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Does your mindfulness benefit others? A systematic review and meta-analysis of the link between mindfulness and prosocial behaviour

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Mindfulness-based meditation practices have received substantial scientific attention in recent years. Mindfulness has been shown to bring many psychological benefits to the individual, but much less is known about whether these benefits extend to others. This meta-analysis reviewed the link between mindfulness – as both a personality variable and an intervention – and prosocial behaviour. A literature search produced 31 eligible studies (N = 17,241) and 73 effect sizes. Meta-analyses were conducted using mixed-effects structural equation models to examine pooled effects and potential moderators of these effects. We found a positive pooled effect between mindfulness and prosocial behaviour for both correlational (d = .73 CI 95% [0.51 to 0.96]) and intervention studies (d = .51 CI 95% [0.37 to 0.66]). For the latter, medium-sized effects were obtained across varying meditation types and intensities, and across gender and age categories. Preliminary evidence is presented regarding potential mediators of these effects. Although we found that mindfulness is positively related to prosociality, further research is needed to examine the mediators of this link and the contexts in which it is most pronounced.

Interest in the psychological effects of meditation has increased rapidly in recent decades (Sedlmeier *et al.*, 2012). Meditation practices based on the cultivation of non-judgemental awareness – or mindfulness – have been at the forefront of this interest, originally in clinical contexts, but more recently as an enabler of well-being and positive functioning among non-clinical populations (Khoury, Sharma, Rush, & Fournier, 2015). Indeed, popular interest in mindfulness meditation is now such that it represents a 'billion-dollar business' serving millions of people around the world (Wieczner, 2016).

Central to the appeal of so-called modern mindfulness practices is their dissociation from any particular belief system, arguably making them attractive to a broad spectrum of the population in secular Western societies (Monteiro, Musten, & Compson, 2014). There is indeed an accumulating body of evidence that mindfulness-based meditation practices provide a range of benefits for the individual, including attenuations in depression and

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anxiety symptoms (Khoury *et al.*, 2013), and improvements in well-being and mental health (Keng, Smoski, & Robins, 2011; Khoury *et al.*, 2015), cognitive abilities (Chiesa, Calati, & Serretti, 2011), and physical health (Keng *et al.*, 2011).

However, in recent times, some have suggested that the benefits of the modern mindfulness movement may be confined to the individual, particularly because secular mindfulness meditation programmes tend not to focus on moral or ethical elements of individuals' choices or behaviour – elements that were central to the traditions from which these meditative practices emerged (Monteiro *et al.*, 2014). If this is the case, the modern mindfulness movement may, paradoxically, serve to *reinforce* individuals' sense of self, rather than transcend it (Monteiro *et al.*, 2014). In contrast, the Eastern contemplative traditions, most notably various sects of Buddhism, which have partly inspired the proliferation of mindfulness interventions in the West, have for millennia emphasized the links between the practice of mindfulness and the promotion of prosocial behaviours (Dalai Lama & Ekman, 2008). A key question, then, is whether secular mindfulness supports the enactment of prosocial behaviours, or whether the benefits of mindfulness are limited to the individual. Recent narrative reviews indicate that mindfulness is indeed linked with various kinds of prosocial behaviour (e.g., Berry & Brown, 2017; Condon, 2017), but there is a need to more systematically examine this emergent line of research.

The present article provides a systematic review and a meta-analysis of the empirical research on the links between mindfulness and prosocial behaviour. We focus on mindfulness as both a personality trait and an intervention, in an effort to provide a multimethod test of our research question. A key advantage of examining links between mindfulness as a personality construct and prosocial behaviour is that we can explicitly measure mindfulness – and therefore isolate its effects – in a way that is more difficult in intervention research, where mindfulness meditation often also seeks to cultivate prosocial emotions, personal values, and intrinsic motivations and also varies considerably in their design and methods (Chiesa & Serretti, 2011; Khoury *et al.*, 2013). To test the directionality and causal relations between our two constructs of interest, we then review findings from intervention studies in this research domain. For both individual difference and intervention studies, we explore potential moderators of obtained effects and lastly examine potential mediating variables. Our overarching goal was to gather and evaluate the relevant empirical studies in this rapidly developing field and provide evidence-based recommendations for future research.

Mindfulness

Mindfulness has been defined as an open and non-judgemental awareness of one's present-moment experience (Brown & Ryan, 2003; Kabat-Zinn *et al.*, 1992) . It describes a way of engaging with one's experience, wherein one's attention is directly oriented to sensations, thoughts, and emotions occurring moment-by-moment. This conceptualization of mindfulness, emanating from Buddhist traditions, is different from other approaches to mindfulness, such as Langer's version (e.g., Langer, Bashnerr, & Chanowitz, 1985), where mindfulness is described as the process of actively seeking out and generating novelty in one's moment-by-moment experience. The latter conceptualization involves mental elaboration and sense-making in a way that Buddhist conceptualizations of mindfulness do not (Chiesa, 2012; Siegling & Petrides, 2014).

Multiple measures of mindfulness derived from Buddhist philosophy have been developed in recent years, each based on related but distinct conceptualizations of the

construct (for a review, see Siegling & Petrides, 2014). Differences in such measures largely pertain to the dimensionality of mindfulness. For example, some scholars view mindfulness as a unidimensional construct, characterized by an attention to and awareness of moment-by-moment experience (Brown & Ryan, 2003). Others view mindfulness as a bidimensional construct, distinguishing between attentional and affective components (e.g., Bishop *et al.*, 2004; Cardaciotto, Herbert, Forman, Moitra, & Farrow, 2008). Still others have operationalized mindfulness as comprising multiple facets, such as observing, acting with awareness, describing present-moment experience, non-judging, and non-reactivity (Baer, Smith, Hopkins, Krietemeyer, & Toney, 2006).

Despite these differences, there is evidence that all these measures of mindfulness converge upon a common, underlying construct, broadly consistent with the Kabat-Zinn *et al.* (1992) definition, and psychometrically distinct from Langer's definition of the construct (Siegling & Petrides, 2014). In the present review, we therefore included all such measures of mindfulness, including single, bifactor, and multifaceted measures.

Further, mindfulness has been studied as both a trait and a state. Trait mindfulness describes a general disposition towards present-centred awareness and attention, while state mindfulness relates to variations in states of present-centred awareness and attention (Brown & Ryan, 2003). While state and trait mindfulness are thus conceptually distinct, they are closely related. Individuals who are high on trait mindfulness also tend to have higher levels of mindfulness at any given moment in time. Several studies have found support for this, with trait and state mindfulness typically correlated between .40 and .50 with one another (e.g., Brown & Ryan, 2003; Tanay & Bernstein, 2013; Weinstein, Brown, & Ryan, 2009). In the present review, we therefore included studies of both trait mindfulness (i.e., via correlational studies) and interventions aimed to cultivate mindful states (i.e., via intervention studies).

