found that the executive network's role in resolving conflict is enhanced by valid orienting cues, but diminished by invalid orienting cues. Because of these interactions, interpreting ANT scores for the task described requires caution (MacLeod et al., 2010; McConnell & Shore, 2011). Ishigami & Klein (2010) also draw attention to practice effects in the executive and the orienting networks, and relatively poor reliability with a single ANT measurement. Galvao-Carmona et al. (2014) suggest that the ANT can be an inaccurate measure of attentional capacity, because difference scores come from non-comparable experimental conditions. Wang et al. (2014) have proposed an improved method of calculating ANT scores to take account of these criticisms. Compared with the original formulas, this method produces larger alerting scores, and smaller executive attention scores. This thesis uses uses the original method of calculating ANT scores, but the validity and reliability of these calculations is discussed further in Chapter 7.

### 2.1.3 Why might focused attention meditation affect the Attention Network Test?

We hypothesised that FAM would improve executive attention. Hölzel et al. (2011) suggest that improved executive attention scores on the ANT provides evidence that meditation regulates attention. Neuroscientific evidence supports this claim. When compared with controls, meditators show different activity in the anterior cingulate cortex, a brain region thought to be responsible for detecting conflict in incompatible streams of information processing (Hölzel et al., 2011). If FAM regulates executive attention, we might expect an improved ability to resolve perceptual conflict on the flanker task. Fewer periods of mind wandering might also regulate executive attention. Mind wandering can be thought of as a switch from the primary task of monitoring the meditation object. Task switching is another type of mental activity known to make extensive demands on executive control resources (Meyer & Kieras, 1997), and responses on experimental tasks are substantially slower following a task switch (Monsell, 2003). Fewer switches between awareness of the meditation object and periods of mind wandering might conserve executive attention resources making them available to improve ANT performance. Fewer periods of mind wandering *during* the ANT could also reduce switch costs, thereby improving performance.

We also hypothesised that FAM would improve orienting and alerting. Disengaging attention from distractions and re-orienting them towards the breath could regulate the orienting network. An improved ability to spatially orient attention might allow participants to make better use of spatial cues, leading to increases in the ANT orienting score. The conflict monitoring aspect of FAM resembles a vigilance task, which could regulate the alerting network. If this improvement were to transfer to the ANT, we might expect increases in the alerting score, because of an improved readiness to respond in the absence of an alerting cue.

## 2.2 Experiment 1: Effects of brief focused attention meditation on the Attention Network Test in novices

There is evidence that ANT performance improves in novices after meditation training (Ainsworth et al., 2013; Becerra et al., 2016; Burger & Lockhart, 2017; Jha et al., 2007; Kwak et al., 2020; Schanche et al., 2019; Tang et al., 2007; Tsai & Chou, 2016; Walsh et al., 2019), principally on executive attention scores. Novices also show improvements after a single, brief meditation on a variety of other attention measures. Using fMRI, Dickenson et al. (2013) found that five minutes of meditation lead to greater recruitment of attentional networks, Schofield et al. (2015) found that seven minutes of meditation reduced inattentional blindness, Watier & Dubois (2016) found that 10 minutes of meditation improved emotional Stroop performance, and Colzato et al. (2015) found that 17 minutes of meditation reduced the attentional blink. Only one ANT study involving novices shows signs of improved executive attention after 10 minutes of guided FAM (Norris et al., 2018). In Experiment 1 we explored whether the effect reported by Norris et al. (2018) could be replicated in novices who completed 15 minutes of FAM.

A procedure was designed to induce two contrasting mental states; mindfulness and mind-wandering. A breath counting task was used to induce mindfulness, because it is a meditation technique frequently given to novices, notably in the Zen tradition. In this technique, meditators mentally speak a word, for example ‘rising’ when they are aware of inhaling, and ‘falling’ to note exhaling. Additional noting words are used to remain mindful of experiences other then respiration, as a support for OMM or *vipassanā*.] (Chiesa & Malinowski, 2011). Accuracy on this breath counting task has also been validated as a measure of mindfulness. Participants were instructed to count their breaths in groups of nine, pressing one key on breaths 1–8, and a different key on breath 9. Higher counting accuracy is associated with higher mindfulness, and less mind-wandering (Levinson et al., 2014). The breath counting task was described as a mindfulness meditation, and the instructions stressed that by "letting go" of thoughts and feelings, participants would find it easier to accurately count their breaths. This instruction is central to Buddhist meditation. The control condition consisted of a reading and comprehension test, which has been used in previous research to test for mind-wandering (Sayette et al., 2009).

Two measures were used as an induction check i.e. to test that breath counting did in fact increase mindfulness. The State Mindfulness Scale (SMS, Tanay & Bernstein, 2013) is a self-report measure, with subscales that measure mindfulness in relation to the mind, and the body. Tanay & Bernstein (2013) validated the SMS using mindfulness of breathing, and there is evidence that it can detect increases in mindfulness after brief interventions. Paz et al. (2017) compared smokers deprived of nicotine for 18 hours, who completed either seven minutes of guided present moment attention and awareness (PMAA) training, or listened to a seven minute audio educational track about jazz music. Total SMS score increased to a greater extent in the PMAA group, than in the control group. Lueke & Gibson (2016) compared undergraduate psychology students after they did a 10 minute guided body scan, or one of two audio listening control conditions, with, and without a word-detection task. The SMS score was higher in the body scan condition than either of the control conditions, which did not differ from each other. Bravo et al. (2018) compared 300 psychology undergraduates who completed a guided, four minute body scan, followed by four minutes mindful breathing, or an audio listening task. They found a significant difference between groups on the SMS body subscale, but not on the SMS mind subscale

The second induction check used in Experiment 1 was accuracy on the breath counting task. This addresses the limitations associated with self-report scales (Baer, 2019), and there is preliminary evidence that it predicts important outcome variables such as meta-awareness, over and above self-report mindfulness measures (Hadash & Bernstein, 2019).

We predicted that both SMS subscales, and breath counting task accuracy would be higher after the FAM intervention, relative to the reading control condition. As the SMS and breath counting task accuracy are both measures of state mindfulness, we also predicted that they would be positively correlated. We hypothesised that after FAM, ANT scores would be higher for alerting and orienting, and lower for executive attention relative to the control condition. Experiment 1 was pre-registered at `https://osf.io/j37d4`.

### 2.2.1 Method

#### Participants

Forty psychology students from the University of Plymouth volunteered to participate in exchange for course credits. Nineteen had never meditated before, 13 had meditated twice or less, and the remainder had not meditated more than a few times.

### Attention Network Test

The Attention Network Test (ANT; see Figure 2.2) was adapted from The Experiment Factory library (Sochat, 2018) and ran in a full-screen web browser with a white background.

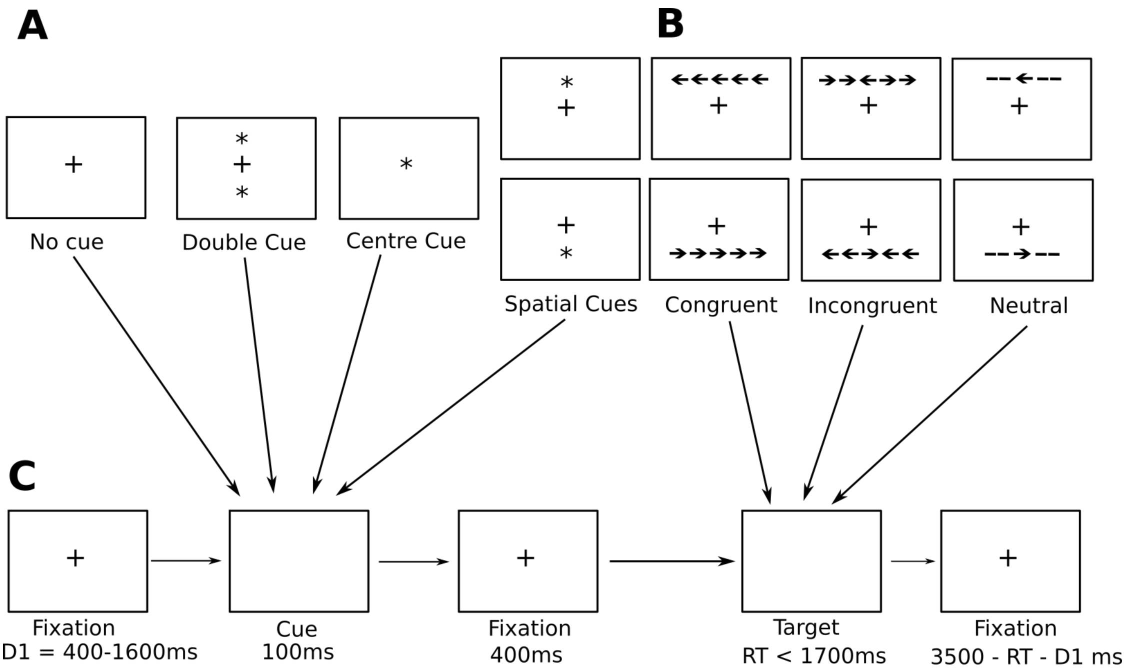


**Figure 2.2:** Attention Network Test. (A) cue types (B) target types (C) trial timings.

Trials began with a central fixation cross displayed for a random interval between 400–1600ms. One of four cue types was then displayed for 100ms. These were a fixation cross (no cue), two asterisks displayed above and below fixation, a central asterisk, or a single asterisk displayed above or below fixation. The single asterisk (spatial) cues were always displayed at the same location as the subsequent target stimulus. The fixation cross and cues were displayed in black, sans serif font at 60px.

The cues were then replaced with a second, central fixation cross for 400ms, followed by the target stimulus, which was displayed centrally above or below fixation. Targets consisted of an arrow pointing left or right, flanked on each side by congruent, incongruent or neutral stimuli. Congruent stimuli were arrows pointing in the same direction as the central arrow, incongruent stimuli were arrows pointing in the opposite direction, and neutral stimuli were lines with no arrow head. The complete target stimulus was 25% of the width, and 7% of the height of the browser window size.

Participants were instructed to press the arrow key matching the direction of the central arrow as quickly and accurately as possible. The target was removed when the participant responded, or after 1700ms if there was no response. A final fixation cross was displayed to make the total trial length 4000ms, by subtracting the response duration and initial fixation cross duration from 3500.

A practice block consisted of 6 nocue trials with targets above fixation, 6 center cue trials with targets above fixation, 6 double cue trials with targets below fixation and 6 spatial cue trials, 3 with targets above fixation and 3 with targets below fixation. The 24 practice trials were presented in a randomised order, and feedback was presented centrally for one second in green for correct responses, and red for incorrect responses.

Each experimental block consisted of a randomised sequence of 48 trials: 2 target locations (above and below fixation) x 4 cue conditions (none, central, double and spatial) x 2 target directions (left and right) x 3 flanker conditions (congruent, incongruent and neutral). There was no accuracy feedback on experimental trials. The experimental task consisted of three blocks, separated by two optional rest periods, making a total of 144 trials. Overall, the task took approximately 15 minutes to complete.

### Breath Counting Task

The breath counting task ran in a full-screen web browser, with a black background. Participants were instructed to sit in a comfortable, upright posture and silently count their breaths, incrementing the count after each in and out cycle. They were instructed to gently place their attention back on the breath, if they noticed it had wandered elsewhere. Participants pressed the down arrow on breaths one to eight, and the right arrow on breath nine, repeating the counting and button pressing, starting from one, each time they reached breath nine. They pressed the down arrow after their first exhale as they counted 'one', then closed their eyes for the remainder of the task. Data was recorded from the first press of the down arrow. The task lasted 15 minutes, during which the screen was blank. A tone sounded to signal the end of the task. The breath counting task was based on an ePrime experiment provided by Levinson et al. (2014, Study 2), and implemented using jsPsych (Leeuw, 2015). Mindfulness was operationalised as correct 9 counts / total cycles.

### Reading task

The control task was based on a 'zoning out' task (Sayette et al., 2009) used to measure mind-wandering. Participants were given 15 minutes to read up to 17 pages of the first chapter of Leo Tolstoy's (1864-1869/2016) War and Peace. The text was displayed on a computer screen in black font against a white background. Each screen contained approximately 22 lines of text. Participants could freely navigate forwards and backwards through pages, by clicking `Next` and `Previous` buttons[3].

[3] https://github.com/earcanal/zoning-out-task

After 15 minutes, or when they had navigated past the final page (whichever was first), participants completed a number of comprehension questions, to test whether they had engaged with the reading task. Each question required a true or false response, and the correct answers to half of the questions were true. The number of questions asked varied according to how many pages the participant had accessed. If all pages were accessed, there were 10 comprehension questions. Questions were presented in the same pseudorandom order.

**Audio instructions**

Instructions for the breath counting task and reading tasks were recorded as audio files. These were matched for number of words and duration (4m 23s). Both recordings began with identical instructions explaining how to sit with a relaxed, alert posture. In the breath counting task condition, participants were told that they would be meditating. The recording explained that letting go of distracting thoughts and feelings would make the breath counting task easier. In the control condition, participants were told they would be doing a reading and comprehension task. Both recordings ended with identical instructions, reminding participants how to maintain a relaxed, alert posture. Scripts for the audio instructions can be found in Appendix A.

**Survey measures**

The State Mindfulness Scale (SMS; Tanay & Bernstein, 2013) is a reliable measure of state mindfulness, consisting of 21 self-report items ranging from 1 (not at all) to 5 (very well). The SMS has two subscales, state mindfulness of mind (15 items, e.g. *I noticed thoughts come and go*), and state mindfulness of body (6 items, e.g. *I noticed some pleasant and unpleasant physical sensations*). Total scores are calculated by adding individual item scores, none of which are reversed. Higher total scores mean higher levels of mindfulness. A demographics survey measured age, gender assigned at birth, dominant hand and meditation experience. Surveys were developed to run in a web browser, using The Experiment Factory (Sochat, 2018).

**Procedure**

Participants were assigned to one of the two experimental groups (breath counting or reading) using a random sequence generated by an R script (R Core Team, 2020). They gave informed consent, then completed the demographics survey, the SMS and the ANT. Next, they used headphones to listen to the audio instructions for the task in their experimental condition. The experimenter checked that the participant had understood the instructions before they began the task. Participants in the reading group were instructed to sit at a distance from the computer screen where they found reading comfortable. After their assigned task, participants completed another ANT, with no practice block, followed by a second SMS. Finally, they were debriefed to end the experiment, which lasted approximately one hour.

### 2.2.2 Results

One additional person was tested to replace a participant who did not complete the second SMS. Data for three participants were excluded from all analyses, based on their response rates in the breath counting task. If the number of button presses was matched to respiration rate, then two of these participants were breathing abnormally slowly (2.5 and 3.4 breaths per minute), and one abnormally fast (54.5 breaths per minute). After exclusions, data from 37 participants (21 female; 17 FAM, 20 controls) were included in the following analyses.

Randomisation was successful. There was some evidence that mean age in the two groups (FAM = 19.76 years, reading = 21.15 years) was similar ($F(1, 35) = 0.59$, $p = .448$, $BF = 0.40$). There were 7 females and 10 males in the FAM group, and 14 females and 6 males in the reading group. There was minimal evidence that these numbers were different to those to be expected by chance ($\chi^2(1, N = 37) = 2.05$, $p = 0.153$, $BF = 1.74$). In the reading group, accuracy for the comprehension questions was above 50% (m = 82.6%, sd = 19.8%), indicating that participants had engaged with the reading task.

**Attention Network Test**

Figure 2.3 shows that alerting scores increased in both groups between time 1 and time 2. Orienting scores increased marginally in both groups. Executive attention scores decreased to a greater extent after reading, than after FAM.

Individual group (2) x time (2) frequentist and Bayesian ANOVAs on alerting, orienting and executive attention scores, were used to test for differences between the FAM and reading interventions. Trials with incorrect responses, RT < 50ms, or RT > three standard deviations above the participant's mean were excluded. Alerting was calculated by subtracting mean RT on double cue trials from mean RT on no cue trials. Orienting was calculated by subtracting mean RT on spatial cue trials from mean RT on center cue trials. Executive attention was calculated by subtracting mean RT on congruent trials from mean RT on incongruent trials, summed across all cue types. Bayesian tests were calculated using the BayesFactor package (Morey & Rouder, 2018).

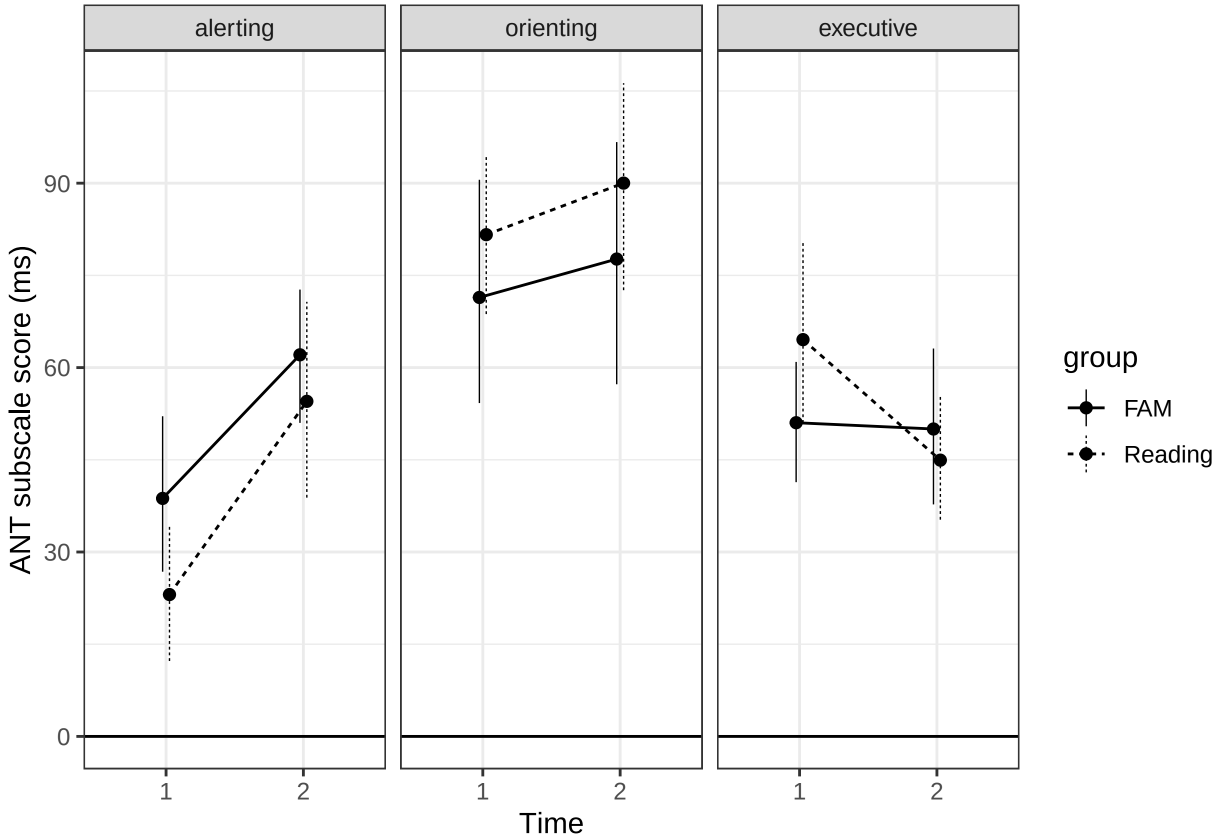


**Figure 2.3:** ANT scores for FAM and Reading groups pre and post intervention. Vertical bars are bootstrapped 95% confidence intervals.

When interpreting Bayes Factors, values > 3 are considered to be evidence supporting a hypothesis, and values < 0.33 as evidence for the null hypothesis[4].

There was decisive evidence that alerting scores increased over time ($F(1, 35) = 30.19$, $p < .001$, $BF = 7{,}168$). There was no evidence of group differences in alerting scores ($F(1, 35) = 1.85$, $p = .182$, $BF = 0.60$). The group x time interaction showed no evidence that the tasks had different effects on alerting ($F(1, 35) = 0.64$, $p = .431$, $BF = 0.47$).

There was no evidence that orienting changed over time ($F(1, 35) = 1.62$, $p = .212$, $BF = 0.46$). There was no evidence of group differences on orienting scores ($F(1, 35) = 1.03$,

[4]Evidence for and against hypotheses based on Bayes Factors is reported following advice in Raftery (1995).

$p$ = .317, $BF$ = 0.56). The group x time interaction showed substantial evidence against the tasks having different effects on orienting ($F(1, 35)$ = 0.03, $p$ = .856, $BF$ = 0.30).

There was minimal evidence that executive attention improved over time ($F(1, 35)$ = 5.51, $p$ = .025, $BF$ = 2.00). There was some evidence that the groups did not differ on executive attention ($F(1, 35)$ = 0.29, $p$ = .592, $BF$ = 0.37). The group x time interaction showed little evidence that the tasks had different effects on executive attention ($F(1, 35)$ = 3.86, $p$ = .058, $BF$ = 1.49).

**State Mindfulness Scale**

SMS reliability, measured before the inductions, was good. Cronbach's alpha for the combined FAM and reading groups was 0.94 for the mind subscale, and 0.86 for the body subscale. Table 2.1 shows that scores on the mind and body subscales increased in both groups after the inductions. The increases were greater for the mind subscale.

**Table 2.1:** SMS scale scores for FAM and Reading groups pre and post intervention. M = mean, SD = standard deviation, MD = mean difference (post-pre).

| | Pre | | | | Post | | | | Post-Pre | |
|---|---|---|---|---|---|---|---|---|---|---|
| | FAM | | Reading | | FAM | | Reading | | FAM | Reading |
| SMS subscale | M | SD | M | SD | M | SD | M | SD | MD | MD |
| body | 19.82 | 5.28 | 18.55 | 5.50 | 20.82 | 4.86 | 19.5 | 5.32 | 1.00 | 0.95 |
| mind | 43.06 | 14.97 | 44.85 | 12.22 | 53.71 | 11.69 | 48.3 | 13.47 | 10.65 | 3.45 |

Individual group (2) x time (2) Bayesian ANOVAs were carried out for the SMS mind and body subscales[5]. Although the mean difference for the mind subscale was higher after FAM, the group x time interaction did not provide evidence for a difference between FAM and Reading ($BF$ = 1.38). For the body subscale, the group x time interaction

[5]Throughout this thesis, frequentist statistics are only reported where they make an additional contribution to an analysis.

showed substantial evidence against a difference in effects of the interventions (*BF* = 0.32). Between subjects Bayesian t-tests for the SMS after the interventions, found no evidence of a difference for the mind (*BF* = 0.67, *d* = -0.44) or body (*BF* = 0.40, *d* = -0.25) subscales.

Figure 2.4 shows the relationship between breath counting task accuracy and the SMS subscales. There was no evidence for a correlation between breath counting task accuracy and SMS mind (*r* = -0.01, *BF* = 0.51), or SMS body (*r* = 0.01, *BF* = 0.51).

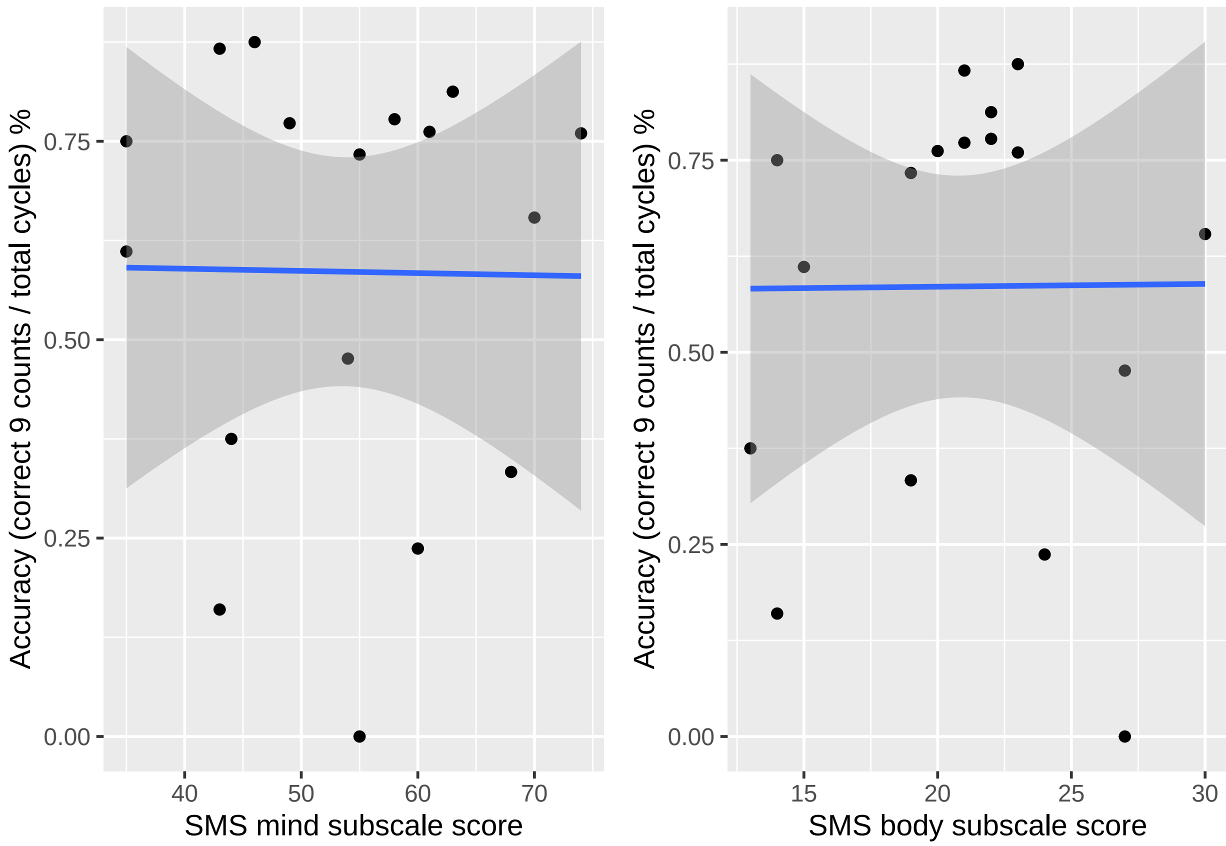


**Figure 2.4:** Scatterplot showing breath counting task accuracy against post-intervention SMS subscales.

### 2.2.3 Discussion

Evidence of group x time interactions on one or more ANT scores would be required to support the hypothesis that meditation regulates attention. We did not observe such interactions, but this might be expected, as there were also no group differences in mindfulness. The main effect of alerting could simply reflect an increase in alertness in both groups, due to engaging in the experiment.

Contrary to our other hypothesis, the SMS did not indicate that mindfulness was greater after FAM than after the reading task. The simplest explanation for this outcome is that the FAM was not long enough to establish a mindful state. This result contrasts with Paz et al. (2017), who found SMS differences after a guided meditation of less than half this duration. However, participants in this study were nicotine deprived smokers. In healthy undergraduates, 15 minutes of breath counting may be insufficient FAM to elevate mindfulness to the extent that differences can be detected using the SMS. There was no positive correlation between breath counting accuracy and the SMS (Figure 2.4). Again, this could be because the degree of mindfulness induced by breath counting was too weak to register on these measures. The common factor here could be one of expertise. In people who are otherwise healthy, mindfulness could be induced more quickly and to a greater degree in long-term meditators, because they are familiar with the method of inducing this mental state.

The test for elevated mindfulness might also have failed because the ANT took place before the second SMS measurement. The ANT was measured immediately after the inductions on the basis that the mental states induced are likely to be temporary, especially in novices. Administering the SMS before the ANT risked altering these states before participants completed the ANT, which was the more important outcome measure. The task order meant that when the second SMS was administered, effects of the inductions could have been reduced due to the passage of time, or to effects of completing the ANT.

*2.2. Experiment 1: Effects of brief focused attention meditation on the Attention Network Test in novices*

Paz et al. (2017) administered the SMS before, and immediately after their mindfulness and control interventions, which could explain why they observed differences at much shorter durations. If a mindful state is short lived, then detection may require measurement immediately after an intervention, especially where participants are not experienced in the technique used to increase mindfulness. To be certain that our meditation induced mindfulness over and above the control task would require a test for elevated mindfulness immediately after the inductions.

These results are inconclusive. If the effects of meditation on mindfulness and attention are small, then the study lacked statistical power to detect group differences. One way to address this would be to increase sample sizes with the aim of providing evidence for the absence of mindfulness and attention regulation effects after brief FAM in novices. However, the primary aim was to find circumstances were there *were* group differences in mindfulness, to establish whether these caused differences in attention regulation. For this reason, an alternative approach was taken by exploring the effects of meditation experience. Experiment 2 used the same paradigm to test whether a brief period of FAM can increase mindfulness and regulate attention in long-term meditators.

## 2.3 Experiment 2: Effects of brief focused attention meditation on the Attention Network Test in long-term meditators

Experiment 1 tested participants with no meditation experience, and the results indicated that insufficient mindfulness was induced to observe differences on the ANT. To test whether these results were due to participants' lack of meditation experience, we ran a similar experiment with experienced meditators. As in Experiment 1, we hypothesised that the SMS would be higher after the FAM intervention, relative to the reading control condition. We also hypothesised that increases in mindfulness after FAM would correspond with improved ANT scores. If supported, these hypotheses would provide evidence that mindfulness mediates attention regulation in experienced meditators.

### 2.3.1 Method

**Participants**

People living near to Plymouth or Exeter, UK were offered £8 to participate in a study investigating meditation and attention. Inclusion criteria were at least 12 months of meditation experience, and a minimum of three meditations per week, using a meditation technique which included some time focusing attention on a single object, such as the breath. Twenty-seven people (15 female) meeting these criteria were included in the study.

**Breath Counting Task**

The breath counting task was identical to Experiment 1 (section 2.2.1), except participants' eyes were open throughout the task. The end of the task was signaled by a message displayed on the screen, rather than an audio tone. The breath counting task was provided by Levinson et al. (2014, Study 2), and implemented in ePrime.

**Survey Measures**

The SMS and demographics measures were as described in Experiment 1 (section 2.2.1). The Five Facet Mindfulness Questionnaire (Baer et al., 2008, FFMQ; 2006) was used as an additional check for baseline differences in trait mindfulness between the FAM and control groups. The FFMQ measures trait mindfulness using 39 self-report questions, with scores ranging from 1 (never or very rarely true) to 5 (very often or always true). The five mindfulness factors measured are observing (e.g. *I notice the smells and aromas of things*), describing (e.g. *I am good at finding words to describe my feelings*), acting with awareness (e.g. *I find myself doing things without paying attention*, reverse scored), nonjudging of inner experience (e.g. *I think some of my emotions are bad or inappropriate and I should not feel them*, reverse scored), and nonreactivity to inner experience (e.g. *I perceive my feelings and emotions without having to react to them*).

## Procedure

Participants were randomly assigned to meditation or control groups using computer software[6]. Variation in meditation experience was balanced between groups based on years of practice, amount of weekly practice, and percent of time spent focusing on a single meditation object. Meditation experience was categorised as high if it was above the median number of self-reported years of practice, otherwise it was categorised as low. Weekly practice intensity (days/week × minutes/day) was used to place participants into high, medium or low intensity groups. Time spent focusing on a single meditation object was classified as high if participants rated this as >= 50%, or low if rated as < 50%.

Testing took place in a psychology lab at Plymouth University, or at a quiet location in the participant's home. After the participant gave informed consent, the investigator conducted a semi-structured interview lasting approximately five minutes. This was to confirm the accuracy of values for number of years of meditation experience, days of practice per week, minutes of practice per day, and percent of time spent focusing on a single meditation object. It was also assumed that the interview was sufficient for participants in the breath counting task condition to treat the task as a meditation.

Next, participants completed the demographics survey. In contrast with Experiment 1, they then completed the FFMQ (rather than the SMS) before the first ANT. As in Experiment 1, participants completed either the breath counting task or the reading task, followed by a second ANT and the SMS. Participants were debriefed to end the experiment, which took approximately one hour.

[6] `http://minimpy.sourceforge.net/`

### 2.3.2 Results

Two participants' data were excluded due to technical failures with the ANT, so data from 25 participants (14 female; FAM, controls) were analysed. Randomisation was successful. There was some evidence that mean age in the two groups (FAM = 47.46 years, reading = 52.75 years) was similar ($F(1, 23) = 1.18$, $p = .288$, $BF = 0.57$). There were 8 females and 5 males in the FAM group, and 6 females and 6 males in the reading group. There was some evidence that these numbers were the same as to be expected by chance ($\chi^2(1, N = 25) = 0.03$, $p = 0.859$, $BF = 0.54$).

Table 2.2 shows that groups were balanced on years of meditation experience, days of meditation per week, minutes of meditation per day, and percentage of their meditation time spent focusing on a single object, such as the sensations of breathing. In the reading group, accuracy for the comprehension questions was above 50% (m = 70.7%, sd = 28.4%), indicating that participants had engaged with the reading task.

**Table 2.2:** Meditation experience for FAM and Reading groups.

| | FAM | | | | Reading | | | | t |
|---|---|---|---|---|---|---|---|---|---|
| Meditation | Min | Max | M | SD | Min | Max | M | SD | BF |
| Days/week | 4 | 7 | 6.23 | 1.09 | 3 | 7 | 5.67 | 1.23 | 0.63 |
| Experience (years) | 1 | 22 | 7.38 | 6.78 | 1 | 41 | 15.33 | 16.89 | 0.37 |
| Minutes/day | 10 | 90 | 36.92 | 30.72 | 10 | 70 | 37.92 | 20.17 | 0.37 |
| Single object (%) | 5 | 100 | 60.00 | 30.96 | 5 | 90 | 60.83 | 22.45 | 0.89 |

Reliability was acceptable for the FFMQ in the combined FAM and reading groups, with Cronbach's alphas of 0.79 for observing, 0.9 for describing, 0.83 for acting with awareness, 0.89 for nonjudging of inner experience, and 0.88 for nonreactivity to inner experience. Descriptive statistics for the FFMQ are shown in Table 2.3. Individual Bayesian t-tests were carried out for each of the FFMQ factors. There was no evidence of group differences for the actaware (*BF* = 0.38), nonjudge (*BF* = 0.45), and observe (*BF* = 0.45) factors. There was little evidence for group differences for the describe (*BF* = 0.75) and nonreact factors (*BF* = 1.94). These results indicate that randomisation successfully balanced trait mindfulness in the two groups.

**Table 2.3:** Pre-induction mean (M) and standard deviation (SD) FFMQ subscale scores for FAM and Reading groups.

| | FAM | | Reading | |
|---|---|---|---|---|
| FFMQ factor | M | SD | M | SD |
| actaware | 29.46 | 3.82 | 29.83 | 3.74 |
| describe | 30.46 | 6.41 | 33.67 | 4.74 |
| nonjudge | 32.15 | 4.96 | 30.33 | 7.23 |
| nonreact | 27.77 | 3.61 | 24.42 | 4.06 |
| observe | 34.15 | 3.72 | 33.00 | 4.13 |

**Attention Network Test**

ANT exclusions and calculations were as described in Experiment 1 (Section 2.2.2). Figure 2.5 shows that alerting scores increased by a similar amount in both groups between time 1 and time 2. Orienting scores increased in both groups, with a greater increase after reading than after FAM. Executive attention scores decreased to a similar extent in both FAM and reading groups.

Individual group (2) x time (2) frequentist and Bayesian ANOVAs on alerting, orienting and executive attention scores, were used to test for differences between the FAM and reading groups. There was moderate evidence that alerting scores did not change over time ($F(1, 23) = 0.88$, $p = .358$, $BF = 0.43$). There was moderate evidence against group differences in alerting scores ($F(1, 23) = 0.01$, $p = .920$, $BF = 0.36$). The group x time interaction also provided moderate evidence against the tasks having different effects on alerting scores ($F(1, 23) = 0.00$, $p = .948$, $BF = 0.37$).

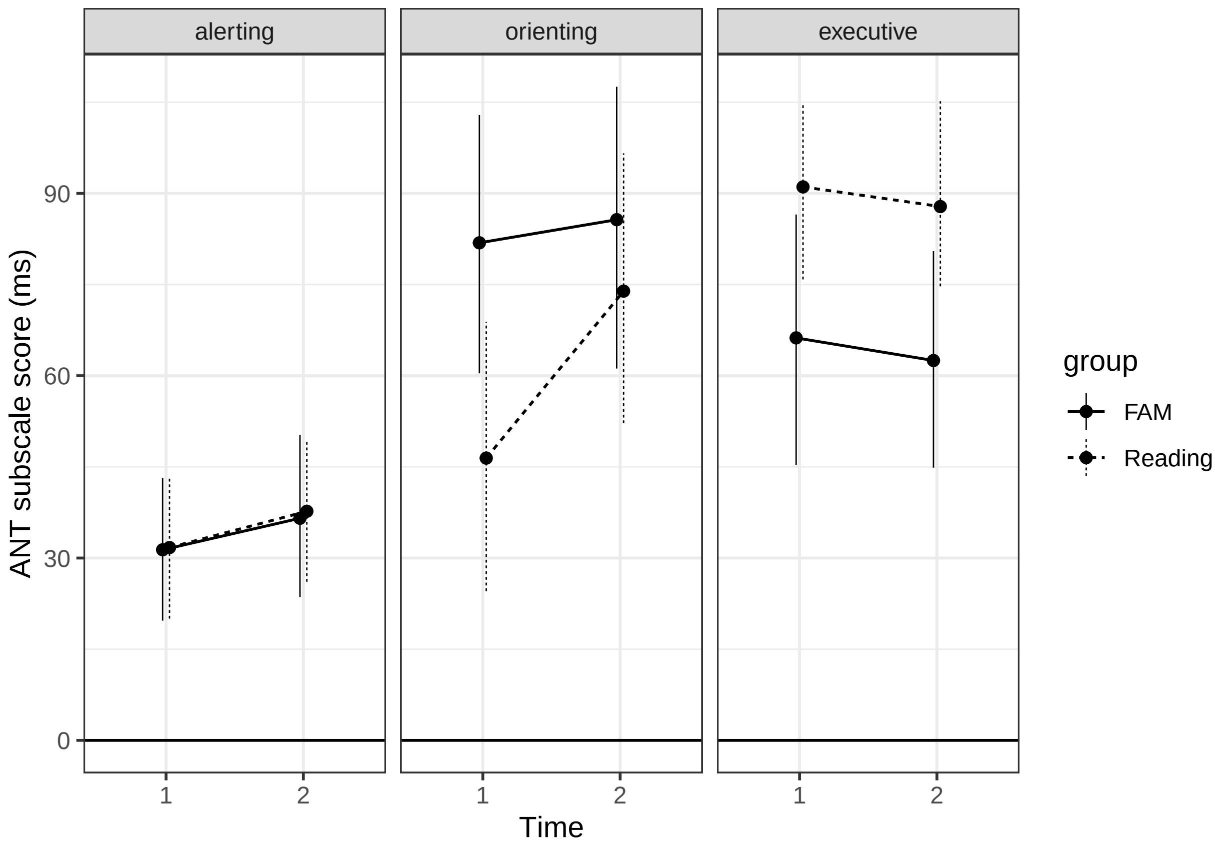


**Figure 2.5:** ANT scores for FAM and Reading (control) groups pre and post intervention. Vertical bars are bootstrapped 95% confidence intervals.

