In Press at *Personality and Social Psychology Bulletin*

Does Mindfulness Training without Explicit Ethics-Based Instruction Promote Prosocial Behaviors? A Meta-Analysis

Daniel R. Berry, Jonathan P. Hoerr, Selena Cesko, Amir Alayoubi, Kevin Carpio, Hannah Zirzow, Wesley Walters, Genny Scram, Katie Rodriguez, and Vanessa Beaver

California State University San Marcos

Authors Notes

Daniel R. Berry, Selena Cesko, Amir Alayoubi, Kevin Carpio, Hannah Zirzow, Wesley Walters, Jonathan P. Hoerr, Genny Scram, Katie Rodriguez, and Vanessa Beaver, Department of Psychology, California State University San Marcos.

Ideas and findings presented in the manuscript have been disseminated in presentations at the Society for Personality and Social Psychology

The authors declare that they have no conflicts of interest. The authors declare that no funding was used to conduct this research. The authors declare that this manuscript is not currently being considered by another publication. The findings in this manuscript have been presented as a poster at the 2019 Society for Personality and Social Psychology Convention.

Corresponding author: Daniel R. Berry, Department of Psychology, California State University San Marcos, 333 Twin Oaks Valley Road, San Marcos, California, 92096. E-mail: drberry@csusm.edu

Abstract

Scholarly discourse has raised concerns about the gravitas of secular mindfulness trainings in promoting prosocial outgrowths, as these trainings lack ethics-based concepts found in contemplative traditions. Random effects meta-analyses were conducted to test whether mindfulness trainings absent explicit ethics-based instructions promote prosocial action. There was a range of small to medium standardized mean difference effect sizes of mindfulness training on overt acts of prosociality when compared to active and inactive controls (k = 29, N = 3100, g = .426, 95% CI(g) = [.304 .549]). Reliable effect size estimates were found for single-session interventions that measured prosocial behavior immediately after training. Mindfulness training also reliably promotes compassionate (but not instrumental or generous) helping and reliably reduces prejudice and retaliation. Publication bias analyses indicated that the reliability of these findings was not wholly dependent on selective reporting. Implications for the science of secular mindfulness training on prosocial action is discussed.

Keywords: meta-analysis; mindfulness, mindfulness training, prosocial

Does Mindfulness Training without Explicit Ethics-Based Instruction Promote Prosocial Behaviors? A Meta-Analysis

Research on the benefits of *mindfulness*—a sustained, receptive attention to present-moment experiences (Anālayo, 2003)—has grown exponentially over the past 20 years (Brown, Creswell, & Ryan, 2015). While early research on mindfulness has focused on its correlates and consequences for personal well-being (e.g., Davidson, 2010), contemplative theories have long emphasized the value of mindfulness-based (and other forms of) meditation practice for facilitating prosocial action (e.g., generosity, compassion, gratitude; Davidson & Harrington, 2002), acts that promote others' well-being (Tomasello, 2009). The more recent science of mindfulness lends support for these theories (see Condon, 2018; Karremans & Papies, 2017 for reviews). Specifically, brief secularized forms of mindfulness trainings, relative to active and inactive controls, have been shown to promote prosocial emotions and/or behavior (e.g., Berry et al., 2018; Condon, Desbordes, Miller, & DeSteno, 2013; Lim, Condon, & DeSteno, 2015).

Scholarly debate has raised concerns, however, about the gravitas of secular mindfulness trainings used in recent studies. Secularized mindfulness trainings often lack grounding in explicit ethics-based instructions found in contemplative traditions (see Bodhi, 2011; Montiero et al., 2015 for reviews), and writers have suggested that such practices may serve pursuit of malevolent, self-centered action (e.g., greed; Dunne, 2015) or may lack appropriate context for enhancing prosocial action (Condon, 2018; Montiero et al., 2015). In a recent experiment, Chen and Jordan (2018) found that pairing mindfulness training with explicit instructions in ethics (but not mindfulness training itself) increased a charitable donation given to a stranger relative to an active control. Another important finding from Chen and Jordan's research indicates that mindfulness training (with and without explicit instruction in ethics) promoted charitable

donations, relative to active controls, among participants high in trait empathy. These findings provide initial evidence that the effect of mindfulness on prosociality may be contingent on an ethical framework either provided through didactic instruction or facilitated by pre-existing proclivities toward prosociality.

Boundaries to the prosocial outgrowths of meditation, which include mindfulness-based practices, have also been reported. Kreplin, Farias, and Brazil (2018) meta-analyzed experiments pitting meditation interventions against various control interventions on an amalgam of prosocial outcomes (e.g., compassion, gratitude, forgiveness, prejudice). Kreplin and colleagues found that meditation promoted prosociality only when one of the study co-authors facilitated the meditation, and when inactive controls were used. This meta-analysis is important for informing about the limitations of mindfulness-integrated interventions in enhancing prosocial action. Specifically, training in mindfulness and related meditative practices may not enhance prosociality at all or may do so with less potency.

As important as this work has been, we note, and so have Kreplin and colleagues (2018), that there are two serious threats to the internal validity of the recent literature on the prosocial outgrowths of mindfulness not addressed in recent meta-analyses on the topic. Internal validity refers to the extent to which changes in an outcome are attributable to an experimental manipulation and not extraneous variables (Brewer, 2000; Campbell, 1957), and in the present research, it refers to the extent to which increases in prosociality are attributable to manipulating mindfulness. First, the trainings in these studies are multimodal in form, including various types of meditation not specific to mindfulness. These multimodal trainings can include components alongside mindfulness that explicitly encourage prosociality (e.g., loving-kindness, social support, social emotion regulation), such as the “ethical mindfulness” intervention used by Chen

and Jordan (2018). Use of explicit ethics-based language may introduce differential demands for prosociality compared to mindfulness-based practices. That is, ethics-based meditation intervention participants may show enhanced prosociality because they think that is an expected outcome of meditation practice, and one recent experiment highlights this problem. Williams, Poljacik, Decety, and Nusbaum (2018) showed that exposure to loving-kindness language (but not loving-kindness practice itself), relative to a tightly matched control, increased sensitivity to the imagined pain of others. This may indicate that the prosociality-enhancing effects of mindfulness-based meditation training with explicit ethics-based instruction could be due in part or completely to priming prosocial responsiveness. Thus, scholars concerned about the fidelity of secular mindfulness training have primarily focused on the apparent lack of construct validity of secular mindfulness interventions—the extent to which a meditation intervention produces a state of mindfulness. By adding ethical concepts alongside mindfulness training, however, we argue that we may threaten the construct validity of our interventions and more importantly the internal validity of claims made about the effects of mindfulness interventions on prosocial behavior.