In considering studies of mindfulness *interventions* and prosocial behaviour, we included a relatively broad range of mindfulness-based interventions. Mindfulness-based interventions have developed in a relatively diverse way, with some focusing more narrowly on mindfulness (i.e., self-regulated attention, curiosity, and acceptance of one's moment-by-moment experience), and others combining the teaching of mindfulness skills with the cultivation of prosocial emotions such as kindness, empathy, and compassion (Galante, Galante, Bekkers, & Gallacher, 2014; Keng *et al.*, 2011). The latter type of intervention typically starts by training mindful attention and acceptance of present-moment experience, before moving into the conscious cultivation of prosocial emotions as a core focus of the intervention (see Galante *et al.*, 2014 for a review). In the present review, we included both types of studies (i.e., 'mindfulness-only' and 'mindfulness-plus-prosocial-emotion'), but report results for both study types separately.

Prosocial behaviour

Prosociality is said to be a central feature of human adaptive success, as it fosters cooperation and cohesion among groups (Fehr & Fischbacher, 2003). Prosocial behaviour has been defined as 'voluntary behaviour intended to benefit another' (Eisenberg, Fabes, & Spinrad, 2007, p. 646). As such, prosocial behaviour can be distinguished from affective responses to another's suffering such as empathic concern, as well as from cognitive responses such as the capacity to take another's perspective or appraisals of deservingness, which may or may not lead to actions aimed at benefitting others (Eisenberg *et al.*, 2007). Additionally, the motivation for the act is not considered in determining whether a behaviour is prosocial or not (Penner & Orom, 2010). Prosocial behaviour may be

motivated by altruism, in which case the act is undertaken with no expectation of personal reward (Eisenberg *et al.*, 2007). However, in many cases, prosocial behaviour may be engendered from non-altruistic motivations such as conforming to norms or rules (Penner & Orom, 2010), adhering to one's sense of fairness, or enhancing status and reputation (Eisenberg *et al.*, 2007). Finally, prosocial behaviour is an act *intended* to benefit another: Whether or not it does provide the intended benefits, or any kind of benefit at all, is seen as irrelevant to classifying an act as prosocial (Eisenberg *et al.*, 2007).

How might mindfulness foster prosocial behaviour?

Theories of how mindfulness operates have emerged from a relatively diverse body of behavioural, cognitive, and neuroscientific studies (for reviews, see Gu, Strauss, Bond, & Cavanagh, 2015; Hölzel *et al.*, 2011; and Vago & Silbersweig, 2012). There are a number of proposed mechanisms by which mindfulness might increase prosociality. First, mindfulness might foster prosocial behaviour by increasing individuals' capacity to sustain and direct attention (Condon, 2017). Studies have demonstrated that mindfulness training leads to increases in sustained attention (for a review, see Chiesa *et al.*, 2011). In social contexts, greater attentional capacities may increase the likelihood that an individual observes the needs of others, meaning they are more likely to respond to them (Brown & Ryan, 2003; Condon, 2017).

Second, mindfulness practices are associated with greater awareness of bodily sensations, or interoceptive awareness (Hölzel *et al.*, 2011; Vago & Silbersweig, 2012). There is evidence that meditation training increases activity in the insula, a brain region involved in interoceptive awareness (Farb *et al.*, 2007). The insula is also involved in processing others' emotional experiences (see Singer, Critchley, & Preuschoff, 2009 for a review), meaning that greater interoceptive awareness may increase individuals' awareness of the needs of others in the social environment.

Third, mindfulness may change an individual's affective experience. Cameron and Fredrickson (2015) found that dispositional mindfulness was associated with more positive emotions such as love/closeness, joy, gratitude, and interest and fewer negative emotions such as anger, fear, guilt, and stress. These emotions were in turn associated with, respectively, greater and lesser self-reported helping behaviour (Cameron & Fredrickson, 2015).There is also evidence that meditators, relative to non-meditators, display the activation of neural networks associated with prosocial emotions (Lutz, Brefczynski-Lewis, Johnstone, & Davidson, 2008).

Fourth, mindfulness may enhance affect regulation, meaning that the experience of negative emotions, such as personal distress when faced with the suffering of another, is less likely to inhibit compassionate and behaviourally flexible responses to such situations (Condon, 2017; Donald, Atkins, Parker, Christie, & Ryan, 2016), and individuals are more likely to respond with interpersonal warmth and kindness (Fredrickson, Cohn, Coffey, Pek, & Finkel, 2008) and act in values-consistent ways (Donald, Atkins, Parker, Christie, & Guo, 2016). Consistent with this, reductions in emotional interference (assessed as the delay in reaction time after being presented with affective versus neutral pictures) have been shown to follow mindfulness training (Ortner *et al.*, 2007). Further, brain regions implicated in emotion regulation, including increased prefrontal cortical activity, and reduced amygdala and threat-system activation, have been found to be positively associated with dispositional mindfulness (Creswell, Way, Eisenberger, & Lieberman, 2007) and meditation (Weng, Fox, Shackman, & Stodola, 2013). In turn, affect-regulating

behaviours such as impulse control and modulating emotional states have been linked with prosocial behaviour across several studies (Eisenberg *et al.*, 2007).

Fifth, mindfulness may enhance the expression of prosocial behaviour by perceiving thoughts as mental events rather than literal truths, meaning that judgements, assumptions, and biases are less likely to inhibit the expression of helping behaviour (Condon, 2017). This process has been described as 'dereification' (Condon, 2017), 'reperceiving' (Shapiro, Carlson, Astin, & Freedman, 2006), and 'cognitive defusion' (Hayes, Strosahl, & Wilson, 1999). In support of this, there is evidence that cognitive defusion predicts less avoidance behaviour when facing suffering, and a greater likelihood of approach behaviours, such as active coping and positive reinterpretation (Donald, Atkins, Parker, Christie, & Guo, 2016).

Finally, it may be that mindfulness facilitates prosocial behaviour by altering one's sense of self from a rigid entity that needs protecting, to one that is interdependent, flexible, and non-attached (Berry & Brown, 2017; Hölzel *et al.*, 2011; Sahdra, Ciarrochi, & Parker, 2016; Vago & Silbersweig, 2012). With less attachment to the self, individuals are more likely to respond helpfully to the needs of others, including to outgroup others (Berry & Brown, 2017). In support of this, studies have shown that mindfulness is associated with less experiential attachment (Sahdra *et al.*, 2016), less defensiveness following threats to the self (Niemiec *et al.*, 2010), and less intergroup bias (Lueke & Gibson, 2015, 2016; Tincher, Lebois, & Barsalou, 2016).

Despite considerable theorizing, there has been relatively little empirical testing of the possible mechanisms by which mindfulness might enhance prosociality. The present review therefore sought to conduct a narrative review of the studies that measured mediators of the effects of mindfulness interventions on prosocial behaviour.

Moderators of the link between mindfulness and prosocial behaviour

Modern approaches to meta-analytic techniques utilize moderation analyses to explain potential sources of variation in effect sizes across studies (Borenstein, Hedges, Higgins, & Rothstein, 2009). By understanding the variables that moderate the association of mindfulness with prosocial behaviour, mindfulness interventions can be targeted in a way that enhance their potential impact on prosociality. We next consider moderators of both correlational and intervention studies.