There was some evidence that orienting scores increased over time ($F(1, 23) = 6.96$, $p = .015$, $BF = 2.66$). There was no evidence of group differences in orienting scores ($F(1, 23) = 2.35$, $p = .139$, $BF = 0.95$). The group x time interaction provided little evidence that the tasks had different effects on orienting scores ($F(1, 23) = 4.23$, $p = .051$, $BF = 1.48$).

There was substantial evidence that executive attention scores did not change over time ($F(1, 23) = 0.28$, $p = .603$, $BF = 0.31$). There was minimal evidence for group differences in executive attention scores ($F(1, 23) = 4.49$, $p = .045$, $BF = 1.86$). The group x time interaction provided some evidence against the tasks having different effects on executive attention scores ($F(1, 23) = 0.00$, $p = .971$, $BF = 0.38$).

**State Mindfulness Scale**

SMS reliability was good. Cronbach's alpha in the combined FAM and reading groups was 0.94 for the mind subscale, and 0.85 for the body subscale. Descriptive statistics for the SMS are shown in Table 2.4. Bayesian t-tests found no evidence of group differences on either the SMS mind (*BF* = 0.51, *d* = 0.38), or body (*BF* = 0.37, *d* = 0.04) subscales.

**Table 2.4:** Post-induction mean (M) and standard deviation (SD) SMS scores for FAM and Reading groups.

| | FAM | | Reading | |
|---|---|---|---|---|
| SMS subscale | M | SD | M | SD |
| body | 22.69 | 5.69 | 22.92 | 4.52 |
| mind | 53.85 | 13.53 | 58.58 | 11.14 |

Figure 2.6 suggests that breath counting task accuracy was negatively associated with both the SMS mind and body subscales. However, the Bayes Factors did not provide evidence for the moderate, negative correlations between breath counting task accuracy and SMS mind (*r* = -0.4, *BF* = 1.07), or SMS body (*r* = -0.39, *BF* = 1.03) subscales.

### 2.3.3 Discussion

The absence of group x time interactions on the ANT indicated that there were no differences in attention between FAM and reading groups in this sample of experienced meditators. In common with Experiment 1, this finding is best explained by the corresponding absence of differences in mindfulness. The ANT comparisons lacked statistical power due to the small sample size. With 13 participants per group, power = .8, and $\alpha$ = .05, these tests would only detect moderate to large group differences (*r* = 0.7).

Contrary to our other hypothesis, neither of the SMS subscales indicated higher mindfulness after FAM relative to the reading control task. This was the same finding

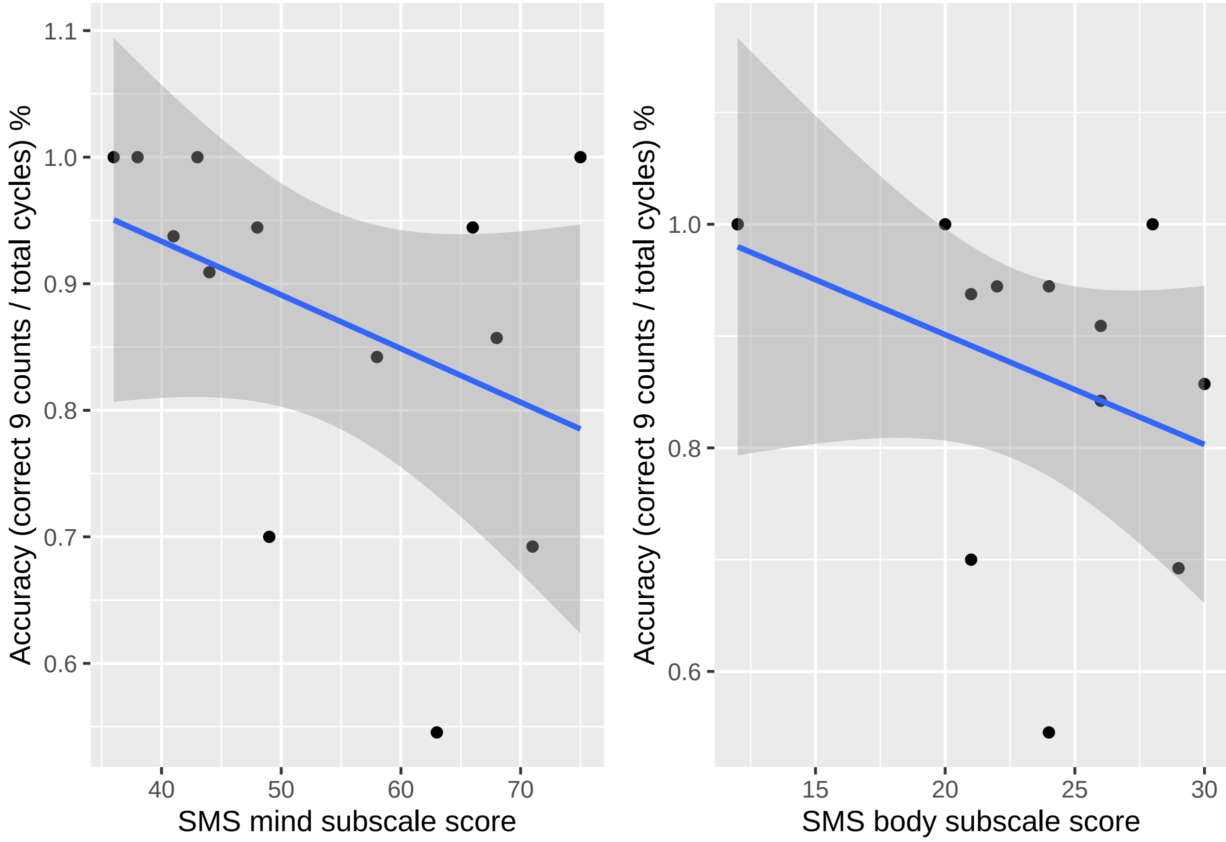


**Figure 2.6:** Scatterplot showing breath counting task accuracy against SMS subscales..

as in Experiment 1. Again, the simplest explanation for this outcome is that 15 minutes of FAM is not sufficient to induce a mindful state, even for participants with a moderate amount of meditation experience. This interpretation is provisional, because the SMS comparisons were underpowered. With 13 participants per group, power = .8, and $\alpha$ = .05, the *t*-tests used to compare SMS scores would only detect large group differences (*d* = 1.15). Additional power would be required to establish whether or not this brief FAM successfully induced mindfulness in experienced meditators. It could also be argued that the comparison of SMS scores was less accurate than the comparison of differences between baseline and post-ANT SMS in Experiment 1. However, as the randomisation checks were successful, the post-intervention SMS comparison theoretically estimates the same quantity as a comparison of changes from baseline (Higgins et al., 2019, Section 6.5.2.8).

## 2.4 Experiment 3: Effects of brief focused attention meditation on the Attention Network Test in long-term meditators

Experiment 3 aimed to address the statistical power limitations in Experiment 2, which prevented clear conclusions being drawn regarding the effects of brief FAM on the ANT in experienced meditators. This was done by replicating Experiment 2 with a larger sample size. Because people with meditation experience are relatively rare and geographically dispersed, recruitment was done online, and participants were asked to complete the experiment in their homes. Hypotheses were the same as Experiment 2. Experiment 3 was pre-registered at `https://osf.io/v4me9`.

### 2.4.1 Method

**Participants**

Regular meditators were recruited via Facebook using virtual snowball sampling (Baltar & Brunet, 2012), and personal contacts. Inclusion criteria were that participants should understand written English, and have meditated at least once per week for the previous six months. People were not paid to participate in the experiment.

Of the 101 people recruited, 69 began the experiment. Twenty participants with incomplete datasets were excluded, as were three participants who took a long break between the first ANT and the breath counting task. Three participants with large numbers of no-response trials on either ANT were also excluded. One participant was excluded because their button press rate in the breath counting task indicated an abnormally slow respiration rate (one breath every 1.25 minutes). There were 42 participants (12 female; 20 FAM, 22 controls) after these exclusions.

**Materials**

The ANT, breath counting task, reading task, demographics survey and State Mindfulness Scale were as described in Experiment 1 (Section 2.2.1). The Five Facet Mindfulness Questionnaire was as described in Experiment 2 (Section 2.3.1). Participants used a questionnaire to indicate their weekly meditation frequency (daily or almost daily, 2-4 times, approximately once, less than once), the duration of their daily meditations (5-15m, 15-30m, >30m), and the number of years of they had meditated at this frequency and duration. They answered the same questions if they had a previous meditation practice where the meditation type, or frequency differed. For each meditation retreat they had attended, participants were asked to calculate the duration in days multiplied by the number of hours of meditation per day, and to report a grand total. Lifetime meditation experience was estimated using the formula described by Hasenkamp and Barsalou (2012, p. 11).

The experiment ran online in a web browser. Participant access, task sequencing, and data collection was managed using an instance of The Experiment Factory (Sochat, 2018), hosted in a Docker container at a UK data centre. Surveys, tasks and forms were developed using HTML and JavaScript.

**Procedure**

Two hundred, single-use experimental tokens (one hundred per condition) were randomised using an R script. Upon recruitment, participants were emailed the next available token, and the website address for running the experiment. They were informed that the experiment would take approximately one hour to complete. After entering their token, participants completed a consent form. They were advised that they would need a desk, a comfortable, straight-backed chair, a computer (not a phone or tablet) with a web browser, and an Internet connection. They were reminded, before starting the experiment, to ensure that they would not be disturbed for one hour. After consenting to participate, they completed the demographics survey, the FFMQ and the ANT. After the breath counting task or reading task, they completed a second ANT, followed by the SMS. Finally they completed the meditation expertise questionnaire, and ticked a box on a debrief form to end the experiment.

### 2.4.2 Results

Randomisation was successful. There was no evidence of a difference in mean age (FAM = 39.7 years, reading = 46.77 years) between the two groups ($F(1, 40) = 2.86$, $p = .099$, $BF = 0.93$). There were 3 females and 17 males in the FAM group, and 9 females and 13 males in the reading group. There was some evidence that these numbers were the same as to be expected by chance ($\chi^2(1, N = 42) = 2.29$, $p = 0.130$, $BF = 2.08$). There was no evidence ($BF = 0.32$) of differences in meditation expertise between the FAM (m = 892 hours; range = 51 – 3845 hours), and reading (m = 994 hours; range = 27 – 3156 hours) groups. Comprehension accuracy was above 50% (m = 80.8%, sd = 27.4%), indicating that participants in the reading group had engaged with the task.

Randomisation successfully matched the two groups on mindfulness traits. Reliability was good for the FFMQ in the combined FAM and reading groups, with Cronbach's alphas of 0.86 for observing, 0.85 for describing, 0.86 for acting with awareness, 0.88 for nonjudging of inner experience, and 0.8 for nonreactivity to inner experience. Descriptive statistics for the FFMQ are shown in Table 2.5. Individual Bayesian t-tests were carried out for each of the FFMQ factors. There was substantial evidence of no group differences for the actaware (*BF* = 0.31) and observe (*BF* = 0.30) factors, some evidence of no group differences for nonjudge (*BF* = 0.32) and describe (*BF* = 0.32), and minimal evidence that the nonreact factor was higher in the FAM group (*BF* = 2.02).

**Table 2.5:** Pre-induction mean (M) and standard deviation (SD) FFMQ subscale scores for FAM and Reading groups.

| | FAM | | Reading | |
|---|---|---|---|---|
| FFMQ factor | M | SD | M | SD |
| actaware | 27.65 | 4.09 | 27.36 | 4.11 |
| describe | 30.55 | 5.39 | 30.00 | 4.36 |
| nonjudge | 30.25 | 6.50 | 30.91 | 4.99 |
| nonreact | 26.90 | 3.18 | 24.68 | 3.31 |
| observe | 29.80 | 5.93 | 29.64 | 3.97 |

**Attention Network Test**

ANT exclusions and calculations were as described in Experiment 1 (Section 2.2.2). Figure 2.7 shows that alerting scores declined in both groups between time 1 and time 2, and to a greater degree in the reading group. Orienting scores decreased marginally both groups. Executive attention scores decreased in both groups.

Individual group (2) x time (2) ANOVAs on alerting, orienting and executive attention scores, were used to test for differences between the FAM and reading interventions. There was no evidence that alerting efficiency changed over time ($F(1, 40) = 2.40$, $p = .129$,

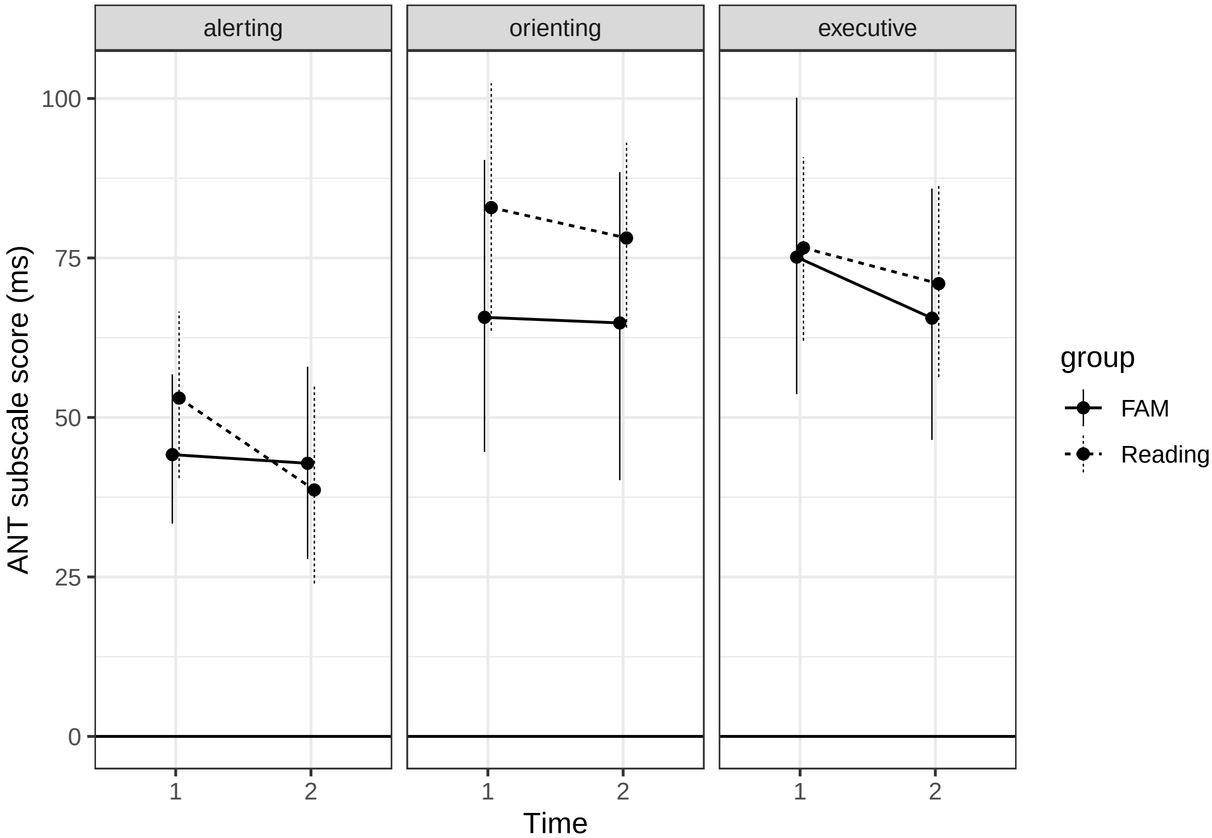


**Figure 2.7:** ANT scores for FAM and Reading (control) groups pre and post intervention. Vertical bars are bootstrapped 95% confidence intervals.

*BF* = 0.62). There was some evidence against group differences in alerting efficiency ($F(1, 40) = 0.07$, $p = .795$, $BF = 0.35$). The group x time interaction provided no evidence that the tasks had different effects on alerting efficiency ($F(1, 40) = 1.52$, $p = .225$, $BF = 0.58$).

There was substantial evidence that orienting efficiency did not change over time ($F(1, 40) = 0.26$, $p = .615$, $BF = 0.25$). There was no evidence of group differences in orienting scores ($F(1, 40) = 1.23$, $p = .274$, $BF = 0.65$). The group x time interaction provided substantial evidence against the tasks having different effects on orienting scores ($F(1, 40) = 0.11$, $p = .738$, $BF = 0.32$).

There was no evidence that executive attention scores changed over time ($F(1, 40) = 1.47$, $p = .233$, $BF = 0.43$). There was some evidence against group differences in executive attention efficiency ($F(1, 40) = 0.08$, $p = .779$, $BF = 0.39$). The group x time interaction provided substantial evidence against the tasks having different effects on executive attention efficiency ($F(1, 40) = 0.10$, $p = .751$, $BF = 0.27$).

**State Mindfulness Scale**

SMS reliability, was good. Cronbach's alpha in the combined FAM and reading groups was 0.91 for the mind subscale, and 0.84 for the body subscale. Descriptive statistics for the post-intervention SMS are shown in Table 2.6. Bayesian t-tests found no evidence of differences between the FAM and reading groups on the SMS mind ($BF = 0.32$, $d = 0.11$), or body ($BF = 0.34$, $d = -0.16$) subscales.

**Table 2.6:** Post-induction mean (M) and standard deviation (SD) SMS scores for FAM and Reading groups.

| | FAM | | Reading | |
|---|---|---|---|---|
| SMS subscale | M | SD | M | SD |
| body | 20.65 | 5.13 | 19.82 | 5.12 |
| mind | 54.00 | 12.88 | 55.23 | 8.95 |

One participant reversed the response buttons in the breath counting task. Their responses for this task were reversed prior to analysis. Figure 2.8 appears to show a small, positive relationship between breath counting task accuracy and both SMS subscales. However, Bayes Factors did not provide evidence for correlations between breath counting task accuracy and SMS mind ($r = 0.05$, $BF = 0.48$), or SMS body ($r = 0.05$, $BF = 0.48$) subscales.

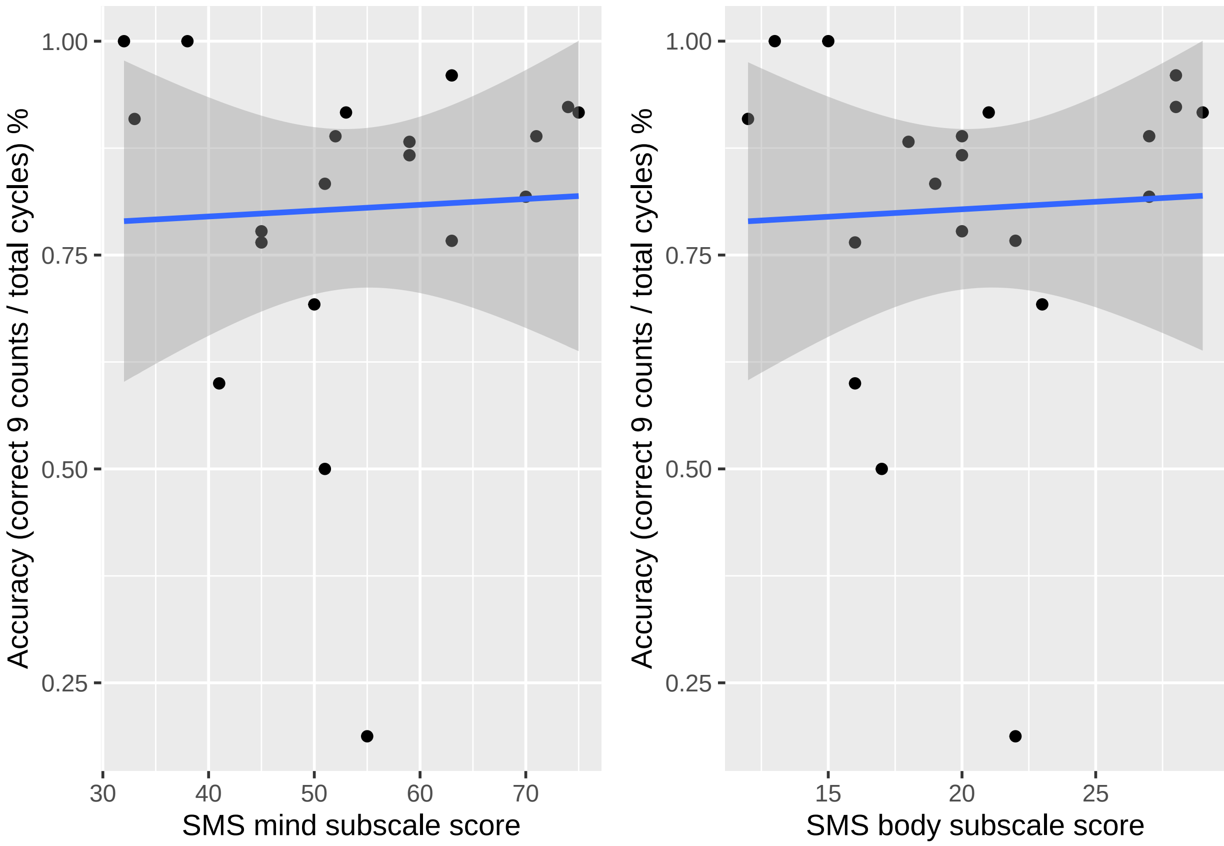


**Figure 2.8:** Scatterplot showing breath counting task accuracy against SMS subscales..

### 2.4.3 Discussion

As in Experiment 2, there was either no evidence, or evidence against group x time interactions on the ANT. This indicated that there were no differences in attention between the breath counting task and reading groups, and would be expected if there were also no group differences in mindfulness. As in Experiment 2, the ANOVAs used to compare ANT scores were underpowered. With 20 participants per group, power = .8, and $\alpha$ = .05, these tests would only detect moderate group differences ($r$ = 0.58).

The SMS indicated that there was no increase in mindfulness after the breath counting task relative to the reading control task. This was contrary to our hypothesis, but consistent with Experiments 1 and 2. As with the previous experiments, this might indicate that 15 minutes of breath counting is not sufficient to produce a mindful state distinct from the state induced by the reading task.

However, Experiment 3 lacked the power to detect group differences in mindfulness. The SMS mind subscale in Experiments 2 and 3 was higher in the reading group, with a mean effect size of $d = 0.25$. The SMS body subscale was higher in the reading group in Experiment 2, but higher in the FAM group in Experiment 3, with a mean effect size of $d = -0.06$. With these effect sizes, $\alpha = .05$ and power = .8, reliably establishing whether the breath counting task or reading task produced higher levels of mindfulness would require 261 participants per group for the SMS mind subscale, and 4452 participants per group for the SMS body subscale.

The sample sizes in Experiment 3 were too small to resolve the power issues it aimed to address. There were only 7 more participants in the FAM group, and 10 more in the control group than in Experiment 2. Enough participants were recruited for 50 per group, but more than half were excluded because they signed up but did not subsequently participate, or did not complete the experiment correctly. This is in line with known attrition rates of 30–50% in online studies (Zhou & Fishbach, 2016). Recruiting large enough samples to adequately power a between subjects design with small effect sizes is challenging. Online studies can make it easier to reach participants with meditation experience, but attrition rates and noisy data should be considered in this type of design.

## 2.5 General Discussion

The paradigm in Experiments 1–3 was designed to test whether mindfulness mediates attention regulation, by comparing two theoretically distinct mental states: mindfulness and mind-wandering. Mindfulness was induced using a form of FAM (the breath counting task), and mind-wandering using a reading and comprehension task. The ANT was chosen as a measure of attention regulation to allow comparison with similar studies. We tested claims that brief mindfulness inductions affect attention, which have yet to be validated using the ANT. In Experiment 1 we tested novices, and in Experiments 2 and 3, we tested experienced meditators.

The simplest conclusion to be drawn from the results of these experiments is that 15 minutes of breath counting does not affect ANT scores in either novices, or meditators with moderate levels of experience. For novices, the study most similar to Experiment 1 is Norris et al. (2018), who claim that executive attention improves after 10 minutes of guided FAM, relative to a listening control task. However, evidence that interventions have different effects would require a group x time interaction for ANT measurements before and after the interventions. Norris et al. (2018) found a moderate RT improvement on both congruent and incongruent flanker trials in their meditation group ($\eta_p^2$ = .09.), but there was no group x time interaction. In this respect, Experiment 1 was consistent with Norris et al. (2018), because it found no group x time interaction for executive attention, alerting or orienting.

This finding conflicts with other studies involving novices, which find that attention improves after brief meditation on tasks other than the ANT. This could be because the ANT is less sensitive to the effects of meditation, or because the study designs or samples are not comparable.

Turning to the experienced meditators, no previous study has tested whether brief meditation affects the ANT in a similar population. Consistent with Experiment 1, there were no group x time interactions on the ANT in Experiments 2 and 3, which suggests that the meditation may have been too short to affect mindfulness and attention, regardless of participants' meditation experience.

The SMS mean differences provide some interesting numerical contrasts, even though there were were no statistical differences between groups. Table 2.7 shows that overall, effect sizes were small, but tended to be slightly higher for the mind subscale than the body subscale. In Experiment 1, both subscales indicated that mindfulness improved after the breath counting task relative to the reading task. On the contrary, experienced meditators showed higher mindfulness levels after the reading task except on the body subscale in Experiment 3. This pattern of results could indicate that the paradigm is suitable for novices, but not for groups with moderate levels of meditation experience.[7] A higher powered replication of Experiment 1 could establish whether the paradigm is suitable for novices.

These conclusions are provisional because comparisons on both the SMS and the ANT were underpowered. In all experiments, our SMS measurements indicated that there were no group differences in mindfulness after the inductions. However, as effect sizes on the SMS were small, mindfulness and mind-wandering may have been induced, but we may not have had enough statistical power to detect this. The ANT was also

[7]The effects might also differ in participants with higher levels of lifetime meditation experience than the 4,000 hour maximum in Experiment 3.

underpowered in all experiments. The results are consistent with previous research, which predominantly only finds differences on the ANT after extended mindfulness training. However, the power issues would need to be addressed to have confidence that this is in fact that case. For example, given the effect sizes in Table 2.7, $\alpha$ = .05 and power = .8, 83 participants would be required to detect differences on the SMS mind subscale, and 253 participants to detect differences on the SMS body subscale.

**Table 2.7:** Effect sizes (Cohen's *d*) for breath counting task and Reading groups in Experiments 1–3. Negative effects indicate higher mindfulness in breath counting task group.

| Experiment | SMS mind | SMS body |
|---|---|---|
| 1 (novice) | -0.44 | -0.25 |
| 2 (experienced) | 0.38 | 0.04 |
| 3 (experienced) | 0.11 | -0.16 |

Using the breath counting task to both induce mindfulness and act as an induction check had some limitations. Breath counting accuracy was used as a behavioural measure to address the self-report limitation of the SMS, and also to compare the relationship between these two measures of mindfulness. The effect size of this relationship is unknown, and it is likely that these studies did not have sufficient power to detect such a relationship. For example, the 20 FAM participants in Experiment 3 would allow us to detect correlations with $r$ = 0.58 at $\alpha$ = .05 and power = .8. Correlations between the SMS and breath counting task in Experiment 3 were much lower than this ($r$ = 0.05). It would be better to use separate experiments to establish the relationship between the SMS and breath counting task.

A guided meditation might have been more powerful than the breath counting task as a mindfulness induction. Because the breath counting task needed to be completed in silence, the instructions which framed it as a meditation were listened to before the task.

In contrast, meditation instructions are often spread throughout a guided meditation, and controlled for in an audio track without mindfulness instructions (Paz et al., 2017; Schofield et al., 2015; Zeidan et al., 2015). Carefully timed instructions amongst periods of silence help meditators to build and maintain mindfulness, especially those meditating for the first time. In addition, guided meditation is rarely combined with a secondary task, such as the button pressing aspect of the breath counting task. This secondary task might benefit participants with experience of breath counting during meditation, but it could also distract people, especially novices, from applying the meditation instructions.

There were also limitations relating to the control task. First, we did not include a manipulation check for mind-wandering. Because the breath counting task was primarily a mindfulness induction, we did not include self-caught and probed mind-wandering measures (Levinson et al., 2014, Study 1). Similarly, we excluded these measures from the reading and comprehension task described by Sayette et al. (2009). Including mind-wandering probes would have allowed us to directly compare mind-wandering after the inductions. Second, the comprehension questions were quite challenging, and might have reduced mind-wandering induced by the reading task. This could be addressed by testing reading comprehension after the final SMS measurement.

Taken together, these experiments do not provide evidence that FAM induces mindfulness, or regulates attention. However, they also don't provide evidence against mindfulness mediating attention regulation, as we would not expect effects on the ANT if the FAM inductions were not powerful enough to induce mindfulness.

In these experiments, the largest differences in mindfulness were in novices. Previous research indicates that novices require some training before their ANT performance differs from control groups. Therefore, in the next experiment we tested the whether ANT performance improved after novices received a period of mindfulness training.

# 3

# The effects of focused attention meditation training on the Attention Network Test

## Contents



## 3.1 Introduction

The amount of meditation in Experiments 1–3 was probably insufficient to detect attention regulation differences. After 15 minutes of FAM, there were no group differences in state mindfulness or the ANT, in either novices or experienced meditators. These results do not provide evidence *against* mindfulness mediating attention regulation, because group differences on attention would not be expected without corresponding differences in mindfulness. They also correspond with many people's subjective experience of meditation, in that it normally takes more than one session to both understand meditation instructions, and develop confidence in applying them.

Other findings also suggest that novices need more than a single, brief meditation session to improve their ANT performance. Table 3.1 summarises training durations and ANT outcomes for all research involving novice meditators. Executive attention improved across a range of mindfulness interventions at durations of 100 minutes or greater. Differences for alerting and orienting only appeared at longer durations. Tsai & Chou (2016) found that 600 minutes of FAM training improved all ANT scores. With almost double this amount of training, Becerra et al. (2016) also found that FAM improved alerting and executive attention. Unlike Tsai & Chou (2016) however, Becerra et al. (2016) found that orienting scores were higher for the control group than for the FAM group[1]. The reason for these orienting differences is unclear, but could be due to differences in either meditation frequency or the control group. Participants in Tsai & Chou (2016) meditated once per week for 50 minutes for 12 weeks, and were compared with a no-training control group. Participants in Becerra et al. (2016) meditated daily for 24 minutes over 8 weeks, and were compared with a waitlist control group. It is also unclear why the study with the highest training duration (Kwak et al., 2020) found effects for executive attention, but not for alerting or orienting, when there were effects in these attentional networks with less meditation. The consistent pattern in

[1] Outcomes for these studies in Table 3.1 differ from the original authors' interpretations. They are based on an analysis of data provided by Tsai & Chou (2016) and a re-interpretation of the results reported by Becerra et al. (2016).

**Table 3.1:** Controlled mindfulness training (MT) studies involving novices.

| | | Group with better ANT score | | |
|---|---|---|---|---|
| Publication | MT minutes | Alerting | Orienting | Executive |
| Tang et al. (2007) | 100* | = | = | IBMT |
| Burger and Lockhart (2017) | 260 | = | = | MM |
| Walsh et al. (2019) | 334 | = | = | MT |
| Tsai and Chou (2016, Experiment 2) | 600* | FAM | FAM | FAM |
| Becerra et al. (2016) | 1075 | FAM | waitlist | FAM |
| Kwak et al. (2020) | 1140* | = | = | MM |

*Note:*
FAM = Focussed Attention Meditation, MM = Mindfulness Meditation, MT = Mindfulness Training, IBMT = Integrative Body-Mind Training
* Maximum suggested minutes (compliance data not reported).

this data is that for novices, executive attention improves after 100 or more minutes of mindfulness meditation.

Experiments 1–3 suggested that a single, brief FAM session was not sufficient to differentiate groups in terms of mindfulness or the ANT. We addressed this issue by running two experiments with more extensive mindfulness training.

## 3.2 Experiment 4: The effects of four weeks of FAM training on the ANT

Experiment 4 was a partial replication of Tsai and Chou (2016, Experiment 2). The experimental designs were identical, but in Experiment 4 participants were asked to complete slightly less meditation than the 600 minutes in Tsai & Chou (2016). In Experiment 4, participants meditated daily for four weeks, rather than weekly for twelve weeks, and the FAM training focused on mindfulness of breathing rather than body awareness. We predicted that participants would have better alerting, orienting and executive attention scores after four weeks of daily FAM training, relative to a control group waiting for the same training.

### 3.2.1 Method

#### Participants

Posters and online advertisements at the University of Plymouth invited people to participate in a free meditation course and research study, in exchange for course credits or £16. Respondents who self-reported having meditated 10 times or less were recruited and offered a course. Those who reported having meditated more than 10 times were included in the study if the experimenter judged them to have minimal experience of breath meditation. The 63 participants recruited were divided into five cohorts, in order to limit meditation training groups to a maximum of six people.

#### Materials

**Survey Measures** State mindfulness was measured using the SMS (see Section 2.2.1). Trait mindfulness was measured using the FFMQ (see Section 2.3.1). The FFMQ was used as a randomisation check for baseline levels of trait mindfulness, and to compare group differences in trait mindfulness after mindfulness training. Demographics were collected using the survey described in Section 2.2.1.

**Behavioural Measures** State mindfulness was measured using a breath counting task. Cohorts 1-3 were measured using the eyes-open breath counting task (see Section 2.3.1). These cohorts were taught to meditate with eyes open, the rationale being that this would enhance the MT group's ability to remain mindful during the post-training breath counting task. Cohorts four and five were measured using the eyes-closed breath counting task (see Section 2.2.1). These cohorts were not taught to meditate with eyes open, the rationale being that minimising visual distractions would help the MT group remain mindful during the post-training breath counting task. Attention was measured using the ANT (see Section 2.2.1).

**Meditation Training** The meditation training (MT) was designed around the first three stages of Ajahn Brahm's Basic Method of Meditation (Brahm, 2017); sustained attention on the present moment, silent awareness of the present moment, and silent present moment awareness of the breath. These instructions were chosen because they are derived from the *Ānāpānasati Sutta*, are suitable for training beginners, and there are many freely available recordings in which Ajahn Brahm guides meditation using this approach.

Groups attended four, weekly, one-hour sessions at Plymouth University, run by the experimenter. Each session consisted of meditation instruction, discussion, and a guided meditation. Participants were asked to meditate daily between sessions, and record data about these sessions in a practice diary. Daily meditation duration was gradually increased as the training progressed, starting at 10 minutes in week one, and increasing by five minutes at each subsequent session. This meant that participants in the MT group were encouraged to meditate for at least 25 minutes per day in the week before their second experimental session. There were some minor differences in the MT completed by cohorts 1-3 and cohorts 4-5.

**Cohorts 1–3** Cohorts 1–3 used a 15 minute and a 25 minute meditation guided by Ajahn Brahm, selected from recordings available on YouTube. Participants recorded the duration, whether they used guidance, and notes about their daily meditations on a one-page, printed practice diary. The diary suggested a meditation duration for each day. There were four 10 minute meditations, seven 15 minute meditations, seven 20 minute meditations, seven 25 minute meditations, and three 30 minute meditations. The diary encouraged participants to use the guided meditation for their first four meditations outside the group, after which guidance was optional. For days when the duration was shorter than the guided meditation recording, participants were invited to use a timer to end the guided meditation early[2].

The first two weeks were used to build familiarity with mindfulness of breathing. In the second half of the course, participants were taught how to maintain awareness of breathing by counting breaths. Written materials and YouTube videos were used to support the meditation itself. These explained how to establish a meditation practice, meditation posture, the three stages of the Basic Method of Meditation, breath counting, how to 'watch' the breath, mindfulness, 'letting go', detecting peaceful states, and how to combine mindfulness with kindness. Materials were presented in the group sessions, and also emailed to participants every few days to reinforce the previous session, and to encourage participants to meditate every day. Because the eyes-open breath counting task was used in cohorts 1–3, these participants were taught how

[2]Guided meditations and practice sheets are archived at `https://osf.io/38vpz/files/`.

to meditate with eyes open, and instructed to practice this technique in four of the meditations during their final week of training.

**Cohorts 4–5** Some minor improvements to the MT were made for cohorts four and five. Participants were given four guided meditations as MP3 audio files with durations matched to those of the suggested daily meditation. This avoided the use of a timer to end guided meditations early, and removed the dependency on YouTube being accessible. Week one used a 10 minute meditation guided by the experimenter. Weeks 2–4 used 15, 20 and 25 minute meditations respectively, all guided by Ajahn Brahm. Participants were not taught to meditate with eyes open, as the experimenter felt that this was an unnecessary challenge for beginners. Instead, an audio tone was used to signal the end of the breath counting task, so it could be completed with eyes closed. Breath counting was introduced in week two, slightly earlier than for cohorts 1–3.