A second limitation to the studies on the prosocial consequences of mindfulness is that many use self-report measures of prosociality, introducing possible contamination by social desirability bias (see Kreplin et al., 2018). This second limitation is important to consider by itself, and because it may compound the biases in compassion-based meditations. Differential priming of prosocial concepts may exacerbate socially desirable reporting when self-report measures are used (e.g., Cialdini et al., 1987).

Donald and colleagues (2018) recently conducted a meta-analysis that incorporated moderators to isolate demand characteristics associated with explicit training in ethics and social

desirability biases tied to self-report of prosocial behavior. Their analyses showed that mindfulness training with and without explicit ethics-based instructions were equally likely to promote prosociality, and that the mindfulness effects on prosociality were not specific to self-report and overt indicators of prosociality. Though promising, we note that these moderator analyses do not obviate common threats to internal validity found in this research area. As shown in Figure 1, these analyses are akin to main effects in a 2x2 between subjects factorial design, in which only the marginal means are compared. Social desirability biases are not isolated in the "ethics-based" and "mindfulness only" subdistributions. It is also not possible to parse the influence of demand characteristics associated with ethics-based mindfulness training when comparing overt and self-reported prosocial behavior outcomes. Furthermore, the Donald et al. (2018) meta-analysis included studies with quasi-experimental designs, which compared meditators to non-meditators, thereby introducing selection bias and limiting the causal inferences that may be drawn from it. In sum, systematic error has been introduced into the summary findings of Donald et al. (2018), compromising internal validity. To date, the role of a mindful quality of attention in promoting prosocial action has not been meta-analyzed free of these serious threats to internal validity.

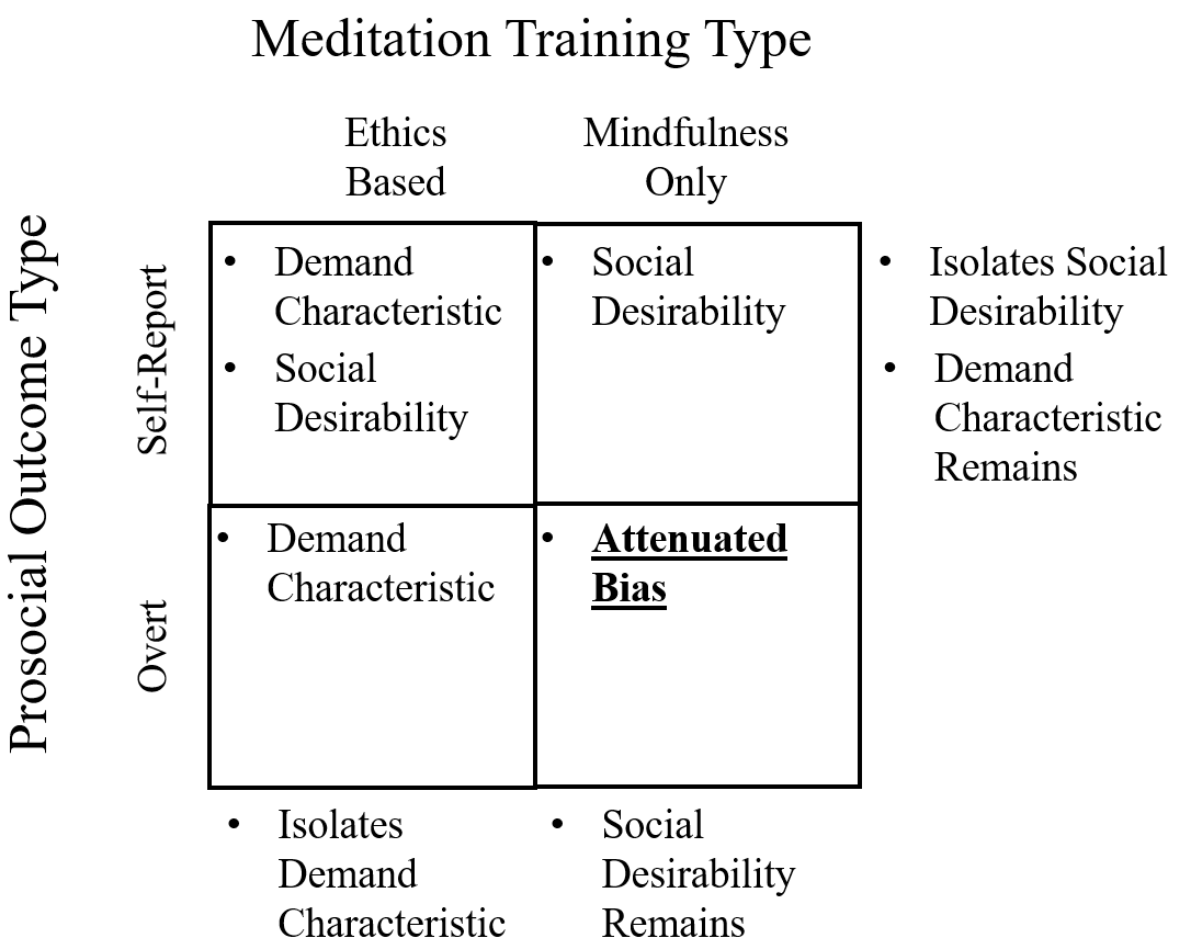

Figure 1. Moderator analyses in a previous meta-analysis by Donald and colleagues (2018) did not isolate demand characteristics and social desirability biases. The present meta-analysis removes studies with self-reported prosociality outcomes and studies using kindness-based meditation practices to attenuate these biases.

Considering this concern with demand characteristics, we meta-analytically tested whether various forms of mindfulness training, absent explicit ethics-based instructions, promote prosocial action. Furthermore, to reduce bias associated with self-reporting of prosocial action, we limited the focus of this meta-analysis to overt measures of prosocial action. Prior to detailing

this meta-analytic review, we offer a theoretical perspective on the phenomenological features of mindfulness that may catalyze prosociality.

Mindfulness as an "Empathic Attentional Set"

Several theories from developmental psychology (Eisenberg, 1988), humanistic/clinical psychology (Rogers, 1959; Schuster, 1979), social psychology (e.g., Goleman, 2013; Latané & Darley, 1970), and neuroscience (Singer, Critchley, & Preuschoff, 2009) suggest one common intrapsychic factor that promotes prosociality: the quality of attention directed toward others in social interactions. Humanistic psychotherapeutic conceptualizations of prosociality have long posited that prosocial action is more dependent on *how* we pay attention to others in need than merely *that* we pay attention to them (e.g., Rogers, 1975). Godfrey Barret-Lennard (1981) suggested that providing care to others in a clinical context is facilitated by an "empathic attentional set," in which one "opens him- or herself in a deeply responsive way to another person's feelings" (p. 92). Adding nuance to this proposed attention-related intrapsychic antecedent of prosocial action, Carl Rogers (1959) speculated from observations in his clinical practice that "being empathic" is about "perceiv[ing] an internal frame of reference of another [person]… with the emotional components and meanings which pertain thereto as if one were the person, but without ever losing the 'as if' condition" (pp. 210-211). Rogers makes a significant distinction between two attentional antecedents of prosocial action—namely, when witnessing another person in need, one must understand and feel *for* the other person but not necessarily *as* they do.