Moderators of correlational effects

Measure of prosocial behaviour

Self-report measures can be subject to social desirability and other biases, inflating correlations through the halo effects (Donaldson & Grant-Vallone, 2002). Such effects may mean that mindfulness is more strongly linked to prosocial behaviour when the behaviour is self-reported as opposed to rated by an observer.

Relationship with recipient of the prosocial act

There is substantial evidence that individuals are more likely to help known others such as one's mate, family member or a member of one's broader social network, than strangers, due to the ongoing nature of these relationships, in which helping behaviours are mutually beneficial(Maner & Gailliot, 2007). Based on this theorizing, we might expect

that mindfulness will have larger effects on helping behaviour towards known than unknown others. However, it may also be that mindfulness attenuates differences in prosocial behaviour towards known others and strangers. In support of this, mindfulness has also been shown to predict less intergroup bias (Berry *et al.*, 2018; Lueke & Gibson, 2015, 2016; Tincher *et al.*, 2016).

Age

The capacity to take the perspective of others is a developmental process that is less advanced among children and adolescents than adults (Kegan, 1982). It may be, therefore, that mindfulness has differential effects on prosocial behaviour for adolescents, emerging adults, and adults, respectively. Indeed, some studies have shown relatively modest relations between mindfulness and prosocial behaviour among adolescents (Sahdra, Ciarrochi, Parker, Marshall, & Heaven, 2015) and children (e.g., Flook, Goldberg, Pinger, & Davidson, 2015), while others have found relatively large effects among adults (e.g., Cameron & Fredrickson, 2015; Geurtzen, Scholte, Engels, Tak, & Zundert, 2014), as well as non-significant effects (e.g., Parent *et al.*, 2010).

Gender

Research has shown that women are more likely to engage in prosocial acts than men, although this may depend on the type of prosocial act (Espinosa & Kováˇ, 2015). Moreover, some systematic reviews have found evidence of different effects of mindfulness interventions by gender (e.g., Katz & Toner, 2013), while others have failed to find such gender differences (e.g., Sedlmeier *et al.*, 2012). It is therefore important to consider whether mindfulness has differential effects on prosociality in females and males.

Moderators of intervention effects

Type of mindfulness intervention

Our inclusive approach to studies of mindfulness allowed us to examine differences in content across mindfulness-based interventions. A key distinction in this regard is between mindfulness-based interventions that target other-oriented positive emotions such as kindness, empathy, and compassion as their primary focus and those that target mindful awareness of present-moment experience (Galante *et al.*, 2014; Keng *et al.*, 2011). Because of their more direct focus on cultivating positive emotions towards others, other-focused compassion interventions may be more strongly predictive of prosocial behaviour than those targeting mindful awareness alone (Weng *et al.*, 2013).

Intervention intensity

Mindfulness intervention research has developed in a diffuse way, with a broad range of intervention formats, intensities, and delivery modes employed (for reviews, see Chiesa & Serretti, 2011; Hofmann, Sawyer, Witt, & Oh, 2010; Khoury *et al.*, 2013). In the present context, there was considerable variation in the intensity of interventions studied, ranging from brief, 9-min experimental inductions of mindfulness(e.g., Berry *et al.*, 2018; Studies 2–4) to a month-long retreat (Montero-Marin, Puebla-Guedea, & Herrera-Mercadal, 2016).

There is meta-analytic evidence that intervention length makes a difference to the efficacy of mindfulness interventions among healthy adults in relation to outcomes such as well-being, stress, anxiety, and depression (e.g., Khoury *et al.*, 2015), while other meta-analyses have failed to find such effects (Carmody & Baer, 2009). We therefore tested whether more intensive interventions had differential effects upon prosociality relative to more brief interventions.

Type of control condition

Due to placebo effects, studies with waitlist controls are expected to yield higher effect sizes than those with specific and non-specific active controls, and there is evidence for this from mindfulness research across a range of outcome measures(for a review, see Goyal *et al.*, 2014). We therefore tested whether there were systematic differences in effect sizes between studies with waitlist controls, non-specific active controls, and specific active controls designed to serve as treatment-as-usual comparisons.

Randomization

Although non-randomization significantly limits the inferences that can be drawn from an intervention study (Campbell & Stanley, 1966), we included both randomized and non-randomized studies in this review, given the nascence of this field and the resultant value in taking a maximally inclusive approach. We conducted sensitivity analyses with non-randomized studies both included and excluded from the analysis and examined differences in effect sizes between both sets of studies.

The present study-2

The primary aim of the present study was to provide a meta-analytic review of the research on the association between mindfulness and prosocial behaviour. Based on the theoretical arguments and the empirical evidence discussed above, our hypotheses were as follows:

Hypothesis 1**:** Trait mindfulness will be positively correlated with prosocial behaviour.

Hypothesis 2**:** Mindfulness-based interventions will increase prosocial behaviour.

Hypothesis 3**:** Mindfulness-based interventions that cultivate positive, other-oriented emotions will have a larger effect on prosocial behaviour than those that cultivate mindfulness alone.

Method

Eligibility criteria

To be included in this review, studies needed to meet the following criteria: (1) Include quantitative, not qualitative, measures of mindfulness and prosocial behaviour. (2) Measure prosocial behaviour either through self-report (e.g., individual reports of their own incidence of prosocial acts) or through other-report (e.g., teacher or peer reports of students' prosocial behaviour). Prosocial attitudes and emotions, such as empathic concern and perspective-taking, were not included as outcome measures in this review, as they do not measure observable prosocial behaviour. (3)

Include either a manipulation or a quantitative measure of mindfulness. (4) Assess the relations between mindfulness and prosocial behaviour, including either an effect size (e.g., Cohen's *d*), sufficient information to calculate one, or have a corresponding author provide such information upon request. (5) Use an experimental (i.e., randomized controlled trial), longitudinal, or cross-sectional study design. Intervention studies with no control condition (i.e., case series designs) were excluded from the review because there is no way of knowing whether observed effects are a result of the intervention or an unrelated process (Kempen, 2011). (6) Full-text access available in English.

Literature search

Literature searches were conducted in the PubMed, EMBASE, CENTRAL, PsycINFO, and ProQuest Psychology databases in September 2017. Three sets of search terms were combined (1) 'mindful'* (for mindfulness and mindful), 'meditat'* (for meditation and meditative), and 'contemplative'; (2) 'prosocial', 'altruis'* (for altruistic and altruism), 'compassion'* (for compassion and compassionate), 'help'* (for help and helping), and 'care'; and to capture the focus of this paper on behavioural rather than cognitive or affective responses; (3) 'behav'* (for behavior and behavioral, as well as their UK-spelling equivalents), 'responding', 'response', and 'action'. This search produced 13,167 studies.

Study selection

Titles and abstracts of studies identified from the database searches were independently screened for eligibility by two authors. This process resulted in 196 studies. The full-text versions of the remaining studies were then screened by the same two authors for eligibility, and differences of opinion were resolved by consultation with three experienced mindfulness researchers. This resulted in the identification of 30 studies for inclusion.