The printed practice diaries were replaced by shared Google spreadsheets to reduce the risk of missing data due to printed sheets being lost. The spreadsheet suggested seven 10 minute meditations, seven 15 minute meditations, nine 20 minute meditations, and five 25 minute meditations. Participants were encouraged to use audio guidance for the first three meditations after the group session, after which guidance was optional. Because the experimenter could access the spreadsheets, missed meditations could be detected daily. Participants were emailed to deal with any questions and encourage them to return to the schedule. To make it easier for participants to access course materials, they were combined into a reference website[3], rather than being distributed as email attachments.

[3] `https://earcanal.github.io/learn-to-meditate`

**Procedure**

Each time enough participants had been recruited to form a new cohort, they were randomly assigned to either the mindfulness training (MT) group, or a waitlist control group, using an R script. The MT:control ratio was varied between 1:1 and 2:1 to balance overall numbers in the two groups. Participants who could not attend all of the group sessions in the course they were offered, were offered a place in a subsequent cohort.

All participants completed two experimental sessions, both of which lasted for approximately 45 minutes. The first session took place immediately before the start of the MT group's training course. Participants gave informed consent, then completed the demographics survey, breath counting task, SMS, ANT and FFMQ. They were then given the approximate start date for their course. For participants allocated to MT, this was a few days after all participants in the MT group had completed their first experimental session. For the control group, this was approximately four weeks after the start of the MT group's course.

Participants' second experimental session took place approximately 28 days after their first session i.e. just after the MT group's four-week meditation course was complete. In this session participants completed the breath counting task, SMS, ANT and FFMQ. They were debriefed to end the experiment. The waitlist group's course began a few days after all participants in this group had completed their second experimental session.

### 3.2.2 Results

Six participants withdrew from the study[4]. Two participants' data were excluded because their breath counting data suggested they had misunderstood, or did not engage with the task, at one or both of the testing sessions[5]. One participant didn't complete the FFMQ at time 1. Two participants in cohorts 1-3 didn't complete the FFMQ at time 2. After exclusions, data from 55 participants (46 female; 29 MT, 26 controls) were included in the following analyses.

**Baseline measures**

Randomisation was successful. There was no evidence of differences in mean age between the MT (26.59 years) and control (21.88 years) groups ($F(1, 53) = 3.11$, $p = .083$, $BF = 0.98$), and no evidence of a relationship between sex and assigned group ($\chi^2(1, N = 55) = 0.30$, $p = 0.582$, $BF = 0.61$).

Descriptive statistics for the FFMQ are shown in Table 3.2. Reliability was good for the baseline FFMQ in the combined MT and control groups, with Cronbach's alphas of 0.81 for observing, 0.92 for describing, 0.92 for acting with awareness, 0.91 for nonjudging of inner experience, and 0.82 for nonreactivity to inner experience. Individual Bayesian t-tests were carried out for each of the FFMQ factors in the first experimental session. Overall, the two groups were well matched on mindfulness traits. Scores were similar for the actaware ($BF = 0.37$), nonjudge ($BF = 0.27$), observe ($BF = 0.27$) and nonreact ($BF = 0.28$) factors. The describe factor was numerically higher in the FAM group, but the Bayes Factor of 1.48 provided little evidence of a difference.

[4]One from cohorts 1-3, five from cohorts 4-5.
[5]Both from cohorts 1-3.

**Table 3.2:** FFMQ subscale for MT and control groups before MT.

| | MT | | Control | |
|---|---|---|---|---|
| FFMQ factor | M | SD | M | SD |
| actaware | 20 | 6.3 | 19 | 5.9 |
| describe | 26 | 6.1 | 23 | 6.8 |
| nonjudge | 22 | 7.1 | 22 | 7.1 |
| nonreact | 20 | 5.0 | 20 | 4.0 |
| observe | 28 | 6.4 | 28 | 5.1 |

*Note:* M = mean, SD = standard deviation.

**Attention Network Test**

The two MT participants who pressed the incorrect button in the breath counting task at time 1 were included in these analyses. These participants responded correctly on the second breath counting task, suggesting they had misunderstood the instructions at time 1, but had otherwise engaged with both breath counting tasks and the MT itself. ANT exclusions and calculations are described in Section 2.2.2. To maximise statistical power, data from all cohorts was combined[6], meaning 29 MT participants and 26 control participants were included in the analyses.

Figure 3.1 shows changes in ANT scores between times 1 and 2, for the two experimental groups. Alerting scores increased in the control group and increased marginally in the MT group. Orienting scores increased in both groups, with a greater increase in the control than the MT group. Executive attention scores reduced by a similar amount in both groups.

These data were analysed using individual group (2) x time (2) ANOVAs on alerting, orienting and conflict scores. For alerting, there was substantial evidence against a

[6]Individual cohort (1–3, 4–5) x group (MT, control) x time (1, 2) ANOVAs were run for the three ANT scores. There were no interactions involving cohort for alerting or executive attention. For orienting, there was a cohort x time interaction, $F(1,51) = 5.51$, $p = 0.03$.

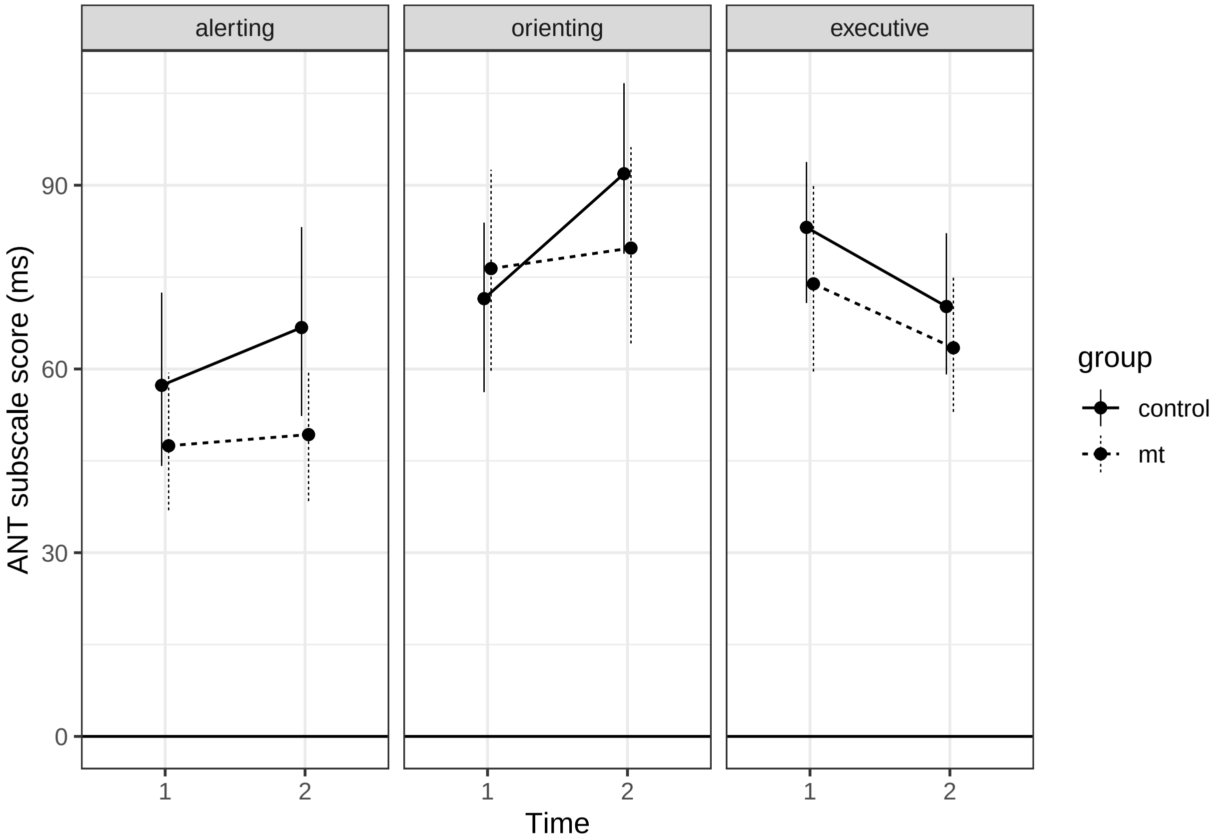


**Figure 3.1:** ANT scores for MT and control groups immediately before (time 1) and after (time 2) the MT group's intervention. Bars are bootstrapped 95% confidence intervals.

main effect of time ($F(1, 53) = 1.00$, $p = .322$, $BF = 0.31$), and no evidence for a main effect of group ($F(1, 53) = 3.13$, $p = .083$, $BF = 1.05$). There was also no evidence for a group x time interaction ($F(1, 53) = 0.49$, $p = .486$, $BF = 0.34$).

For orienting, there was no evidence for main effects of time ($F(1, 53) = 4.41$, $p = .040$, $BF = 1.29$), group ($F(1, 53) = 0.14$, $p = .711$, $BF = 0.33$), or a group x time interaction ($F(1, 53) = 2.46$, $p = .123$, $BF = 0.72$).

For executive attention, there was no evidence for main effects of time ($F(1, 53) = 5.10$, $p = .028$, $BF = 1.92$) or group ($F(1, 53) = 1.09$, $p = .302$, $BF = 0.44$). There was substantial evidence against a group x time interaction ($F(1, 53) = 0.06$, $p = .811$, $BF = 0.30$).

**State Mindfulness**

The breath counting task and SMS indicated that mindfulness did not increase in the MT group relative to the waitlist group. Two participants in the MT group were excluded from the analysis of the breath counting task, because they pressed the incorrect button to record their breath counts during the breath counting task in the first experimental session. Table 3.3 shows that mean breath counting accuracy improved after MT by 7.16%, with no change in the control group. However, a group (2) x time (2) Bayesian ANOVA found no evidence for main effects of time (*BF* = 0.37), group (*BF* = 0.49), or a group x time interaction (*BF* = 0.50).

**Table 3.3:** Breath counting accuracy before (time 1) and after (time 2) mindfulness training (MT).

| | MT | | | Control | | |
|---|---|---|---|---|---|---|
| Time | n | M | SD | n | M | SD |
| 1 | 27 | 76 | 17 | 26 | 75 | 19 |
| 2 | 27 | 83 | 13 | 26 | 75 | 15 |

*Note:* Accuracy = percent accuracy of correct 9 counts, M = mean, SD = standard deviation.

Baseline SMS reliability was good, with Cronbach's alpha for the combined MT and reading groups of 0.89 for the mind subscale, and 0.77 for the body subscale. Table 3.4 shows the changes in the body and mind subscales before and after MT. Scores on the body subscale increased slightly after MT in the MT group and decreased slightly in the control group. The mind subscale increased after MT in the MT group, and was relatively unchanged in the control group.

The SMS mind and body subscales were analysed using group (2) x time (2) Bayesian ANOVAs. For the mind subscale, there was no evidence for a main effect of time (*BF* =

**Table 3.4:** SMS scale scores for MT and control groups before and after MT.

| | Pre | | | | Post | | | | Post-Pre | |
|---|---|---|---|---|---|---|---|---|---|---|
| | MT | | Control | | MT | | Control | | MT | Control |
| SMS subscale | M | SD | M | SD | M | SD | M | SD | MD | MD |
| body | 18 | 5.7 | 20 | 4.2 | 20 | 5.7 | 19 | 5.3 | 2.0 | -1.42 |
| mind | 48 | 13.0 | 50 | 9.2 | 54 | 10.7 | 51 | 9.0 | 6.5 | 0.65 |

*Note:* M = mean, SD = standard deviation, MD = mean difference (post-pre).

0.89), and substantial evidence against a main effect of group (*BF* = 0.20). Although the mean difference for the mind subscale was numerically higher in the MT group than the control group, there was no evidence of a group x time interaction (*BF* = 0.65). For the body subscale, there was substantial evidence against main effects of time (*BF* = 0.22) and group (*BF* = 0.20). There was no evidence of a group x time interaction (*BF* = 0.95).

**Associations between breath counting accuracy and SMS**

Figure 3.2 shows the relationships between breath counting accuracy and the SMS for the MT group. There was no evidence for correlations between breath counting accuracy and either the mind (*r* = 0.15, *BF* = 0.53), or body (*r* = 0.01, *BF* = 0.42) subscales. Figure 3.3 shows the relationships between breath counting accuracy and the SMS for the control group. There was no evidence for correlations between breath counting accuracy and either the mind (*r* = 0.03, *BF* = 0.43), or body (*r* = -0.03, *BF* = 0.43) subscales.

**Trait Mindfulness**

Table 3.5 summarises the post-intervention FFMQ. The pre and post intervention FFMQs were analysed using group (2) x time (2) Bayesian ANOVAs for each factor. For actaware, there was no evidence for main effects of time (*BF* = 0.58) or group (*BF* = 0.65), and no

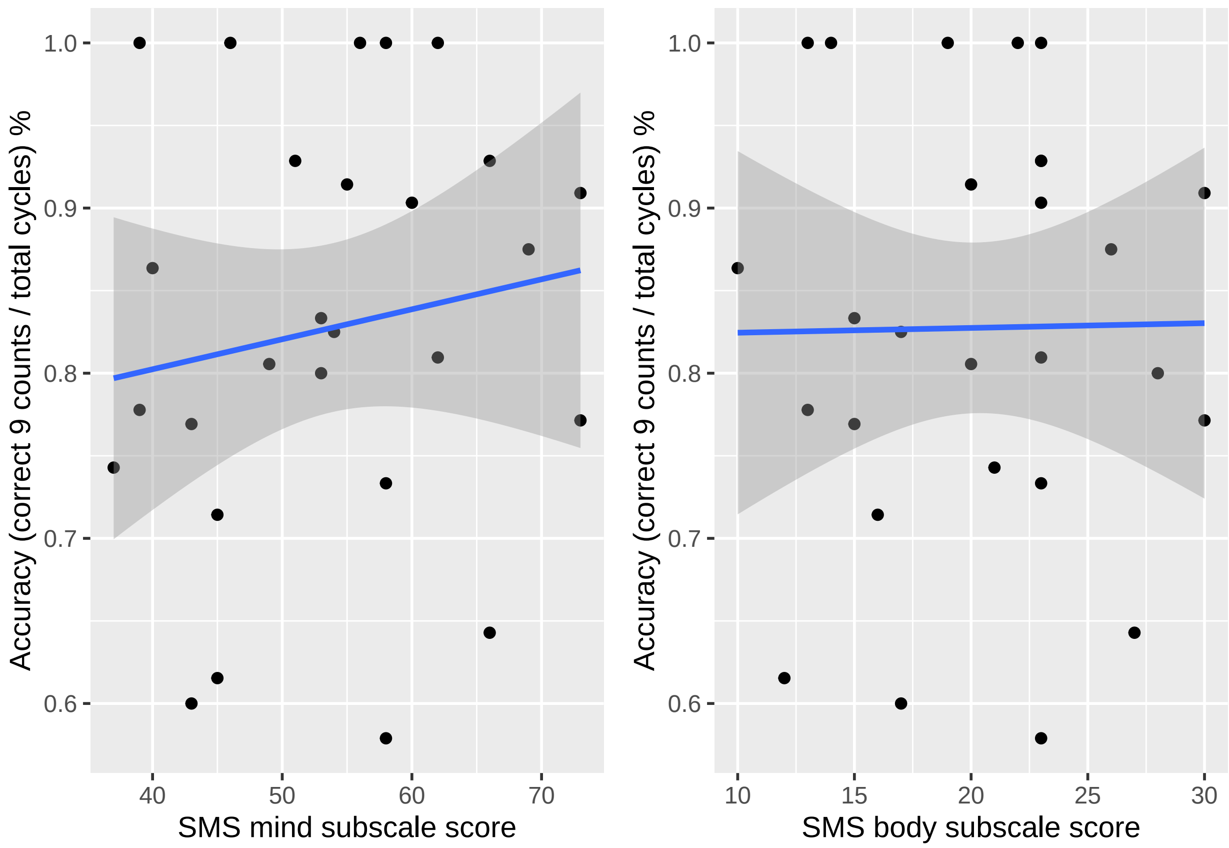


**Figure 3.2:** Scatterplot showing breath counting accuracy against SMS subscales for the MT group at time 2.

evidence for a group x time interaction (*BF* = 0.50). For describe, there was evidence against a main effect of time (*BF* = 0.29), no evidence for a main effect of group (*BF* = 0.74), and evidence against a group x time interaction (*BF* = 0.30). For nonjudge, there was evidence against main effects of time (*BF* = 0.27) and group (*BF* = 0.29), and no evidence for a group x time interaction (*BF* = 0.72). For nonreact, there was no evidence for main effects of time (*BF* = 0.44) or group (*BF* = 0.38), and no evidence for a group x time interaction (*BF* = 1.42). For observe there was evidence against main effects of time (*BF* = 0.21) and group (*BF* = 0.33), and no evidence for a group x time interaction (*BF* = 1.31).

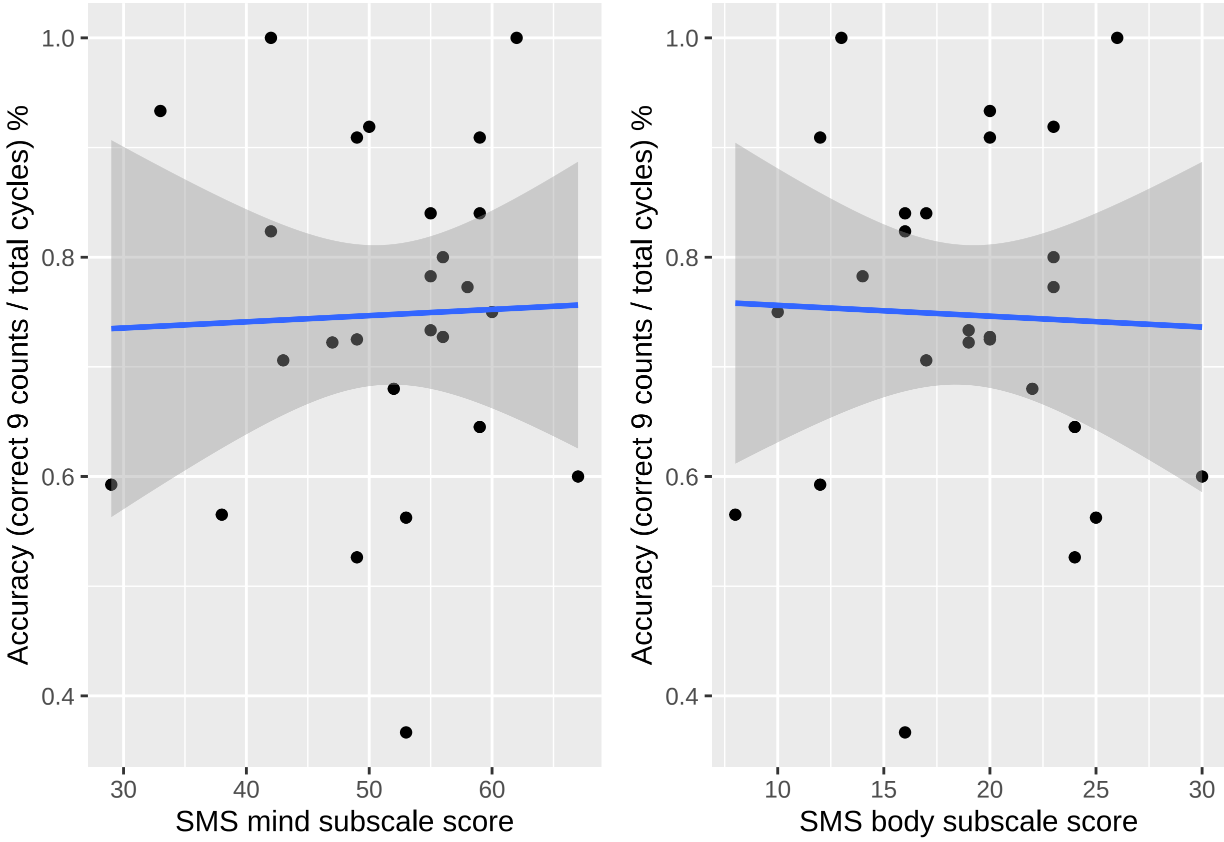


**Figure 3.3:** Scatterplot showing breath counting accuracy against SMS subscales for the control group at time 2.

**Table 3.5:** FFMQ subscale mean (M) and standard deviation (SD) for MT and control groups after MT.

| | MT | | Control | |
|---|---|---|---|---|
| FFMQ factor | M | SD | M | SD |
| actaware | 23 | 6.0 | 19 | 6.0 |
| describe | 27 | 6.9 | 24 | 8.0 |
| nonjudge | 25 | 8.1 | 21 | 6.3 |
| nonreact | 23 | 4.8 | 19 | 3.6 |
| observe | 30 | 5.1 | 27 | 4.5 |

### Meditation duration

The minor adjustments to the MT in cohorts 4 and 5 meant that the total suggested meditation time for these cohorts (490 minutes) was slightly less than for cohorts 1-3 (550 minutes). Table 3.6 summarises the actual amount of meditation completed and 'compliance' with the suggest durations[7].

**Table 3.6:** Meditation quantity and duration.

| | Compliance (%) | | Meditation (minutes) | | Days between experimental sessions | | |
|---|---|---|---|---|---|---|---|
| Condition | M | SD | M | SD | Median | Min | Max |
| MT | 78 | 20 | 400 | 102 | 29 | 27 | 34 |
| control | 44 | 15 | 217 | 73 | 27 | 25 | 34 |

### 3.2.3 Discussion

These results differ from those of Tsai and Chou (2016, Experiment 2). Most importantly, where Tsai & Chou (2016) found that weekly MT over 12 weeks improved all ANT scores, relative to waitlist controls, we found no improvements on the ANT after four weeks of daily MT. We also found no improvement in state mindfulness in the MT group relative to controls on either the breath counting task or the SMS, and no improvement in trait mindfulness on the FFMQ. The most likely explanation for the difference between these two studies is that participants in Experiment 4 completed about two thirds as much meditation (400 minutes) as the those in Tsai & Chou (2016). The longer training duration (12 weeks) in Tsai & Chou (2016), than in Experiment 4 (4 weeks) may also have had an effect.

There were no differences between the MT and control groups on either state mindfulness measure, and no correlations between breath counting task accuracy and the SMS mind or body subscales. Both measurements should have been reliable, as the SMS was measured immediately after the breath counting task. We can conclude from this that there were no group differences in state mindfulness. It could be argued that the breath counting task would have increased mindfulness to a greater extent at time

[7] Twelve control participants did not attend their meditation course.

2 if the MT group were explicitly told that the breath counting task was a meditation. However, it seems unlikely that they would not have recognised the breath counting task as meditation, given that breath counting was explicitly taught in the second half of their mindfulness training. It is more likely that increases in state mindfulness are not measurable after 400 minutes of FAM training.

Compared with this study, the originators of the breath counting task used much more intensive and focused training to demonstrate that meditation improves breath counting task accuracy. In Experiment 4, Levinson et al. (2014) found increased breath counting accuracy after approximately 1,000 minutes of breath counting training (two 25 minute sessions per day, for four weeks), compared with similar amounts of working memory training or no training. As there was little evidence of group differences in mindfulness, it is unsurprising that FAM training did not improve ANT scores relative to the control group.

The trait mindfulness results were consistent with Tsai and Chou (2016, Experiment 2). Experiment 4 found no pre-post differences on any of the FFMQ factors. Similarly, Tsai & Chou (2016) found no pre-post increases on a Chinese version of the Mindful Attention and Awareness Scale (MAAS, Brown & Ryan, 2003) in either their MT or control groups. In addition to increased breath counting accuracy, Levinson et al. (2014, Experiment 4) only found improved FFMQ scores in the breath counting training group. This is further evidence that the MT in Experiment 4 did not increase mindfulness.

From a different perspective, we *should* be surprised that this amount of meditation did not improve ANT performance. Participants in Experiment 4 meditated 25 times longer than those in Experiment 1, and 40 times longer than those in Norris et al. (2018). Why were there no differences on the ANT, when novices show improvements on other attentional measures after a single meditation of between 5-17 minutes (Colzato et al., 2015; Dickenson et al., 2013; Schofield et al., 2015; Watier & Dubois, 2016)? It could be that these measures are more sensitive to the effects of meditation than the ANT, or that their effects are overstated. However, an alternative hypothesis is that guided meditation is needed to induce mindfulness in novices. Schofield et al. (2015), Watier & Dubois (2016), Colzato et al. (2015) and Norris et al. (2018) all involved audio-guided meditations, whereas Experiments 1 and 4 used the breath counting task as a (non-guided) mindfulness induction. Perhaps mindfulness is elevated in novices because audio guidance is needed to periodically re-orient them towards their meditation subject.

Our findings also conflict with Tang et al. (2007), Burger & Lockhart (2017), and Walsh et al. (2019), who all found effects on executive attention with much less meditation (see Table 3.1). However, the type of meditation, training intensity and control conditions in these experiments were quite different to those in Experiment 4, and Tsai & Chou (2016). In Experiment 5 we addressed two possible reasons why the FAM training in Experiment 4 did not improve ANT performance, by increasing both the the total amount, and the overall duration of FAM training.

## 3.3 Experiment 5: The effects of 8 weeks of FAM training on the ANT

When novices are trained exclusively in FAM, effects on the ANT have been found after 600 minutes of training delivered weekly over 12 weeks (Tsai & Chou, 2016, Experiment 2), and 1075 minutes of training delivered over 8 weeks (Becerra et al., 2016). These amounts and durations of FAM training are greater than in Experiment 4, where 400 minutes of FAM training delivered daily over four weeks produced no effects on the ANT. Experiment 5 was a pre-registered replication of Becerra et al. (2016)[8]. This was carried out to test whether a slightly more demanding period of FAM training is necessary to detect effects on the ANT.

Becerra et al. (2016) compared ANT measurements before and after non-meditators completed eight weeks of FAM training, with a waitlist control group. Their results are plotted in Figure 3.4. Becerra et al. (2016) reported significant group x time interactions for alerting, orienting, and executive attention. Table 3.7 shows the directions of these effects.

**Table 3.7:** Baseline ANT scores subtracted from ANT scores after FAM training (Becerra et al., 2016, Table 2).

| ANT | FAM | Control |
|---|---|---|
| Executive | -18.44 | 0.65 |
| Alerting | 2.96 | -0.17 |
| Orienting | -11.87 | -0.83 |

Becerra et al. (2016) interpret their results as improvements in orienting and executive attention after FAM training, relative to controls. If improved performance is reflected in higher alerting and orienting scores, and lower executive attention scores (Fan et al., 2002), then this interpretation is only partially accurate. The results show a large improvement in executive attention scores after FAM training, relative to controls, but they also show a small improvement in alerting. The orienting scores suggest that this attentional network *worsened* after FAM training relative to controls. Based on this re-interpretation of Becerra et al. (2016), we predicted that the FAM

[8] `https://osf.io/c5n97`

group would have higher alerting and executive attention scores, and that the control group would have a higher orienting score[9].

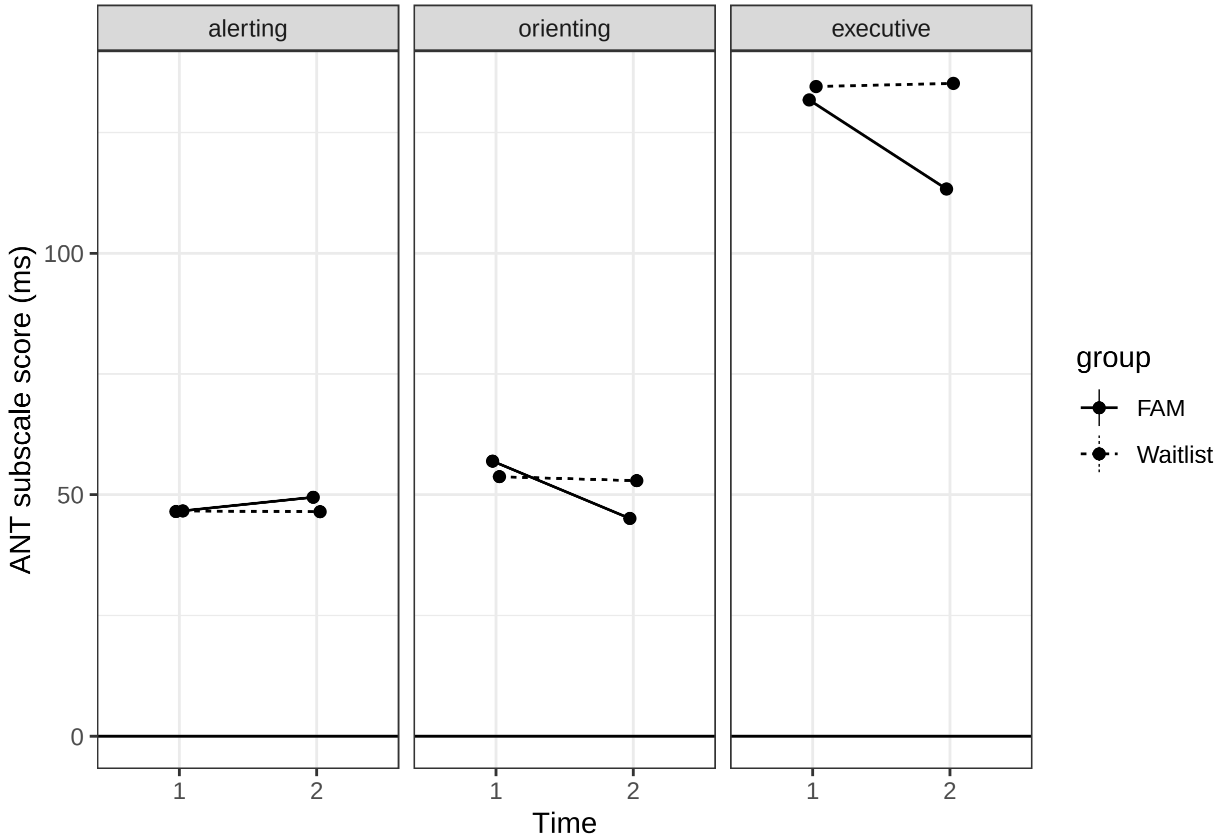


**Figure 3.4:** Results from Becerra et al. (2016).

In the most important respects, Experiment 5 was a direct replication of Becerra et al. (2016). The FAM training content differed, but the duration of the daily meditations were identical. Two measures were added to the second testing session[10]. An FFMQ measurement was taken after the ANT to compare against the same measurement in Experiment 4. A short personality measurement was taken before the FFMQ, to test whether any group differences on the ANT corresponded with differences in personality factors.

[9]Our reinterpretation of Becerra et al. (2016) was carried out after preregistration, so these hypotheses differ from those in the preregistration.

[10]These were also added after preregistration.

### 3.3.1 Method

#### Participants

Posters and online advertisements invited people at the University Plymouth to participate in a free meditation course and research study, in exchange for course credits or £16. Two cohorts were recruited, one in each semester of the academic year. Twelve people withdrew from the study. In total, 50 participants (38 female) completed the study, 23 randomised to the MT group, and 27 to the waitlist control group.

#### Materials

**Attention Network Test** The ANT is described in Section 2.2.1.

**Surveys** Following Becerra et al. (2016), the 21-item Depression Anxiety and Stress Scale (DASS-21, Lovibond & Lovibond, 1995) was used to compare baseline levels of depression, anxiety and stress. The DASS-21 is a reliable measure of negative affect, consisting of 21 self-report items, 7 each for measuring depression, anxiety and stress[11]. Questions relate to a person's experiences over the past week (e.g. *I found it difficult to relax*), with scores ranging from 0 (never) to 3 (almost always). Henry & Crawford (2005) report Cronbach's alphas of .82 for the Anxiety scale, .88 for the Depression scale, .90 for the Stress scale, and .93 for the Total scale.

The Ten-Item Personality Inventory (TIPI, Gosling et al., 2003) is a shortened measure of the Big-Five personality domains, for situations where personality is not the primary measure of interest[12]. It consists of 10 self-report items (e.g. *I see myself as extraverted, enthusiastic*) rated from 1 (disagree strongly) to 7 (agree strongly) . The measure has

[11] `https://github.com/earcanal/dass-21`
[12] `https://github.com/expfactory-experiments/ten-item-personality-survey`

adequate test-retest reliability, convergence with self, observer, and peer reports using longer Big-Five measures, patterns of predicted external correlates, and convergence between self and observer ratings.

The FFMQ is described in Section 2.3.1 and the demographics survey in Section 2.2.1.

**Meditation Training** The guided meditation and associated training materials for Becerra et al. (2016) were unavailable, so a simplified form of the course delivered to cohorts 4 and 5 in Experiment 4 was used. Participants were given a document containing meditation instructions abridged from Brahm (2017), guidance on when and where to meditate, meditation posture, and instructions for using the Insight Timer meditation app[13]. The guided meditation for the course was created from the audio track of a YouTube video from the Buddhist Society of Western Australia, edited to 24 minutes and uploaded to Insight Timer[14]. Participants accessed the guided meditation using Insight Timer on their phone or tablet. The app automatically recorded the number and duration of each participant's meditation sessions. These variables are often not measured (Parsons et al., 2017), and they are subject to memory or social desirability biases when measured using meditation diaries. As with the shared spreadsheets in Experiment 4, the experimenter had daily access to each participant's Insight Timer activity, which was used to encourage them to return to the schedule when a daily meditation was missed.

The experimenter designed the eight week meditation training around the first three stages of Ajahn Brahm's Basic Method of Meditation (Brahm, 2017). The guided

[13]Meditation instructions are archived at `https://osf.io/qnxd6/files/`

[14]`https://insighttimer.com/paulsharpe/guided-meditations/ajahn-brahm-guided-meditation`

meditation was selected to help participants apply the three stages. Stage one, sustained attention on the present moment, was developed using a body scan and posture adjustment exercise. Stage two, silent awareness of the present moment, was developed using a visualisation of a still, mountain lake. Stage three, silent present moment awareness of the breath, was supported using a mantra.

Groups attended four, bi-weekly, one-hour sessions run by the experimenter. Session one introduced the three meditation stages, and a video[15] was played to emphasise that each stage involved progressively 'letting go' of 'doing'. Participants were instructed to adopt a comfortable, relaxed, upright seated posture when meditating. They were instructed to watch their breath by simply knowing whether they were breathing in or out, rather than focusing on sensations at the nostrils or abdomen. Participants then meditated for the first time, following the guided meditation which the experimenter played through a speaker.

At the end of the first session, participants installed the Insight Timer app and logged in using an account created by the experimenter. They were asked to meditate daily, using the same 24-minute guided meditation on Insight Timer. This reinforced the instructions and automatically recorded the number and duration of practice sessions for each participant. Over the first two weeks, participants were encouraged to experiment to find their preferred posture, and a place and time for their daily meditation which minimsed interruptions and sleepiness.

Group sessions were designed to match the format in Becerra et al. (2016), i.e. discussion and clarification regarding any practice issues, followed by a group meditation using the daily guided meditation recording. The experimenter emailed participants every

[15] https://www.youtube.com/watch?v=T6wIWJ6cDl0

few days between sessions, to reinforce the instructions from the previous session, and to motivate daily practice. In subsequent sessions, the experimenter began by answering any questions arising from the previous two weeks of home practice. In sessions two and three, the experimenter gave a short talk to further explain and reinforce the second and third stages, respectively, in both the written and audio instructions. Each session ended with a group meditation, using the Insight Timer guided recording. The total duration of the 56 daily meditations was 1346 minutes.

**Procedure**

The study was scheduled to minimise conflicts with University holidays and exams. Before agreeing to participate, people who had expressed an interest in the course attended an orientation session. The experimenter gave a short presentation describing the course and key dates, so that people could decide if they were able to commit the time involved in participating. Those who wanted to participate completed a paper version of the DASS-21, and the experimenter scheduled their pre-testing session, and assigned them to either to the MT group, or the waitlist control group, using an allocation sequence pre-generated using R.

Both groups were tested immediately before, and after the MT group's meditation training. The two experimental sessions took place in a psychology lab. At the first session, each participant gave informed consent, then completed the demographics survey. A small number of participants completed the DASS-21, if they had not done so at the orientation session. Finally, participants completed the ANT. The MT group's first training session was scheduled a few days after the final participant in this group had completed the first experimental session. Control participants were given an approximate date for their second experimental session, and informed that their course would begin a few days after this. The second experimental session took place approximately 56 days after the first. Participants completed the ANT, the TIPI and the FFMQ, before being debriefed to end the experiment. Each experimental session took approximately 30 minutes.

### 3.3.2 Results

**Baseline measures**

Randomisation was successful. Mean age was not significantly different for the MT (23.48 years) and control (22.89 years) groups, $F(1, 48) = 0.08$, $p = .774$, and there was no evidence of a relationship between sex and assigned group, $\chi^2(1, N = 50) = 0.00$, $p = 0.989$, $BF = 0.41$. Table 3.8 summarises the DASS-21 results. Chronbach's $\alpha$ was good for for depression, anxiety, and stress in the combined MT and control groups. Individual Bayesian t-tests found substantial evidence against group differences for anxiety and stress, and no evidence of differences for depression.

**Table 3.8:** DASS-21 subscale for MT and control groups before MT.

| | MT | | | | Control | | | | | |
|---|---|---|---|---|---|---|---|---|---|---|
| DASS-21 factor | M | SD | Min | Max | M | SD | Min | Max | $\alpha$ | *BF* |
| anxiety | 5.7 | 4.7 | 0 | 18 | 5.7 | 3.4 | 0 | 13 | 0.81 | 0.28 |
| depression | 5.1 | 3.9 | 1 | 17 | 6.2 | 3.7 | 0 | 14 | 0.77 | 0.41 |
| stress | 9.0 | 4.4 | 2 | 17 | 9.0 | 3.6 | 1 | 18 | 0.83 | 0.28 |

*Note:* M = mean, SD = standard deviation.

**Attention Network Test**

ANT exclusions and calculations are described in Section 2.2.2. Figure 3.5 shows that for both groups there were similar increases in alerting and orienting scores, and similar decreases in executive attention scores.