In social psychology and social neuroscience these distinct states of *feeling for* and *feeling as* an affected person are referred to as empathic concern (or compassion) and empathic distress, respectively (Goetz, Keltner, & Simon-Thomas, 2010; Zaki, 2014). Empathic concern is

defined as an other-oriented emotion that entails feeling for a person in need and a reliable promotor of prosocial action (Batson, 2009, Batson et al., 1987). On the contrary, empathic distress is a self-oriented emotional response to an afflicted person and tends to lead to withdrawal from the situation at hand or leads to helping merely to reduce one's own negative affect (Batson, O'Quinn, Fultz, Vanderplas, & Isen, 1983; Toi & Batson, 1982).

Bringing mindfulness into social interactions diverges from the way humans typically interact with each other (Kang, Gruber, & Gray, 2013)—operating on automatic pilot without much awareness of what we are doing (Bargh & Chartrand, 1999). Instead, abiding in a state of mindfulness involves suspending habitual or automatized ways of processing experience through learned patterns of thought and emotion, memories, and appraisals (Brown, Ryan, & Creswell, 2007). Mindfulness is an open attentiveness that simply processes what is occurring moment by moment. This context of consciousness is said to bring greater clarity to experiences (Varela & Depraz, 2003). When deployed in social interactions, two interrelated phenomenological outgrowths of mindful awareness parallel conceptualizations of the "empathic attention set": (1) de-automatization, and (2) (dis)identification (see Berry & Brown, 2017 for review). As a result, we suggest that mindfulness itself should encourage prosociality in a variety of circumstances.

De-automatization. The instantiation of a prosocial act often involves overcoming automatic reactivity to interpersonal and situational factors that hinder it (Penner, Dovidio, Piliavin, & Schroeder, 2005). For example, people are generally more likely to come to the aid of family members and in-group members than to help those who do not share biological relative or social in-group statuses (e.g., Burnstein, Crandall, & Kitayama, 1994). Prosociality can also be suppressed by the inactions of proximal others (e.g., Darley & Latané, 1968; Latané & Rodin, 1969) or being in a hurry (Darley & Batson, 1973). Barrett-Lennard alludes to the impact of

automaticity on prosociality when he suggested that "opening [one]self in a deeply responsive way" to another person serves as the gateway to the empathic attentional set (1981, p. 92). As a receptive attention to present-moment experiences (e.g., Anālayo, 2003) mindfulness may exemplify this quality of attention. Mindfulness allows one to notice mental processes as they arise, or to notice the psychological effects of those processes on one's behavior, perhaps allowing one to slow, interrupt, change, or override these automatic cognitions, emotions, and behaviors (Berry & Brown, 2017). Therefore, we anticipate that training in this capacity should promote prosocial behavior in circumstances that typically inhibit it.

Recent research lends support to this claim. Condon and colleagues (2013; also see Lim et al., 2015) found that training in mindfulness (or loving-kindness) meditation promoted helping behavior (i.e., offering one's seat) to an ostensible participant on crutches. More importantly, in this real-world simulation two bystanders did nothing to help—a common social occurrence that typically suppresses helping (Darley & Latané, 1968). Along this vein, Lueke and Gibson (2015) asked whether brief mindfulness-based meditation could reduce biases in implicit race and age attitudes. Lueke and Gibson (2015) found that training in mindfulness, relative to an active control condition, reduced implicit race and age biases by dampening automatically activated associations on the IAT. In a follow-up experiment, Lueke and Gibson (2016) showed that mindfulness trainees, relative to active and inactive controls, showed less biased charitable giving toward racial outgroup members in the dictator game.

(Dis)identification. Several theories of prosocial behavior converge to suggest that before engaging in a prosocial act, humans first take on the sensory, motor, visceral, and affective states of others through bottom-up cognitive processes (e.g., Decety & Jackson, 2004). Though mindfulness can involve focusing attention on and/or openly monitoring (Lippelt,

Hommel, & Colzato, 2014) one's own somatic, cognitive, and emotional experiences (Baer, 2009; Hölzel et al., 2011), and brief mindfulness training can increase empathic accuracy (Tan et al., 2014), evidence that training in mindfulness increases "perceiv[ing] an internal frame of reference of another [person]" (Rogers, 1959; pp. 210-211) is mixed (e.g., Lim et al., 2015).

The empathic attentional set not only entails "perceiv[ing] an internal frame of reference of another [person]," but also requires that one do so "with the emotional components and meanings which pertain thereto as if one were the person, but without ever losing the 'as if' condition" (Rogers, 1959; pp. 210-211). Self-related cognitions are typically very accessible and in fact may be part of our default mental functioning (Killingsworth & Gilbert, 2010); these cognitions help to support conceptual boundaries between self and other that can hinder empathy (Fennis, 2011). For instance, Batson et al. (1997) demonstrated that imagining how a suffering other feels increases empathic concern, whereas imagining how one would feel in place of a suffering other produced empathic concern and empathic distress (Batson, et al., 1983; Toi & Batson, 1982). Mindfulness is thought to facilitate dis-identification from mental content, and even a partial suspension of self-referential thought and emotion (Farb et al., 2007). When less ego-involved, conceptual boundaries between self and other may become less predominant, possibly giving way to empathic concern for and helping behavior toward others in need.

In line with this theorizing, incipient evidence that a mindful quality of attention facilitates this component of the empathic attentional set comes from three experiments by Berry and colleagues (2018). These experiments showed that mindfulness trainees, relative to attention-based, relaxation, and inactive controls, wrote more comforting emails to ostracized strangers (also see Tan et al., 2014) and then included them more in an online game. Most importantly, empathic concern (but not empathic distress) mediated the effect of mindfulness

training on helping behavior (Berry et al., 2018). This mediation finding is important, as it suggests that mindfulness trainees are less likely to lose the "as if" precondition of the empathic attentional set. Thus, like the empathic attentional set, mindfulness facilitates concern for others in need (but not concern for the self).