As a secondary step, the corresponding authors of the studies identified from the database searches were emailed, seeking additional contributions (including unpublished data), consistent with the study's eligibility criteria. We received responses from nine authors (within the 1-month response window specified), resulting in an additional three studies that met our eligibility criteria (Parent, McKee, Rough, & Forehand, 2016; Parent, McKee, Anton, *et al.*, 2016; Schonert-Reichl *et al.*, 2015). Lastly, the reference lists of the articles identified from the literature search were inspected for any additional relevant articles. This did not yield any further studies for inclusion. On 15 October 2017, we concluded the literature search, with a total of 33 included studies.

Data extraction

Two researchers independently extracted data from the included studies. The following data were extracted: (1) publication author(s) and year; (2) study design (cross-sectional, longitudinal or experimental); (3) number of participants; (4) cell sizes (if experimental); (5) mean participant age and gender; (6) instrument used to measure or manipulate mindfulness; (7) instrument used to measure prosocial behaviour; and (8) the statistical result measuring the relationship between mindfulness and prosocial behaviour. There was 98% consistency between the two raters. These data, except for the effect sizes, are

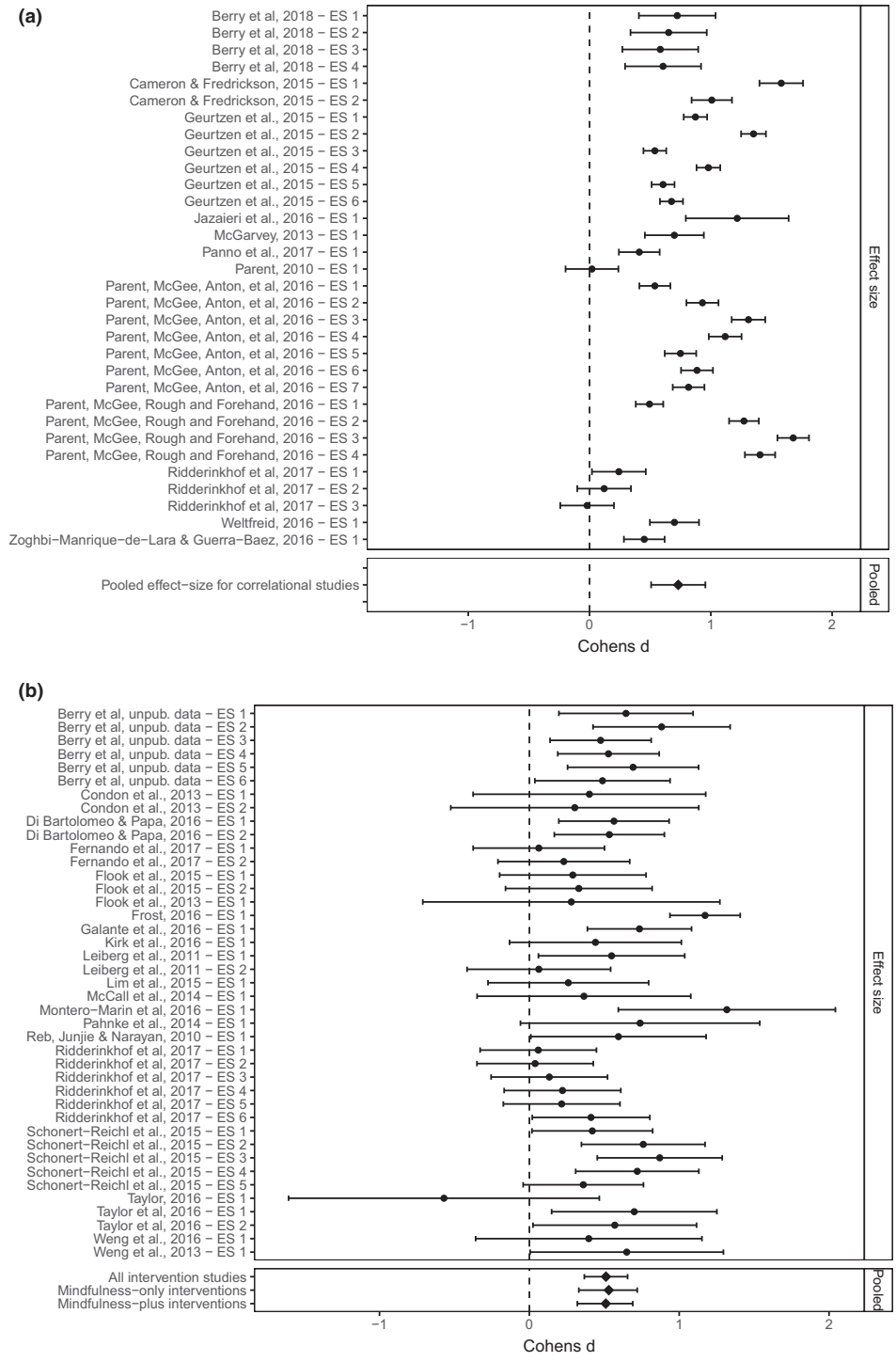

Figure 1. (a) Forest plot showing effect sizes and 95% confidence intervals for each correlational study plus pooled effects. (b). Forest plot showing effect sizes and 95% confidence intervals for each intervention study plus pooled effects. *Note.* ES = effect size.

included in Table S1. Effect sizes for all included studies are displayed in Figure 1, below. A description of the measures of mindfulness, mindfulness-based interventions, and prosocial behaviour included in this review also appears in Table S1.

Summary measures

All summary measures were converted to Cohen's *d* using Rosenthal's (1994) and Rosenthal's (1991) conversion formulas. Cohen's *d* effect sizes were defined as .2 (small), .5 (medium), and .8 (large). Cohen's *d* effect sizes from correlational studies were derived from Pearson's *r* coefficients, while those from intervention studies were derived from an odds ratio, an eta-squared statistic, an adjusted mean difference score (i.e., in pre-test–post-test-control group designs; Morris, 2008), or a post-test-only mean difference (i.e., where baseline scores on the outcome variable were not measured, as in laboratory studies of brief mindfulness-based interventions). This way, all available information for calculating effect sizes was used. In calculating Cohen's *d* from pre-test–post-test-control group designs, we used standard deviations for each condition, combining both pre-test and post-test standard deviations, per the methods outlined in Rosenthal (1991; 1994). Where a study did not report the information needed to convert relevant summary measures to Cohen's *d*, we contacted the lead author to request this information. The authors of six studies were contacted in relation to a total of nine effects. Three authors provided the required information for five of these effects within the 4-week window that was specified.

Of the three remaining studies, one study reported two effects as standardized regression coefficients (Cameron & Fredrickson, 2015), so could not be precisely converted to Cohen's *d*. Using the methods outlined by Peterson and Brown (2005), Pearson's *r* correlations were imputed for these effects and sensitivity analyses were conducted to test whether the inclusion of these imputed statistics changed the results of the meta-analysis. There was no evidence that inclusion of these two effects, which were correlational, significantly changed the pooled effect size for correlational studies, relative to a model where they were excluded from the analyses ($\Delta\chi^2 = 2.57$, $p = .283$). The imputed data were therefore included in the present analyses. Finally, there were two remaining studies that did not report effects that could be used, and we were unable to obtain the required information from the authors (Kemeny *et al.*, 2012; Lueke & Gibson, 2016). These studies were dropped from the meta-analysis. The final analysis consisted of 31 studies and 73 effects.