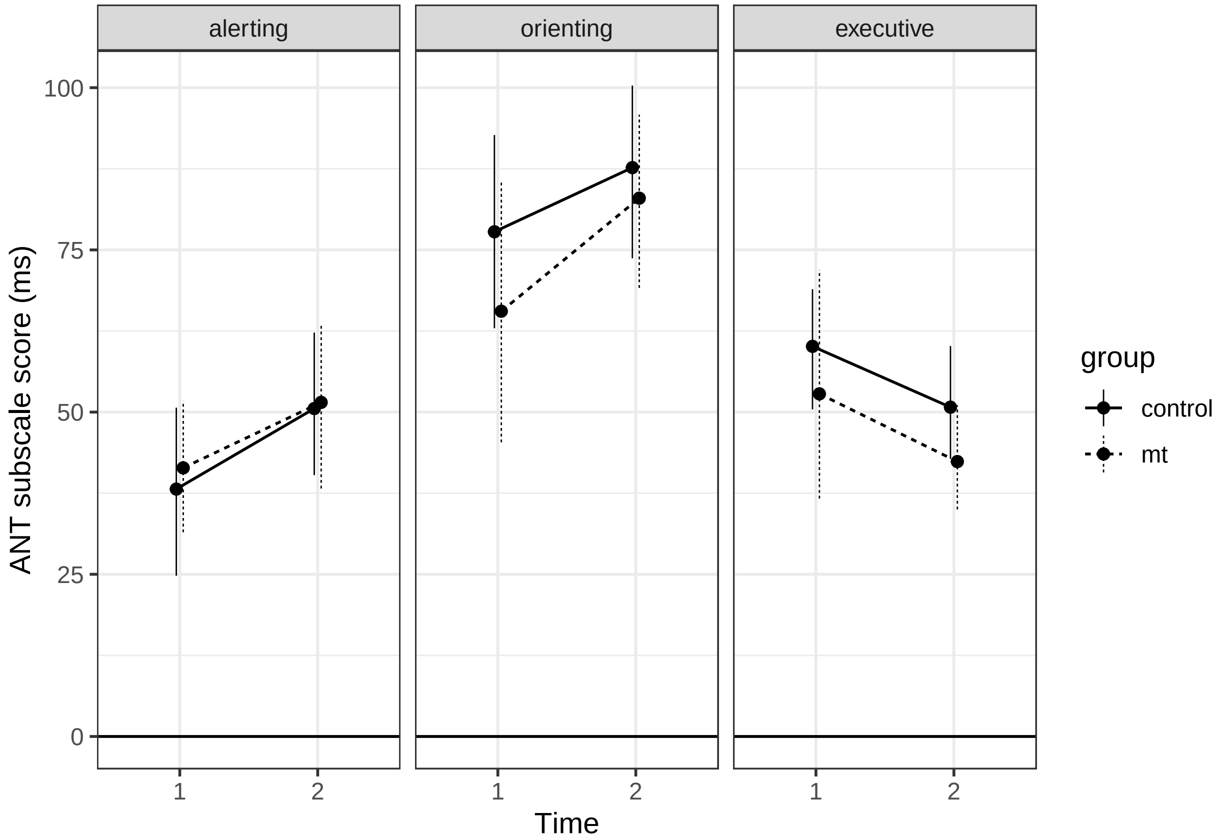


**Figure 3.5:** ANT scores for FAM and control groups immediately before and after the MT intervention. Bars are bootstrapped 95% confidence intervals.

Differences for the three ANT scores were tested using individual group (2) x time (2) ANOVAs. Alerting scores showed no evidence of a main effect for time ($F(1, 48) = 3.28$, $p = .077$, $BF = 1.16$), and substantial evidence against a main effect of group ($F(1, 48) = 0.10$, $p = .751$, $BF = 0.26$). There was substantial evidence against a group x time interaction ($F(1, 48) = 0.03$, $p = .854$, $BF = 0.29$).

Orienting scores showed was no evidence of a main effect of time ($F(1, 48) = 4.73$, $p = .035$, $BF = 1.64$), and substantial evidence against a main effect of group ($F(1, 48) = 0.81$, $p = .372$, $BF = 0.40$). There was substantial evidence against a group x time interaction ($F(1, 48) = 0.37$, $p = .544$, $BF = 0.31$).

*3. Effects of FAM training on the ANT*

For executive attention, there was no evidence for main effects of time ($F(1, 48) = 3.33$, $p = .074$, $BF = 1.03$), or group ($F(1, 48) = 1.34$, $p = .254$, $BF = 0.44$). There was substantial evidence against a group x time interaction ($F(1, 48) = 0.01$, $p = .923$, $BF = 0.27$).

**Meditation duration**

The total suggested meditation time was 1346 minutes. Table 3.6 summarises the meditation completed in each group, and the compliance with the suggested minutes[16].

**Table 3.9:** Meditation compliance, quantity (minutes) and training duration.

| | Compliance (%) | | Meditation | | Days between test sessions | | |
|---|---|---|---|---|---|---|---|
| Condition | M | SD | M | SD | Median | Min | Max |
| MT | 58 | 26 | 776 | 343 | 63 | 57 | 65 |
| control | 13 | 15 | 171 | 206 | 61 | 57 | 65 |

*Note:*
M = mean, SD = standard deviation.

**FFMQ**

Table 3.10 summarises the post-MT FFMQ results. Chronbach's $\alpha$ was good in the combined FAM and control groups, for all subscales. Individual Bayesian t-tests were run to compare the groups on each subscale. There was no evidence of differences for actaware, describe, nonreact, and observe factors, and substantial evidence against a difference for nonjudge.

[16]Seven control participants did not attend their meditation course.

**Table 3.10:** FFMQ subscale for FAM and control groups after MT.

| | FAM | | Control | | | |
|---|---|---|---|---|---|---|
| FFMQ factor | M | SD | M | SD | $\alpha$ | *BF* |
| actaware | 24 | 5.1 | 23 | 5.2 | 0.86 | 0.41 |
| describe | 26 | 7.7 | 23 | 5.8 | 0.93 | 0.54 |
| nonjudge | 24 | 7.3 | 24 | 7.4 | 0.93 | 0.29 |
| nonreact | 21 | 4.6 | 20 | 4.0 | 0.79 | 0.39 |
| observe | 28 | 5.1 | 26 | 5.0 | 0.78 | 0.98 |

*Note:*
M = mean, SD = standard deviation.

**TIPI**

Table 3.11 summarises the post-MT TIPI results. Chronbach's $\alpha$ was good, in the combined FAM and control groups, for all subscales except Agreeableness. Individual Bayesian t-tests were run to compare the groups on each subscale. There was substantial evidence that the groups did not differ on Openness to Experience, Conscientiousness and Emotional Stability, and there was no evidence of group differences for Extroversion and Agreeableness.

**Table 3.11:** TIPI subscales for FAM and control groups after MT.

| | FAM | | Control | | | |
|---|---|---|---|---|---|---|
| TIPI factor | M | SD | M | SD | $\alpha$ | *BF* |
| Openness to Experience | 11.0 | 2.4 | 10.6 | 2.3 | 0.56 | 0.31 |
| Conscientiousness | 9.4 | 3.2 | 9.6 | 2.8 | 0.64 | 0.29 |
| Extraversion | 8.0 | 2.9 | 8.8 | 3.2 | 0.74 | 0.39 |
| Agreeableness | 9.5 | 2.1 | 10.2 | 2.0 | 0.08 | 0.46 |
| Emotional Stability | 8.1 | 3.4 | 7.8 | 2.7 | 0.75 | 0.30 |

*Note:*
M = mean, SD = standard deviation.

### 3.3.3 Discussion

Experiment 5 did not replicate the findings of Becerra et al. (2016). We found no differences on the ANT in participants who completed eight weeks of FAM training compared with a group waiting for the same training. There was no evidence of trait mindfulness or personality differences which might explain this finding.

The total meditation duration was almost twice that of Experiment 4, almost 30% longer than Tsai and Chou (2016, Experiment 2) but only 70% as long as Becerra et al. (2016). At this intermediate duration we would have expected to see an improvement in executive attention and alerting, as these effects were present in both Tsai & Chou (2016) and Becerra et al. (2016).

## 3.4 General Discussion

Experiment 4 found no effects of 4 weeks of FAM training on the ANT relative to a waitlist control group. This contrasts with Tsai and Chou (2016, Study 2), who found that FAM training improved all ANT scores. The differences in these results could be explained by the shorter training duration (400 minutes) in Experiment 4. Tsai & Chou (2016) did not report compliance levels, but if we assume that participants attended all of the group sessions, then they would have meditated for a minimum of 600 minutes. Participants were not asked if they meditated outside of the group sessions, so it is also possible that the total duration was greater than 600 minutes. It is also possible that training FAM by focusing on body parts and acupoints (Tsai & Chou, 2016) was more powerful than the breath meditation which was the focus in Experiment 4. On the other hand, the meditations in Experiment 4 began with a brief body scan, so were matched with Tsai & Chou (2016) to some extent.

In contrast with Tsai & Chou (2016), Experiment 4 included the breath counting task in both experimental sessions. We might have expected this to improve mindfulness and therefore improve ANT performance relative to an experiment without a mindfulness induction before the ANT, but this was not the case. Experiment 4 was consistent with a model in which mindfulness mediates attention regulation, to the extent that there was no increase in mindfulness after FAM training, so we would not expect improvements on the ANT.

A different explanation is needed to account for the studies which found increases in executive attention with less meditation than Experiment 4 (Burger & Lockhart, 2017; Tang et al., 2007; Walsh et al., 2019). One possibility is that in these studies, the meditations, MT or both were simply more effective than those in Experiment 4. An alternative, intriguing idea is that the meditation in these experiments was more effective for novices, because it was less *intensive*. In Experiment 4, daily meditations began at 10 minutes, and the duration was progressively increased, peaking at 25 minutes per day. This contrasts with shorter daily meditations lasting 10 minutes (Burger & Lockhart, 2017; Walsh et al., 2019), or 20 minutes (Tang et al., 2007)[17]. It may be that daily low intensity training (Burger & Lockhart, 2017; Tang et al., 2007;

[17] In Burger & Lockhart (2017), participants completed MT designed and delivered by the first author, and meditated daily for four weeks by following a guided, 10 minute mindfulness of breathing exercise. Experiment 4 was very similar to Burger & Lockhart (2017) in that ANT performance was compared with a waitlist control group. The mindfulness condition in Tang et al. (2007) consisted of five, 20 minute recordings instructing people in body posture adjustment, breathing practice, guided imagery, and mindfulness training, accompanied by background music. The control recordings instructed people in relaxing parts of their body. In Walsh et al. (2019), each day, participants chose a 10 minute daily activity from a mindfulness app based on MBSR, or played a game that involved reasoning and perception for ten minutes. Training lasted for three weeks.

Walsh et al., 2019), and weekly high intensity (Tsai & Chou, 2016) training were more effective at improving executive attention than the meditation 'doses' in Experiment 4. More research is needed to establish the most effective titrations of meditation and MT, especially in novices.

Experiment 5 found no effects of 8 weeks of FAM training on the ANT relative to a waitlist control group. In fact, the Bayes Factors provided substantial evidence *against* group x time interactions for alerting, orienting and executive attention. This contrasts with Becerra et al. (2016), who found that FAM training improved alerting and executive attention scores, but worsened orienting scores.

Details of the mindfulness training and content of the guided meditation in Becerra et al. (2016) were insufficient to assess whether these were responsible for the difference in results. The total amount of meditation in Experiment 5 was only 70% as long as in Becerra et al. (2016), but this was still almost 30% longer than Tsai & Chou (2016). As both Becerra et al. (2016) and Tsai & Chou (2016) found that FAM improved executive attention and alerting scores, we would have expected similar results in Experiment 5. In fact, Experiment 5 replicated the results of Experiment 4, finding no effects on the ANT with double the length of mindfulness training and meditation practice.

Differences between experimental and control conditions could also explain why the findings of Tsai & Chou (2016) and Becerra et al. (2016) were not replicated. Experiment 4 used a waitlist group to control for expectancy of receiving MT. In contrast, Tsai & Chou (2016) compared MT with no training. Waiting for MT could have had negative (nocebo) effects. A meta-analysis of psychotherapy trials found that waitlist conditions can have nocebo effects when compared with no treatment control groups (Furukawa et al., 2014). The mindfulness training in Becerra et al. (2016) was structurally similar to therapeutic mindfulness interventions, which consist of individual meditation practice between regular group sessions. The ANT effects in Becerra et al. (2016) could be attributed to a nocebo effect in their waitlist, which was absent (or smaller) in Experiment 5.

Mindfulness intervention studies need more reliable measures of meditation duration and compliance. In a meta-analysis of mindfulness-based interventions, Parsons et al. (2017) found that participants' home practice was 64% of the assigned amount, equating to about 30 minutes per day, six days per week. Compliance in Experiment 4 was 78%. As this was self-reported using printed and online meditation diaries, it could be slightly inflated due to memory and social desirability biases. Compliance in Experiment 5 was 57%, a figure which should be highly accurate as it was automatically recorded using an app. This suggests that compliance declines as the schedule becomes more demanding. Tsai & Chou (2016) didn't report compliance for the group meditation sessions, or measure whether participants meditated outside of these sessions. Becerra et al. (2016) reported 80% compliance, which is high compared to the 57% in Experiment 5 and the meta-analytic figure of 64% (Parsons et al., 2017). High compliance in Becerra et al. (2016) could also explain effects on the ANT which were absent in Experiment 5. Declines in compliance over longer studies, may be a sign of fatigue. Further research is needed to establish the meditation durations and frequencies which optimise outcomes, especially in beginners.

To test whether mindfulness mediates the effects of MT on improved ANT performance (or any other outcome), mindfulness should be measured prior to the post-intervention ANT. In Experiment 4, there were no increases in state mindfulness in the FAM group, which could explain why this group also showed no performance improvements on the ANT. The type and duration of mental states induced by mindfulness meditation are yet to be established, but it seems highly likely that they are ephemeral, and affected by subsequent mental activity. Therefore, the most reliable state mindfulness measures should occur immediately before the ANT. It is possible that Experiment 4 failed to detect increased mindfulness in the FAM group because the SMS was administered after the ANT, and the intervening task affected this measurement. In order to precisely replicate Becerra et al. (2016), Experiment 5 did not measure state mindfulness. The design of Experiment 5 could be improved by adding the breath counting task immediately prior to the post-intervention ANT. Instructions could also be manipulated to describe the task as a meditation in the MT condition, but not the control condition. This improved experimental design could establish whether mindfulness mediates improved ANT performance. Finally, a trait mindfulness measure (the FFMQ) did not appear to be useful in these experiments. There were no group by time interactions on the FFMQ in Experiment 4, and no post-MT differences on the FFMQ in Experiment 5. In experiments with novices, state mindfulness measures should be preferred over trait measures.

In conclusion, these experiments did not decisively establish how much FAM training and practice novices need to complete for it to affect ANT performance. However, they do suggest an obvious next step, which is to estimate effect sizes for the effects of MT on the ANT. The results of these and similar studies may be unreliable because the designs lack statistical power. Effect size estimates could be used to calculate sample sizes which address this limitation in future replications. The next chapter reviews mindfulness studies which use the ANT as an outcome measure, and meta-analyses these studies in order to estimate effect sizes.

# 4

# The effects of mindfulness meditation on the Attention Network Test: a systematic review and meta-analysis of reaction times

## Contents

## 4.1 Introduction

The experiments discussed so far suggest that ANT scores are not improved by either a single, brief FAM session (Experiments 1-3, Chapter 2), or more extensive FAM training (Experiments 4-5, Chapter 3). The latter finding conflicts with studies which have shown differences between FAM training and controls on all ANT scores, e.g. Becerra et al. (2016) and Tsai & Chou (2016).

The wider literature looks at the effects of meditation on the ANT from a few different perspectives. Some studies compare novices (people who have never meditated before) who do a brief, guided meditation, with novices who do a control task. Others compare novices who complete a period of meditation training, with a group waiting for the same training, or who are trained for the same duration in something other than meditation. Many experiments compare long-term meditators and non-meditators, who are matched on other characteristics such as age, sex and education. A few studies look for differences between long-term meditators who do a brief, guided meditation with those who do a control task. Occasionally, comparisons are made between meditators who do an intense period of practice, with novices who do a less intense period of practice.

These studies may not have had the statistical power to reliably detect effects on the ANT, because sample sizes were not calculated *a priori* using an effect size estimate. Between-subjects designs are common in meditation studies, which means that well powered experiments may require large numbers of participants to detect effects. This limitation can be addressed by using meta-analysis to estimate effect sizes. Future studies could recruit more appropriate sample sizes by using the estimated effect sizes in power calculations.

Previous meta-analyses have established that mindfulness meditation affects attention (Sedlmeier et al., 2012; Verhaeghen, 2020), but they do not focus exclusively on the ANT. To address this gap in the literature, a review and Bayesian meta-analysis was carried out for all studies which measure the effects of meditation on the ANT.

## 4.2 Methods

### 4.2.1 Eligibility criteria

This meta-analysis focuses on the effects of mindfulness on the ANT in healthy populations. Experimental studies of healthy adult populations (age 18 and above) with a mindfulness group, and ANT response times (RT) as an outcome variable were eligible for inclusion. Studies involving tai chi, or hatha yoga were excluded unless it was clear that mindfulness was central to the training. Studies were drawn from English language peer reviewed journal articles, published up to and including 2019.

Most meditation studies fall into one of two types. Cohort studies compare ANT performance in groups who complete a mindfulness intervention, with groups who complete one or more control interventions. An 'intervention' could be a single meditation, or a longer period of mindfulness training. Case-control studies are used to compare groups with differing levels of meditation experience. Groups are normally matched on a range of factors, notably age, sex and education. Due to the small number of studies in this field, both cohort and case-control studies were included.

### 4.2.2 Information sources and search

The research was conducted in accordance with the Preferred Reporting Items for Systematic Reviews and Meta-analyses (PRISMA, Page et al., 2020) guidelines. The term `("attention network test" OR "attention network task") AND (meditat* OR mindfulness)` was used to search CINAHL (CINAHL Plus with Full Text, AMED, MEDLINE), Cochrane Library, PsycInfo, PubMed, Scopus, and Web of Science databases in December 2019. Full text search options were selected where available.

### 4.2.3 Study selection, data extraction and synthesis

Studies were selected by applying the inclusion criteria to titles, abstracts and full text of eligible studies (see Appendix B). Effect sizes and their confidence intervals were calculated for alerting, orienting and executive attention reaction times (RT). Effect sizes were not estimated for ANT error rates, as they are rarely reported. Tables 4.1–4.3 summarise the randomised, non-randomised and non-controlled studies included.

### 4.2.4 Subgroup analysis

Studies were stratified by study type (randomised or non-randomised) and the meditation experience of participants (novice or experienced).

**Table 4.1:** Summary of randomised, controlled meditation studies which include an ANT RT outcome.

| Publication | Age (mean/range) | Meditation experience | Treatment (*n*) | Treatment duration | Control (*n*) | Control duration | Minutes after meditation | | Task | Trials | ANT calculation | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | ANT 1 | ANT 2 | | | Alerting | Orienting | Executive |
| Ainsworth et al. (2013) | 20.3 | 0 | FAM (24), OMM (25) | 8 days; 3 * 1hr group training + 8 * 10m/day | Relaxation (27) | 8 days | NA | 0 | Emotional ANT | 480 | NC - DC | CC - SC | I - C |
| **Becerra et al. (2016)** | **33.9** | **0** | **FAM (23)** | **8 weeks; 4 * bi-weekly group sessions + 24m/day** | **Waitlist (23)** | **8 weeks** | **NA** | **NI** | **ANT** | **288** | **NC - DC** | **CC - SC** | **I - C** |
| **Burger and Lockhart (2017)** | **18-40 (69 %)** | **92% little or none** | **MM (28)** | **4 weeks; 10m/day** | **Waitlist (24)** | **4 weeks** | **NI** | **NI** | **ANT** | **NA** | **NI*** | **NI*** | **NI*** |
| Elliott et al. (2014) | 53.17 | 46.5-56.5 daily minutes; 40.7-44.3 retreat days | Samatha retreat (19) | 5-6 days | Samatha retreat (22) | 1-2 days | NA | NI | Modified ANT | NA | NA | (a) reflexive: cue (valid, invalid) x congruency x SOA x group (b) volitional: cue (valid, invalid) x congruency x SOA x group | (a) I-C (b) neutral cue RT task x congruency x SOA x group |
| **Kwak et al. (2020)** | **MM (30.9), relaxation (31.43)** | **0** | **MM (23)** | **19 hours over 4 days** | **Relaxation (14)** | **4 days** | **NA** | **NI** | **ANT** | **210** | **NC - CC** | **CC - SC** | **I - C** |
| Norris et al. (2018) | 19.52 | Minimal | Guided FAM (29) | 10m | Spoken word listening (27) | 10m | NI | 0 | ANT | 288 | NA | NA | RT |
| **Sharpe (2021, Experiment 1)** | **20.65** | **Minimal** | **FAM (17)** | **15 minutes** | **Reading (20)** | **NA** | **NI** | **0** | **ANT** | **144** | **NC - DC** | **CC - SC** | **I - C** |
| **Sharpe (2021, Experiment 2)** | **FAM (47.46), Reading (52.75)** | **Meditators: 1-41 years, 3-7 days/week, 10-90 mins/day** | **FAM (13)** | **15 minutes** | **Reading (12)** | **NA** | **NI** | **0** | **ANT** | **144** | **NC - DC** | **CC - SC** | **I - C** |
| **Sharpe (2021, Experiment 3)** | **FAM (39.7), Reading (46.77)** | **27-3156 hours** | **FAM (21)** | **15 minutes** | **Reading (22)** | **NA** | **NI** | **0** | **ANT** | **144** | **NC - DC** | **CC - SC** | **I - C** |
| **Sharpe (2021, Experiment 4)** | **NA** | **NA** | **FAM (28)** | **4 weeks** | **Waitlist (26)** | **4 weeks** | **NA** | **0** | **ANT** | **144** | **NC - DC** | **CC - SC** | **I - C** |
| **Sharpe (2021, Experiment 5)** | **NA** | **NA** | **FAM (23)** | **8 weeks** | **Waitlist (27)** | **8 weeks** | **NA** | **NI** | **ANT** | **144** | **NC - DC** | **CC - SC** | **I - C** |
| Tang et al. (2007) | 21.8 | 0 | IBMT (40) | 5 days * 20m/day | Body relaxation (40) | 5 days * 20m/day | NA | NI | ANT | 248 | NI* | NI* | NI* |
| **Tsai and Chou (2016, Experiment 2)** | **FA (20), Control (20)** | **NI** | **FA (20)** | **12 weeks * 50m/week** | **No training (20)** | **12 weeks** | **NI** | **NI** | **ANT** | **192** | **NC - DC** | **CC - SC** | **I - C** |
| **Walsh et al. (2019)** | **20.01** | **NI** | **MT app (45)** | **3 weeks * 10m/day** | **CT app (41)** | **3 weeks** | **NA** | **NA** | **CRSD-ANT** | **NA** | **NC - DC** | **CC - SC** | **I - C** |

*Note:*
CT = cognitive training; FAM = focused attention meditation; IBMT = integrative body-mind training; MM = mindfulness meditation; MT = mindfulness training; OMM = open monitoring meditation; ANT = Attention Network Test; NC = no cue; CC = centre cue; DC = double cue; SC = spatial cue; C = congruent flankers; I = incongruent flankers; RT = reaction time; CRSD-ANT = Centre for Research on Safe Driving-ANT; NI = no information; NA = not applicable.
* Assume calculations specified in Fan et al. (2002)

**Table 4.2:** Summary of non-randomised, controlled meditation studies which include an ANT RT outcome.

| | | | | | | | Meditation lag | | | | ANT calculation | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Publication | Age (mean/range) | Meditation experience | Treatment (*n*) | Treatment duration | Control (*n*) | Control duration | ANT 1 | ANT 2 | Task | Trials | Alerting | Orienting | Executive |
| **Isbel and Mahar (2015)** | **42.65** | **Meditators=average 10 years, 8.1 hours/week. Non-meditators=0.** | **Meditators (23)** | **NA** | **Non-meditators (21)** | **NA** | **NI** | **NA** | **ANT** | **288** | **NC - DC** | **CC - SC** | **I - C** |
| Jha et al. (2007) | MBSR (24), Retreat (35), Control (22) | MBSR=0, Retreat=4-360 months, control=0 | MBSR (17), Reteat (17) | MBSR: 8 weekly 3-hr group sessions + 59 * 30m/day; Retreat: 30 days * 10-12h/day | No mindfulness (17) | 0 | MBSR=NA, Control=NA, Retreat=NI | NI | ANT | 288 | NC - DC | CC - SC | I - C |
| **Jo et al. (2016)** | **Meditators (41), controls (40.8)** | **Meditators=aveage 13.1 years, 247.8 minutes/week. Non-meditators=0.** | **Meditators (20)** | **NA** | **Age and gender matched controls (20)** | **NA** | **NI** | **NA** | **ANT** | **288** | **NC - CC** | **CC - SC** | **I - C** |
| **Otten et al. (2015)** | **Meditators (39.7), Controls (39.5)** | **Meditators: >= 3 years, >= 2 hours/week. Controls: 0** | **Meditators (22)** | **NA** | **Sex, age, education matched controls (22)** | **NA** | **NI** | **NA** | **ANT** | **NA** | **NI*** | **NI*** | **NI*** |
| **Schötz et al. (2015)** | **21-50** | **Meditators: >= 3 years, >= 2 hours/week. Controls: 0** | **TM (20)** | **NA** | **Sex, age, education, BMI, physical activity, stress, impulsiveness matched controls (20)** | **NA** | **NI** | **NA** | **ANT** | **288** | **NI*** | **NI*** | **NI*** |
| Sperduti et al. (2016) | OAN (67.12), OAE (67.69), YAN (27.16) | 25.5 years | OAE (16) | NA | Sex, education, age, cognition matched controls. OAN (16). YAN (19). | NA | NI | NA | Modified ANT | 96 | NC - DC | DC - SC | I - C |
| **Tsai and Chou (2016, Experiment 1)** | **FA/OM (46), control (44)** | **Meditators: 3-30 years. Controls: 0** | **FA/OM (11/19=30)** | **NA** | **Age, education, social status matched controls (30)** | **NA** | **NI** | **NA** | **ANT** | **192** | **NC - DC** | **CC - SC** | **I - C** |
| van den Hurk et al. (2010) | Meditators (48.1), Controls (48.1) | Meditators: 3 months-30 years, 60-420 minutes/week. Controls: NI | Meditators (20) | NA | Age and gender matched controls (20) | NA | NI | NA | ANT | 282 | NC - DC | CC - SC | I - C |
| **Wittmann et al. (2015)** | **21-50** | **Meditators: >= 3 years, >= 2 hours over last 8 weeks. Controls: 0** | **Meditators (42)** | **NA** | **Age, sex, education matched (42)** | **NA** | **NI** | **NA** | **ANT** | **NA** | **NI*** | **NI*** | **NI*** |

*Note:*
FA = focused attention; MBSR = mindfulness based stress reduction; OM = open monitoring; ANT = Attention Network Test; OAE = Older adults meditation-experts; OAN = Older adults meditation-naïve; Younger adults meditation-naïve; NC = No cue; CC = Centre cue; DC = Double cue; SC = Spatial cue; C = Congruent flankers; I = Incongruent flankers; NI = No information; NA = Not applicable.
* Assume calculations specified in Fan et al. (2002)

**Table 4.3:** Summary of non-randomised, non-controlled meditation studies which include an ANT RT outcome.

| | | | | | | | Meditation lag | | | | ANT calculation | | |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| Publication | Age (mean/range) | Meditation experience | Treatment (*n*) | Treatment duration | Control (*n*) | Control duration | ANT 1 | ANT 2 | Task | Trials | Alerting | Orienting | Executive |
| Schanche et al. (2019) | 23.12 | 0 | MBCT (25) | 2 weeks. 3 * 2.5 hours + 6 hours + 1 hr / day. | NA | NA | NA | NI | ANT-R | 288 | NI† | NI† | NI† |
| Spadaro and Hunker (2016) | ”The age of the participants varied between the ages of under 25 and over 60.” | 0 | MBSR (23) | 8 weeks | NA | NA | NA | NI | ANT | NA | NI* | NI* | NI* |

*Note:*
MBCT = mindfulness based cognitive therapy; MBSR = mindfulness based stress reduction; ANT = Attention Network Test; ANT-R = Revised Attention Network Test; NC = No cue; CC = Centre cue; DC = Double cue; SC = Spatial cue; C = Congruent flankers; I = Incongruent flankers; NI = No information; NA = Not applicable.
* Assume Fan et al. (2002)
† Assume Fan et al. (2009)

### 4.2.5 Risk of bias within studies

Strengths and weaknesses of the included studies were assessed using two risk of bias tools. The individually-randomized, parallel-group trials were assessed for bias arising from the randomization process, deviations from the intended intervention, missing outcome data, outcome measurement, and selection of the reported result, using the RoB 2 tool (Sterne et al., 2019). The non-randomised studies were assessed for bias due to confounding, selection of participants, classification of interventions, deviations from the intended intervention, missing data, outcome measurement, and selection of the reported result, using the ROBINS-I tool (Sterne et al., 2016). Funnel plots were used to test for publication bias (Sterne et al., 2011).

### 4.2.6 Meta-analysis

Bayesian meta-analyses were conducted on the three ANT outcomes for randomised and non-randomised studies. Effects were pooled for experienced and novice subgroups within each ANT outcome. A pooled standardised mean difference (SMD) was estimated using a model which gave greater weight to studies with larger sample sizes. Study was included as a random effect, meaning that effect sizes were assumed to vary by study (Harrer et al., n.d.)[1]. A normal (0,1) prior was specified for the pooled effect estimate, on the basis that small effects are more likely than large effects, and that positive or negative effects are equally likely. A half-Cauchy (0,.3) prior was specified for the standard deviation in effect sizes. This gave most prior weight to small variations in the pooled effect, but still allowed some likelihood for wider variance from the average effect size. A 95% highest-density continuous interval (HDCI) was calculated for each

[1] Appendix C lists the formulas used in the meta-analysis.

study and also for the pooled effects. The HDCI is the shortest single range[2] in which 95% of the posterior density falls. In other words, it is the range within which we are 95% sure that the true effect lies, conditional on the data and model. Models were built using `brms` (Bürkner, 2017).

Effect sizes for each study were calculated so that a positive number represented a better ANT score for the mindfulness condition, where 'better' means higher alerting and orienting scores, and lower executive attention scores (see Section 2.1.2). For case-control studies this means better ANT scores in the meditation condition relative to the control condition. For cohort studies, this means a greater improvement in ANT scores over time in the meditation condition relative to the control condition. Requests were made to the original investigators where data required for calculating effect sizes were unavailable in the published article.

## 4.3 Results

### 4.3.1 Search results, and study selection

The database search produced 222 results, 85 of which were duplicates. From the remaining 137 items, 88 were excluded by screening at the title and abstract level. Thirty of the remaining 49 items were excluded during full text screening, because they did not meet eligibility criteria. These items included non-mindfulness training studies (n=17), studies which used an outcome measure other than ANT (n=5), study protocols without published data (n=3), review articles (n=2), and studies where participants were under 18 (n=1), or reported subjective cognitive complaints (n=1). One study with negative

[2]The HDCI is different to the Highest Density Interval which can create multiple intervals if the posterior were bimodal (Kruschke, 2015).

baseline alerting and orienting scores (Quan et al., 2018) was excluded, because the ANT is not expected to produce negative scores (MacLeod et al., 2010, p. 646). After adding five studies from the current thesis (Chapters 2 & 3), twenty-four items were included in this systematic review, one of which contained two experiments (Tsai & Chou, 2016).

Sixteen of the twenty five studies were included in the meta-analysis. Of the nine exclusions, RT data was unavailable for Jha et al. (2007), Norris et al. (2018, Study 2), Schanche et al. (2019), Spadaro & Hunker (2016), Sperduti et al. (2016), Tang et al. (2007), and Hurk et al. (2010). Both the emotional ANT (Ainsworth et al., 2013), and the modified ANT described by (Elliott et al., 2014) were considered to be too different to the majority of ANT tasks to be included in the meta-analysis. For Tsai and Chou (2016, Experiment 1), the comparison with the largest sample sizes was chosen. This was the comparison between meditators and non-meditators, rather than the comparison between FA and OM in the meditation group.

### 4.3.2 Results of individual studies

Table 4.4 summarises the hypotheses and outcomes for all studies. Bold cells indicate that the effect of mindfulness was greater than the control condition. The adjacent cell is also bold if this finding matched the hypothesis. The outcomes in Table 4.4 are based on this author's interpretation of the results of each study. In four cases (Becerra et al., 2016; Hurk et al., 2010; Tsai & Chou, 2016, Experiments 1 and 2), this interpretation differed from that of the original author(s).

Effects of mindfulness on the ANT are consistently low, with no group differences in most studies. There were 13 comparisons where meditation improved executive attention, three where it improved alerting, two where it improved orienting, and one where the control group scored higher on orienting than the meditation group.

**Table 4.4:** Results of ANT studies.

| Publication[*] | Treatment 1 (T1) | Treatment 2 (T2) | Control | ANT[†] | Alerting Hypotheses | Alerting Outcomes | Orienting Hypotheses | Orienting Outcomes | Executive Hypotheses | Executive Outcomes |
|---|---|---|---|---|---|---|---|---|---|---|
| **Novice meditators** | | | | | | | | | | |
| Becerra et al. (2016) | MT (FAM) | NA | Waitlist | 1 vs. 2 | **T1 > C** | **T1 > C[‡]** | T1 > C | **C > T1[§]** | **T1 > C** | **T1 > C** |
| Burger and Lockhart (2017) | MT (MM) | NA | Waitlist | 1 vs. 2 | None | T1 = C | None | T1 = C | None | **T1 > C** |
| Kwak et al. (2020) | MM | NA | Relaxation | 1 vs. 2 | None | T1 = C | None | T1 = C | **T1 > C** | **T1 > C** |
| Sharpe (2021, Experiment 1) | FAM | NA | Reading | 1 vs. 2 | T1 > C | T1 = C | T1 > C | T1 = C | T1 > C | T1 = C |
| Sharpe (2021, Experiment 4) | MT (FAM) | NA | Waitlist | 1 vs. 2 | T1 > C | T1 = C | T1 > C | T1 = C | T1 > C | T1 = C |
| Sharpe (2021, Experiment 5) | MT (FAM) | NA | Waitlist | 1 vs. 2 | T1 > C | T1 = C | C > T1 | T1 = C | T1 > C | T1 = C |
| Tsai and Chou (2016, Experiment 2) | MT (FAM) | NA | No training | 1 vs. 2 | None | **T1 > C[¶]** | None | **T1 > C[¶]** | None | **T1 > C** |
| Walsh et al. (2019) | MT app | NA | CT app | 1 vs. 2 | T1 > C | T1 = C | T1 > C | T1 = C | **T1 > C** | **T1 > C** |
| **Experienced meditators** | | | | | | | | | | |
| Isbel and Mahar (2015) | Meditators | NA | Non-meditators | 1 | T1 > C | T1 = C | T1 > C | T1 = C | T1 > C | T1 = C |
| Jo et al. (2016) | Meditators | NA | Matched | 1 | None | T1 = C | T1 > C | T1 = C | T1 > C | T1 = C |
| Otten et al. (2015) | Meditators | NA | Matched | 1 | T1 > C | T1 = C | T1 > C | T1 = C | T1 > C | T1 = C |
| Schötz et al. (2015) | TM | NA | Matched | 1 | T1 > C | T1 = C | T1 > C | T1 = C | T1 > C | T1 = C |
| Sharpe (2021, Experiment 2) | FAM | NA | Reading | 1 vs. 2 | T1 > C | T1 = C | T1 > C | T1 = C | T1 > C | T1 = C |
| Sharpe (2021, Experiment 3) | FAM | NA | Reading | 1 vs. 2 | T1 > C | T1 = C | T1 > C | T1 = C | T1 > C | T1 = C |
| Tsai and Chou (2016, Experiment 1) | FA | OM | Matched | 1 | None | T1 = C | None | C > T1[**] | None | **T1 > C** |
| Wittmann et al. (2015) | Meditators | NA | Matched | 1 | None | T1 = C | None | T1 = C | None | T1 = C |
| **Not included in meta-analysis** | | | | | | | | | | |
| Ainsworth et al. (2013) | FAM | OMM | Relaxation | 1 vs. 2 | T1 = T2 = C | T1 = T2 = C | (a) T1 = T2 = C (b) T2 > T1 > C (valence) | (a,b) T1 = T2 = C | **T1 > C; T2 > C** | **T1 > C; T2 > C** |
| Elliott et al. (2014) | Samatha (post) | NA | Samatha (pre) | 2 | NA | NA | None | (a) T1 = C (volitional) (b) T1 = C (reflexive) | **Alerting-Executive decoupling** | **T1 > C** |
| Jha et al. (2007) | MBSR | Retreat | No mindfulness | 1 | T2 > T1; T2 > C | T1 = T2 = C | T2 > T1; T2 > C | T1 = T2 = C | **T2 > T1; T2 > C** | **T2 > T1 + C** |
| | MBSR | Retreat | No mindfulness | 2 | **T2 > T1; T2 > C** | **T2 > T1 + C** | **T2 > C; T1 > C** | **T1 > C + T2** | T2 > C; T1 > C | T1 = T2 = C |
| Norris et al. (2018) | FAM (guided) | NA | Listening | 2 | NA | NI | NA | NI | **T1 > C** | **T1 > C[‡‡]** |
| Schanche et al. (2019) | MBCT | NA | NA | 1 vs. 2 | post > pre | post = pre | post > pre | post = pre | **post > pre** | **post > pre** |
| Spadaro and Hunker (2016) | MBSR | NA | NA | 1 vs. 2 | None | post = pre | None | post = pre | None | post = pre |
| Sperduti et al. (2016) | OAE | YAN | Matched OAN | 1 | None | T1 = T2 = C | None | T1 = T2 = C | **T1 > C** | **T1 > C** |
| Tang et al. (2007) | IBMT | NA | Body relaxation | 1 vs. 2 | None | T1 = C | None | T1 = C | **T1 > C** | **T1 > C** |
| van den Hurk et al. (2010) | Meditators | NA | Matched | 1 | None | T1 = C | T1 > C | C > T1[††] | T1 > C | T1 = C |

*Note:*
**Bold** = effect of meditation/supported hypothesis; CT = cognitive training; FAM/FA = focused attention meditation; IBMT = integrative body-mind training; MBCT = mindfulness based cognitive therapy; MBSR = mindfulness based stress reduction; MM = mindfulness meditation; OMM/OM = open monitoring meditation; ANT = Attention Network Test; OAE = Older adults meditation-experts; OAN = Older adults meditation-naïve; Younger adults meditation-naïve; NI = No information; NA = Not applicable.