Present Research

Based on the foregoing literature review, we hypothesize that, as an exemplar of the empathic attentional set, training in mindfulness itself, relative to controls, will promote prosocial action. As a strong, precise test of our hypothesis, we meta-analyzed experiments in which mindfulness training without explicit ethics-based language was compared to active and inactive controls only on overt prosocial outcomes. Again, Figure 1 shows that removing explicit ethics-based language and self-reported measures of prosociality from meta-analyses reduces demand characteristics and social desirability biases, thereby allowing greater specificity in testing our hypothesis. As an extension to previous meta-analyses on the topic of mindfulness and prosociality, we used several indicators of publication bias to assess the prevalence of selective reporting and whether the effects of mindfulness training on prosociality were driven by selective reporting. Furthermore, novel moderation analyses were used to explain heterogeneity in study effect sizes. The hypothesis, eligibility criteria, and analytic plan were pre-registered on the Open Science Framework (Berry, 2019) after the literature search began but prior to analyses.

Method

Literature Search Procedure and Study Selection

From May 2018 to September 2018, we searched PsycINFO, PsycARTICLES, ScienceDirect, PubMed, and ProQuest Thesis and Dissertations databases to locate relevant

records published between January 2000 and September 2018. Two preprint databases, PsyArXiv and MindRxiv, were also searched, the former dedicated to psychological science and the latter dedicated to contemplative studies. Like Donald and colleagues (2018), *Mindfulness* and *meditation* served as search keywords. These terms were combined with the keywords *prosocial*, *helping behavior*, *altruism*, *empathy*, *compassion*, *antisocial*, *aggression*, *anger*, and *retaliation* in separate searches. When filtering options allowed for greater specificity in search queries, we excluded records with the term *self-compassion*, as this term is conceptualized as attitudes, cognitions, and emotions toward oneself (Neff, 2003) but not toward others, per se (but see Lindsay & Creswell, 2014). We also excluded the term *social mindfulness*; Van Doesum, Van Lange, and Van Lange (2013) define social mindfulness as a "means to safeguard other people's control over their own behavioral options in situations of interdependence," which deviates substantially from the conceptual definition of mindfulness used in the present review.

Article titles and abstracts were screened for their use of (a) mindfulness interventions without explicit ethics-based instruction, (b) experimental designs with random assignment to conditions, (c) at least one overt behavioral indicator of prosocial behavior, and (d) non-clinical population (coded by third, fourth, fifth, and eight authors). If a record met these criteria or if it was unclear that it met these criteria, the article was retrieved, read in full, and coded. Consistent with Quaglia, Braun, Freeman, McDaniel, and Brown (2016), we cross-checked our search records of studies published after 2010 in Mindfulness Research Monthly (Black, 2010). Mindfulness Research Monthly is a newsletter published by the American Mindfulness Research Association that tracks peer-reviewed literature on mindfulness science. We checked references of retrieved articles and emailed corresponding authors of retrieved articles to request unpublished data that met our search criteria. Corresponding authors of retrieved articles were

also contacted to request additional information to compute effect sizes when necessary information was lacking.

Eligibility Criteria

Studies were included in the meta-analysis if they (a) were an experiment that compared mindfulness training to an active and/or inactive control condition and (b) randomized participants into those conditions. Studies that (c) implemented mindfulness interventions with either focused attention, open monitoring, or a combination of these two forms of mindfulness instruction were included to specify the type of training received (Lutz, Jha, Dunne, & Saron, 2015). Focused attention instructions encourage participants to concentrate on an event or object, while open monitoring asks participants to pay attention intentionally to awareness itself, attending to any thought, feeling, somatic event, or object that comes to mind (see Lippelt et al., 2014, for review). We excluded studies that implemented loving-kindness meditation (Salzberg, 1995), the four immeasurables (Wallace, 1999), and/or compassion meditation (Hofmann, Grossman, & Hinton, 2011)—practices that promote changes in social emotional functioning and contain unambiguous teaching in ethics-based concepts (e.g., Ekman, Davidson, Ricard, & Wallace, 2005; Fredrickson, Cohn, Coffey, Pek, & Finkel, 2008; Mascaro, Rilling, Negi, & Raison, 2012; Rosenberg et al. 2015). Studies that provided ethics-related instruction not part of the meditation practice itself were also excluded. This criterion is important, as it helps to rule out the possibility of demand characteristics inherent in compassion-based meditations.

Studies that (d) used at least one overt outcome of prosocial behavior were included in this meta-analysis. For an outcome to be considered overt prosocial behavior, it needed to concern an act that promotes another person's (or multiple people's) well-being (e.g., instrumental helping behavior, compassion, generosity, cooperation; Tomasello, 2009) or one

that reduces the well-being of another person or other people (e.g., aggression, theft, retaliation; Anderson & Huesmann, 2007). Although acts that reduce well-being are seemingly antithetical to prosocial behaviors, we define withholding or engaging less in these acts as a prosocial act. Task-based measures, such as the implicit association test (Greenwald, McGhee, Schwartz, 1998), and self-report measures of prosocial behaviors were not included in the meta-analysis. Studies with third-person reports of participants' behaviors were included in the meta-analysis insofar as they described an overt act of prosocial behavior. Finally, studies were included if they (e) focused on normally developing children[1] or normally developed adults, (f) were published in the English language, and (g) if they were published during or after January 2000.

Moderators

Sedlmeier, Loße and Quasten (2018) found that shorter-term training in mindfulness produced larger effect sizes for positive psychological variables than long-term training, and the authors tentatively suggested these findings could be attributable to participants' expectations (i.e., a placebo effect). Thus, we compared single session interventions to multi-session interventions. We also surmised that as a mental capacity that exemplifies the empathic attentional set, mindfulness may show more potent effects on prosociality when mindfulness is made salient during a social interaction. Thus, we compared the immediacy in which prosociality was probed after training concluded (i.e., immediate vs. one day or more). Outcome valence (prosocial vs. antisocial) was also tested as moderator and in a follow-up analysis we compared outcome types (i.e., compassionate, instrumental, generosity, prejudice, & retaliation) to determine if the type of prosociality that mindfulness promotes is of a specific kind.

[1] No studies involving children met all eligibility criteria.

A recent meta-analysis indicates that the impact of (broadly defined) meditation interventions on prosociality are limited to studies that implement inactive controls and that include intervention facilitators as the article co-authors (Kreplin, Farias, & Brazil, 2018). Consistent with this, we accounted for potential differences in mindfulness effects on prosociality relative to active versus inactive controls. The type of active control used was also tested as a moderator, as some controls groups (e.g., relaxation, mind-wandering, and attentional control) more clearly isolated a mindful quality of attention by holding constant common factors that may promote prosociality. To control for biases introduced by live facilitators, we examined whether manuscript co-authors facilitating the meditation (co-author facilitator vs. no co-author facilitator) and live facilitation[2] (live facilitation vs. non-live facilitation) moderated the mindfulness—prosociality effects. Focused attention and focused attention plus open monitoring forms[3] of mindfulness training were compared to examine if the type of training explained heterogeneity in effect sizes (Lutz et al., 2015). Studies that recruited community versus student samples were also compared.