Risk of bias in individual studies

To assess the risk of bias in studies using both experimental and cross-sectional designs, the Strengthening the Reporting of Observational Studies in Epidemiology (Von Elm *et al.*, 2007) guide and the Consolidated Standards of Reporting Trials (Schulz, Altman & Moher, 2010), or CONSORT, statement were adapted. The risk of bias criteria included (1) description of participant eligibility criteria, (2) random allocation of study participants (intervention studies), (3) among correlational studies, a psychometrically valid assessment of mindfulness (i.e., at least one published study supporting reliability and validity of the instrument), (4) a psychometrically valid assessment of prosocial behaviour, (5) valid assessment of prosocial behaviour, (6) power calculation reported and study adequately powered to detect hypothesized relations, and (7) relevant covariates included in the analyses.

Two researchers independently rated studies on each of the above criteria, assigning either a 1 (present and explicitly described) or a 0 (absent or inadequately described) to each. Ratings were consistent between raters in 221 of the 234 cases. The 13 discrepancies were resolved by discussion between the two researchers. Studies that met less than half of the criteria were considered to have a high risk of bias (Higgins, Altman, & Sterne, 2011). Studies that met at least half of the criteria were deemed as having a low risk of bias implying that results represent unbiased estimates of the true effect (Higgins *et al.*, 2011).

Meta-analytic procedures

We used a multilevel structural equation modelling approach to meta-analysis (see Cheung, 2014; for an introduction to this method). This approach allows the researcher to fit models with dependent effect sizes nested within studies (e.g., multiple comparisons within an experimental study, or multiple correlation coefficients within a cross-sectional study), using latent variables (see Cheung, 2014). To do this, Level 2 (effect sizes within-study) and Level 3 (between-study) sources of variance are estimated, using a structural equation modelling (SEM) approach. In the SEM, studies are treated as 'individuals' and effect sizes are treated as non-independent 'variables', thereby enabling non-independence to be explicitly estimated in the model (Cheung, 2014). A key advantage of this approach is that it enables the use of all available information from non-independent effect sizes within studies, in contrast to many alternative approaches to handling dependence in meta-analyses, in which information is lost (Ahn, Ames, & Myers, 2012; Cheung, 2014). Other notable advantages of this approach include handling missing covariates using the full information maximum-likelihood approach, rather than less precise *ad hoc* approaches; placing flexible constraints on parameters; and constructing more accurate confidence intervals using likelihood-based approaches (Cheung, 2014).

To estimate the degree of 'true' heterogeneity in pooled effect sizes, as opposed to variation due to sampling error, the I^2 statistic was used (Borenstein *et al.*, 2009). As a rule of thumb, 25%, 50%, and 75% have been identified as low, medium, and high levels of heterogeneity respectively (Higgins & Thompson, 2002). Following similar approaches elsewhere, moderation analysis was conducted where $I^2 > 25\%$ and with a minimum of 4 effect sizes (Fu *et al.*, 2011). For completeness, we also report the τ statistic, which is the variance in effect sizes at the population level (Cheung, 2015). For each moderation analysis, we calculated the proportion of variance explained by the inclusion of the moderating variable (R^2) and the chi-squared test of whether the moderated model differed significantly from the model excluding the moderator (Borenstein *et al.*, 2009; Cheung, 2014). Because we modelled within-study and between-study sources of variation separately, we report moderation (R^2) and heterogeneity statistics (I^2 and τ) for each level of analysis.

To ensure that effects based on a relatively large sample had a greater influence on pooled effects, effect sizes were weighted using the reciprocal of each effect's sampling variance. We utilized a three-level approach, which includes a random-effects component, a between-study fixed effects component, and a within-study fixed effects component(Cheung, 2014).

All analyses were conducted in R Version 3.1.2 (R Core Team, 2018), and meta-analyses were conducted using the *metaSEM* package (Cheung, 2015). Unconditional mixed-effects structural equation models were used to calculate the overall pooled effect size

(pooled Cohen's *d*). For each pooled effect, 95% confidence intervals were calculated using a likelihood-based approach (Cheung, 2014).

Results

Study characteristics

Of the 31 studies included in the meta-analysis, there were 12 correlational (n = 13,820) and 21 intervention studies (n = 3,421), with a total of 17,241 participants.[1] Of the 12 correlational studies, 10 were published and two were dissertations (McGarvey, 2010; Weltfreid, 2016). Of the 21 intervention studies, 18 were published, two were unpublished conference papers (Frost, 2016; Reb, Junjie, & Narayanan, 2010), and one was a dissertation (Taylor, 2016). Nineteen of the 21 intervention studies used a randomized controlled design, while two studies used a non-randomized matched controlled design (McCall, Steinbeis, Ricard, & Singer, 2014; Montero-Marin *et al.*, 2016). The interventions used can be classified into mindfulness-plus-prosocial-emotions interventions (k = 9) and mindfulness-only interventions (k = 13).[2] Mindfulness-plus-prosocial-emotions interventions ranged in length from 6 (Leiberg, Klimecki, & Singer, 2011) to 36 hours(Taylor *et al.*, 2016). Mindfulness-only interventions ranged from 5 to 10 min (Berry *et al.*, 2018; Ridderinkhof *et al.*, 2017) to 224 hours (Montero-Marin *et al.*, 2016). Control conditions for the 21 interventions studies were waitlist (k = 8), non-specific active (k = 6), and specific active (k = 7). The non-specific active control interventions were goal-setting training (k = 2), memory training (k = 2) a mental imagery exercise (k = 1), and a writing task (k = 1). Specific active controls were cognitive reappraisal training (k = 2), relaxation exercises (k = 2), social responsibility training (k = 1), giving a speech on civic service (k = 1), and light exercise training (k = 1). Study characteristics are summarized in Table S1.