[*] 1 = ANT before intervention(s) or between groups; 2 = ANT after intervention(s).
[†] Jha (2007): ANT=1 is a hypothesis of meditation experience; ANT=2 are hypotheses of dorsal function (Orienting, Executive), and ventral function (Alerting).
[‡] Becerra et al. (2016) report MT = control.
[§] Becerra et al. (2016) report MT = control.
[¶] Tsai and Chou (2016) report MT = control.
[**] Tsai and Chou (2016) report Meditators > controls.
[††] van den Hurk et al. (2010) report Meditators > controls.
[‡‡] Main effect of group in condition x trial type (congruent, incongruent) ANOVA.

### 4.3.3 Risk of bias within studies

#### Randomised studies

Figure 4.1 summarises the risk of bias assessments for the randomised studies. The traffic-light plots in this and Figure 4.2 were generated using `robvis` (McGuinness, 2019). Concerns in Domain 1 related to the randomisation method, which was either unspecified, or potentially non-random. Only Walsh et al. (2019) specified that the allocation sequence was concealed until participants were assigned to interventions. In Domain 2, one study (Ainsworth et al., 2013) was assessed as being at high risk of bias, because there was no information about whether participants complied with the assigned daily meditation. There were some concerns that the lack of supervision in the online procedure described by Sharpe (2021, Experiment 3)[3] risked non-adherence to interventions that could have affected participants' outcomes. In Domain 5, concerns regarding bias in the reported result were for studies which did not pre-register an analysis plan.

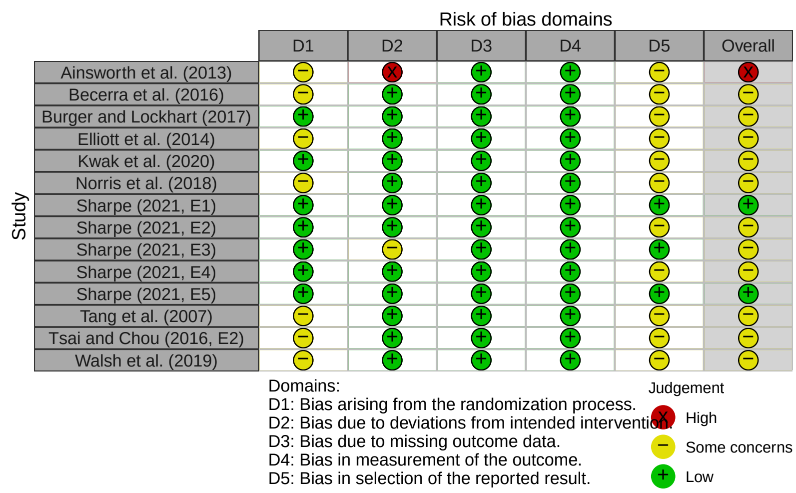


**Figure 4.1:** Risk of Bias in individually-randomised studies, assessed using RoB 2.

[3]Experiment 3, Chapter 2.

**Non-randomised studies**

Figure 4.2 summarises risk of bias assessments for the non-randomised studies, including the two studies without control conditions (Schanche et al., 2019; Spadaro & Hunker, 2016). Many of these were case-control studies involving long-term meditators. Long-term meditators might systematically differ from controls in many ways other than their lifetime meditation duration. For example, an *innate* ability to focus attention might attract people to meditation, but their long-term practice may have little or no effect on attention. In Domain 1, studies which included a long-term meditator group (Hurk et al., 2010; Isbel & Mahar, 2015; Jha et al., 2007; Jo et al., 2016; Otten et al., 2015; Schötz et al., 2016; Sperduti et al., 2016; Tsai & Chou, 2016 Experiment 1; Wittmann et al., 2015) were rated as being at serious risk of confounding, because these additional variables were not measured or controlled for. For similar reasons, these studies were classified as being at serious risk of bias due to deviation from intended interventions (Domain 4). Long-term meditators are also likely differ from controls in ways which should be treated as "co-interventions" (Sterne et al., 2016). For example, many Buddhist meditators maintain a number of ethical precepts e.g. a commitment to restrain their speech and actions, potentially leading to cognitive and behavioural differences which should also be controlled for. A complete list of co-interventions would be extensive, difficult to measure and impossible to control for. Also in Domain 4, Schanche et al. (2019) and Spadaro & Hunker (2016) were assessed as being at moderate risk of bias, due to their lack of a control condition. All studies were assessed as being at moderate risk or above for bias in selection of the reported result (Domain 7), as they did not pre-register an analysis plan.

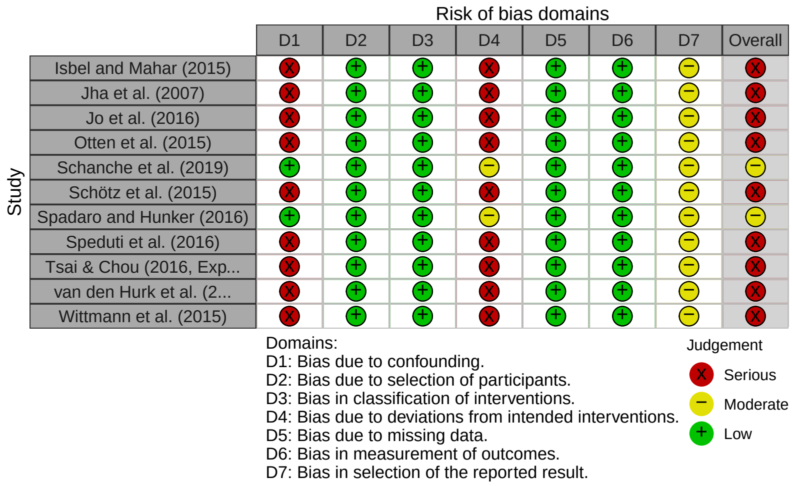


**Figure 4.2:** Risk of bias in non-randomised studies, assessed using ROBINS-I.

### 4.3.4 Risk of bias across studies

Figures 4.3 and 4.4 summarise risks of bias across the randomized and non-randomized studies respectively[4]. For the randomized studies, overall risk of bias was moderate, and driven by three domains. Half of the studies showed some concerns regarding randomisation procedures (Domain 1). A quarter of studies showed concerns or high risk of bias due to deviations from the intervention (Domain 2). Three quarters of studies showed some concerns regarding selection of the reported result (Domain 5), primarily because analysis plans were not pre-registered. Overall risk of bias in the non-randomized studies was serious, and was driven three domains. Three quarters of studies were at serious risk of bias due to confounding (Domain 1). All studies were at moderate or serious risk of bias due to deviations from the intended interventions (Domain 4). Finally, all studies were at moderate risk of bias in selection of the reported result, again because they did not pre-register an analysis plan.

[4] All studies were equally weighted in these analyses.


**Figure 4.3:** Risk of bias across randomised studies.

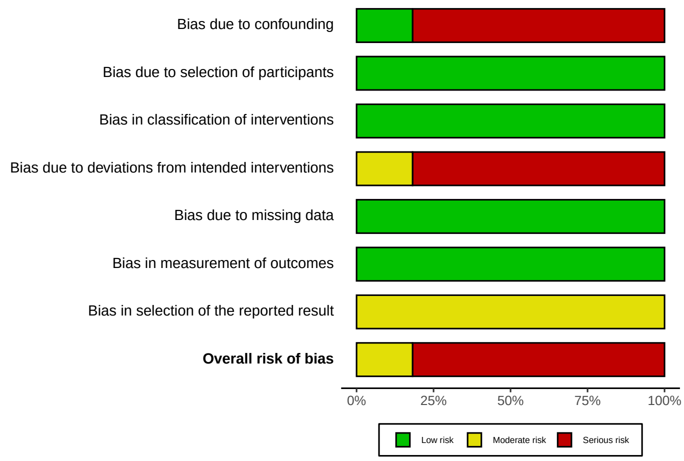


**Figure 4.4:** Risk of bias across non-randomised studies.

### 4.3.5 Meta-analysis

The following meta-analyses estimate the true effect of meditation on the ANT in novices and experienced meditators. Bayesian meta-analysis produces a posterior distribution which summarises the probabilities we assign to a range of effect sizes. The HDCI is derived from this distribution, and shows the range within which we are 95% sure that the true effect lies. Bayesian meta-analyses also allow us to make probability statements about effect sizes, which place reasonable upper and lower bounds on the true effect of meditation on attention. Where effect sizes are not qualified, they refer to the SMD

(Hedges' *g*). Traditionally, SMDs of 0.2, 0.5 and 0.8 are are considered small, medium and large respectively (Cohen, 1992). When interpreting the ANT, improved performance is traditionally indicated by higher alerting and orienting scores, and lower executive attention lower scores (Fan et al., 2002). To simplify the meta-analysis results, values have been adjusted so that positive values for all ANT scores reflect improvements in the meditation condition. A HDCI which does not overlap zero provides certainty over the direction of an effect.

The meta-analyses included eight effects for novices, all from randomised studies, and five effects for experienced meditators from non-randomised studies. The only randomised experiments involving experienced meditators were Experiments 2 and 3 in the current thesis.

**Executive attention**

The pooled effect for novices in Figure 4.5 shows evidence of a small improvement (*SMD* = 0.14) in executive attention. Based on the data observed, there is a 33% chance that the effect is > 0.2, and a 1% chance that it is > 0.5. For the randomised studies involving experienced meditators, Figure 4.5 shows a very small (*SMD* = 0.05) pooled effect. There is a 21% chance that the effect is > 0.2, and a 3% chance that it is > 0.5.

Figure 4.6 shows results for the non-randomised studies. There is evidence of a slightly larger effect of meditation (*SMD* = 0.15) than in the randomised studies. There is a 30% chance that the effect is > 0.2.

The funnel plot in Figure 4.7 shows, for each study, the estimate of the SMD for executive attention, against the study's standard error. A funnel plot is a scatterplot of study effect size estimates against their sample size or precision (Sterne et al., 2011). The

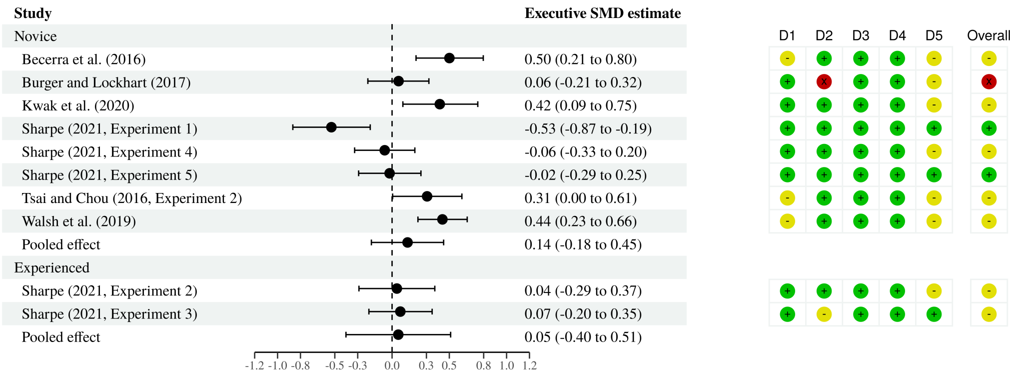


**Figure 4.5:** Meta-analytic effect sizes for executive attention (randomised studies).

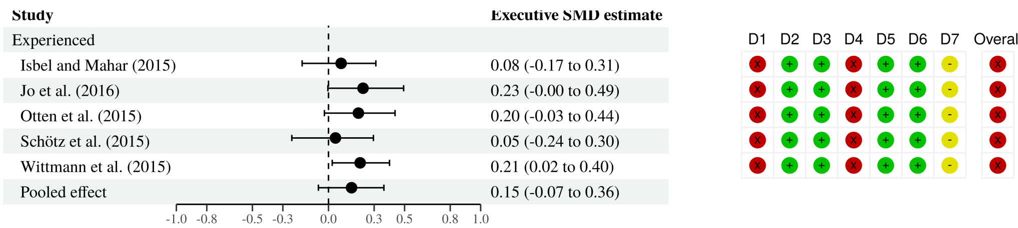


**Figure 4.6:** Meta-analytic effect sizes for executive attention (non-randomised studies).

standard error is representative of sample size, so studies with larger samples and power are positioned higher on the y axis (as the standard error decreases). Effect estimates from smaller studies should scatter more widely at the bottom of the plot, and narrow as sample sizes increase, creating an inverted funnel shape. In the absence of bias the solid and dotted triangles, centred on the pooled effect size estimate, will include about 95%, and 99% of studies respectively. Asymmetry around the effect size estimate can indicate publication bias (Sterne et al., 2011). Figure 4.7 is symmetrical, which indicates that the executive attention SMD estimates were not affected by publication bias.

### Alerting

The pooled effect for novices in Figure 4.8 shows minimal effects (*SMD* = 0.01) of meditation on alerting in novices. Based on the data observed, there is a 5% chance

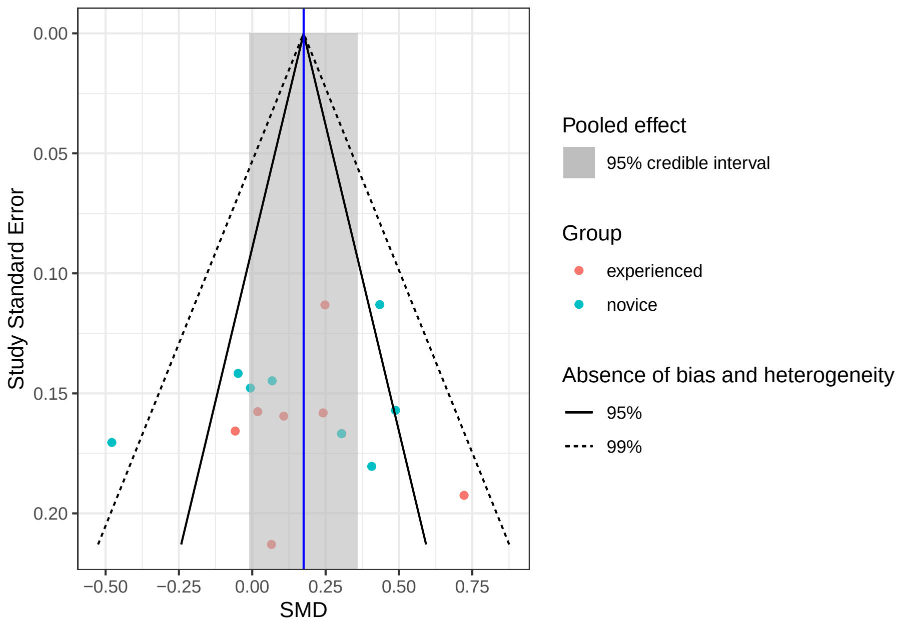


**Figure 4.7:** Funnel plot showing meta-analytic executive attention SMD against study size/precision.

that the effect is > 0.2. One study (Sharpe, 2021, Experiment 4) found the reverse effect – that alerting was better in the control condition than the experimental condition. For the randomised studies involving experienced meditators, Figure 4.8 also shows a small (*SMD* = 0.18) improvement in alerting. There is a 50% chance that the effect is > 0.2, and a 9% chance that it is > 0.5. There is also a 7% chance that meditation has a negative effect on alerting, with an effect size > 0.2.

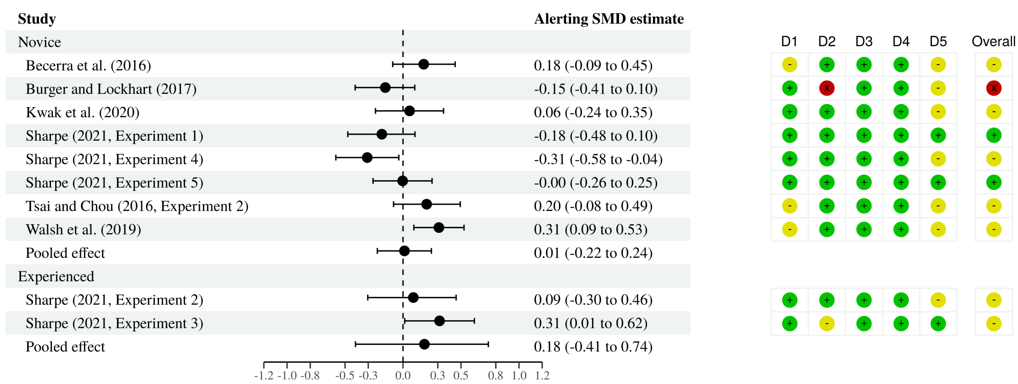


**Figure 4.8:** Meta-analytic effect sizes for alerting (randomised studies).

Figure 4.9 shows results for the non-randomised studies. There is minimal evidence ($SMD = 0.04$) that meditation improves alerting. There is a 9% chance that the effect is > 0.2, and a 3% chance that meditation has a negative effect on alerting, with an effect size > 0.2.

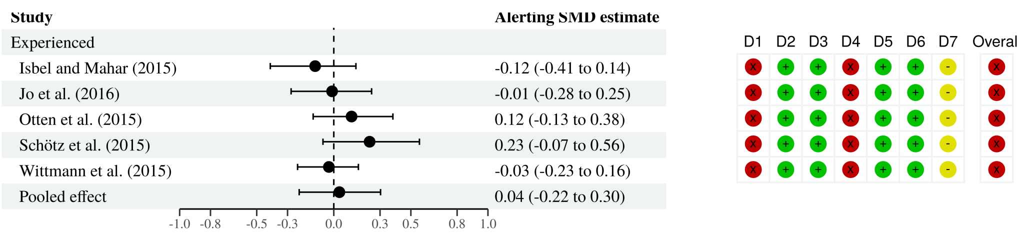


**Figure 4.9:** Meta-analytic effect sizes for alerting (non-randomised studies).

The funnel plot in Figure 4.10 is symmetrical about the pooled effect size, so there is no evidence that the alerting SMD estimates were affected by publication bias.

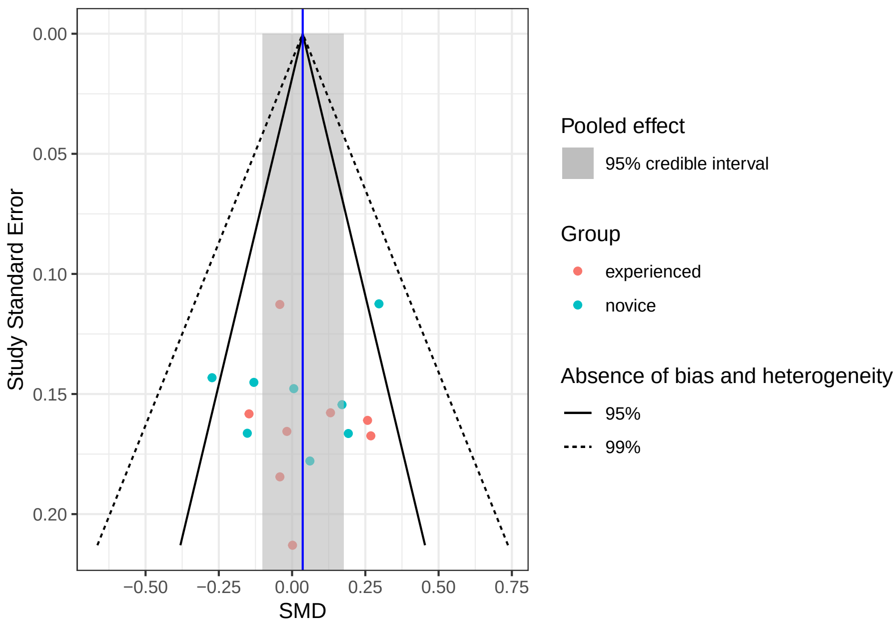


**Figure 4.10:** Funnel plot showing meta-analytic alerting SMD against study size/precision.

**Orienting**

The pooled effect for novices in Figure 4.11 shows that meditation has a very small (*SMD* = -0.07), negative effect on orienting. Based on the data observed, there is a 21% chance that this effect is > 0.2, and a 1% chance that it is > 0.5. There is also a 5% chance that meditation has a positive effect on orienting, with an effect size > 0.2. For the randomised studies involving experienced meditators, Figure 4.11 also shows a small (*SMD* = -0.19), negative orienting effect, mostly driven by Sharpe (2021, Experiment 2). There is a 48% chance that the effect is > 0.2, and a 14% chance that it is > 0.5. There is also a 10% chance that meditation improves orienting in experienced meditators, with an effect size > 0.2, and a 3% chance that this effect is > 0.5.

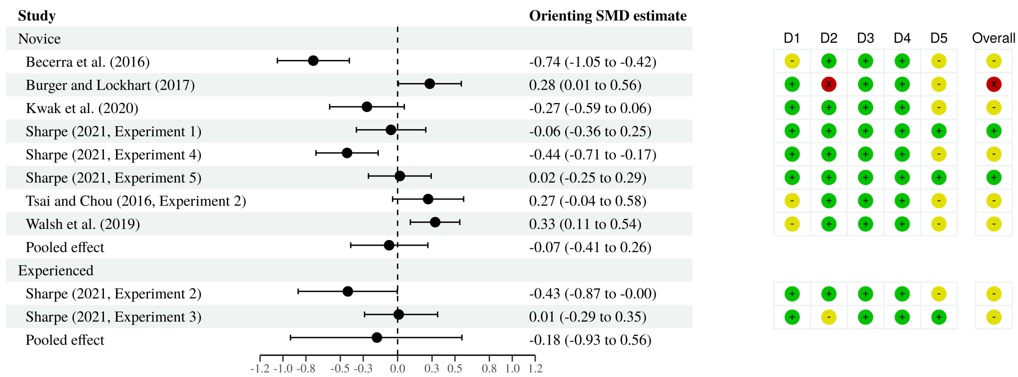


**Figure 4.11:** Meta-analytic effect sizes for orienting (randomised studies).

Figure 4.12 shows results for the non-randomised studies. There is minimal evidence that meditation improves alerting (*SMD* = 0.03). There is a 2% chance that the effect is > 0.2.

The funnel plot in Figure 4.13 is symmetrical about the pooled effect size, so there is no evidence that the orienting SMD estimates were affected by publication bias.

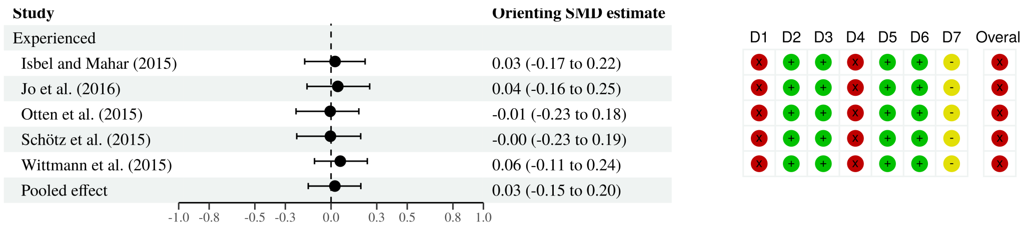


**Figure 4.12:** Meta-analytic effect sizes for orienting (non-randomised studies).

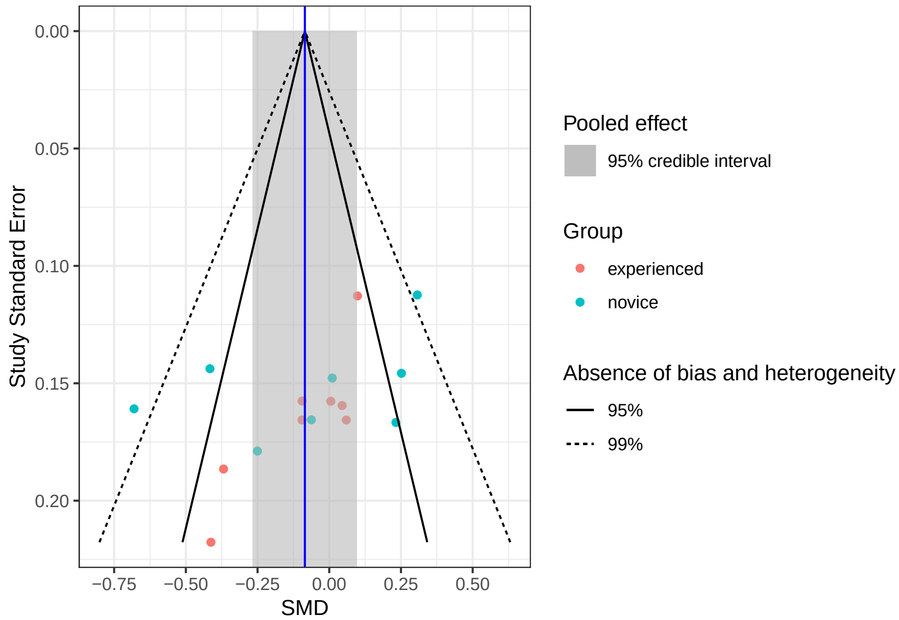


**Figure 4.13:** Funnel plot showing meta-analytic orienting SMD against study size/precision.

## 4.4 Discussion

The meta-analyses show that mindfulness meditation produces small executive attention improvements. In the randomised studies, there was evidence for a small effect in novices, but no evidence of an effect in experienced meditators. There was a large degree of uncertainty over the pooled effect estimate in experienced meditators. In the non-randomised studies, there was evidence for a small executive attention effect in experienced meditators. These estimates mean that all of the reviewed studies were underpowered. For example, a between-subjects design with $SMD$ = .15, $\alpha$ = .01, and power of .96[5], would require 1479 participants per group to detect differences. This is over 50 times the sample sizes used in these studies. A next step might be to try to replicate the studies which show the largest improvements to executive attention (Becerra et al., 2016; Kwak et al., 2020; Tsai & Chou, 2016, Experiment 2; Walsh et al., 2019). All of these studies tested the effects of training novices (the easiest participants to recruit) to meditate. A replication of Becerra et al. (2016) would need about 135 participants per group to detect differences in executive attention.

There was minimal evidence that mindfulness meditation improves alerting. The randomised studies provided no evidence for an effect in novices, and for experienced meditators the pooled effect was small. In the non-randomised studies, there was no evidence that meditation improved alerting in experienced meditators. There was a large degree of uncertainty over all three pooled alerting effect estimates.

There was also minimal evidence that mindfulness meditation improves orienting. The randomised studies suggested that meditation has a very small detrimental effect on orienting, in both novices and experienced meditators. There was a large degree of uncertainty for the pooled estimates in the randomised studies. In the non-randomised studies, there was no evidence that meditation improved orienting in experienced meditators.

Care is needed when reporting ANT outcomes, because the rationale underlying ANT score calculations make it easy to reverse outcome directions, especially when calculating difference scores before and after an intervention. This can lead to misinterpretations which potentially inflate the perceived effects of meditation (or any other

[5]Bayes Factors tend to be between about 3 and 5 with $\alpha$ = .01. Type I and Type II errors are balanced with $\alpha$ = .01, power=.96 to the same extent as the more traditional $\alpha$ = .05, power = 0.8. https://ajwills72.github.io/rminr/effsize_from_papers.html#96power

intervention). In four studies, our interpretations of one or more outcomes differed from the those reported in the original article.

Researchers should measure, report and potentially control for participants' most recent meditation, because the mental states induced by mindfulness meditation are likely to be ephemeral. For example, ANT scores might only be affected if a participant had meditated within an hour of an experiment. Recency could easily be controlled for by including a meditation as part of an experimental procedure. This is an important variable which was not reported or controlled for in most of the studies in this (Tables 4.1-4.3, columns ANT1 and ANT2), and previous (Chiesa et al., 2011, Table 4) reviews.

The meta-analyses were limited to an extent by the low quality of the underlying data. Data were unavailable for over a third of studies, which was indicative of a relatively poor quality of reporting that can be seen in the overall risk of bias ratings. These ratings raised some concerns over bias in the randomised studies, and identified serious risks of bias in the non-randomised studies. All non-randomized studies, and three quarters of randomized studies showed moderate risk of bias in selection of the reported result. All non-randomized studies, and a quarter of randomized studies were at risk of bias due to deviations from the intended interventions. Randomisation procedures were at risk of bias in half of the randomised studies, and there was risk of confounding in three quarters of non-randomized studies. These observations align with broader critical evaluations of the mindfulness and meditation literature (Van Dam et al., 2017).

In conclusion, these meta-analyses provide some certainty that the effects of meditation on the ANT are much smaller than previously thought. Consequently, the ANT does not provide strong evidence to support theories which associate mindfulness with improved attention regulation (Hölzel et al., 2011; Lindsay & Creswell, 2019; Malinowski, 2013; Teper et al., 2013).

# 5

# The effects of brief meditation on the attentional blink

**Contents**



## 5.1 Introduction

This chapter signals a change in direction from measuring attention regulation using the ANT. Chapter 4 found a small effect of meditation on executive attention, but little evidence of effects on either alerting or orienting. One approach would be to use sample sizes large enough to detect the small effects on executive attention established by the meta-analysis in Chapter 4. However, the sample sizes required to detect such small effects were too large for this to be feasible within the scope of this thesis. Therefore, the effects of meditation were tested on a different, potentially more sensitive, attentional task.

Novices who complete brief periods of mindfulness meditation show improved performance on a range of attentional tasks. Dickenson et al. (2013) found that approximately 5 minutes of breath meditation recruited an attentional brain network which was not recruited by an equivalent period of mind wandering. Schofield et al. (2015) found that inattentional blindness was reduced after a 7 minute audio-guided mindfulness exercise using raisins, compared with a control condition where participants listened to a factual description of raisins. Compared with 10 minutes of arithmetic exercises, 10 minutes of guided mindfulness meditation improved performance on the emotional Stroop task, a test of enhanced attentional control and inhibition of elaborative processing (Watier & Dubois, 2016). Gorman & Green (2016) compared undergraduates who interleaved three, 10-minute sessions of either a breath counting task (Levinson et al., 2014) or web browsing, with a battery of cognitive tasks. The breath counting group performed better on the flanker task component of the ANT, and tasks which tested attentional filtering, impulsivity and task-switching. The attentional blink (Shapiro et al., 1997) is a phenomenon which measures attention allocation over short time periods. With 17 minute interventions, Colzato et al. (2015) found a reduced attentional blink after OMM, and an increased attentional blink after FAM, relative to a relaxation control condition. This consistent pattern of results could mean that tasks other than the ANT are more sensitive measures of the effects of brief mindfulness meditation in novices.

### 5.1.1 The attentional blink

Amongst these outcomes, the attentional blink (AB) is of particular interest, because mindfulness theories consider reduced AB to be evidence of improved attention regulation (Hölzel et al., 2011; Malinowski, 2013; Vago & Silbersweig, 2012). The AB takes its name from an analogy with the lapse in vision which occurs when the eyes blink. The effect can be demonstrated in a task where two target numbers must be detected amongst a larger number of distractor letters presented sequentially, and very rapidly, in the same location on a screen (see Figure 5.1).

Each stimulus is presented for 70ms, and the number of letters between the first target (T1) and second target (T2) is varied on each trial. The interval between T1 and T2 is called the 'lag'. At the end of a trial, if participants have detected T1, their accuracy at reporting T2 tends to be worse at lags between 200-500ms. This is the AB effect. It is thought to occur because attentional resources are allocated to processing T1, and intervening distractors, to the extent that T2 is not perceived; hence, the analogy with a visual blink. From approximately 200ms, conditional T2 accuracy (T2 accuracy given T1 detection) declines and then returns to baseline in an approximate 'U' shape. Experiments have shown that the AB is not due to limitations in perception or memory span (Shapiro et al., 1997), providing further evidence that the effect measures how attention is allocated over time.

### Meditation expertise

The AB appears to be smaller in people who have practised meditation over many years. Leeuwen et al. (2009) found a smaller AB in adults (mean age 49.8) with 1–29 years lifetime meditation experience (a mixture of OMM and FAM), than in two control groups of non-meditators. One control group was matched for age, sex and education, the other contained younger participants (mean age 24.3). These results suggest that practising meditation over a number of years may delay cognitive decline; AB performance, sustained attention, and inhibitory control are all known to decline with age (Leeuwen et al., 2009). Furthermore, as participants did not meditate in the experimental sessions, the reduced AB in the meditation group indicates that the effects of meditation on attentional processing were stable over time.

### Extended meditation training

Extended periods of meditation training have also been shown to reduce the AB. Slagter et al. (2007) compared the effects on the AB of extended meditation training with moderate meditation training. The experimental group were experienced meditators who were tested twice, before and after a three month Vipassana retreat, during which they meditated for between 10-12 hours each day. Some Vipassana meditation practices involve noting successive sensory impressions of seeing, hearing etc. (Anālayo, 2003, p. 95, note 8), making them similar to OMM. The control group, who were matched on age and education with the experimental group, received one hour of Vipassana training and were asked to meditate daily for 20 minutes in the week before each of their two AB testing sessions. After their respective training, the experienced meditators showed a reduced AB in comparison with the control group. Furthermore, EEG measures showed reduced allocation of brain resources when the experienced meditators were processing T1. These reductions were correlated with reduced AB magnitude, indicating more even allocation of attention between T1 and T2 in the experienced meditators. From these results, Slagter et al. (2007) inferred that intensive Vipassana meditation training reduces the AB by increasing T1 processing efficiency.

**Brief meditation training**

Some studies suggest that even short meditations can affect the AB. Vugt & Slagter (2014) had experienced meditators interleave meditation with the AB task. This was a within-subjects design, consisting of counterbalanced FAM and OMM blocks. They found a smaller AB in the OMM condition than in the FAM condition, but only in a sub-sample who had an average lifetime meditation experience of approximately 10,000 hours. The lack of AB differences in the less experienced sub-sample could have been due to carry over effects when switching between FAM and OMM, or to difficulties in distinguishing between the meditation instructions.

To address these points, Colzato et al. (2015) used a between-subjects design, in which groups of non-meditators completed 17 minutes of either OMM or FAM, followed by the AB task. They found a smaller AB after OMM than after FAM or a relaxation control condition. Their interpretation of this result is that OMM induces a parallel processing style, which allows multiple targets to be selected at once. In contrast, FAM induces a more serial style in which targets are processed one at a time, making conditional T2 accuracy worse than OMM and relaxation at shorter lags.

### 5.1.2 Does mindfulness meditation affect the AB in novices?

No study showing a reduction in AB as a result of meditation has been replicated. The easiest claims to test are that the AB is affected after novices undertake a brief period of meditation. Therefore, we began this line of enquiry with a precise replication of Colzato et al. (2015). Theories which use reduced AB as a proxy for the regulatory effects of mindfulness on attention rely exclusively on studies with experienced meditators. If Colzato et al. (2015) could be replicated, this would strengthen accounts that mindfulness mediates attention regulation, by confirming that a brief period of OMM reduces the AB in novices, as well as experienced meditators.

## 5.2 Experiment 6: Do brief FAM and OMM affect the attentional blink?

We ran two direct replications of Colzato et al. (2015), with a total of 120 participants (40 per group), double the sample size of the original study. We tested the claim that, relative to relaxation, the AB is reduced after brief OMM, and increased after brief FAM. To rule out pleasure or arousal as mediators of any observed effects, we predicted no group differences for these variables.

### 5.2.1 Method

#### Design

This was a mixed design, with group (OMM, FAM, relaxation) as a between participants independent variable, and T2 lag (1, 3, 5, 8) as a within participants independent variable. Dependent variables were conditional T2 accuracy (T2|T1), and T1 accuracy.

## Participants

One hundred and twenty psychology students from the University of Plymouth (mean age = 21.55 years, 90 female), combined from two samples of 60, were included in the study. Forty participants were randomly assigned to each of the three experimental groups. Sample sizes were not calculated using an *a priori* power analysis.

Students volunteered to participate in exchange for course credits. They were asked not to participate if they had ever meditated regularly (>=10 sessions), or were concerned that meditation might have a negative effect on their mental health. Five participants were excluded, one who failed to achieve 50% accuracy on the AB task, and four due to equipment failure. Additional participants were recruited to balance the number in each experimental group.

## Materials and Measures

**Affect Grid** The affect grid (Russel et al., 1989) simultaneously measures affect and arousal on a 9 x 9 grid. An online version of the affect grid was developed, which allowed participants to click in the cell to rate their current levels of pleasure and arousal (-4 to +4)[1]. A short instruction sheet explained how to position a cell in the grid.

[1]A demonstration and source code for the affect grid is available at `https://github.com/paulsharpeY/affect-grid`.

**Attentional Blink** The Rapid Serial Visual Presentation (RSVP) task used to induce the AB is shown in Figure 5.1. Participants were instructed to identify and report two target digits (T1 and T2) among 18 distractor letters presented sequentially. Letter stimuli were capitalized and drawn randomly from the alphabet without replacement. Number stimuli were drawn randomly from the set 2–9. Each trial began with a 2000ms fixation cross ("+") followed by a blank interval of 250ms. The 20 stimuli were then displayed sequentially, each item appearing for 70ms followed by a 30ms inter-stimulus interval. To reduce predictability of onset, T1 varied randomly between positions 7–9. T2 appeared directly after T1 (lag 1) or after 2, 4, or 7 intervening letters (lag 3, 5 and 8 respectively). Stimuli were presented at a resolution of 1920 by 1080 pixels on a 22" LCD monitor. The fixation cross and all items were presented centrally, in 60 pixel, black Times New Roman font, on a grey (RGB 128, 128, 128) background. Participants were then prompted to report the numbers they had seen, in either order. The first question read "Which two targets did you see? (press a number key)". When a number was pressed, or after 5000ms if no number was pressed, the prompt was replaced with "Which two targets did you see? (press another number key)". The next trial began when a number was pressed, or after 5000ms had elapsed if no number was pressed.