Publication bias

Publication bias[4], a phenomenon in which the total population of studies are not represented in a meta-analysis (Rothstein, Sutton, & Borenstein, 2005), can produce inflated effect sizes in systematic reviews (Light & Pillemer, 1984). Trials that fail to confirm hypotheses are less likely to be published and are cited less frequently (Egger, Zellweger-Zahner et al.,

[2] Immediacy of the prosocial probe, prosociality type, type of control, form of mindfulness intervention, and population were added in response to reviewer comments and not hypothesized at pre-registration.
[3] We intended to compare focused attention to open monitoring, but only one study trained participants in open monitoring.
[4] Unpublished studies were not included in analyses of publication bias

1997); in turn, negative findings are more likely to be missed in meta-analysis literature searches. Sample size and year of publication were also used to assess publication bias. Systematic variation in effect sizes by a trial sample size is an indicator of publication bias (Egger, Davey Smith, Schneider, & Minder, 1997) when smaller samples yield larger effect sizes. Year of publication can reveal presence of early publication of extremely large effect sizes that dwindle or reverse in subsequent publications as the research area matures (Ioannidis & Trikalinos, 2005). Duval and Tweedie's (2000) trim-and-fill analysis was used and Egger's regression (Egger et al., 1997) were used to test for asymmetry in the funnel plot. Funnel plot asymmetry may reflect suppression of findings with null effects and effects antithetical to the hypothesis (Sterne & Egger, 2005).

Simonsohn, Nelson, and Simmons (2014a) have shown that applying Duval and Tweedie's trim-and-fill analysis (2000) often fails to correct for publication bias. *p*-curve analysis[5] (cf., Simonsohn, Nelson, & Simmons, 2014b) is a set of two inferential statistics that examine whether statistically significant findings in a research area are not reliant on publication bias. The first statistic tests the shape of the observed distribution of statistically significant *p*-values ($ps < .05$) against a flat distribution of *p*-values. If this first test is statistically significant, it indicates that the *p*-curve is positively skewed, and that one can infer the effect size in the set of studies is not completely driven by selective reporting (Simonsohn et al., 2014b). The second test in *p*-curve analysis examines whether a distribution of statistically significant *p*-values in a research area is significantly flatter than would be expected if the studies had 33% power. If this

[5] As we describe below, the present meta-analysis uses a random-effects meta-analysis, and *p*-curve analysis applies to fixed-effect meta-analysis. Therefore, inference made from this *p*-curve analysis are limited to the set of studies examined, and broader generalizations about the effect of mindfulness on prosociality cannot be made.

second inferential test is statistically significant, one may infer that statistically significant effects in a set of studies is driven by selective reporting (Simonsohn et al., 2014).

Coding Procedures

The first and second author supervised and trained four coders (third, fourth, fifth and eight authors) to screen record titles and abstracts for study eligibility of inclusion in this meta-analysis (see *Eligibility Criteria*). The first author confirmed study relevance in this initial coding phase. Thereafter, manuscripts were read in full by six coders (authors three through eight). To obtain high interrater reliability, two coders independently reviewed each manuscript. Studies were selected based on eligibility criteria (detailed above). The two project supervisors then reviewed all manuscripts in full and confirmed study relevance in this final coding phase. Additionally, the two project supervisors corrected typographical errors and miscoding. Disagreements were resolved in round table discussions among the two authors who coded the manuscript and the two project supervisors.

Risk of Bias Analyses

To assess the overall methodological rigor in the sampled studies, we coded for 18 biases (described in Methods Reporting), some identified in previous meta-analyses on the topic (cf., Donald et al., 2018; Luberto et al., 2018; Schulz, Altman, & Moher, 2010) and some identified as recurring issues in the literature by authors one, five, nine, and ten in and a round table discussion. Authors five, nine, and ten coded an initial six studies to assess interrater reliability (89% consistency in ratings), and then coded the remaining 23 studies (7 or 8 each).

Meta-Analytic Approach

Effect size considerations. Some records included two control groups—most often an active and inactive control (e.g., Lueke & Gibson, 2016). In these circumstances, three effect

sizes were obtained. In the first meta-analysis, mindfulness training was compared to the average of the two control groups or the average of a single control group if only one was used. The second two meta-analyses compared effect sizes of mindfulness training to those of active and inactive trainings separately.

When records reported multiple eligible outcomes (e.g., Berry et al., 2018), the coder determined if these effect sizes were generated in two unique samples. All effect sizes generated from unique samples were included in the analyses. Special considerations were taken when multiple effect sizes were generated from the same sample. Records included multiple overt prosocial behavioral outcomes (e.g., Ridderinkhof et al., 2017) and repeated measures of an eligible outcome (e.g., Kirk et al., 2016; Yusainy & Lawrence, 2015). Moreover, some records included a manipulated variable (Long & Christian, 2015; Yusainy & Lawrence, 2015) unrelated to mindfulness. Although in this third situation it could be argued that comparisons of mindfulness at each level of the second manipulated variable are unique samples, effect sizes were averaged in all three situations accounting for the correlation between two outcomes or repeated measurement of one outcome. When the correlation was not reported, every effort was made to obtain this information from corresponding authors. If unable to obtain this information, we used a correlation of 1, which generated the most conservative variance estimates of the effect size (Borenstein, Hedges, Higgins, & Rothstein, 2009). When effect sizes were averaged across levels of a variable unrelated to mindfulness, a correlation of 0 was used.

Effect sizes from studies using antisocial outcomes were reverse coded, so that positive scores reflected withholding an antisocial behavior.

Model choice. Given the variability in study methodologies, we assumed a distribution of true effect sizes, and sought to estimate this distribution (Brockwell & Gordon, 2001; Hedges &

Vivea, 1998) of standardized mean differences between mindfulness and control trainings. Therefore, we constructed multiple random effects meta-analyses using Comprehensive Meta-analysis software (CMA; version 3); studies were weighted by the inverse of the sampling error variance. Hedge's *g*—an effect size estimate of standard mean differences (Hedges, 1981)—was used to correct for the influence of small sample sizes. The first meta-analyses compared mindfulness training to the average of all control groups in a study. Thus, effect sizes were generated by comparing mindfulness to a single control group in some studies and to the average of two control groups when appropriate. Follow-up analyses also tested for publication and availability bias, and moderation effects (see *Moderators*).