Risk of bias and publication bias

Risk of bias assessment was conducted by study (n = 31). There was 98% agreement between the two raters on risk of bias ratings, and all discrepancies were resolved by discussion between the two raters. Six studies were rated as having a high risk of bias and 25 studies were rated as having a low risk of bias. Risk of bias did not moderate pooled effects for either correlational ($\Delta\chi^2$ = 1.34, p = .249, R^2 = .09) or intervention studies ($\Delta\chi^2$ = 0.72, p = .390, R^2 = .12). Further, we assessed publication bias across studies using funnel plots, Egger's test, testing moderation by publication status, and moderation by the standard errors of effect sizes. We found no evidence of publication bias for correlational studies, but some evidence among intervention studies. Although several contemporary approaches to assessing publication bias have been proposed, such as p-curve, p-uniform, PET-PEESE, 3PSM (for reviews, see Carter, Schönbrodt, Gervais, & Hilgard, 2018; and McShane, Böckenholt, & Hansen, 2016), a key advantage of the moderation tests we undertook is that they do not assume independence among effect sizes, which was a significant issue in the data included in this review. The

[1] *Two papers included both correlational and intervention studies, meaning they were counted twice in this calculation (Berry* et al.*, 2018; Ridderinkhof, de Bruin, Brummelman, & Bögels, 2017).*

[2] *One study (Condon, Desbordes, Miller, & DeSteno, 2013) included both a mindfulness-only and a compassion-focused intervention, meaning it was counted twice in these calculations.*

Table 1. Results of the meta-analysis and moderator analyses for correlational studies

Variable	*k*	#ES	*n*	*d*	95% CI		I_2^2	I_3^2	*Q*-stat	ANOVA		
					Lower	Upper				$\Delta\chi^2$	R_2^2	R_3^2
Overall relationship	12	32	13,820	.73***	0.51	0.96	.46	.51	895.03			
Measure of prosocial behaviour												
Self-report	10	25	13,018	.89***	0.80	0.98	.97	.00	748.07	2.99†	0%	53%
Observer-report	2	7	802	.37***	0.19	0.79	.00	.78	29.13			
Relationship with recipient												
Known person	6	20	11,695	.91***	0.76	1.05	.97	.00	628.94	4.11*	0%	75%
Stranger	3	8	1,081	.38***	0.13	0.63	.00	.73	29.88			
Age												
Adolescence (12–18)	2	4	753	.24***	0.01	0.47	.18	.43	10.25	4.56*	0%	77%
Emerging adult (18-25)	2	5	528	.66***	0.55	0.78	.00	.00	0.65			
Adult (25 +)	7	22	12,259	.94***	0.76	1.12	.97	.00	682.9			
Gender												
Mixed gender (33–66%)	8	22	7,575	.67***	0.41	0.92	.47	.49	588.56	0.64	0%	0%
Majority female (66%+)	7	16	6,245	.87***	0.45	1.28	.52	.46	305.65			

Notes. *k* = number of studies;#ES = effect size; *n* = number of participants; *d* = Cohen's *d*; I_2^2 = non-error heterogeneity within studies; I_3^2 = non-error heterogeneity between studies; R_2^2 = Explained variance within studies; R_3^2 = Explained variance between studies.
'Mindfulness-only' = interventions that solely train mindfulness; 'Mindfulness-plus' = Interventions that train both mindfulness and prosocial emotions.
†$p < .10$; *$p < .05$; ***$p < .001$.

Table 2. Results of the meta-analysis and moderator analyses for intervention studies

					95% CI					ANOVA		
Variable	*k*	#ES	*n*	*d*	Lower	Upper	I_2^2	I_3^2	*Q*-stat	$\Delta\chi^2$	R_2^2	R_3^2
Intervention studies	21	41	3,421	.51***	0.37	0.66	.00	.53	82.94			
Intervention type												
Mindfulness-only	13	30	2,834	.53***	0.33	0.72	.00	.63	72.71	0.94	0%	0%
Mindfulness-plus	9	11	587	.51***	0.32	0.69	.00	.00	10.22			
Measure of prosocial behaviour												
Self-report	5	6	264	.61***	0.35	0.88	.00	.00	5.96	0.61	0%	2%
Observer-report	17	35	3,157	.48***	0.32	0.64	.00	.55	76.35			
Relationship with recipient												
Known person	5	10	722	.49***	0.22	0.79	.00	.09	11.69	0.25	37%	0%
Stranger	14	28	2,577	.49***	0.33	0.66	.00	.58	65.52			
Age												
Childhood (<12)	2	7	631	.55***	0.39	0.71	.00	.00	7.03	1.92	0%	21%
Adolescence (12–18)	3	8	708	.34***	0.13	0.56	.00	.28	9.28			
Emerging adult (18–25)	5	13	1,028	.43***	0.36	0.50	.00	.21	12.79			
Adult (25+)	8	9	736	.68***	0.37	1.00	.00	.59	22.8			
Gender												
Mixed gender (33–66%)	10	21	1,995	.52***	0.27	0.77	.00	.69	68.34	0.14	0%	0%
Majority female (66%+)	7	15	1,108	.54***	0.48	0.61	.00	.00	13.47			
Intervention intensity												
<1 hr	6	18	2,033	.54***	0.25	0.82	.00	.74	59.04	0.01	0%	0%
1–10 hrs	7	13	1,034	.51***	0.34	0.69	.06	.04	13.32			
More than 10 hrs	7	9	321	.55***	0.30	0.81	.00	.00	10.21			
Control condition type												
Waitlist	10	15	832	.40***	0.23	0.57	.00	.00	13.95	0.30	0%	0%
Non-specific active	7	14	1,582	.52***	0.25	0.78	.00	.72	51.8			
Specific active	7	12	1,007	.53***	0.36	0.71	.00	.24	13.83			
Allocation to condition												
Non-randomized	4	5	226	.40***	0.38	0.83	.57	.00	11.8	0.07	0%	2%
Randomized	17	36	3,195	.53***	0.37	0.68	.00	.52	70.38			

Notes. *k* = number of studies;#ES = effect size; *n* = number of participants; *d* = Cohen's *d*; I_2^2 = non-error heterogeneity within studies; I_3^2 = non-error heterogeneity between studies; R_2^2 = Explained variance within studies; R_3^2 = Explained variance between studies.
'Mindfulness-only' = interventions that solely train mindfulness; 'Mindfulness-plus' = Interventions that train both mindfulness and prosocial emotions.
***$p < .001$.

complete results of the risk of bias and publication bias assessments are reported in Appendix S1.

Main analysis

Next, we ran an unconditional multilevel model and found support for both Hypotheses 1 and 2. As displayed in Table 1, dispositional mindfulness was positively associated with prosocial behaviour, with a medium-to-large effect size (d = .73 CI 95% [0.51 to 0.96]), using the criteria proposed by Cohen (1992). Further, as shown in Table 2, mindfulness-based interventions predicted greater prosocial behaviour, with a medium-sized pooled effect(d = .51 CI 95% [0.37 to 0.66]). These effect sizes were not statistically different from one another (d = .14, SE = .11, CI [−0.06, 0.35]).

Correlational effects

Individual study effect sizes, along with pooled effect sizes, are shown in Figure 1a. As the figure shows, there was consistency among effects, with all but three effects larger than zero at the 95% CI level. There was a moderate level of heterogeneity across effects for the 12 correlational studies (I^2 = .51), indicating that exploring potential moderators of these effects was warranted (see Table 1).

Measure of prosocial behaviour

We found some support for the proposition that self-reports of prosociality are larger than observer ratings (Donaldson & Grant-Vallone, 2002), with differences in the way prosocial behaviour was measured moderating the association between mindfulness and prosocial behaviour but at the $p < .10$ level ($\Delta\chi^2 = 2.99$; $p < .08$; $R^2_3 = 53\%$). As shown in Table 1, mindfulness had a small-to-medium association with other-report measures of prosocial behaviour (d = .37, 95% CI [0.19, 0.79]) and a large association with self-report measures of prosociality (d = .89, 95% CI [0.80, 0.98]), and the confidence intervals around these estimates did not overlap.