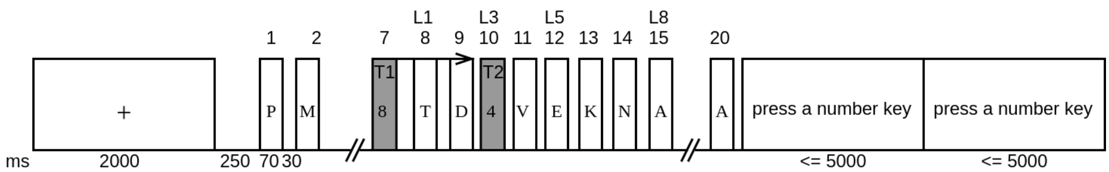


**Figure 5.1:** Rapid Serial Visual Presentation (RSVP) trial. Participants try to detect two target numbers (T1 and T2, shown in grey) amongst 18 distractor letters. Each stimulus appears for 70ms with a 30ms inter-stimulus interval. On each trial, T1 varies between positions 7-9. T2 appears, relative to T1, at lag L1 (T1+1), L3 (T1+3), L5 (T1+5), or L8 (T1+8). In this example, T2 appears at Lag 3. After stimulus 20, participants are asked to recall T1 and T2, in either order

To ensure the instructions were clear, the task began with a single trial where stimuli were presented at a rate three times slower than in the actual task. This was followed by 24 practice trials at full speed. The practice block was repeated until 50% or more responses were correct. Practice was followed by a single experimental block of 144 trials (3 locations of T1 x 4 T2 lags x 12 repetitions). The task lasted for approximately 15 minutes[2]. The tasks were implemented using jsPsych (Leeuw, 2015). Computer task sequencing and data collection were implemented using The Experiment Factory (Sochat, 2018).

**Interventions**

OMM and FAM instructions were translated from Dutch (Colzato et al., 2015) to English by a person fluent in both languages, and recorded in a male voice. Participants in the OMM group were instructed to pay continuous attention to their present moment experience, beginning with the sensations of breathing, before extending this to their thoughts, body sensations and feelings. In the FAM group, participants were instructed to pay attention to the sensations of their breath at their nostrils. The instructions then guided participants through progressively more challenging methods of attending to the breath, beginning by counting and labeling in and out breaths, and ending by simply noticing the sensations themselves. In both meditation conditions, the instructions frequently reminded participants where to direct their attention and awareness, towards the breath (FAM) or the breath and other aspects of their experience (OMM). Participants in the relaxation group were instructed to sit comfortably and relax. They were invited to read magazines covering local events and news, if they found that relaxing. The audio

[2]A demonstration and source code for the AB task is available at https://github.com/paulsharpeY/rsvp-task.

recording for the relaxation participants was silent during the period which contained meditation instructions in the other two groups.

**Procedure**

Experimental sessions were conducted in individual laboratory rooms. Computer tasks ran in a Google Chrome web browser on the Windows 10 operating system. Participants gave informed consent and read the affect grid instructions, before completing the first affect grid. Next, they used headphones to listen to an 18 minute recording of either OMM, FAM, or relaxation instructions. Immediately after the recording ended, participants completed a second affect grid. Next, they completed the AB task, sitting at a viewing distance of approximately 50cm from the computer screen. They were instructed to make their best guess if they were unsure of the identity of the targets. Participants who failed to achieve 50% accuracy after three practice blocks were immediately debriefed and informed that the experiment was complete. Finally, participants completed a third affect grid and were debriefed to end the experiment.

### 5.2.2 Data Analysis and Statistics

Previous literature has operationalised the AB in different ways. In the analysis presented here, we follow the approach taken by Raymond et al. (1995), in which AB magnitude is defined as the difference in mean T2|T1 accuracy between lags where accuracy is typically low, and lags where accuracy is near its maximum. We selected lags 3 and 8 because the AB effect is generally observed when T2 is presented between 200-500ms after detection of T1. Lag 3 is 300ms following T1 presentation and should, therefore, be subject to AB. In contrast, lag 8 is 800ms following T1 presentation, and so this condition serves as a control for the AB effect observed at lag 3. It should be noted that Colzato et al. (2015) analysed the T2|T1 accuracy across all four lags (1, 3, 5 and 8). In doing this, they defined AB as a difference in accuracy between any of the four conditions. We prefer the approach taken by Raymond et al. (1995) because it focuses on the AB effect. That is, it avoids comparisons of conditions (e.g. lag 1 vs. lag 8) which would be not be expected to produce an AB. Results of the Colzato et al. (2015) analysis (all lags) were the same as with the comparison of lags 3 and 8, and are reported in Appendix D. Finally, as well as analysing the level of AB, and in order to assess participants' simple ability to detect targets, we examined participants' accuracy in detecting T1.

To maximise statistical power, we present results for the combined sample of 120 participants. We justify this on the basis that ANOVAs showed no experiment (2) x group (3) interactions for either AB magnitude ($F(1, 2) = 0.14$, $p = .872$, $BF = 0.15$), or T1 accuracy ($F(1, 2) = 0.29$, $p = .746$, $BF = 0.08$).

### 5.2.3 Results

A one-way ANOVA showed that mean age (FAM = 20.57, OMM = 21.4, control = 22.68) did not differ between groups, $F(2, 117) = 1.19$, $p = .309$, $BF = 0.21$. Figure 5.2 suggests that an AB effect was present in all groups. Conditional T2 accuracy (T2|T1) was lower at lag 3, than at lags 1, 5 and 8 (only lag 3 is within the critical range of 200-500ms post T1 presentation). Importantly, the three slopes between lag 3 and lag 8 are almost parallel. This provides a strong indication that there were no group differences in AB magnitude (see Table 5.1 for the mean difference between lags 3 and 8 across groups). However, overall T2|T1 accuracy was higher in the meditation groups (FAM and OMM) than in the control group. Table 5.1, in addition to confirming the data pattern shown in Figure 5.2, also suggests that T1 accuracy was greater in the FAM and OMM groups than in the relaxation group.

### Attentional Blink

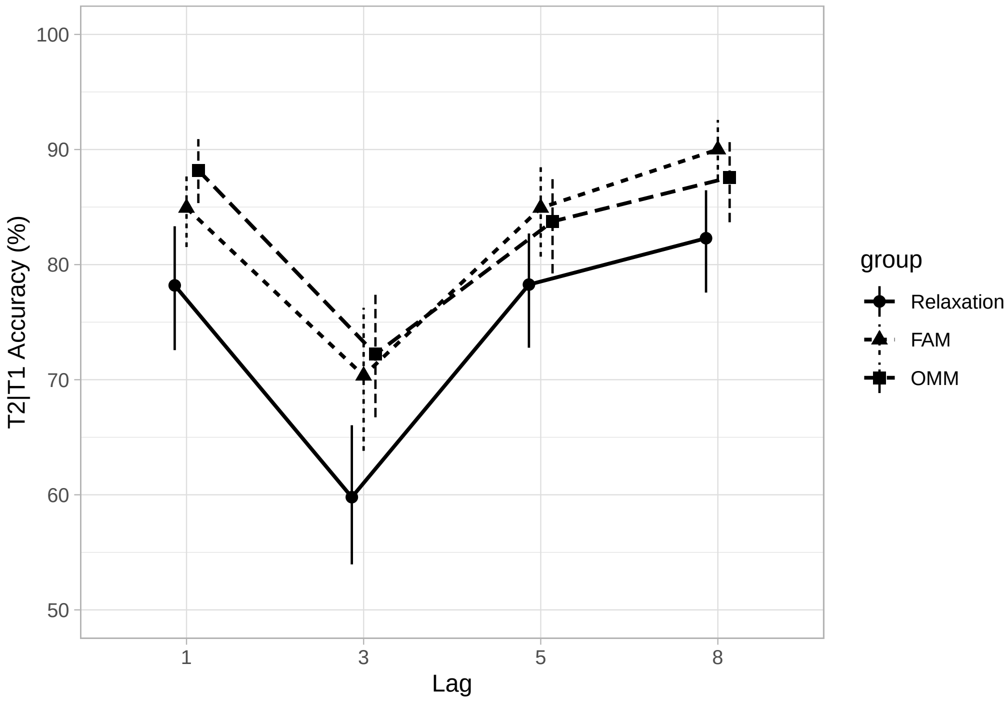


**Figure 5.2:** Mean T2|T1 accuracy for FAM, OMM and relaxation (control) at lags 1, 3, 5 and 8. Vertical lines are bootstrapped 95% confidence intervals.

**Table 5.1:** Accuracy (%) for T1, T2|T1, and AB magnitude

| | | | T2\|T1 | | | | | |
|---|---|---|---|---|---|---|---|---|
| | T1 | | Lag 3 | | Lag 8 | | AB | |
| Group | M | SD | M | SD | M | SD | M | SD |
| Relaxation | 89.90 | 10.40 | 59.79 | 18.60 | 82.29 | 14.33 | 22.50 | 14.29 |
| FAM | 94.20 | 3.98 | 70.35 | 19.77 | 90.00 | 8.82 | 19.65 | 16.68 |
| OMM | 94.15 | 4.76 | 72.22 | 18.37 | 87.57 | 11.36 | 15.35 | 16.37 |

*Note:* M = mean, SD = standard deviation, AB = attentional blink (lag 8 - lag 3)

We tested for an AB effect using a group (3) x lag (2) ANOVA on T2|T1. This showed a main effect of lag ($F$(1.00, 117.00) = 176.26, $p$ < .001, $BF = 1.15 \times 10^{22}$), confirming the presence of the AB. There was also a main effect of group ($F(2.00, 117.00) = 5.83$, $p = .004$, $BF = 5.28$). To conclude that there is a modulation of the AB across groups, requires evidence of a group x lag interaction (MacLean & Arnell, 2012). No group x lag interaction was observed. Hence there was no evidence of an AB difference between FAM, OMM or Relaxation ($F(2.00, 117.00) = 2.07$, $p = .130$, $BF = 0.41$). Table 5.2 shows post-hoc contrasts consistent with this interpretation. Most significantly, they show no evidence of a difference between FAM and OMM in terms of the magnitude of the AB effect. Furthermore, they show no evidence of differences between Relaxation and OMM, or Relaxation and the combined meditation conditions, and evidence of no AB difference between FAM and Relaxation.

**Table 5.2:** t-tests comparing AB magnitude (lag 8 - lag 3) differences between relaxation and meditation.

| Contrast | Mean AB difference (%) | *df* | *t* | *p* | *BF* | *d* | 95% CI |
|---|---|---|---|---|---|---|---|
| Relaxation - FA | 2.9 | 117 | 0.81 | 0.70[a] | 0.31 | 0.18 | [-0.26, 0.62] |
| Relaxation - OM | 7.2 | 117 | 2.02 | 0.11[a] | 1.49 | 0.45 | [0.01, 0.9] |
| FA - OM | 4.3 | 117 | 1.22 | 0.45[a] | 0.42 | 0.27 | [-0.17, 0.72] |
| Relaxation - Meditation | 5.0 | 117 | 1.63 | 0.16[b] | 0.67 | 0.32 | [-0.07, 0.7] |

[a] Tukey adjusted for 3 tests
[b] FDR adjusted for 3 tests

**Target Accuracy**

In addition to the effect of meditation on AB, is the issue as to whether overall target detection differed as a result of meditation. To analyse this, because it is not an examination of AB, we took a slightly different approach and included all trials (all lags) in the analysis. Figure 5.2 shows that T2|T1 accuracy across all lags was greater in both meditation groups than in the relaxation group. This difference was not hypothesized at pre-registration, but was confirmed by a t-test which showed that T2|T1 accuracy was greater in the combined meditation groups (FAM and OMM) than in the relaxation group ($t(62.03) = -3.06$, $p = 0.003$, $BF = 27.48$, $d = -0.65$). Figure 5.3 shows a similar pattern of results for T1 accuracy. A t-test confirmed that T1 accuracy (again including all lags) was also greater in the combined meditation groups (FAM and OMM) than in the relaxation group ($t(45.98) = -2.50$, $p = 0.016$, $BF = 16.68$, $d = -0.61$).

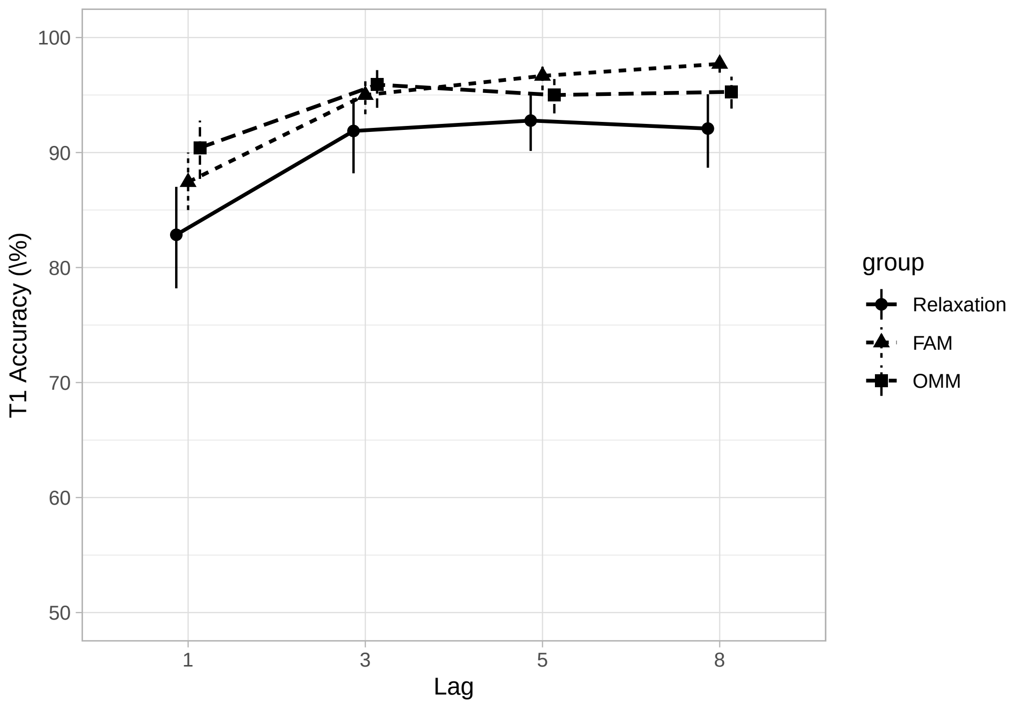


**Figure 5.3:** Mean T1 accuracy for FAM, OMM and relaxation (control) at lags 1, 3, 5 and 8. Vertical lines are bootstrapped 95% confidence intervals.

Figure 5.4 shows the results of the three affect grid measurements. We ran individual group (3) x time (3) ANOVAs for pleasure and arousal. For pleasure, there was a main effect of time ($F(1.76, 205.34) = 69.36$, $p < .001$, $BF = 6.22 \times 10^{21}$), but no main effect of group ($F(2.00, 117.00) = 0.03$, $p = .971$, $BF = 0.05$), or group x time interaction ($F(3.51, 205.34) = 0.85$, $p = .485$, $BF = 0.05$). Because pleasure remained similar in all groups, we can infer that differences in pleasure were not masking evidence of AB differences.

For arousal, there was a main effect of time ($F$(1.93, 226.34) = 88.21, $p$ < .001, $BF$ = $1.61 \times 10^{29}$), but no main effect of group ($F$(2.00, 117.00) = 0.92, $p$ = .402, $BF$ = 0.06). There was some evidence for a group x time interaction ($F$(3.87, 226.34) = 3.08, $p$ = .018, $BF$ = 2.44). Figure 5.4 suggests that this was driven by a larger increase in arousal in the meditation groups than in the control group, between the measurements immediately before (time 2) and after (time 3) the AB task. An ANOVA comparing arousal at time 2 and time 3 found strong evidence against a main effect of group ($F$(2.00, 117.00) = 0.35, $p$ = .706, $BF$ = 0.06). There was decisive evidence that arousal increased between time 2 and time 3 ($F$(1.00, 117.00) = 158.72, $p$ < .001, $BF$ = $1.96 \times 10^{26}$). The group x time interaction provided substantial evidence that arousal increased to a greater extent in the meditation groups than in the control group ($F$(2.00, 117.00) = 4.44, $p$ = .014, $BF$ = 6.13).

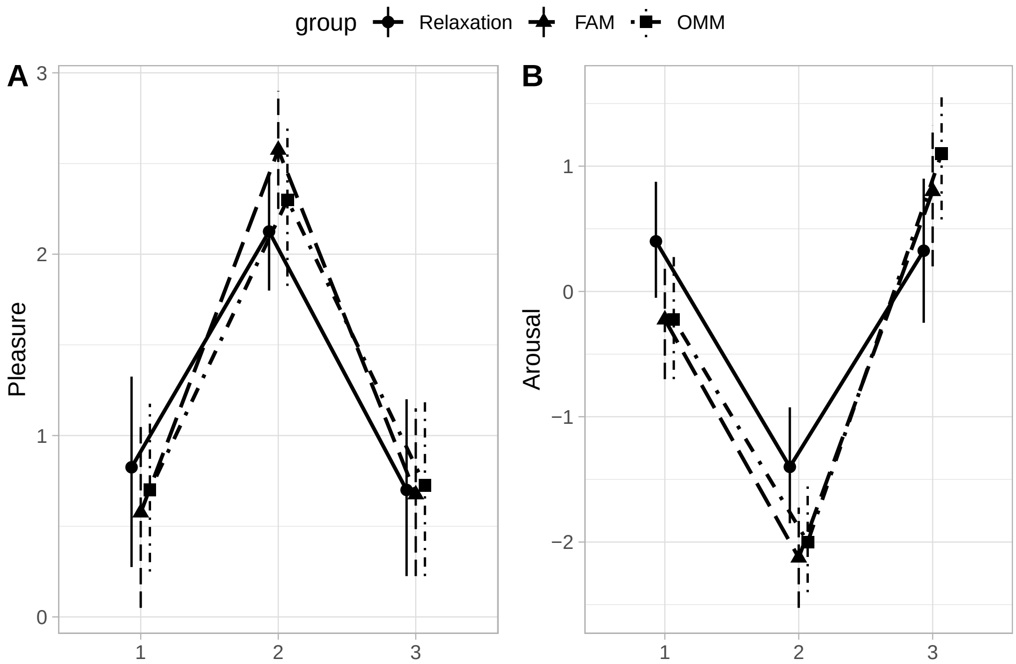


**Figure 5.4:** Affect grid pleasure and arousal ratings (-4 to +4) for FAM, OMM and Relaxation groups. Time 1=before group intervention, Time 2=after group intervention, Time 3=after AB

### 5.2.4 Discussion

Colzato et al. (2015) reported that brief OMM resulted in a diminished AB effect relative to both FAM and a relaxation control. This pattern was not observed in the data presented here. In our exact, procedural replication of Colzato et al. (2015), we found no effect of meditation on the AB. Furthermore, post-hoc comparisons found no evidence for AB differences between FAM and OMM, or between either of the meditation and control conditions. This failure to replicate, with twice as many participants as the original study, calls into question conclusions in Colzato et al. (2015). Our current data, therefore, weaken the argument that, in the case of non-meditators, brief meditations induce a more parallel processing mode after OMM, and a more serial processing mode after FAM. This result contrasts with the reduced AB found in experienced meditators after brief (Leeuwen et al., 2009) and extensive (Slagter et al., 2007) periods of meditation. The absence of AB differences in novices, suggests that experience, longer meditations, or both are necessary for attentional allocation which allows both T1 and T2 to be detected at short lags.

There are clear reasons why we might not expect the AB to differ between FAM and OMM in this experiment. We were careful to precisely replicate the meditation instructions described by Colzato et al. (2015). The OMM instructions began with approximately 12 minutes of FAM, which is a common approach to initially calm the mind (Lutz et al., 2008, Box 2). However, this meant that the the OMM component only lasted for approximately six minutes, which may have been insufficient to induce a mental state distinct from the 18-minute FAM intervention. Furthermore, Isbel & Summers (2017) point out that prolonged practice is required to transition from FAM to

OMM, making it unlikely that this was consistently achieved by the novice meditators in this sample. More fundamentally, according to their cognitive model we would not expect FAM and OMM to have different effects on attention, because "[it] is only the object taken in the present moment that changes, not the cognitive processes underlying that attention" (Isbel & Summers, 2017, p. 87).

Improved target accuracy in the FAM and OMM groups could mean that meditation improved executive attention. Target accuracy is distinct from the AB effect. There were medium sized (*d*s > .6) accuracy improvements for both T1 and T2|T1 in the meditation groups, relative to the relaxation group. It is interesting to note here that although arousal levels were intermediate in all groups between Time 2 and Time 3, arousal increased to a greater extent in the meditation groups between these two time points (before and after the AB task). Perhaps a brief period of meditation increases the availability of attentional resources, the ability to flexibly allocate attention, or both. In other words, it improves executive attention. If this were the case, the meditators may have remained more engaged with the AB task, increasing both accuracy, and arousal. This interpretation is speculative, but consistent with accounts of mindfulness as a metacognitive process which mediates attention allocation (Isbel & Summers, 2017). It is also consistent with the theory that AB performance is affected by cognitive processes which rely on a common, limited-capacity attentional resource (Dux & Marois, 2009).

An alternative explanation for the differences in task accuracy, is that the control task may have induced a mental state which reduced performance. Relaxing with the option of reading magazines could induce a relatively wide range of mental states, making it unsuitable as control condition for meditation. Reading is commonly used as a task which induces mind wandering (Smallwood & Schooler, 2006), a process which might *deplete* attentional resources. Other forms of listening tasks are more commonly used to control for listening to a guided meditation. For example, Zeidan et al. (2015) had participants listen to an audio-book, and Schofield et al. (2015) had participants listen to a factual description of raisins, to control for a mindfulness exercise involving raisins. The argument that meditation reduces depletion of attention would be strengthened if the same result was found using listening, rather than reading, as a control condition.

Although our sample size was double that of Colzato et al. (2015), this was still too small to decisively establish whether or not OMM reduces the AB to a greater extent than FAM. Colzato et al. (2015) report $F = 3.07$ for the interaction between group and lag. This is equivalent to a medium sized reduction in AB in OMM relative to FAM ($d = 0.54$). We found a smaller reduction AB in OMM relative to FAM ($d =$ ). A decisive replication requires an estimate of the true size of AB effects, so adequate sample sizes are used to test for differences. Consequently, the next chapter reviews mindfulness studies which use the AB as an outcome measure, and estimates associated effect sizes. This follows the format of the review and meta-analysis of ANT studies in Chapter 4. A broader discussion of AB effects is postponed until the final chapter.

# 6

# The effects of meditation on the attentional blink: a systematic review and meta-analysis

## Contents

## 6.1 Introduction

Pooling the effects from Colzato et al. (2015) and Experiment 6 indicates that $d$ = 0.36 is a more realistic estimate of AB differences between FAM and OMM. This is two thirds of the magnitude ($d$ = 0.54) reported by Colzato et al. (2015). This means that other AB studies which recruited less than 40 participants may also have been underpowered. It also means that the literature may suffer from publication bias. If statistical significance is used as a publication criterion, and studies assume larger effect sizes that the true effect, then positive findings will be over-represented in the literature. This would skew the impression of the extent to which meditation reduces the AB.

Significant differences with small samples, small effect sizes and noisy data can overestimate the magnitude and sign (direction) of effects. Gelman & Carlin (2014) refer to these as 'Type M' and 'Type S' errors respectively. Type S errors are especially important as they might lead us to conclude, incorrectly, that meditation has negative effects on attention. A more reliable pooled estimate of AB effects would ensure that future studies are adequately powered, and reduce uncertainty over the magnitude and direction of effects. A meta-analysis would estimate the effect at the highest possible precision, and allow a test for publication bias in this field.

Chapter 5 provided preliminary evidence that the AB may more sensitive to the effects of meditation on executive than the ANT. The ANT and AB are quite different tasks. The ANT involves spatially orienting attention in order to resolve perceptual conflict, whereas the AB fixes attention on a single location and requires inhibition of distractors in order to recall targets. Even this superficial analysis suggests that the two tasks are likely to measure slightly different aspects of (executive) attention. Consequently, effect size estimates should be made using data from a single task.

General estimates of the effects mindfulness on attention exist (Lao et al., 2016; Sedlmeier et al., 2012; Verhaeghen, 2020), but these include only a small number of AB studies, and are combined with effects from many different experimental tasks. There are no meta-analytic estimates for the effects of meditation on the AB. A pure estimate of AB effects would support hypotheses which relate to ways in which meditation affects the allocation of attention over time, the aspect of attention which is most associated with the AB. To address this gap in the literature, we carried out a systematic review and meta-analysis of all mindfulness studies which have used the AB as an outcome measure.

## 6.2 Methods

### 6.2.1 Protocol and registration

A pre-registered review protocol can be accessed at `https://osf.io/syhm8/`.

### 6.2.2 Eligibility criteria

This meta-analysis focuses on the effects of mindfulness training on the AB in healthy populations. Studies of healthy adult populations (age 18 and above) which reported the AB in terms of accuracy as an outcome measure were eligible for inclusion. Studies involving tai chi, or hatha yoga were excluded, unless it was clear that mindfulness was central to the training. Studies were drawn from English language peer reviewed journal articles, published up to and including 2019. Articles covering patient populations, or those which provided exclusively EEG data were excluded.

Due to the small number of studies in this area, both experimental, prospective cohort, and retrospective case-control designs were included. In the case of cohort studies, comparisons are between groups after the mindfulness intervention. For case-control studies, AB comparisons are between groups with mindfulness experience and control groups.

### 6.2.3 Information sources and search

The research was conducted in accordance with the Preferred Reporting Items for Systematic Reviews and Meta-analyses (PRISMA, Moher et al., 2009) guidelines. The term *"attentional blink" AND (meditat* OR mindfulness)* was used to search CINAHL (CINAHL Plus with Full Text, AMED, MEDLINE), Cochrane Library, PsycInfo, PubMed, Scopus, and Web of Science databases in October 2019. Full text search options were selected where available.

### 6.2.4 Study selection, data extraction and synthesis

The study selection process is illustrated in Appendix E. For each study meeting the inclusion criteria, a single outcome was chosen which compared a difference in AB magnitude to test the primary hypothesis. These are summarised in the final column of Table 6.1. AB magnitudes were included in the meta-analysis irrespective of whether the original researchers interpreted the difference as statistically significant.

The AB was operationalised as the difference between the lags nearest 300ms and 800ms, because T2|T1 accuracy is typically largest at lags around 200-300ms (Slagter & Georgopoulou, 2013), and absent beyond around 500ms. These are referred to as short and long lags respectively. This approach was taken because the RSVP task varied between studies in terms of stimulus duration, ISI, number of T2 lags, and the method of calculating the AB. Table 6.1 shows the short and long lags closest to these values for each study.

### 6.2.5 Subgroup analysis

Studies were analysed according to whether they were randomised or non-randomised. Meta-analytic effects were pooled according to whether participants were experienced or novice meditators.

**Table 6.1:** Mindfulness studies with attentional blink as outcome measure.

| Publication | Type | Age (mean/range) | Meditation experience | Treatment (*n*) | Treatment duration | Control (*n*) | Control duration | Minutes after meditation | | Lag (ms) | | AB Trials | Tri-als | Lags | Authors' interpretation of outcome |
|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|---|
| | | | | | | | | RSVP 1 | RSVP 2 | Short | Long | | | | |
| **Sharpe (2021, Experiment 6)** | **RCT** | **21.55** | **0** | **FAM (40), OMM (40)** | **18 minutes** | **Relaxation (40)** | **18 minutes** | **NA** | **1 minute (approx.)** | **300** | **800** | **36** | **144** | **1,3,5,8** | **No AB differences between FAM, OMM and relaxation. Accuracy on T1 and T2\|T1 greater in combined meditation conditions.** |
| Polsinelli et al. (2020) | RCT | 75.7 | 0 in previous 5 years | FAM (26) | 6 weeks * 22m/day | Mind-wandering (22) | 6 weeks * 22m/day | NA | <= 1 week | 370 | 706 | NI | NI | 2,4,8 | Smaller AB in FAM for lags at 706ms and 370ms, but not for lags at 706ms and 202ms. |
| Fabio et al. (2018) | CT | 45.31 | > 6 years | Meditators (18) | NA | Age, gender, education matched (18) | NA | NA | NI | 300 | 700 | 10 | 70 | 1–7 | Smaller AB in meditators with > 6 years of experience, and average of 14 hours practice per week. |
| **Colzato et al. (2015)** | **RCT** | **20.07** | **0** | **FAM (20), OMM (20)** | **17 minutes** | **Relaxation (20)** | **17 minutes** | **NA** | **1 minute (approx.)** | **300** | **800** | **36** | **144** | **1,3,5,8** | **Smaller AB in OMM than FAM. No difference between FAM and relaxation.** |
| **van Vugt and Slagter (2014)** | **CT** | **18-64** | **m=6041 hours; range=786-31,937 hours** | **FAM (30), OMM (30)** | **5.5 minutes** | **NA (within-subjects)** | **5.5 minutes** | **NA** | **0 minutes** | **336** | **672** | **60** | **180** | **2,4,8** | **No differences between OMM and FAM in full sample. Smaller AB in OMM than FAM in more experienced meditators (median split).** |
| **Braboszcz et al. (2013)** | **NCT** | **37.5** | **1-6.8 years** | **Isha yoga (82)** | **3 months: 6 weeks * 4 hours/day + 6 weeks * 2–3 hours/day** | **NA (no control group)** | **NA** | **NI** | **NI** | **332** | **664** | **40** | **80** | **3,7** | **Post-treatment improvements on T1, T2\|T1 short, and T2\|T1 long. Largest improvement for T2\|T1 short.** |
| **May et al. (2011, Experiment 1)** | **CT** | **22.64** | **0** | **LKM (13)** | **8 weeks (mean = 485.15 minutes)** | **No meditation (14)** | **0 minutes** | **NA** | **NI** | **300** | **800** | **52** | **104** | **3,8** | **No significant group differences.** |
| **May et al. (2011, Experiment 2)** | **CT** | **22.64** | **8 weeks** | **LKM (13)** | **10 minutes** | **No meditation (14)** | **0 minutes** | **NA** | **0 minutes** | **300** | **800** | **52** | **104** | **3,8** | **Smaller AB after 10 minutes LKM following 8-weeks LKM training, than for non-meditating controls.** |
| Burgard and May (2010) | RCT | 21.65 | 0 | LKM (19) | 21.5 minutes | Relaxation (20) | 19.8 minutes | NA | 0 minutes | 300 | 800 | 65 | 130 | 3,8 | No significant difference. |
| van Leeuwen et al. (2009) | CT | 49.9; 24.3 | 1-29 years | Meditators (17) | NA | Age, sex, education matched (17), young (17) | NA | NI | NA | 300 | 700 | 40 | 280 | 1–7 | AB less in meditators than age matched controls. No differences between meditators and young controls. |
| **Slagter et al. (2007)** | **CT** | **20-64** | **NI** | **Vipassana, metta (17)** | **3 months, 10-12 hours/day** | **Age, education matched Vipassana trainees (23)** | **7 days * 20m/day at pre-test, 14 days * 20m/day at post-test** | **NI** | **NI** | **336** | **672** | **192,72** | **408** | **4,8** | **AB less in meditators than controls.** |

*Note:*
**Bold studies** were included in meta-analysis. Type: CT = Controlled trial; RCT = Randomised controlled trial; NCT = Non-controlled trial. Treatment/control: n = sample size; FAM = Focused Attention Meditation; OMM = Open Monitoring Meditation; LKM = Loving Kindness Meditation. RSVP 1 = Pre-treatment RSVP; RSVP 2 = Post-treatment RSVP; ET = End of treatment. Lag: Short = Lag closest to 300ms; Long = Lag closest to 800ms. AB Trials: RSVP trials available for AB calculation (Long - Short). Trials: RSVP trials per participant. Lags: T2 presentations relative to T1. Outcome: T1 = T1 accuracy; T2|T1 = conditional T2 accuracy. NA = Not applicable. NI = No information.

### 6.2.6 Risk of bias within studies

Risk of bias was assessed using two tools. The individually-randomized, parallel-group trials were assess using the RoB 2 tool (Sterne et al., 2019). The non-randomised studies were assessed using the ROBINS-I tool (Sterne et al., 2016)

### 6.2.7 Meta-analyses

Bayesian meta-analyses were conducted on AB outcomes for randomised and non-randomised studies. Effects were pooled for experienced and novice subgroups. A pooled standardised mean difference (SMD) was estimated using a model which included study SMD as a random effect. Models were built using `brms` (Bürkner, 2017) with a normal (0,1) prior for the pooled effect size, and a half-Cauchy (0,.3) prior for the between-study standard deviation.[1]. A 95% highest-density continuous interval (HDCI) was calculated for each study and the pooled effects. Requests were made to the original investigators where data required for calculating effect sizes were unavailable in the published article.

## 6.3 Results

### 6.3.1 Search results, and study selection

Our initial database search produced 219 results. Three articles were added to these results; one from a saved search using the same terms, and two which didn't match the search terms, but were known to the authors. From the 222 results, 33 duplicates were removed and 172 items were excluded during screening at the title and abstract level. Six of the remaining 17 items were excluded during full text screening. One was a

[1]Appendix F lists the formulas used in the meta-analysis.

review article, two did not use the AB as an outcome measure, one used an emotional variant of the AB, one was a conference abstract with no published results, and one (Slagter et al., 2009) contained only EEG results for behavioural data reported in Slagter et al. (2007). Table 6.1 summarises the characteristics of the included studies.

Seven of the 11 studies were included in the meta-analysis. Four articles (Burgard & May, 2010; Fabio & Towey, 2018; Leeuwen et al., 2009; Polsinelli et al., 2020) were excluded because AB effect sizes could not be calculated from the available data.

### 6.3.2 Results of individual studies

Table 6.2 summarises study hypotheses and outcomes for the AB (the difference in T2|T1 accuracy at long and short lags). There were group differences on the AB in 9 of the 11 studies. The results of one study (Polsinelli et al., 2020) could not be interpreted because the AB was calculated as a difference in proportional accuracy (average T2 accuracy / average T1 accuracy) at short and long lags, rather than a difference in conditional T2 accuracy (T2|T1). Leeuwen et al. (2009) and Fabio & Towey (2018) calculated the AB using seven lags, beginning at 100ms, and increasing at 100ms intervals. Data was unavailable for these studies, so it is unclear whether a difference between lags at 300ms and 700ms would produce the same results.

**Table 6.2:** Effects of meditation on AB differences.

| Study | Mean AB | | AB difference | Outcome | Hypothesis supported |
|---|---|---|---|---|---|
| | Group 1 | Group 2 | | | |
| **Novice meditators** | | | | | |
| Burgard and May (2010) | NA | NA | NA | No group (Metta, relaxation) x Lag (3, 8) interaction | N |
| **Colzato et al. (2015)** | **NA** | **NA** | **NA** | **OMM < FAM** | **Y** |
| **May et al. (2011, Experiment 1)** | **2.3** | **4.1** | **-1.8** | **Group 2 (no meditation) < Group 1 (8 weeks loving-kindness meditation training)** | **N** |
| **May et al. (2011, Experiment 2)** | **21.8** | **10.5** | **11.3** | **Group 1 (non meditators, no meditation) > Group 2 (trainees, 10 minutes loving-kindness meditation)** | **Y** |
| Polsinelli et al. (2020)* | NA | NA | NA | Meditators < controls | Y |
| **Sharpe (2021, Experiment 6)†** | **22.5** | **17.5** | **5.0** | **Group 1 (18 minutes relaxation) > Group 2 (18 minutes FAM or OMM)** | **Y** |
| **Experienced meditators** | | | | | |
| **Braboszcz et al. (2013)** | **6.7** | **4.5** | **2.2** | **Group 2 (3 months Isha yoga) < Group 1 (baseline)** | **Y** |
| Fabio et al. (2018) | NA | NA | NA | Meditators < controls | Y |
| **Slagter et al. (2007)** | **10.5** | **2.5** | **8.0** | **Group 1 (experienced, 3 months Vipassana) < Group 2 (trainees, 2 weeks Vipassana)** | **Y** |
| van Leeuwen et al. (2009) | NA | NA | NA | Meditators < controls | Y |
| **van Vugt and Slagter (2014)** | **30.4** | **22.9** | **7.5** | **Group 2 (OMM) < Group 1 (FAM)** | **Y** |

*Note:*
**Bold** items included in meta-analysis. AB difference = % accuracy. NA = No data available.
* Non-standard AB calculation.
† Numeric (not statistical) difference.

### 6.3.3 Risk of bias within studies

#### Randomised studies

Figure 6.1 summarises the risk of bias assessments for the randomized studies. The traffic-light plots in this and Figure 6.2 were generated using `robvis` (McGuinness, 2019). In Domain 1, there were some concerns over the randomisation process for Sharpe (2021), because the enrolling investigators had knowledge of the forthcoming allocation. There were also some concerns in Domain 1 for Burgard & May (2010) and Colzato et al. (2015), because there was no information regarding the method of randomisation, or whether the allocation sequence was concealed until participants were assigned to interventions. Polsinelli et al. (2020) gave details on the method of randomisation, but there were some concerns over whether the allocation sequence was concealed until assignment to interventions. There were some concerns regarding bias in the reported result (Domain 5) for Burgard & May (2010), Colzato et al. (2015) and Polsinelli et al. (2020), because the analyses carried out were not pre-registered.