Two follow-up meta-analyses addressed the active versus inactive control moderator by comparing mindfulness training to active and inactive controls separately. This approach was taken, as including all effect sizes in one meta-analysis would have violated the assumption of independence of error terms[6]. Calculations of Hedge's *g* used in meta-analysis models are available on Open Science Framework (https://doi.org/10.17605/OSF.IO/MSP6T).

Results

Literature Search Results and Included Studies

Figure 2 shows results from the literature search according to PRISMA guidelines (Moher, Liberate, Tetzlaff, & Altman, 2009). From the initial 522 records identified, 156 duplicates were removed, and 335 studies were removed for failing to meet eligibility criteria in the initial reading of abstracts and titles. The 31 remaining records were read in full. Fourteen were removed for failing to meet eligibility criteria, and three unpublished records were

[6]Like our active vs. inactive control moderator analyses, in the outcome type moderator analysis, some effect sizes were relevant to two or more types of prosociality (e.g., compassion and prejudice). Thus, separate meta-analyses were conducted.

included. After an invitation to resubmit this manuscript, a second, smaller-scale search was conducted by authors five, nine, and ten on records published between September 2018 and September 2019. Three additional records were found (Frost, 2017; Hafenbrack et al., 2019; Schindler, Pfattheicher, & Reinhard, 2019). The remaining 23 manuscripts included 29 unique samples that met all eligibility criteria. Study design characteristics are shown in Table 1. Our search results data file is publicly available on Open Science Framework (https://doi.org/10.17605/OSF.IO/MSP6T).

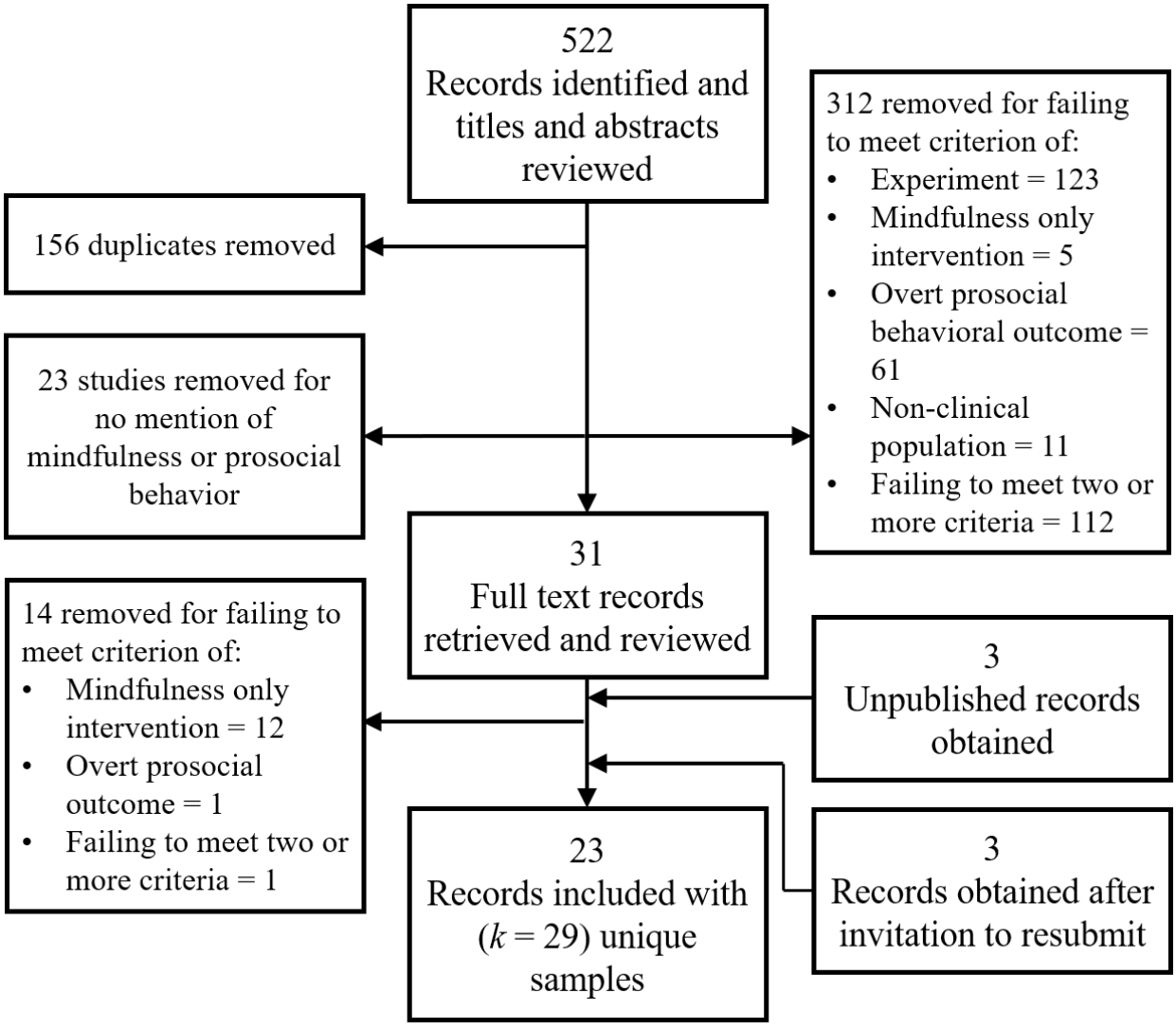

Figure 2. Flow chart of literature search results and study selection for meta-analysis.

Seventy-two percent of studies used single session interventions (28% multi-session). Intervention duration was almost perfectly confounded with studies measuring prosocial behavior immediately after training concluded. Seventy-two percent of studies use focused attention meditation practice. Three percent used open monitoring practice, and 20% of studies combined focused attention with open monitoring—most often Mindfulness-Based Stress Reduction (Kabat-Zinn, 2004). For 6% of studies the training type was not specified.

Seventy-two percent of studies measured prosociality as increasing prosocial behavior, and 28% measured it as reducing antisocial acts. Forty-five percent of studies measured compassionate helping, 10% instrumental helping, 17% generosity-related helping, 21% reduction of prejudice, and 21% reduction of retaliation. For six-percent of studies we were not able to classify the nature of the outcome. Eighty-three percent of studies used an active control, 21% only implemented an inactive control, and 28% used both an inactive and active control. Active control groups were 24% attention-based, 6% relaxation, 14% mind-wandering, 6% immersion, 6% placebo, 6% cognitive skills, and 17% non-specific. In many variables, sum percentages are greater than 100%, as a study could fit within two or more categories.