Recipient of prosocial behaviour

We next tested whether mindfulness was more strongly related to prosocial behaviour when the recipient of the prosocial act was known as opposed to being a stranger and found support for this ($\Delta\chi^2 = 4.11$; $p < .05$; R^2 = 46%). As Table 1 shows, the association between mindfulness and prosociality was large when the recipient was a known person (d = .91, 95% CI [0.76, 1.05]), and small-to-medium when they were a stranger (d = .38, 95% CI [0.13, 0.63]). The 95% confidence intervals around these estimates did not overlap, suggesting that replication studies would find a similar difference, with a relatively high likelihood (Cumming & Maillardet, 2006). To explore the possibility that differences in helping behaviour towards known versus unknown recipients were an artefact of the way in which prosocial behaviour was measured (i.e., self- versus other-reports), we conducted supplementary analyses, with self-report measures of prosociality included in the model as a secondary moderator. However, we did not find evidence for this ($\Delta\chi^2 = 1.48$; $p = .223$; R^2 difference = .171).

Age

Differences in subjects' age moderated the relations between trait mindfulness and prosocial behaviour ($\Delta\chi^2 = 4.56$; $p < .05$; R^2 = 77%). We found small effects for adolescents(d = .24, 95% CI [0.01, 0.47]), medium-sized effects for emerging adults (d = .66, 95% CI [0.55, 0.78]), and large effects for adults(d = .94, 95% CI [0.76, 1.12]).[3]

Gender

In contrast with meta-analytic evidence that gender moderates the link between mindfulness and other outcomes(e.g., Katz & Toner, 2013), and as shown in Table 1, we did not find evidence for this in relation to prosocial behaviour.

[3] *There were no studies of trait mindfulness among pre-adolescents included in the review.*

Intervention effects

We found support for our second hypothesis, with a medium-sized pooled effect of mindfulness interventions predicting increases in prosocial behaviour, relative to controls. Figure 1b illustrates the effect sizes, and confidence intervals around each, for these studies.

We next tested our third hypothesis that interventions that explicitly trained prosocial emotions in addition to mindfulness would have larger effects on prosociality than those that trained mindfulness alone. As shown in Table 2, effect sizes from studies of these two intervention types were both medium-sized and non-different from one another. To test whether these differences in intervention emphases lead to different effects on prosocial behaviour, we conducted sensitivity analyses with the mindfulness-plus-prosocial-emotions interventions (k = 9) excluded from the analyses. The difference between the two models was non-significant ($\Delta\chi^2$ = 0.095, p = .948, ΔR^2 = .001), suggesting that combining the two intervention types in subsequent moderation analyses was warranted.

Table 2 shows that there was moderate heterogeneity associated with the pooled effect between intervention studies (I^2_3 = .53). Further, there was moderate heterogeneity associated with the pooled effect across the 'mindfulness-only' studies (I^2_3 = .63), but not between the 'mindfulness-plus' studies (I^2_2 = .00). To be conservative, we conducted moderation analyses on both intervention types combined, as it is possible that study samples were not representative of the overall population, meaning that a low I^2 statistic does not rule out the possibility that variation in effects can be explained by third variables (Borenstein *et al.*, 2009).

Results from the moderation analyses are presented in Table 2. We did not find evidence that any of the variables we tested moderated intervention effects. Instead, we found consistent, medium-sized effects of mindfulness interventions on prosociality across recipient relationship, gender, age categories, intervention intensity, and type of control condition.

Mediation effects

We identified five mindfulness studies that examined potential mechanisms of change. Lim, Condon, and Desteno (2015) tested for but did not find differences in empathic accuracy between individuals who completed a 3-week online mindfulness meditation programme and those who undertook a cognitive training programme with the same format, suggesting that empathic accuracy could not be a mediator of the effect of mindfulness meditation on prosociality(Lim *et al.*, 2015). In another study, Montero-Marin *et al.* (2016) tested whether individual differences in non-attachment mediated the effect of a 1-month Vipassana retreat on self-reported cooperativeness, but similarly did not find support for this.

Most recently, Berry *et al.* (2018; Studies 2-4) tested the role of empathic concern, a construct closely related to compassion, as a mediator of the effect of mindfulness meditation on prosociality. Across three studies, the authors found that brief (9 min) mindfulness meditation enhanced prosocial responding and that these effects were consistently mediated by empathic concern.

A further two studies of interventions combining mindfulness and prosocial emotions examined potential mechanisms of change. Weng *et al.* (2013) found that increases in altruistic behaviour following mindfulness-based compassion meditation were associated with the activation of neural networks associated with understanding others' affective states, executive function, and the experience of positive emotions. Further, Reb *et al.*

(2010) found that increases in altruistic behaviour following loving-kindness meditation were completely accounted for by increases in positive affect.

Discussion

Relatively little is known about whether and how mindfulness – as both a personality variable and as a meditative practice – enhances prosocial behaviour. This review is timely, given recent debates about whether or not secular mindfulness supports the cultivation of ethical behaviour(e.g., Monteiro *et al.*, 2014), and increasing research interest in the interpersonal effects of mindfulness practice (see Berry & Brown, 2017; Condon, 2017).

We found support for our hypotheses that individual differences in mindfulness would be linked to greater prosocial behaviour (H1) and that mindfulness interventions would predict increases in helping behaviours (H2). These effects were medium-sized, non-different from one another, and provide converging, multimethod evidence for the links between mindfulness and prosociality. In relation to our first hypothesis, our findings suggest that by having a non-judgementally aware disposition towards one's experience, individuals are more likely to respond to the needs of others in helpful ways. Preliminary mediation analysis indicates that this may happen via increases in empathic concern, emotion regulation, and positive affect.

Results from our moderation analyses provide a number of insights regarding the links between trait mindfulness and helping behaviour. First, we found that that mindfulness positively correlated with both self- and other-reports of prosociality. Second, we found that trait mindfulness was associated with more helping behaviour towards known than unknown others, but that both effects were positive and greater than zero. Notably, we did not find evidence that these differences were an artefact of the way in which prosocial behaviour was measured (i.e., self- versus other-reports). Our findings suggest that mindfulness may reinforce differences in cooperation towards kin versus strangers that are consistently displayed by humans (Maner & Gailliot, 2007). However, intriguingly, these findings run counter to a recent body of research suggesting that mindfulness meditation serves to inhibit intergroup biases (Berry *et al.*, 2018; Lueke & Gibson, 2015, 2016; Tincher *et al.*, 2016). Possible reasons for this are discussed below.

Third, we found evidence that mindfulness was most strongly linked to prosociality among adults as compared to emerging adults and adolescents. This finding is consistent with the idea that perspective-taking ability develops with age and is more advanced among adults than children (Kegan, 1982). However, it is notable that the mindfulness–prosociality link was positive for all age categories.