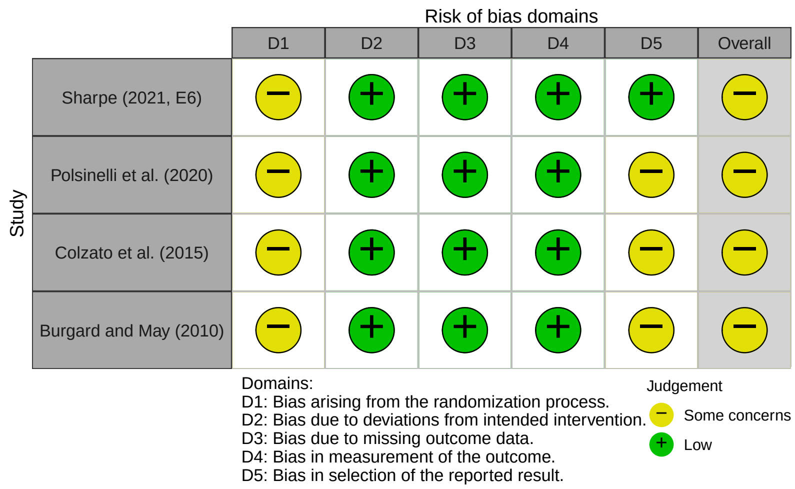


**Figure 6.1:** Risk of Bias in individually-randomised studies, assessed using RoB 2.

### Non-randomised studies

Figure 6.2 summarises risk of bias assessments for the non-randomised studies, and the one within-subjects study (Vugt & Slagter, 2014). In Domain 1, studies which compared long-term meditators with a less experienced control group (Fabio & Towey, 2018; Leeuwen et al., 2009; Slagter et al., 2007) were rated as being at serious risk of confounding, because potentially influential variables were not measured or controlled for. These groups might systematically differ in many ways other than their lifetime meditation duration. For example, higher motivation levels, or an innate ability to focus attention, might also explain their ability to maintain a meditation practice over an extended time period. Two other studies were also rated as being at serious risk of confounding; Braboszcz et al. (2013) did not include a control group, and the LKM group in May et al. (2011), was self-selected and were also the authors of the study.

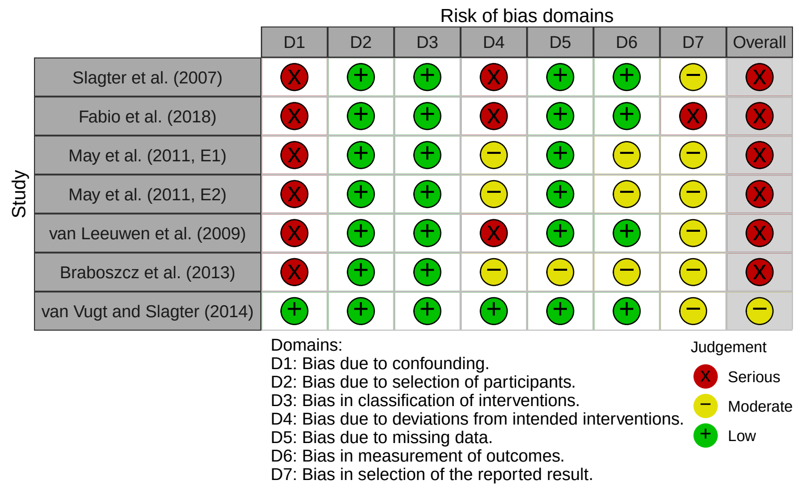


**Figure 6.2:** Risk of bias in non-randomised studies, assessed using ROBINS-I.

Studies which compared experienced and inexperienced meditators (Fabio & Towey, 2018; Leeuwen et al., 2009; Slagter et al., 2007) were rated as being at serious risk of bias due to deviation from intended interventions (Domain 4). Experienced meditators are likely to differ in terms which should be treated as "co-interventions" (Sterne et al., 2016). For example, many Buddhist meditators maintain a number of ethical precepts, potentially leading to cognitive and behavioural differences which should also be controlled for. A complete list of co-interventions would be extensive, difficult to measure and impossible to control for. May et al. (2011) was assessed as being at moderate risk in Domain 4, because there was no active control intervention in either experiment. Braboszcz et al. (2013) was assessed as being at moderate risk in Domain 4, because there was no control condition.

Braboszcz et al. (2013) was assessed as being at moderate risk of bias due to missing data (Domain 5), because data from only 82 of the 103 participants tested at baseline was included in the analyses. Bias in outcome measurement (Domain 6) was assessed as moderate for May et al. (2011), because the investigators were also the participants in the

LKM condition. Braboszcz et al. (2013) was assessed as moderate in Domain 6, because the assessors has knowledge of the intervention received by study participants. Finally, all studies were assessed as being at moderate risk or above for bias in selection of the reported result (Domain 7), as they did not pre-register an operationalised definition of the AB, or an analysis plan.

### 6.3.4 Risk of bias across studies

Figure 6.3 shows a moderate overall risk of bias for the randomized studies. The two domains with clear risk of bias for the randomized studies, were the randomisation process (Domain 1) and the reporting of results (Domain 5). In Domain 1 information regarding randomisation procedures was not specific enough to rank any of the studies as being at low risk of bias. The primary concern for Domain 5 was the lack of pre-registered operationalisation of the AB.


**Figure 6.3:** Risk of bias across randomized studies

Figure 6.4 shows that the serious overall risk of bias in the non-randomized studies was primarily due to potential confounds (Domain 1). In Domain 4, there was a serious risk that 'co-interventions' were not balanced between groups, and moderate risk of bias due to passive (or absent) control conditions. There were minor concerns about bias due to missing data (Domain 5), but concerns in some studies that assessor knowledge could have biased measurement outcomes (Domain 6). The moderate or serious risk in Domain 7 was due to the lack of pre-registered operational definitions of the AB and analysis plans.

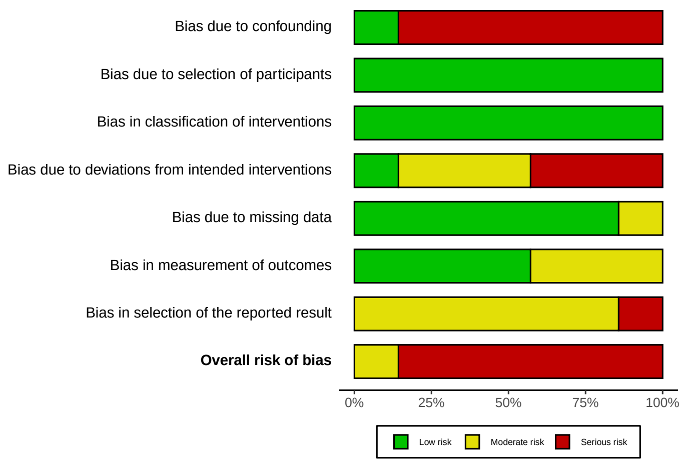


**Figure 6.4:** Risk of bias across non-randomized studies

Finally, Table 6.1 shows that many studies did not report the time between participants' most recent meditation and the RSVP task. It is widely accepted that meditation has both state and trait changes on practitioners (Goleman & Davidson, 2017). In addition to meditation experience, the duration of, and time since, a participant's most recent meditation is likely to affect many outcome measures. As such, these variables should be measured and controlled for[2].

[2]May et al. (2011) addressed this limitation to some extent in Experiment 2, by including an analysis which compared the meditation group with a group from Burgard & May (2010). After this adjustment, the AB in the trained meditator group was 17% lower than in the non-trained meditation group.

### 6.3.5 Meta-analysis

The following meta-analyses estimate the true effect of meditation on the AB, in novices and experienced meditators. Bayesian meta-analysis produces a posterior distribution which summarises the probabilities we assign to a range of effect sizes. The HDCI is derived from this distribution, and shows the range within which we are 95% sure that the true effect lies. Bayesian meta-analyses also allow us to make probability statements about effect sizes, which place reasonable upper and lower bounds on the true effect of meditation on attention. Where effect sizes are not qualified, they refer to the SMD (Hedges' *g*). Traditionally, SMDs of 0.2, 0.5 and 0.8 are are considered small, medium and large respectively (Cohen, 1992). Positive values indicate a reduced AB in the meditation condition. An HDCI which does not overlap zero provides certainty over the direction of an effect.

Figure 6.5 shows a small (*SMD* = 0.33) reduction in the pooled AB for randomised studies involving novices, with a large degree of uncertainty indicated by the wide HDCI. There is a 78% chance that the effect is > 0.2, and a 3% chance that it is > 0.8. There is a also a chance that meditation *increases* the AB, although the probability of this effect being > 0.2 is only 3%.

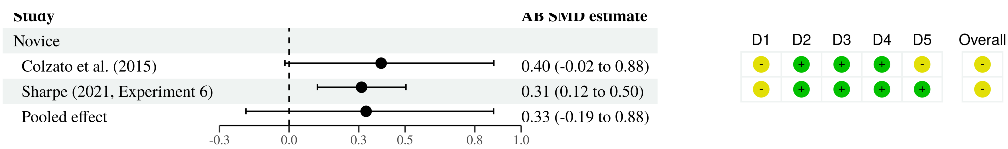


**Figure 6.5:** Meta-analytic effect sizes for AB (randomised studies).

The pooled effect for novices in Figure 6.6, shows a small (*SMD* = 0.29) reduction in AB for the non-randomised studies. The wide HDCI indicates a large degree of uncertainty over this estimate. There is a 62% chance that the benefit of meditation for novices is greater than > 0.2, a 28% chance that it is > 0.5, and a 9% chance that it is > 0.8. Conversely, there is a 11% chance that meditation *increases* the AB with an effect size > 0.2.

For experienced meditators, Figure 6.6 shows that there is a small to medium (*SMD* = 0.44) reduction in AB. The HDCI does not overlap zero, which means we can be certain that this estimate shows a reduced AB in experienced meditators. There is a 30% chance that the effect is > 0.5, and a 1% chance that it is > 0.8.

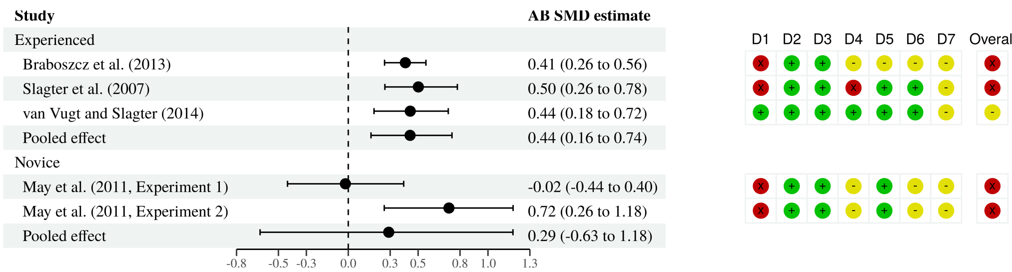


**Figure 6.6:** Meta-analytic effect sizes for AB (non-randomised studies).

The funnel plot in Figure 6.7 is symmetrical about the pooled effect size, so there is no evidence that the AB estimates were affected by publication bias.

## 6.4 Discussion

In novices, the effect of meditation on the AB is small in both randomised and non-randomised studies. There is a good chance (about 70%) that effects are larger than 0.2, but only a small chance (about 10%) that they are greater than 0.8. There is also a small chance (about 10%) that meditation *increases* AB in novices. In experienced meditators there is strong evidence that meditation reduces the AB. Effect sizes are likely to be around 0.4–0.5.

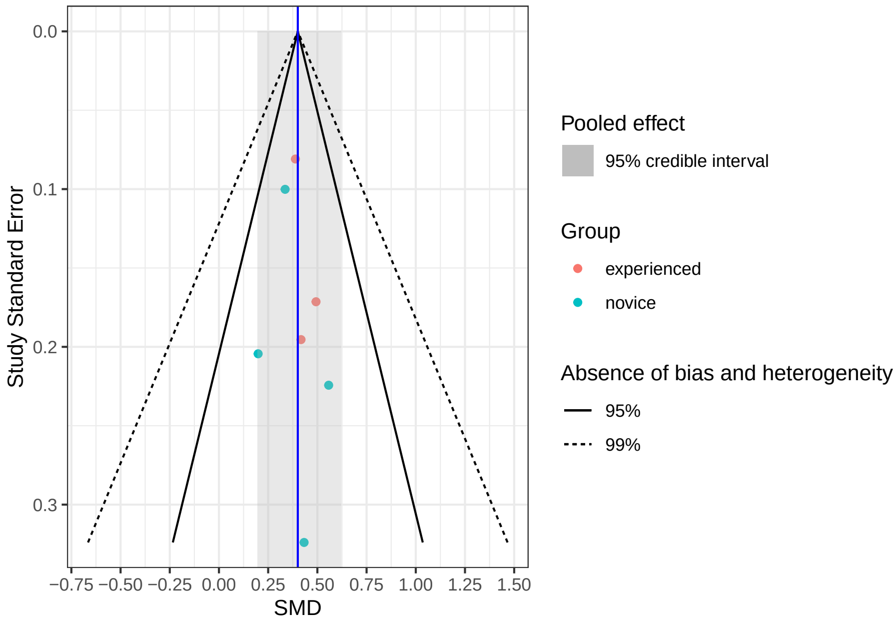


**Figure 6.7:** Funnel plot showing meta-analytic AB SMD against study size/precision.

From these estimates, it is clear that all of the studies reviewed were underpowered. For example, taking the lower end of the estimate for experienced meditators (*SMD* = 0.4), to detect AB differences in a between-subjects design with $\alpha$ = .01, and power of .96, would require 210 participants per group. This number is considerably larger than maximum sample size (n = 82) in the studies reviewed. Adequately powered, pre-registered replications are needed to reliably test the effects of meditation on the AB.

In common with the ANT, the AB meta-analyses were limited to an extent by the low quality of the underlying data. Data was unavailable for over a third of studies, and overall risk of bias ratings showed some concerns over bias in the randomised studies, and serious concerns over bias in the non-randomised studies. Randomisation procedures were at risk of bias in all of the randomised studies. Bias in the results reported was evident in all non-randomized studies, and three quarters of randomized studies. All but one of the non-randomized studies was at risk of bias due to confounding and deviations from the intended interventions. Most studies would have been assessed at much lower risk of bias if they had pre-registered operational definitions of the AB, and analysis plans for testing group differences.

There were methodological limitations in some studies. First, there were inconsistencies in the way that the AB was measured (Table 6.1) and analysed. An agreed operational definition of the AB, and standardised RSVP task would make studies easier to compare. Second, participants' most recent duration, and type of meditation relative to the RSVP task(s) was often not reported (see Table 6.1, columns RSVP 1 and RSVP 2). These variables are important, because the mental states induced by mindfulness meditation are likely to be ephemeral, and may differ according to the type of meditation. In two case-control studies (Fabio & Towey, 2018; Leeuwen et al., 2009) this measurement would have helped to distinguish between trait and state effects of meditation on the AB. Similarly, this would have disentangled trait and state effects in the pre-post comparison of Isha yoga (Braboszcz et al., 2013). Finally, four studies (Burgard & May, 2010; Colzato et al., 2015; Polsinelli et al., 2020; Sharpe, 2021, Experiment 6) would have benefited from a pre-treatment AB measurement to control for baseline differences.

To summarise, there is some evidence that mindfulness meditation can reduce the AB, with larger more reliable effects for experienced meditators than for novices. Pre-registered, well powered studies are needed to validate existing claims that meditation reduces the AB. Methodological limitations should be addressed to improve AB measurements and to distinguish between trait and state effects of mindfulness on the AB.

# 7

# General discussion

**Contents**



## 7.1 Introduction

The main findings from this thesis are summarised below, but they can be quickly described as follows. The claim that mindfulness meditation regulates attention was tested. Chapter 2 found no effects of brief FAM on the ANT in novices (Experiment 1) or long-term meditators (Experiments 2 and 3). Chapter 3 found no effects of four weeks (Experiment 4) or eight weeks (Experiment 5) of FAM training on the ANT. The meta-analyses in Chapter 4 found a small improvement to executive attention scores on the ANT in novices and long-term meditators. Experiment 6 (Chapter 5) found a novel effect on the RSVP task used to measure the AB. This is different from the AB itself, but is another sign that brief mindfulness meditation can regulate attention in novices. The meta-analyses in Chapter 6 found evidence that mindfulness meditation can produce small AB reductions in novices and long-term meditators. Taken together, these findings provide evidence that meditation can have small regulatory effects on executive control.

A secondary aim of this thesis was to test whether mindfulness mediates any effects of meditation on attention regulation. To show such an effect would require evidence of both increases in mindfulness and corresponding improvements in attention regulation. The experiments in Chapters 2 and 3 did not find evidence that mindfulness meditation increased mindfulness. This is absence of evidence for a mediation effect, rather than evidence of absence. Increased mindfulness may not have been detected because the measures were insufficiently sensitive, the mindfulness inductions lacked power, or the effects of meditation had worn off before mindfulness was measured.

These findings should be considered provisional due to limitations in statistical power, the choice of control conditions for mindfulness meditation and training, and risks of bias in the data available for analysis. This chapter discusses how future research could address these limitations, and suggests that mind wandering may be a process which provides an alternative approach to studying the effects of meditation on attention regulation. The chapter ends by considering the roles played by mindfulness, attention regulation and emotion regulation in improving health and wellbeing.

## 7.2 Does mindfulness meditation regulate attention?

In novices, the ANT provides limited evidence that mindfulness meditation regulates attention. Experiment 1 (Chapter 2) found no differences on any ANT scores (executive attention, alerting, orienting), between novices who did 15 minutes of FAM and controls who did a reading and comprehension task. There were also no effects on the ANT after novices completed four or eight weeks of FAM training and practice (Experiments 4 and 5, Chapter 3). More generally, meditation appears to have a small benefit on ANT executive attention scores, but there is little evidence that meditation improves orienting or executive attention in novices. The meta-analysis in Chapter 4 found evidence of small benefits to executive attention, with a SMD likely to be between 0.1–0.2. For alerting, the pooled effect was very small (*SMD* = 0.01). The pooled effect for orienting, was slightly larger (*SMD* = -0.07), but the negative sign indicates that orienting scores were worse in meditation groups. The most consistent finding is that meditation improves executive attention scores on the ANT. However, because this effect is smaller that previously thought, the effects of FAM on executive attention in Experiments 1, 2 and 4 are inconclusive, as they lacked statistical power.

In long-term meditators, the overall pattern of effects on ANT scores was similar to that in novices. Experiments 2 and 3 (Chapter 2) found no differences on the three ANT scores, between long-term meditators who did 15 minutes of FAM just prior to testing and controls who did a reading and comprehension task. The meta-analysis in Chapter 4 found evidence of small benefits to executive attention, with a SMD likely to be between 0.1–0.2. Alerting effects were larger for long-term meditators than for novices. The pooled alerting effect was likely to be between 0.2–0.5, but there was also a chance that meditation has a small, detrimental affect on alerting scores. In common with novices, orienting in long-term meditators worsened in the randomised studies, with a slightly larger pooled effect (*SMD* = -0.18). However, this estimate should be treated with caution, as it was derived from just two studies (Experiments 2 and 3, Chapter 2). In the non-randomised studies, long-term meditators showed a very small improvement ($SMD \simeq 0.03$) in orienting. Overall, the effects of meditation on the ANT in long-term meditators are similar to those in novices, the most consistent finding being a small improvement to executive attention. Because this effect is small, studies with small samples lack the statistical power to conclusively claim that meditation improves executive attention.

Mindfulness meditation can reduce the AB in novices. The meta-analysis in Chapter 6 (four studies) estimated a small AB reduction in novices, with a SMD likely to be between 0.2–0.5. Colzato et al. (2015) found that OMM reduced the AB to a greater extent than FAM. The direction of this effect was the same in Experiment 6 (Chapter 5), but there was no statistical difference between OMM and FAM groups. The small effect size estimate means a further replication is needed to establish whether OMM reduces the AB to a greater extent that FAM. A novel finding in Experiment 6, was that target accuracy was higher in the FAM and OMM groups than the relaxation group on the RSVP task used to detect the AB. This finding suggests that, in novices, OMM and FAM may not have different effects on the way in which attention is allocated, but meditation *in general* may improve attentional performance on some tasks. In Experiment 6 this was a moderate effect ($d = 0.65$). A pooled effect size could be estimated by conducting a meta-analysis of T1 and T2 accuracy where meditation studies which test the AB make this data available. A further replication should be conducted to validate this result, ideally using the meta-analytic effect size to unsure the experiment has adequate power. The other two studies involving novices explored loving-kindness meditation (LKM), which is thought to regulate both attention and emotion. One of these (May et al., 2011, Experiment 2) found a reduced AB in beginners after eight weeks of LKM training relative to untrained controls. However, this effect was only present after the LKM group did a 10 minute meditation immediately prior to the AB measurement (the control group did not meditate).

Mindfulness meditation can reduce the AB in long-term meditators. The meta-analysis in Chapter 6 estimated a small, but reliable effect, with a SMD likely to be between 0.2–0.5. Although these estimates were made from limited data (three studies), they consistently showed a reduced AB in the meditation conditions. One study (Vugt & Slagter, 2014) found that, in long term meditators, OMM reduced the AB to a greater extent than FAM after very brief periods of meditation undertaken just prior to test. It may be that long-term meditators are more proficient than novices at entering differing mental states, using different meditation techniques. Other studies (Braboszcz et al., 2013; Slagter et al., 2007) suggest that intense periods of practice may reduce the AB. These studies did not report the delay between participants most recent meditation and the AB measurement, so it is unclear whether these are enduring effects of intensive meditation, state mindfulness effects from recent meditation, or a combination of both. Two studies (Fabio & Towey, 2018; Leeuwen et al., 2009) found a reduced AB in long-term meditators relative to matched controls, which suggests AB effects in long-term meditators may be enduring. However, neither study reported the time period between most recent meditation and the AB meditation in the meditation group, so it is unclear whether these were trait or state effects.

An argument could be made that the ANT and AB literature both provide evidence that meditation improves executive control. A consistent finding is that resolving conflict (for example between the direction of the central and flanker arrows on the ANT) requires executive control (Fan et al., 2002). This is what the ANT executive attention score measures, and an improvement on this score as a result of meditation is the most consistent, albeit small, result that came out of the ANT studies examined here.

The claim that reduced AB, and higher RSVP target accuracy indicate improved executive control is a little more speculative. In Chapter 5, the argument was made that increased RSVP target accuracy and arousal in the FAM and OMM groups could mean that more attentional resources were available, or that attention was allocated more flexibly after meditation. The reduced AB in long-term meditators is thought to arise from an ability to flexibly allocate attention to both targets (T1 and T2) on the RSVP task. Allocating attentional resources is an aspect of executive control, and there appear to be small improvements to this attentional ability as a result of meditation, in both novices and long-term meditators.

If both tasks measure executive control, then the RSVP task appears to be more sensitive to the effects of mindfulness meditation than the ANT. In novices, the meta-analytic AB effect sizes were approximately twice as large as those for executive attention on the ANT. A well powered experiment is needed to test whether meditation can reduce the AB in novices, or whether, as in Experiment 6, there are only differences in target accuracy on the RSVP task.

In long-term meditators, the AB effect sizes were three times as large as the executive attention effects on the ANT. Therefore the RSVP task may more sensitive than the ANT for detecting the small effects that meditation has on executive control; it is more sensitive to reduced AB in long-term mediators and possibly novices, and also to increased target accuracy in novices.

The ANT may also be unsuitable for meditation research due to ambiguities over the way in which network scores are calculated and interpreted. One highly cited study (Jha et al., 2007) illustrates the problem. Jha et al. (2007) compared long term meditators after a 30 day meditation retreat, with participants combined from two control groups. One group were novices who did a moderate amount of meditation as part of an eight week MBSR course, the others did no mindfulness training. The results for the post-intervention ANT alerting scores are reproduced in Figure 7.1 below[1]. The lower difference score in Figure 7.1 (panel A) was interpreted as evidence that the meditation retreat improved alerting. This contrasts with the traditional interpretation of alerting scores, where a *higher* score would indicate a more "efficient" network (Fan et al., 2002).

These data also violate one of the general assumptions of ANT scores, which is that the subtractions used to calculate ANT scores are expected to produce positive numbers. MacLeod et al. (2010, p. 646) note that "[t]he meaning of these negative [ANT RT] scores is unclear, making interpretation of some individuals' ANT performance difficult". Panel B of Figure 7.1 explains why Jha et al. (2007) interpreted the data in this way. The solid line shows that RTs for a younger subgroup of retreatants were similar for both no cue and double cue trials. The dotted line shows that the control groups were much faster on double cue trials than no cue trials. The squares and circles show that retreatants were similar to control groups on double cue trails, but significantly faster on no cue trials. Jha et al. (2007) argue that this was because retreatants were

[1] *p*-values are for *t*-tests at time 2.

in a more readied state when there was no warning about target onset. This appears to show that meditation may produce attentional states which result in small (even marginally negative) ANT alerting scores which *can* be interpreted[2].

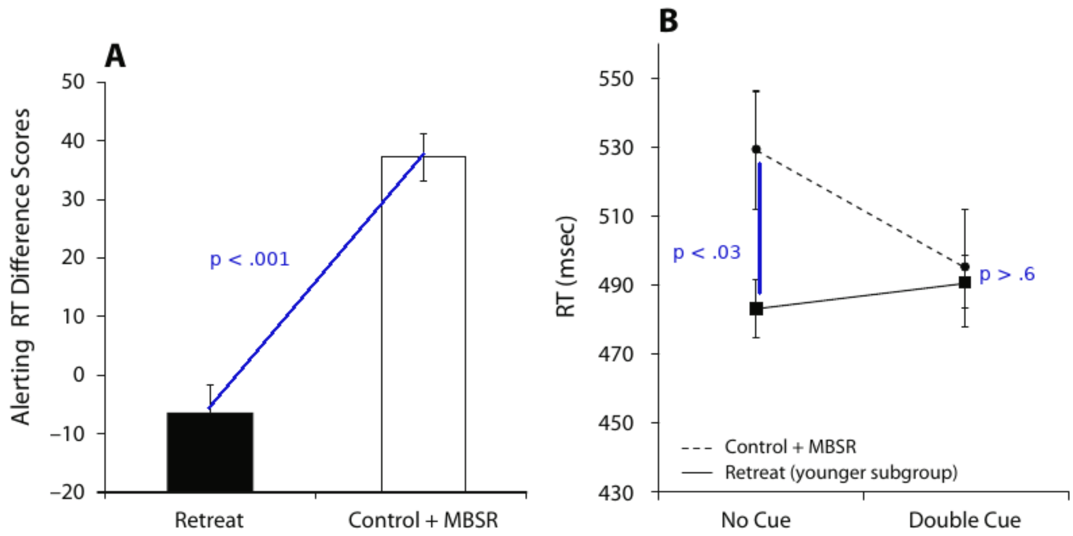


**Figure 7.1:** Post-intervention ANT alerting comparisons (adapted from Jha et al. (2007, Figure 4)).

This alternative interpretation of an ANT alerting score introduces ambiguity over whether better attentional performance is always indicated by a higher alerting score (Fan et al., 2002), or whether situations can arise where a lower alerting score indicates better performance (Figure 7.1, panel A). The second interpretation may be true when long-term meditators meditate for 30 days, but not when novices meditate for 15 minutes. Without some criterion for deciding which interpretation is appropriate, there is a risk of misinterpreting alerting (and possibly orienting) scores. This compounds another issue, which is that the traditional meaning of 'efficiency' in relation to ANT scores is sometimes misinterpreted (see Table 4.4, Chapter 4).

[2]This could mean that there is also a rational interpretation of the negative baseline alerting and orienting scores reported by Quan et al. (2018).

One way to decide whether higher or lower alerting scores imply better performance is to analyse RTs by cue type, rather than using the traditional ANT subtractions (Figure 7.1, panel B). This finer-grained view of the data sheds more light on how the three ANT networks may differ in meditators. In the case of alerting, the retreatants' RTs on no-cue trials indicated that they were more vigilant than controls, because they were faster at the flanker task on trials in which no asterisk prepared them to respond. To avoid ambiguity in the interpretation of ANT scores, where there are group differences on alerting and orienting scores, an analysis by cue type may help to explain how meditation affects the use of visual cues. This means that it may be the subtractions used to calculate ANT *scores* which are inappropriate for meditation research, rather than the ANT *task*.

A different issue relates to the ANT executive attention score, which may be subject to floor effects in RT performance. A surprising finding in Jha et al. (2007) was that there were no differences in executive attention between the retreat group and the control groups. The retreatants would be expected to have higher executive attention scores than controls, given that they were long-term meditators, who had just completed an intense period of meditation. Executive attention (conflict) scores may be highly susceptible to exposure effects (Ishigami & Klein, 2010; Jha et al., 2007). In other words, if people do the ANT twice, their performance tends to improve on the second ANT, regardless of any experimental manipulation. If, as Jha et al. (2007) argue, all groups perform very well on executive attention on a post-intervention ANT, the floor effect may mask differences in executive attention resulting from any experimental manipulations. This could explain why meditation does not always improve executive attention scores, and might also mean that the pooled executive attention effect size is larger than suggested by the meta-analysis in Chapter 4.

In summary, the primary claim tested by this thesis is whether mindfulness meditation regulates attention. The experiments conducted and reviewed in this research operationalised attention regulation as improved performance on the ANT and a reduced AB. With this definition, we can make the qualified claim that mindfulness meditation regulates executive attention. The effect on executive attention is small, and slightly larger in long-term meditators than novices. Compared with the ANT task, the RSVP task appears to be a more sensitive measure of the regulatory effects of meditation on attention. This is because the RSVP task shows larger effect sizes than the ANT when used to measure reductions in AB, and it also shows that meditation can increase T1 and T2 accuracy, with medium-sized effects.

## 7.3 Does mindfulness mediate attention regulation?

A secondary aim of this thesis was to test whether mindfulness mediates attention regulation. Before discussing this, an important distinction needs to be made between the short-term effects of meditation, potentially longer lasting effects of regular meditation training over a number of weeks, and lasting effects of many years of meditation. Measurement issues relating to short-term (state) and long-term (trait) effects of meditation have already been discussed, but other issues can arise if it is unclear whether state or trait mindfulness is of primary interest. For example, in novices we are likely to be interested in the short-term effects induced by meditation. However, if novices undergo a period of training, we might be interested in whether this has different effects on attention depending on whether they meditate immediately prior to measurement.

In long-term meditators, we might be most interested in trait effects, in which case we might ask them to *abstain* from meditating for a known period before measuring attention. The important distinction in the following discussion is that the effects of meditation training may be quite different depending on whether on not participants meditate immediately prior to an attentional measurement.

Testing whether mindfulness mediates attention requires a measurement of mindfulness prior to the primary outcome measure. State mindfulness, measured by the SMS, was not higher after 15 minutes of FAM when compared with a reading and comprehension task, in either novices (Experiment 1, Chapter 2) or long-term meditators (Experiments 2 and 3, Chapter 2). Experiment 4 (Chapter 3) found no differences in state mindfulness, measured by the SMS and a breath counting task, in beginners who had been trained for four weeks and a waitlist control group. There were also no group differences on trait mindfulness (FFMQ) measures taken before and after the beginners' training. In summary, these experiments did not find evidence that brief mindfulness meditation increases mindfulness in novices or long-term meditators, or evidence that mindfulness training increases mindfulness in novices.

Approximately half of the studies reviewed in Chapters 4 and 6 included a mindfulness measure. In all cases this was a self-report measure of trait mindfulness, the limitations of which were discussed in Section 1.5 (Chapter 1). Long-term meditators showed higher trait mindfulness than matched controls on the MAAS (Isbel & Mahar, 2015; Tsai & Chou, 2016, Experiment 1), FMI (Otten et al., 2015; Schötz et al., 2016; Wittmann et al., 2015) and KIMS (Fabio & Towey, 2018)[3]. This could be interpreted

[3]Fabio & Towey (2018) only found the KIMS 'observe' factor to be greater in meditators than controls. Vugt & Slagter (2014) measured trait mindfulness using the FFMQ but did not report the results.

in two opposing ways. On the one hand it could show that long-term meditators are consistently more mindful than controls, or that people who are naturally mindful tend to become meditators. On the other hand, it could show that long-term meditators are more familiar with mindfulness concepts, or have other reasons for self-reporting higher trait mindfulness. For novices who were trained to meditate, trait mindfulness (measured using the FFMQ) was not always higher than control groups. Three studies found improved FFMQ scores after two weeks (Schanche et al., 2019), four weeks (Burger & Lockhart, 2017), and eight weeks (May et al., 2011, Experiment 1)[4] of MT, but one study found no FFMQ differences after six weeks of MT (Polsinelli et al., 2020). On a different trait mindfulness measure, the MASS, meditators were no different to controls after eight days (Ainsworth et al., 2013) or 12 weeks (Tsai & Chou, 2016, Experiment 2) of MT. Overall, this sub-sample of the meditation literature did not provide strong evidence that mindfulness meditation or mindfulness training increases mindfulness.

To claim that mindfulness mediates beneficial effects of meditation on attention regulation requires evidence of both attention regulation, *and* increased mindfulness. The evidence presented so far indicates that mindfulness meditation can make small improvements to the regulation of executive attention, but there is, somewhat surprisingly, less evidence that mindfulness meditation increases mindfulness. On problem is that mindfulness is often not measured, making it unclear whether this, or some other variable mediates attention regulation. A second problem is that mindfulness is not always measured reliably. Most studies evaluated in this thesis used trait mindfulness measures which, it was argued in Chapter 1, are less reliable than state measures.

[4]Trainees in May et al. (2011, Experiment 1) only scored higher than controls on FFMQ observe and describe factors.

There *is* some evidence that mindfulness mediates attention regulation. Long-term meditators have shown greater mindfulness *and* reduced AB relative to matched controls (Fabio & Towey, 2018). On the ANT, long-term meditators consistently showed increased mindfulness, but only one study (Tsai & Chou, 2016, Experiment 1) found a corresponding improvement on the ANT. The inconsistency in ANT improvements could be explained by the greater sensitivity of the AB discussed previously. In contrast with long-term meditators, studies of beginners which found no mindfulness differences, also found no differences on the AB (May et al., 2011, Experiment 1; Polsinelli et al., 2020). Taken together, these data provide some evidence that attention is only regulated when there is a corresponding increase in mindfulness, and that these two conditions may only be observed in long-term meditators.

It may be possible to demonstrate both increased mindfulness and improved attention regulation in novices. Experiment 6 (Chapter 5) found that the RSVP target accuracy was higher in novices after brief meditation. This is not the same as a reduced AB, but perhaps overall RSVP performance is a very sensitive measure of attention regulation in novices – one suitable for the assessment of the mindfulness-attention relationship. Mindfulness was not measured in this experiment because it was a precise replication of Colzato et al. (2015), but the addition of a state mindfulness measure before the RSVP task might demonstrate mediation.

Demonstrating that mindfulness mediates improved ANT performance may be more challenging. None of the experiments in this thesis found effects of meditation or mindfulness training on the ANT (Chapters 2 and 3). This could due to power limitations arising from the small effect sizes associated with the ANT, or because of limitations associated with ANT measurements, both of which were discussed above. Other research involving novices has found inconsistencies between increased trait mindfulness and improved ANT performance. For example, there were ANT differences irrespective of whether mindfulness was (Burger & Lockhart, 2017; Schanche et al., 2019), or was not (Ainsworth et al., 2013; Tsai & Chou, 2016, Experiment 2) greater after mindfulness training. Again, the inconsistencies in ANT outcomes could be due to low statistical power, or measurement issues. The inconsistencies in mindfulness could be due to the use of trait measures. The experiments in this thesis tried to address these issues by using state mindfulness measures – the SMS and breath counting accuracy. However, these measures also showed no improvements in mindfulness after brief FAM (Chapter 2), or four weeks of mindfulness training (Chapter 3). This could mean that SMS and breath counting accuracy only increase after a longer period of meditation training. Because ANT performance was not increased after eight weeks of meditation training (Experiment 5, Chapter 3), a longer training programme may be required to demonstrate both increases in state mindfulness, and improvements to ANT performance.

## 7.4 Future research

This section makes some suggestions for future research. Some of these would address the limitations which have been discussed so far, including issues of low statistical power, improving experimental conditions used to control for mediation, and a number of more general methodological issues that were identified when reviewing the ANT and AB literature in Chapters 4 and 6. Future research might focus on mind wandering as an alternative approach to studying the effects of meditation on attention regulation.

### 7.4.1 Power

Claims that meditation improves performance on attentional tasks should be considered as provisional unless they have sufficient statistical power to detect group differences. All of the experiments conducted in this thesis were underpowered to detect differences on the ANT and AB, as were all of the studies reviewed in Chapters 4 and 6. Power calculations using effect size estimates from the meta-analyses, indicated that much larger samples are needed than have been used in previous ANT and AB studies. To detect differences, a well powered, between participants design would require almost 1,500 participants per group for the ANT, and 135 participants per group for the AB. Samples in the ANT studies were approximately 50 times too small to detect effects. Similarly, the largest sample in the AB studies (Braboszcz et al., 2013) was still only half the size needed to detect an effect. Future research using the AB or ANT should calculate sample sizes using the effect size estimates from the meta-analyses in Chapters 4 and 6.

Power calculations are also required to ensure that samples are large enough to detect effects on mindfulness measurements. Statistical power in Experiments 1–4 was too low to test for differences in state mindfulness. The SMS difference between MT and waitlist groups in Tanay & Bernstein (2013, Study 3) was equivalent to $d = 0.62$. This means that 88 participants per group would be required to detect SMS differences in a between-subjects design with $\alpha = .01$, and power of .96. For breath counting accuracy, Levinson et al. (2014, Study 4) found a difference between four weeks of breath counting training and control groups equivalent to $d = 0.52$. This means that 125 participants per group would be needed to detect breath counting task accuracy differences in a between-subjects design with $\alpha = .01$ and power=.96.

Studies need sufficient power if they aim to show that differences in mindfulness mediate other outcome measures. As 135 participants are required to detect between-subjects AB differences, a study with 135 participants per groups should have sufficient power to detect differences in both breath counting accuracy, and the AB. As a behavioural measure, breath counting accuracy might be more reliable than the (self-report) SMS. However, the SMS may be a more suitable measure if the duration of an experiment is important, because it is quicker to administer. A well powered study is also needed to measure correlations between the SMS and breath counting accuracy, so that the two measures can be calibrated.

Additional, well-powered replications are needed to establish whether or not FAM and OMM have different effects on the AB. In novices, Colzato et al. (2015) found that OMM reduced the AB to a greater extent than FAM. This difference was not replicated in Experiment 6 (Chapter 5). In long-term meditators, OMM has been shown to reduce the AB to a greater extent than FAM when participants complete short periods of meditation before being tested (Vugt & Slagter, 2014). This is a sign that meditation experience is required for OMM and FAM to quickly induce differing mental states. However, all of these studies were underpowered. The small meta-analytic effect sizes that were estimated for the AB in Chapter 6 indicate that much larger samples would be required to substantiate these claims.