Most studies did not include meditation facilitators as manuscript co-authors (86%), and only 28 of studies used live facilitators. Sixty-two percent of studies used student samples, 24% used community samples, and for 6% the sample was not specified. Seventeen percent of the sampled studies had not been published

Table 1.
Study Design Characteristics, Sample Sizes, and Effect Sizes

Author(s)	Publication Year	Sample	Intervention Form	Intervention Format	Control	Active Control Type	Intervention Facilitator Co-author?	Live Facilitation	Outcome Valence	Outcome Type	Specific Outcome	Intervention Duration	Duration between Training and Outcome	ES Mindfulness vs. Active + Inactive (n)	ES Mindfulness vs. Active (n)	ES Mindfulness vs. Inactive(n)
Berry, Cairo, & Brown	Unpublished	Student	FA	Audio	Active	Attentional Control	No	No	Prosocial	Compassion	E-mail Comforting + Inclusion	Single-Session	Immediate		.582(75)	
Berry, Cairo, Goodman et al. Study 2	2018	Student	FA	Audio	Active	Attentional Control	No	No	Prosocial	Compassion	E-mail Comforting + Inclusion	Single-Session	Immediate		.768(83)	
Berry, Cairo, Goodman et al. Study 3	2018	Student	FA	Audio	Active + Inactive	Attentional Control	No	No	Prosocial	Compassion	E-mail Comforting + Inclusion	Single-Session	Immediate	.546(146)	.449(106)	.593(97)
Berry, Cairo, Goodman et al. Study 4	2018	Student	FA	Audio	Active + Inactive	Relaxation	No	No	Prosocial	Compassion	E-mail Comforting + Inclusion	Single-Session	Immediate	.544(131	.599(89)	.441(78)
Berry, Wall et al. Study 1	Unpublished	Student	FA	Audio	Active + Inactive	Attentional Control	No	No	Prosocial	Compassion + Prejudice	E-mail Comforting + Inclusion	Single-Session	Immediate	.468(124)	.705(84)	.193(81)
Berry, Wall et al. Study 2	Unpublished	Student	FA	Audio	Active + Inactive	Attentional Control	No	No	Prosocial	Compassion + Prejudice	E-mail Comforting + Inclusion	Single-Session	Immediate	.573(132)	.566(97)	.564(81)
Berry, Wall, Tubbs et al.	Unpublished	Student(*n*=75) + Community(*n*=4)	FA	Group	Active	Placebo *(Sham Mindfulness)*	No	Yes	Prosocial	Compassion + Prejudice	Offering Seat Or Assisting Experimenter	Multi-day	Immediate		485(75)	
Chen & Jordan	2018	Student	FA + OM	Audio	Active	Poetry Analysis	No	No	Prosocial	Generosity	Donation	Multi-day	1 Day+		−.139(414)	
Condon	2015	Community	FA + OM	Group	Inactive		Yes	Yes	Prosocial	Compassion	Offering Seat	Multi-day	1 Day+			.272(35)
Condon et al.	2013	Community	FA + OM	Group	Inactive		Yes	Yes	Prosocial	Compassion	Offering Seat	Multi-day	1 Day+			.629(26)
DeSteno et al.	2018	Student	FA + OM	Smartphone App	Active	Cognitive Skills	No	No	Antisocial	Retaliation	Hot Sauce Allocation	Multi-day	1 Day+		.867(46)	
Fernando et al.	2016	Medical Professionals	NS	Audio	Active	NS *(Speech on Civic Service)*	No	No	Prosocial	Instrumental	Assisting Experimenter	Single-Session	Immediate		.230(83)	
Frost	2016	Community + Students	FA	Group or Individual In-Person	Inactive		No	Yes	Prosocial	Generosity + Prejudice	Prisoner's Dilemma	Single-Session	Immediate		.158(331)	
Hafenbrack et al. Study 1a	2019	Community	FA	Audio	Inactive		No	No	Prosocial	Instrumental	Self and Employer Spontaneous Coworker Helping	Multi-day	1 Day+			.103(77)
Hafenbrack et al. Study 1b	2019	Community	FA	Audio	Active	NS *(NRR recording on technological advances)*	No	No	Prosocial	Instrumental	Coworker Rated Spontaneous Coworker Helping	Single-Session	Immediate		.447(92)	
Hafenbrack et al., Study 3	2019	Community	FA	Audio	Active	NS *(NY Times article about education initiatives)*	No	No	Prosocial	Compassion	Comforting Writing	Single-Session	Immediate		.701(70)	
Heppner et al. Study 2	2008	Student	FA	In-person	Inactive		Yes	Yes	Antisocial	Retaliation	Noise Blast	Single-Session	Immediate			.626(38)
Kirk et al.	2016	Community	FA + OM	Group + Homework	Active	Placebo *(Relaxation, Stretching + Exercise, Health Enhancement Instruction)*	No	Yes	Prosocial	Retaliation	Ultimatum Game	Multi-day	1 Day+		.246(51)	

Author(s)	Publication Year	Sample	Intervention Form	Intervention Format	Control	Active Control Type	Intervention Facilitator Co-author?	Live Facilitation	Outcome Valence	Outcome Type	Specific Outcome	Intervention Duration	Duration between Training and Outcome	ES Mindfulness vs. Active + Inactive (n)	ES Mindfulness vs. Active (n)	ES Mindfulness vs. Inactive(n)
Liang et al. Study 1	2018	MTurk	FA	Flashcards	Active	Mind Wandering	No	No	Antisocial	Retaliation	Voodoo Doll Task	Single-Session	Immediate		.561(69)	
Lim et al.	2015	Student	FA + OM	Smartphone App	Active	Cognitive Skills	No	No	Prosocial	Compassion	Offering Seat	Multi-day	1 Day+		1.161(54)	
Long & Christian Study 1	2015	Student	FA	Audio	Active	Mind Wandering	No	No	Antisocial	Retaliation	Theft	Single-Session	Immediate		.428(57)	
Lueke & Gibson	2016	Student	FA	Audio	Active + Inactive	Attentional Control	No	No	Antisocial	Generosity + Prejudice	Dictator Game	Single-Session	Immediate	.579(87)	.583(59)	.551(56)
Ramsey & Jones Study 2	2014	Student	FA	In-person	Active	NS *(Typing)*	No	Yes	Antisocial	NS	Ostracism	Single-Session	Immediate		.504(100	
Ridderinkhof et al.	2017	NS	FA	Audio	Active	Relaxation	No	No	Prosocial	Compassion	E-mail Comforting + Inclusion	Single-Session	Immediate	.217(158)	.098(105)	.324(107)
Schindler et al. Study 1	2019	Student	FA	Audio	Active	Mind Wandering	No	No	Prosocial	Generosity	Dictator Game	Single-Session	Immediate		−.116(80)	
Schindler et al. Study 5	2019	Student	FA	Audio	Active	Mind Wandering	No	No	Prosocial	Generosity	Dictator Game	Single-Session	Immediate		−.032(251)	
Tan et al.	2014	Student	FA	Audio	Active	Immersion	No	No	Prosocial	Compassion	E-mail Comforting	Single-Session	Immediate		.615(72)	
Tincher et al.	2015	Student	OM	Audio	Active	Immersion	Yes	Yes	Antisocial	Prejudice	Linguistic Intergroup Bias	Single-Session	Immediate		.203(84)	
Yusainy & Lawrence	2015	Student	FA	Audio	Active	Attentional Control	No	No	Antisocial	Retaliation	Noise Blast Depleted	Single-Session	Immediate		.143(59)	

Note. FA = focused attention; OM = open monitoring; NS = not specified.