Regarding the links between mindfulness *interventions* and prosociality, we did not find support for our third and final prediction (H3) that mindfulness interventions that focus on the cultivation of prosocial emotions would have a larger effect on helping behaviour than those that focus only on cultivating mindful awareness. This suggests that mindfulness by itself is sufficient to produce increases in helping behaviour and that there may not be benefits (in terms of prosocial behaviour) in combining positive, other-oriented emotions with mindfulness, beyond those generated by training mindfulness alone. This explanation is consistent with results from the correlational analyses, suggesting that non-judgemental awareness alone (measured using instruments such as the Mindful Attention Awareness Scale, Brown & Ryan, 2003; and the Five Facets of Mindfulness Questionnaire, Baer *et al.*, 2006) fosters prosocial responding to another's

suffering. More research is needed, however, to more robustly test this preliminary finding, for example, by using more tightly controlled experimental designs.

Our review of mediation results from mindfulness intervention studies suggests that mindfulness meditation enhances prosocial behaviours via increases in empathic concern – also commonly studied as compassion (Goetz, Keltner, & Simon-Thomas, 2010) – and that mindfulness-based compassion meditation may increase prosociality via greater emotion regulation and positive affect. Although very little research has examined possible mediators of the link between mindfulness interventions and prosociality, the evidence to-date does not support the role of more cognitive factors such as empathic accuracy in mediating the effects of mindfulness meditation on prosociality (Lim *et al.*, 2015).

Regarding moderators of intervention effects, we did not find differences in effect sizes between known and unknown recipients of helping behaviour. This contrasts with findings from the correlational studies, where differences ran along expected lines – with larger effects for known relative to unknown others. It may be that the practice of mindfulness leads to states of mind that are able to overcome intergroup biases, and there is a body of experimental evidence to support this (Berry *et al.*, 2018; Lueke & Gibson, 2016; Tincher *et al.*, 2016). However, these experimental effects are for measures that immediately followed mindfulness meditation. It may be, therefore, that the effects of mindfulness meditation on intergroup biases are relatively short-lived, explaining why they are not reflected across studies of trait mindfulness and prosociality. Further research is needed to explore how mindfulness meditation influences prosociality towards known and unknown others over time.

Consistent with findings from correlational studies, effect sizes appeared larger for self- than observer-report measures of prosociality, though these differences were not significant among the intervention studies. Further, we did not find evidence that age or gender moderated intervention effects. Instead, we found medium-sized effects across age and gender categories, suggesting that mindfulness meditation facilitates prosocial responding across a spectrum of individuals and helper–receiver relations.

We did not find evidence that variables relating to study design, namely intervention intensity, type of control used, or randomization, moderated intervention effects. The results regarding intervention intensity are surprising, as they suggest that relatively brief interventions (i.e., <1 hr in duration) have similar-sized effects on prosociality as multisession interventions lasting between 1 and 10 hours, up to a 1-month intensive retreat (Montero-Marin *et al.*, 2016). It is likely that third variables, such as length of follow-up (i.e., longer treatments have longer follow-ups, which allows the mindfulness effects to wear off), or variation in the sensitivity of the dependent variables across studies, account for these results. Future research could explore whether this is the case.

Lastly, we found some evidence of publication bias among intervention studies. While every effort was made to include null and unpublished data in this analysis, including by emailing authors, searching abstracts, and consulting reference lists of included studies, we cannot rule out the possibility that the effects for intervention studies may have been subject to the file-drawer problem. Further intervention research in this domain should aim to adhere to research best practice, including by publishing or registering intervention protocols ex ante, and using manualized protocols.

Limitations

Although this review used state-of-the-art meta-analytic methods, the nascence of this research domain and diversity among included studies resulted in several limitations. First,

it is possible that non-significant effects were due to a lack of statistical power. Meta-analyses generally substantially increase statistical power, especially where individual studies have relatively low sample sizes and the number of studies included in the meta-analysis is large (Borenstein *et al.*, 2009). Further, in random-effects meta-analysis, the method used here, when between-study variation is relatively low, the reduction in power due to random effects is minimal (Borenstein *et al.*, 2009). In the present study, between-study variance was moderate for both correlational (I_3^2 = .51) and intervention studies (I_3^2 = .53). Moreover, for the intervention studies, where no evidence of moderation was found, power to detect actual effects of d = .30 was greater than 95% (assuming a sample-size of n = 25 per condition). A lack of power to detect actual effects was therefore unlikely in this review. However, due to insufficient cell size, we were unable to test the possibility that multiple moderating variables (e.g., measure of prosocial behaviour, relationship to the recipient, and age) combined in distinct ways to predict prosocial behaviour.

A second limitation is that for nearly all studies included in this review, there was no evidence provided that the sample was representative of the broader population from which the sample was drawn. Third, and more specific to the present review, there was substantial diversity in the mindfulness interventions included in this review. This diversity limits the inferences that can be drawn from the pooled effects reported here.

Third, this review was not able to resolve the extent to which high levels of mindfulness, versus increases in mindfulness, lead to more prosocial behaviour. This issue has been recently highlighted as being generally overlooked in social psychology (Bless & Burger, 2016). Experimental manipulations of mindfulness may lead to relatively high mindfulness, and this may in turn cause prosocial behaviour. This is often the assumption of research studies in this area. However, it may be that *changes* in mindfulness, rather than high *levels* of mindfulness lead to prosocial outcomes. For example, increases in non-judgemental awareness may signal safety and the likelihood of social reward, which leads to prosocial behaviour – similar to findings from studies of positive mood (Schwarz & Clore, 1983). While teasing apart these different sources of change in prosocial behaviour was beyond the scope of this review, Bless and Burger (2016) suggest several ways of exploring this, including via pre- and post-measures of the independent variable in an experiment (to isolate change effects), correlational analysis (to isolate level effects), and the use of longitudinal designs (to identify both level and change effects).

Conclusion

We found converging, multimethod evidence for theoretical claims that mindfulness increases the incidence of prosocial acts toward others, with medium-sized pooled effects for both correlational and intervention studies. Interventions that trained mindfulness as their sole focus had similar-sized effects on prosocial responding as those that combined mindfulness with prosocial emotions, suggesting that non-judgemental awareness alone fosters helping behaviour to a similar degree as compassion-oriented meditation. Evidence to-date suggests that these effects may occur via greater empathic concern, emotion regulation and positive affect. Lastly, we found that mindfulness interventions had similar-sized effects on prosociality towards known and unknown others, inline with an emerging body evidence that mindfulness mediation reduces intergroup biases. The results of the present review suggest that mindfulness fosters ethical and cooperative behaviour across a range of interpersonal contexts.

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Supporting Information

The following supporting information may be found in the online edition of the article:

Figure S1. Funnel plot for correlational studies. *Note.* 95% confidence intervals are indicated by parallel lines either side of the pooled effect.
Figure S2. Funnel plot for intervention studies. *Note.* 95% confidence intervals are indicated by parallel lines either side of the pooled effect.
Table S1. Characteristics of studies included in the meta-analysis.
Table S2. Risk of bias assessment.

Appendix S1. R Code.