### 7.4.2 Control conditions

This thesis identified some challenges in selecting a control activity which is well matched to meditation. Control activities should be chosen which isolate the mental activity which is specific to meditation. For example, in a typical FAM, participants would begin by closing their eyes and adopting a comfortably sitting posture. They would then attend to the sensations of breathing, and return attention to these sensations whenever they noticed their attention was oriented elsewhere. An assumption is that it is the monitoring and reorienting of attention towards the breath which regulates attention in this type of meditation, so a control condition should be matched for everything except this. To some extent this can be done by having two groups sit comfortably, in silence, and instructing only one group to meditate. However, control conditions might also contain instructions which induce a mental state which contrasts with the state induced by following meditation instructions.

Actively controlling for meditation requires careful consideration of the mental states which might be induced by alternative activities. Experiments 1–3 (Chapter 2) used a reading and comprehension control condition, and Experiment 6 (Chapter 5) used a relaxation and reading control condition. Meditation is a passive mental activity, so reading and comprehension may be unsuitable as control conditions because they are active tasks involving complex cognitive processes. A better match for mindfulness of breathing could be a "sham meditation", such as a deep breathing task (Zeidan et al., 2015).

Special considerations are needed if mindfulness is induced using guided meditation. Guided meditation, instructions should be standardised to ensure that FAM and OMM are comparable across studies (Isbel & Summers, 2017). To control for the speaking aspect of guided meditation, groups are often asked to listen to an audiobook, or audio content matched to the content of the mindfulness induction. For mindfulness inductions involving audio, periods of silence should be matched in control recordings, as these periods are precisely where mindfulness may reduce mind wandering in meditation conditions. These improvements to control conditions for meditation would improve the validity of experimental comparisons, and allow for more direct comparisons to be made between studies.

Active control conditions should be considered for studies which include meditation training. The distinction between meditation and mindfulness training was made in Section 7.3. The effects of mindfulness training are often measured by comparing an experimental group which completes the training, with a (waitlist) control group who expect to receive the same training. MacCoon et al. (2014) suggest that active control conditions should be used to isolate the effects of meditation training. This approach has advantages over waitlists, as it controls for allegiances which might form with the trainer, and factors common to any successful group intervention. Active control conditions are training programmes which are structurally similar to the mindfulness training and delivered by the same teacher, but which consist of alternative, non-mindfulness training. For example, an active control training could follow the general structure of the mindfulness training described in Chapter 3, but deep breathing could be taught and practiced as a form of sham meditation. A high level of experimental control can be achieved by having experimental and control groups complete identical training, but manipulating the duration or frequency of their meditation (Chin et al., 2018). Active control conditions would increase confidence that it is meditation, rather than some other aspects of mindfulness training which is responsible for any improvements in outcome measures.

### 7.4.3 Other methodological issues

Future research should address uncertainties over the longevity of meditation effects. There is no reliable estimate of how long the effects of meditation last, but it seems highly likely that they are temporary, except perhaps in long-term meditators. Therefore, studies should include a measurement of the time between participants' most recent meditation and the primary outcome measure(s). In many of the studies reviewed in Chapters 2 and 5, the delay between participants' most recent meditation and the outcome measure was either absent, or could only be inferred from the experimental procedure. This can affect the interpretation of outcomes. For example, three studies which showed numerical evidence that meditation reduces the AB in novices (Colzato et al., 2015; May et al., 2011, Experiment 2; Sharpe, 2021, Experiment 6) all measured the effect immediately after a meditation. May et al. (2011, Experiment 1) did not find a difference between eight weeks of LKM training and no training when neither group meditated immediately prior to the AB measurement. This strongly suggests that the effects of meditation on attention in novices are temporary. In long-term meditators, Braboszcz et al. (2013) and Slagter et al. (2007) both found a reduced AB after extensive mindfulness training, but neither study reported the time period between participants most recent meditation and the AB measurement. This makes it unclear whether the reduction in AB was due to the training, or a meditation close to the AB measurement.

If the mental states induced by meditation are of interest, then a conservative approach is to include a period of meditation as close to the primary outcome measure as possible. The longer the delay between a period of meditation and an experimental outcome measurement (e.g. mindfulness or attention), the less likely the effects are to register on these measures. Conversely, if the long-term effects of meditation (years of practice), or the effects meditation training (days or weeks of practice) are the primary focus, then participants could be asked to *abstain* from meditation for a period of time, in order to distinguish short-term from lasting effects of meditation.

Where participants undergo a period of meditation training, accurate measures of the amount of meditation completed should be reported, as this is one of the key variables which predicts outcomes. These measurements are often made by having participants record data in meditation diaries, which makes them susceptible to missing data, and measurement error due to memory and social desirability biases. By automatically recording the time and duration of each mediation, a meditation app avoids participants having to complete diaries, which minimises unintentional or deliberate misreporting of the number or duration of meditation sessions. These measurements are used to calculate compliance with suggested training schedules, another measurement which is often absent or imprecisely reported. Low compliance could indicate that a training programme is too intense for participants. This could affect motivation, mood or other variables in ways that affect mindfulness and other primary outcomes.

Meditation experience should also be measured and reported. Lifetime meditation experience is mostly of interest for studies involving long-term meditators. This is also susceptible to measurement error, because participants rarely keep detailed records of the amount of meditation they have completed. Standardised approaches are available, and should be used for estimating lifetime meditation experience (Hasenkamp

& Barsalou, 2012). Participants' mediation experience is an important predictor of outcomes, so this variable should be measured as precisely as possible.

Meditation practice quality may influence the effectiveness of mindfulness training. The quality of participants' regular meditation sessions is likely to have a large effect on experimental outcomes. There are relatively few measures of meditation quality which explains why it is almost never reported. The Mindfulness Adherence Questionnaire measures a mixture of quantitative and qualitative aspects mindfulness meditation practice (Hassed et al., 2020). An example question from this scale is *When meditating, how much of the time were you practicing an accepting attitude toward what you were experiencing?*. A more objective measure of meditation quality could be accuracy on a breath counting task used as a meditation practice.

Posner et al. (2010) suggest that teacher experience could be an active component in the effectiveness of mindfulness training. The experience of teachers who deliver mindfulness training programmes could be measured more precisely. Van Dam et al. (2017) recommend measuring the number and type of retreats which a teacher has attended, their general and specific experience in contemplative instruction, formal contemplative training, formal clinical qualifications and whether they were blinded to experimental hypotheses.

Training materials which describe and explain meditation techniques, and address common confusions about meditation practice may influence the rate at which novices benefit from training. In Experiments 4 and 5 (Chapter 3), a highly experienced meditation teacher's training materials and guided meditations were chosen with the aim of maximising the effectiveness of these variables. When delivering the training, I was mindful of the need strike a delicate balance between encouraging participants to comply with the training programme, and pushing them too hard. Based on my own meditation experience, and the group discussions, I sensed that getting this balance wrong could reduce the effectiveness of the training. Experienced teachers should be consulted regarding these and similar aspects of designing and delivering effective mindfulness training.

Finally, some general methodological limitations were identified in the literature which tests the effects of meditation on the ANT (Chapter 4) and AB (Chapter 6). The precision of effect size estimates were limited because data were unavailable from published articles or their authors for over a third of studies matching the meta-analysis inclusion criteria. This could be addressed by publishing data on an open repository. Concerns over questionable research practices (John et al., 2012) would be reduced if hypotheses, analyses and details of outcome measurements (for example the lags used to calculate the AB) were preregistered. Another simple approach to improving the quality in this field would be to follow simple principles which reduce false-positive results (Simmons et al., 2011). These include pre-specifying a rule for terminating data collection, collecting at least 20 observations per cell, listing all variables measured, reporting all experimental conditions and reporting data exclusion criteria. Randomised studies should report the randomisation method used, and miminise experimenter bias by ensuring that allocations are concealed until participants are assigned to interventions.

The quality of future studies which test the effects of meditation on the ANT and AB would improved by addressing issues of statistical power, experimental control and general methodological limitations discussed in this section. Many of the points raised may also be relevant to the effects of meditation on other attentional measures, or non-attentional effects of meditation (Chiesa et al., 2011).

### 7.4.4 Mind wandering, mindfulness and attention regulation

If mindfulness meditation reduces mind-wandering, this could also imply that attention is being regulated. Mindfulness meditation could affect mind wandering in ways which support what Randall et al. (2014) call the 'meta-awareness hypothesis'. This is another name for Smallwood and Schooler's (2006) idea that higher meta-awareness reduces mind wandering, by improving the ability to detect off-task thoughts and reorient attention towards a primary task.

Early meditation experiences provide a prosaic example of mind wandering as a 'default state' of unregulated attention. Beginners often notice how frequently the mind wanders in spite of their intention to remain aware of the sensations of breathing (Hasenkamp & Barsalou, 2012), so-called 'monkey mind'. The frequency and duration of 'off breath' episodes tends to reduce with practice. Randall et al. (2014) define attention regulation as the ability to allocate attention towards on-task thoughts, and away from mind wandering. Furthermore, less frequent mind-wandering is associated with improved attention regulation (Hölzel et al., 2011; Vago & Silbersweig, 2012). If mindfulness is sustained meta-awareness, and meditation increases mindfulness, then we would also expect mindfulness meditation to reduce mind wandering. If mindfulness and mind wandering are opposing states/processes, then reduced mind wandering might coincide with increased mindfulness and improved attentional performance.

A key difference in this approach is that it explores mental activity under conditions of low cognitive load. Since the identification of the default mode network (Raichle & Snyder, 2007), it has become clear that the mind frequently wanders when it has nothing else to do (Vago & Zeidan, 2016), for example when a task is undemanding or boring. Also, individuals often lack meta-awareness of their mind wandering (Schooler & Smallwood, 2014). As a relatively passive activity, mindfulness meditation could be the perfect training environment for increasing meta-awareness such that the onset of mind wandering is detected more frequently. Repeatedly reorienting attention towards a meditation object may transfer to an ability to orient away from the contents of spontaneous thought, thereby reducing the duration of mind wandering episodes.

Vigilance tasks, such as the Sustained Attention to Response Task (SART; Robertson et al., 1997) may be more appropriate for studying mind wandering, because they create conditions of low cognitive load, where mind wandering is more likely. Conversely, the attentional demands made by the ANT and AB may make them less suitable tasks for studying mind wandering and mindfulness. The SART is a go-nogo task, which is frequently used to study mind wandering. Digits are displayed every 250ms, and participants must respond by pressing a key each time a new digit appears. They must *withhold* a response each time a particular digit appears. Mind wandering increases responses on nogo trials, when a response should be witheld. More frequent mind wandering could signify *disregulation* in the vigilance attentional network. If meditation increases mindfulness, and mindfulness reduces mind wandering, then mindfulness meditation should improve SART performance.

The SART might simultaneously measure the effects of meditation on mindfulness and vigilance. Existing evidence support the idea that mindfulness meditation reduces mind wandering. Eight minutes of mindful breathing has been shown to improve SART performance compared with passive relaxation and reading (Mrazek et al., 2012). Rahl et al. (2017) found improved SART performance after three days of brief mindfulness and acceptance training, relative to mindfulness only training and a reading control condition. The SART may provide a purer measure of vigilance (alerting/sustained attention) than the ANT, because it lacks the attentional effort required to orient towards the arrows and resolve the conflict in the flanker task. Under these less active conditions, improved SART performance following mediation might be evidence of increased mindfulness *and* vigilance. This may not explain *how* mindfulness regulates attention, but it may show that the processes are related, and it may do this in a way which is not possible with tasks such as the ANT.

## 7.5 Can mindfulness improve wellbeing?

In Chapter 1, improved attentional control was proposed as a mechanism which explains the beneficial effects of MBIs. Given the small effects that meditation has on attention, additional mechanisms probably play a role in positive MBI outcomes, and wellbeing more generally. Candidate mechanisms are enhanced positive emotion regulation strategies, self-compassion levels, and decreased rumination and experiential avoidance (Chiesa et al., 2014). Improved wellbeing may result from interactions between mindfulness, attention regulation and emotion regulation.

Mindfulness is thought to consist of two different components; an enhanced awareness of present moment experience and the adoption of attitudes of curiosity, openness, and acceptance towards experience (Bishop, 2004; Chiesa et al., 2014). The awareness aspect of mindfulness is most closely associated with attention regulation. The types of attitude adopted towards present moment experience are thought to regulate emotion. Both of these hypothetical mechanisms are to be found in all theories of mindfulness, regardless of whether they are derived primarily from psychological science (Hölzel et al., 2011; Isbel & Summers, 2017; Lindsay & Creswell, 2017; Malinowski, 2013; Teper et al., 2013; Vago & Silbersweig, 2012) or Buddhism (Grabovac et al., 2011). Britton (2019) argues that an imbalance between interventions which regulate attention, and those that regulate emotion may explain why meditation produces a mixture of positive, negative and null effects on a range of outcomes. An *underemphasis* on the potential for mindfulness to regulate emotion may limit its potential to improve wellbeing.

The attitude which has dominated research into the effects of mindfulness on emotion regulation is acceptance, also known as equanimity (Lindsay & Creswell, 2017). Equanimity has been defined as "an even-minded mental state or dispositional tendency toward all experiences or objects, regardless of their origin or their affective valence" (Desbordes et al., 2014, p. 1). According to one mindfulness teacher, equanimity is the opposite of apathy and suppression (Young, 2016). In Buddhism, equanimity (Pali: *upekkhā*) is one of four "divine abodes" (Pali: *brahma-vihāra*), wholesome mental states which are commonly taken as meditation objects (Gethin, 1998, pp. 186–187). Another synonym for acceptance/equanimity is 'nonjudgement'. MBIs often define mindfulness as "the awareness that arises from paying attention, on purpose, in the present moment, and non-judgmentally" (Kabat-Zinn, 2011). Ambiguity in the term "non-judgmentally", risks misinterpretation of what Buddhists understand by equanimity. A middle ground can, however, be struck if both terms are thought of as practical ways to minimise anger, hatred, and attachment as responses to experience (Gethin, 2011).

Theoretical and empirical evidence suggests that improvements to wellbeing (reduced stress for example) are more pronounced when mindfulness and acceptance are combined. Monitor and Acceptance Theory (MAT; Lindsay & Creswell, 2017) draws the clearest boundary between the effects of mindful awareness (monitoring) on attention regulation and the effects of acceptance on emotion regulation. Drawing on this theory, Lindsay et al. (2018) found that three weeks of monitoring and acceptance training reduced physiological stress measures to a greater extent than an equivalent period of monitoring training. A similar result has been found for mind wandering. Rahl et al. (2017) found that three days of monitoring and acceptance training improved SART performance (reduced mind wandering) to a greater extent than the same period of monitoring training, relaxation training, or an active reading control condition. This is additional evidence that mindfulness with acceptance may improve wellbeing to a greater extent than mindfulness alone.

Other research suggests that equanimity is multifaceted, and one component can improve as a *result* of mindfulness meditation. Hadash et al. (2016) conceptualise equanimity as the decoupling of desire (wanting/not wanting) from the hedonic tone (pleasant/unpleasant) of current or anticipated experience. They found that equanimity can be divided into lower order factors of acceptance, and automatic reactivity (habitual mental and behavioural reactions) to unpleasant hedonic tone. Hadash et al. (2016) gave novices four, 60 minute sessions of mindfulness training, *without* acceptance instructions. Relative to controls, the trainees had higher state mindfulness, reduced reactivity to unpleasant hedonic tone, but no elevation in acceptance. This suggests that reduced reactivity to unpleasant experience is distinct from acceptance. Interestingly, this study found improvements on the SMS with less overall training than in Experiment 4 (Chapter 3). This could be due to the higher intensity or lower frequency of the training. It might also be due to the type of meditation, which was a mixture of FAM and OMM using a form of mindful noting (Anālayo, 2003, p. 95, Note 8).

Buddhists would agree that wellbeing requires training mindfulness, attention, and other mental factors. One senior teacher glibly comments, “[obviously] mindfulness is not enough!" (Brahm, 2016, p. 4). In Buddhism, the combination of mindful awareness with wholesome attitudes is called wise attention (Pali: *yoniso manasikāra*). In addition to equanimity, other states cultivated by Buddhists are loving-kindness, or friendliness (Pali: *mettā*), compassion (Pali: *karuṇā*), and sympathetic joy (Pali: *muditā* ). *Mettā* meditation, also known as Loving-kindness meditation (LKM), is central to Buddhist practice. It can be considered as a type of FAM, where the object of meditation is *mettā* (feelings of goodwill, happiness for others, and forgiveness), rather than a sensory object such as the breath. More research is needed to establish whether LKM improves wellbeing to a greater extent than other forms of FAM, and whether this is similar to the finding that mindfulness is more powerful with acceptance, than without. Mindfulness and compassion are also combined in meditation, and are key components in Compassion Focused Therapy, a treatment which develops the capacity to cultivate prosocial emotions in mentally healthy ways (Gilbert, 2014). Some research indicates that mindfulness and compassion can regulate emotion. Desbordes et al. (2012) found that eight weeks of mindfulness training, with and without a compassion component, reduced amygdala activity relative to an active control intervention. Interestingly, these differences were found when participants were not in a meditative state. More research is needed to establish whether compassion training enhances attention regulatory effects of mindfulness training.

Finally, low intensity mindfulness training may offer the most enduring improvements to wellbeing. When discussing the results of Experiment 5 in Chapter 3, the intriguing idea was presented that, in novices, ANT performance may improve when training is less intensive. If this is true, then long periods of low intensity mindfulness practices may also improve wellbeing to a greater extent that daily meditation over a few weeks. Meta-analyses suggest that participants comply with about 64% of the assigned amount of meditation in a training programme, meaning they manage about 30 minutes per day, six days per week (Parsons et al., 2017). Anecdotally, there are many obstacles to maintaining a regular meditation practice, especially after a training course ends. Two less intense mindfulness practices which form part of the eight week MBCT programme may provide a more gradual, but sustainable approach to improving wellbeing (Williams et al., 2012). The first is to meditate for shorter periods of time. For example, MBCT has a three minute meditation which it refers to as the 'breathing space'. The second is to use everyday activities, such as washing dishes, driving, or even simply walking up and down stairs as mindfulness practices. These less intense practices might be especially effective for wellbeing if combined with attitudes such as acceptance, kindness and self-compassion which are known to regulate emotion.

A narrow focus on attention risks losing sight of the bigger, possibly more important potential of mindfulness to improve health and wellbeing. Ordinary people may not be interested in the goals of long-term meditators or monastics, or able to invest the time and effort required to achieve them. Many however, are interested in health and wellbeing outcomes which Buddhists consider to be side effects of mindfulness training. These seem most likely to be attained by using types of meditation which regulate both attention *and* emotion. Whilst deep *samādhi* is necessary for the permanent liberation sought by monastics, with less effort, ordinary people may be able to apply these meditation techniques to experience more peace and happiness in their lives.

## 7.6 Conclusion

The primary claim tested by this thesis was whether mindfulness meditation regulates attention, operationalised as improved performance on the ANT and/or a reduced AB. There was some meta-analytic evidence for a small reduction in AB (Chapter 6), and experimental evidence that in novices, brief mindfulness meditation improves attentional performance on the RSVP used to measure the AB effect (Chapter 5). There was no experimental evidence that brief FAM increases mindfulness or improves ANT scores in novices or long-term meditators (Chapter 2). Novices also showed no increases in mindfulness or ANT performance after four or eight weeks of FAM training (Chapter 3). There were small meta-analytic effects of meditation on the executive attention component of the ANT (Chapter 4). This suggests that mindfulness meditation has small regulatory effects on executive control. Compared with the ANT, the RSVP task used to measure the AB appears to be a more sensitive, valid and reliable measure of the regulatory effects of meditation on attention. There was limited evidence that mindfulness mediates these attentional effects. However, this could be because mindfulness is not always accurately measured, lack of sensitivity in mindfulness measures, or because mindfulness inductions were not powerful enough to register on the measures used. These are provisional findings, as studies were limited by low statistical power.

Future research requires larger samples to address statistical power limitations. They should also preregister designs to address risks of bias, and make data available for subsequent meta-analyses. Studies should also ensure that control conditions are well matched to mindfulness meditation, and use active control conditions in studies which include meditation training. The time between participants' most recent meditation and primary outcomes should be measured, as should participants' meditation experience. More research is needed into the quality of participants' meditation practice, and the effectiveness of meditation instruction. Mind wandering should be studied in more depth, as it is a mental process which contrasts with mindfulness and can be measured using the SART. The SART may provide a more fruitful approach for detecting the effects of meditation on attention because, unlike many other tasks, it studies mental activity under relatively low cognitive load.

If the aim of mindfulness is to improve wellbeing, then attention regulation needs to be studied in conjunction with emotion regulation. By stabilising attention, mindfulness appears to provide a foundation for improved wellbeing, but the effects appear to be larger when meditation is combined with attitudes of acceptance, friendliness and compassion. More research is needed to establish how mindful awareness combined with these attitudes towards experience can improve wellbeing.

# Appendices

# A
# Breath counting/reading and comprehension instructions

- These scripts are written in a modular fashion so that sections other than the active component are identical (i.e. controlled for) in different conditions. Once recorded, keeping active wording separate from generic wording allows various combinations of hypothesised active components, at the same time, allowing us to use the same recordings in control conditions.
- When recorded, scripts should be approximately 3 minutes.
- Ps are instructed to press down arrow on breaths <= 8, right arrow on breath == 9.

| Both |
| --- |
| Listen closely to these instructions which prepare you for the next computer task. Start by finding a relaxed posture and close your eyes. Make sure your feet are flat on the floor ... allow your arms to be limp and heavy, and rest your hands comfortably in your lap. Allow your body to relax as much as possible. So let your jaw drop, and your face relax. Release any tension in your shoulders ... and in your arms and hands. And relax your legs, and your feet. Feel the heaviness in your relaxed muscles pulling your body down towards the ground. Now straighten your spine, and take a deep breath in... and now breathe out. And imagine there's a balloon attached to the crown of your head. The pull of this balloon is perfectly balanced with the weight of your relaxed body. So the equal and opposite forces gently lengthen your spine, and bring your head upright so that it's balanced on top. And as your spine straightens, you should find your mind becomes more alert.<br>Notice this simultaneous relaxation in your body, and mental alertness from your upright posture. Just breathe naturally ... so just notice your breath going easily in and out of your body. Try to maintain this state of relaxed alertness, as this will improve your accuracy on the following computer task. |

*A. Breath counting/reading and comprehension instructions*

| Breath Counting (letting go) | War and Peace reading |
|---|---|
| You're going to be doing a type of meditation. Focus your attention on your breath going in and out, but try not to control your breath. You're just watching your natural breathing pattern. All you need to detect whether you're breathing in, or breathing out. | You're going to do a reading and comprehension task. You'll be reading the first chapter of War and Peace, a novel by the Russian author Leo Tolstoy. Try to just read at your normal speed. All you need to do is to pay attention to the events and characters that are introduced in the story. |
| As a way to help you stay focused on your breathing, you'll be counting your breaths. Try this now. Gently rest your attention on detecting your breath going in and out. Each time you exhale, add one to your count. | Read carefully as you will be asked some questions at the end of the task. The questions will only relate to what you have actually read. This is why your reading speed is unimportant. |
| During the task, it's likely that thoughts or feelings will distract you, making it more likely that you'll lose count of your breaths. These distractions are completely natural, and to be expected. Rather than trying to keep your attention on your breath, the key to meditation is to simply let go of all other distractions. | The first chapter of the novel has quite a few characters, so you may find it hard to follow the plot. This is completely natural and to be expected. If this happens, it's fine to go back and re-read earlier pages. There will be buttons on the screen to navigate forwards and backwards through the pages. |
| As soon as you notice a distracting thought or feeling, try to gently disengage from it, and let it go. You can imagine distracting thoughts as hitting a deeply padded wall. Allow them to sink into the padding and remain there so they don't reverberate in your mind. This will help you to stay focused on your breath counting. | So just relax and read the chapter as though you were reading the novel itself. Imagine that a friend wants to ask you some questions about what happened in the chapter. Try to keep track of the plot and people so that you can answer your friend's questions when you've finished reading. |

| Both |
|---|
| Try to maintain the posture I described throughout the next computer task. You can easily regain that feeling of relaxed alertness, by simply relaxing your body and straightening your spine. Now slowly open your eyes, remove the headphones, and contact the researcher. |

# B
# ANT meta-analysis: selection process

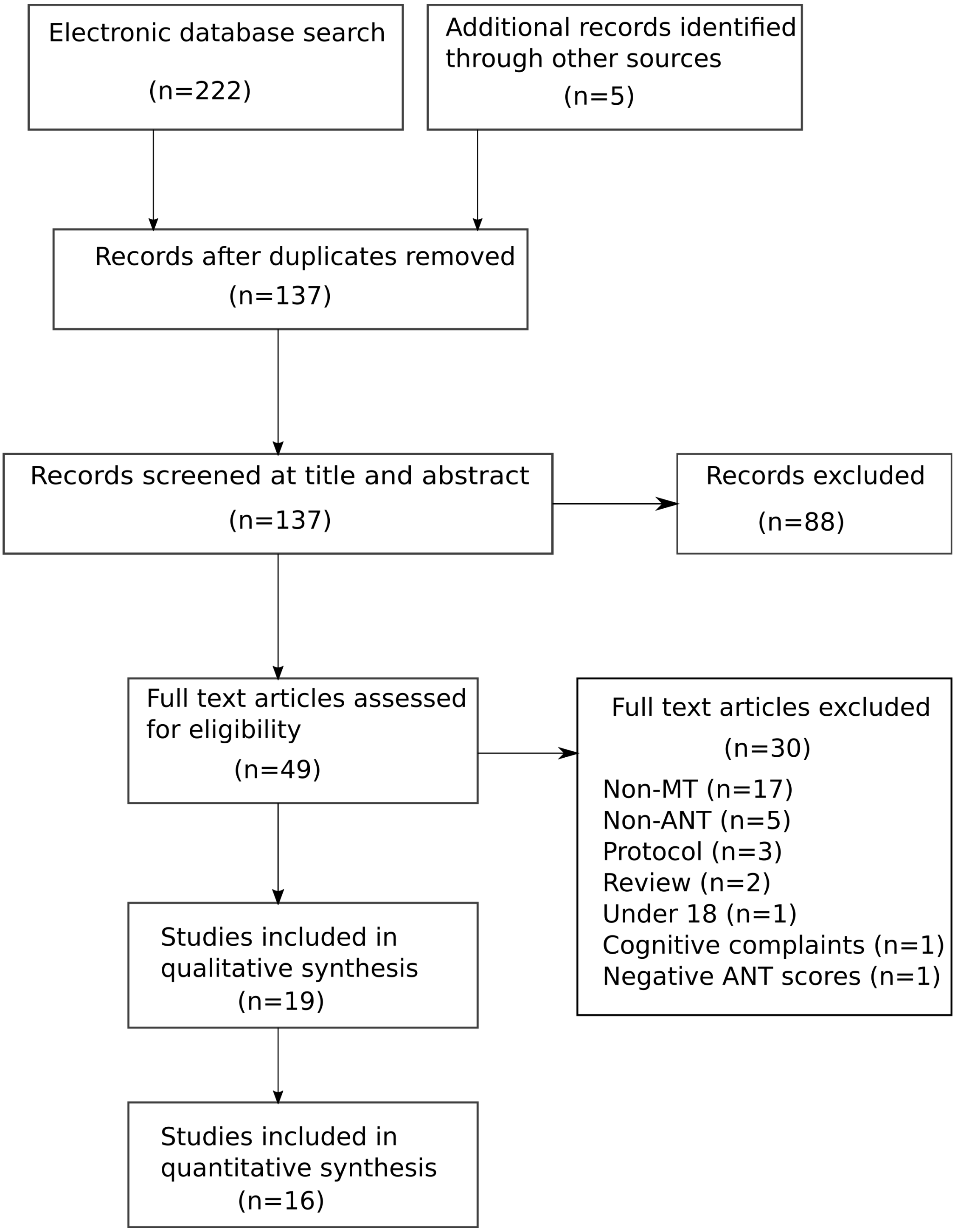


**Figure B.1:** ANT meta-analysis study selection process

# C
# ANT meta-analysis: formulas

## C.1 ANT scores

$$Alerting = RT_{no-cue} - RT_{double\ cue} \tag{C.1}$$

$$Orienting = RT_{centre\ cue} - RT_{spatial\ cue} \tag{C.2}$$

$$Conflict = RT_{flanker\ incongruent} - RT_{flanker\ congruent} \tag{C.3}$$

## C.2 Meta-analysis

The meta-analysis for each ANT variable was built using the `brmsformula`

$$smd|se(se) \sim 1 + (1|study) \tag{C.4}$$

This models the study SMD `smd` and its standard error `se(se)` in terms of an overall intercept `1` (fixed effect), and a random effect for each study `(1 | study)`.

## C.3 Supporting formulas

$$SE = \frac{CI95_{upper} - CI95_{lower}}{3.92} \tag{C.5}$$

95% confidence interval for Cohen's d (Rosnow & Rosenthal, 2009).

$$df = n1 + n2 - 2 \tag{C.6}$$

$$CI95 = \sqrt{\left(\frac{n_1 + n_2}{n_1 n_2} + \frac{d^2}{2df}\right)\frac{n_1 + n_2}{df}} \tag{C.7}$$

$$SD_{pooled} = \sqrt{\frac{(n_1 - 1)SD_1^2 + (n_2 - 1)SD_2^2}{n_1 + n_2 - 2}} \tag{C.8}$$

For each *ANT* score (*Alerting*, *Orienting*, *Conflict*), equation (C.9) defines the standardized mean difference (SMD) between groups. This formula is Hedges' *g* (Higgins et al., 2019, Section 6.5.1.2).

$$SMD = \frac{mean(ANT_{group1}) - mean(ANT_{group2})}{SD_{pooled}(n_{group1}, n_{group2}, sd(ANT_{group1}), sd(ANT_{group2}))} \tag{C.9}$$

Jo et al. (2016) and Tsai & Chou (2016) reported standard errors. These were converted to standard deviations using equation (C.10) (Higgins et al., 2019, Section 6.5.2.2).

$$SD = SE \times \sqrt{n} \tag{C.10}$$

Kwak et al. (2020) did not include standard deviations for pre and post ANT scores by condition. These were imputed using equation (C.11), which is based on Higgins et al. (2019, Section 6.5.2.2). In each calculation, $ant1$ and $ant2$ are the trial types used to compute the alerting, orienting or executive ANT score, and $Corr$ was calculated using data from Experiment 5 (Section 3.3), as the correlation between the mean RT by participant for the same trial types.

$$SD_{imputed} = \sqrt{SD_{ant1}^2 + SD_{ant2}^2 - (2 \times Corr \times SD_{ant1} \times SD_{ant2})} \tag{C.11}$$

# D

# The effects of brief meditation on the attentional blink: supplementary materials

## D.1 Effects of meditation on the AB: four vs. two lags

Our results include a group(FAM, OMM, relaxation) x lag(3,8) ANOVA i.e. without comparisons of conditions (e.g. lag 1 vs. lag 8) which would be not be expected to produce an AB:

- group: $F(2.00, 117.00) = 5.83$, $p = .004$, $BF = 5.28$
- lag: $F(1.00, 117.00) = 176.26$, $p < .001$, $BF = 1.15 \times 10^{22}$
- group x lag: $F(2.00, 117.00) = 2.07$, $p = .130$, $BF = 0.41$

For comparison with Colzato et al. (2015), we include the following group(FAM, OMM, relaxation) x lag(1,3,5,8) ANOVA.

- group: $F(2.00, 117.00) = 5.59$, $p = .005$, $BF = 7.55$

- lag: $F(2.33, 272.36) = 102.32$, $p < .001$, $BF = 7.56 \times 10^{43}$
- group x lag: $F(4.66, 272.36) = 1.75$, $p = .129$, $BF = 0.17$

The results are similar, with substantial evidence against a group x lag interaction.

## D.2 Materials

- Demonstrations and source code for the
  - affect grid `https://github.com/paulsharpeY/affect-grid`
  - AB task `https://github.com/paulsharpeY/rsvp-task`.
- Other materials are archived at `https://osf.io/qjrkb/`

## D.3 Pre-registration

The experiments were pre-registered at `https://osf.io/ps9nr/`, and `https://osf.io/qp74d/`.

# E
# AB meta-analysis: selection process

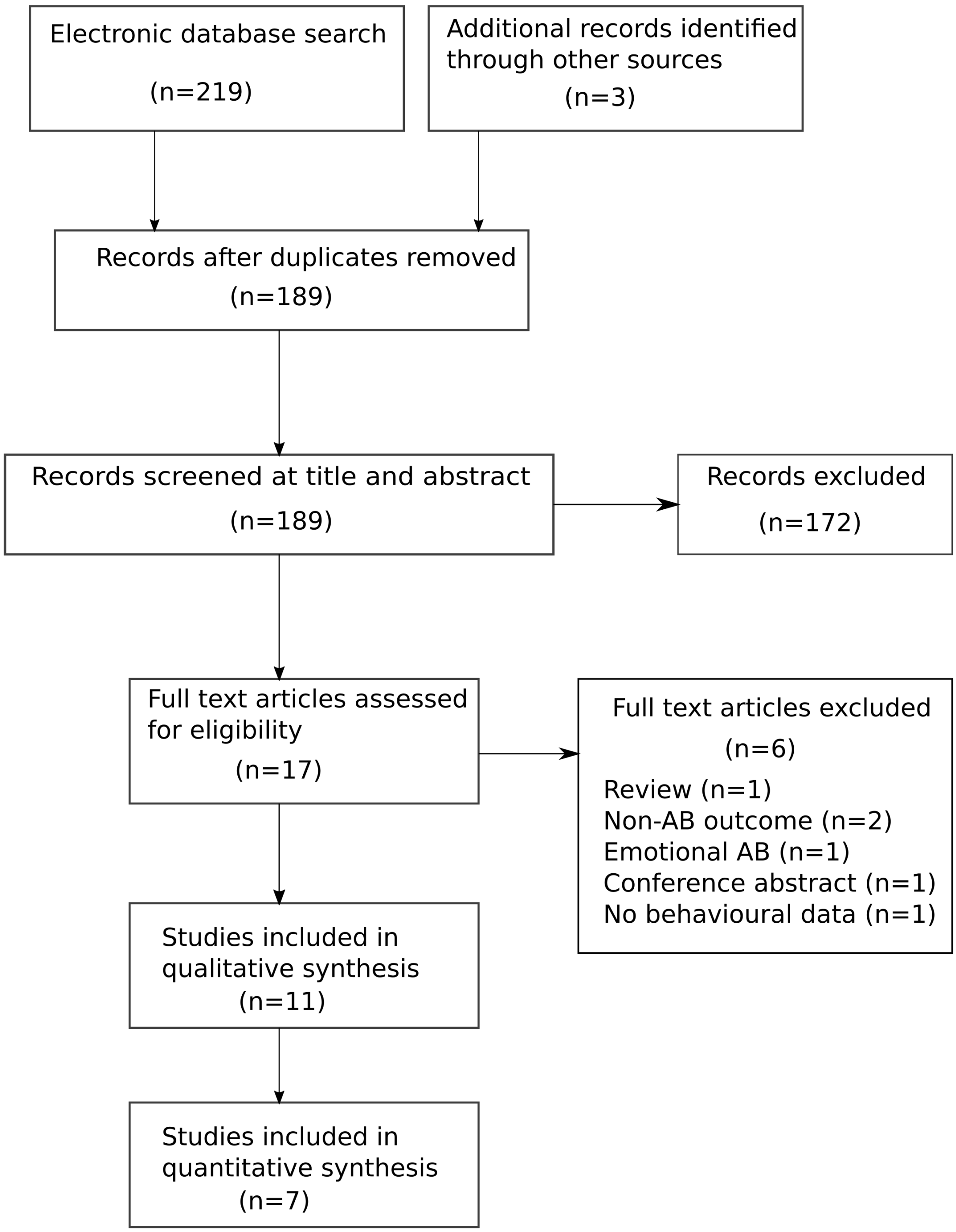


**Figure E.1:** AB meta-analysis study selection process

# F

# AB meta-analysis: formulas

This appendix defines the equations used in the AB meta-analyses. Supporting formulas are shown in Appendix C.3.

## F.1 AB

AB magnitude (henceforth, AB) was defined by equation (F.1).

$$T2|T1 = T2\,accuracy,\ given\,T1\,accurate \tag{F.1}$$

Where raw accuracy data were available, the mean and standard deviation AB for each condition was calculated using equations (F.2) and (F.3), where *i* means all accuracy observations for participant *i*.

$$AB_{mean} = \sum_{i=1}^{n} mean(T2|T1\,long_i - T2|T1\,short_i) \tag{F.2}$$

$$AB_{sd} = \sum_{i=1}^{n} sd(T2|T1\ long_i - T2|T1\ short_i) \tag{F.3}$$

Equations (F.4) and (F.5) were used when means and standard deviations for short and long lags, rather than raw data were available.

$$AB_{mean} = mean(T2|T1_{long}) - mean(T2|T1_{short}) \tag{F.4}$$

$$AB_{sd} = SD_{pooled}(sd(T2|T1_{long}), sd(T2|T1_{short})) \tag{F.5}$$

Equation (F.6) defines the standardized mean AB difference (SMD) between groups. This formula is Hedges' *g* (see Higgins et al., 2019, Section 6.5.1.2). For Colzato et al. (2015), he *SMD* and its 95% confidence interval were derived for the FAM and OMM conditions from the *F* statistic, using `compute.es::fes` (Re, 2013), as we were unable to obtain summary statistics or raw data for this study.

$$SMD = \frac{AB_{mean1} - AB_{mean2}}{SD_{pooled}(AB_{n1}, AB_{n2}, AB_{sd1}, AB_{sd2})} \tag{F.6}$$

## F.2 Meta-analysis

The meta-analysis was built using the `brmsformulas`

$$smd|se(se) \sim 1 + (1|study) \tag{F.7}$$

This models the study SMD `smd` and its standard error `se(se)` in terms of an overall intercept `1` (fixed effect), and a random effect for each study `(1 | study)`.

# G
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