Mindfulness Intervention Effects on Prosocial Outcomes

Table 2 presents meta-analyses, sensitivity analyses, and moderator analyses of mindfulness intervention effects on prosocial outcomes. Mean effect size estimates including 95% confidence intervals and I^2 statistics are shown, the latter indicating heterogeneity among studies not attributable to random sampling error. Leave-one-out sensitivity analyses, which serially remove one study from the analysis, were included to assess the influence of each study on the effect size estimate (Patsopoulos, Evangelou, & Ioannidis, 2008). The effect of mindfulness training, relative to the average of various active and inactive controls was small to moderate in size (cf., Cohen, 1992). Leave-one-out analyses showed that this effect size remained relatively stable and was not meaningfully influenced by any one study.

Table 2.
Meta-Analysis, Sensitivity Analyses, Moderator Analyses Comparing Mindfulness to Active and Inactive Controls on Prosocial Outcomes

	Meta-analysis					Sensitivity analysis									
						Leave-one-out		Duval and Tweedie's Trim-and-fill						Egger's Regression	
Subdistribution	*N*	*k*	*g*	95% CI(*g*)	I^2	g_{min}	g_{max}	*k*	$k_{imputed}$	*g*	g_{adj}	95% CI(g_{adj})	Δg	*t*(*p*)	*Q*(*p*)
Total	3100	29	.426	[.304, .549]	63.707	.406	.449	24	6	.429	.322	[.180, .463]	.107	3.443(.002)	
Intervention Duration															
Single-session	2322	21	.436	[.321, .552]	48.748	.416	.465	17	4	.443	.374	[.240, .508]	.069	1.585(.134)	0.019(.890)
Multi-session	778	8	.411	[.065, .756]	75.431	.290*	.511	7	0	.404	.404	[.014, .795]	.000	3.461(.018)	
Prosocial Probe															
Immediate	2397	22	.437	[.326, .548]	46.348	.418	.464	17	4	.443	.374	[.240, .508]	.069	1.585(.134)	0.025(.875)
1+ days	703	7	.404	[.014, .795]	77.051	.254*	.524	7	0	.404	.404	[.014, .795]	.000	3.461(.018)	
Outcome Valence															
Prosocial	2560	21	.376	[.232, .520]	69.847	.348	.407	16	2	.358	.283	[.097, .469]	.075	2.356(.034)	3.639(.056)
Antisocial	540	8	.603	[.419, .786]	0.000	.573	.629	8	0	.603	.603	[.419, .786]	.000	.862(.422)	
Outcome Type															
Compassionate	1181	13	.548	[.445, .652]	0.000	.529	.585	9	2	.570	.492	[.316, .668]	.078	.865(.416)	
Instrumental	252	3	.273	[.018, .527]	0.000	.173*	.340	3	0	.273	.273	[.018, .727]	.000	1.572(.361)	
Generosity	1163	5	.034	[−.156, .225]	57.199	−.040*	.099*	4	0	.026	.026	[−.228, .280]	.000	1.338(.313)	
Prejudice	833	6	.464	[.302, .626]	30.447	.424	.556	2							
Retaliation	320	6	.536	[.293, .780]	0.000	.477	.602	6	2	.536	.462	[.241, .684]	.074	.752(.494)	
Author Facilitator				.											
Co-author	183	4	.605	[.309, .900]	.000	.493	.697	4	1	.605	.493	[.246, .741]	.112	.874(.474)	1.388(.239)
No Co-author	2917	25	.410	[.257, .563]	66.889	.387	.436	20	5	.408	.301	[.145, .458]	.107	3.207(.005)	
Facilitation Type															
Live	740	8	.389	[.276, .583]	16.675	.305	.514	6	0	.520	.520	[.301, .739]	.000	.430(.689)	.116(.734)
Not Live	2360	21	.430	[.216, .583]	70.902	.401	.461	18	4	.412	.301	[.131, .471]	.109	3.198(.006)	

Table 2. cont'

Meta-Analysis, Sensitivity Analyses, Moderator Analyses Comparing Mindfulness to Active and Inactive Controls on Prosocial Outcomes

	Meta-analysis					Sensitivity analysis									
						Leave-one-out		Duval and Tweedie's Trim-and-fill						Egger's Regression	
Subdistribution	*N*	*k*	*g*	95% CI(*g*)	I^2	g_{min}	g_{max}	*k*	$k_{imputed}$	*g*	g_{adj}	95% CI(g_{adj})	Δg	*t*(*p*)	*Q*(*p*)
Control															
Active	2357	24	.459	[.311, .607]	69.080	.434	.488	20	5	.432	.309	[.148, .469]	.123	4.095(.001)	
Inactive	1022	11	.353	[.243, .484]	9.987	.322	.427	8	1	.440	.428	[.276, .582]	.012	.154(.882)	
Active Control Type															
Attentional Control	706	7	.576	[.445, .707]	0.000	.547	.602	4	2	.621	.555	[.387, .724]	.066	.404(.726)	
Relaxation	289	2	.389	[.070, .709]	52.575	.217*	.544	2							
Mind-Wandering	457	4	.166	[−.145, .478]	57.181	.038*	.276*	4	1	.166	.056	[−.266, .378]	.110	1.386(.300)	
Immersion	156	2	.689	[.360, 1.018]	0.000	.615	.755	2							23.314(.001)
Placebo	126	2	.389	[.032, .746]	0.000	.246*	.485	1							
Cognitive Skills	100	2	1.020	[.588, 1.453]	0.000	.867	1.161	2							
Non-Specific	759	5	.317	[−.034, .668]	78.263	.291*	.458	5	1	.317	.233	[−.080, .547]	.084	5.682(.011)	
Intervention Form															
Focused Attention	2307	21	.420	[.307, .535]	46.886	.373	.440	16	3	.420	.367	[.227, .508]	.053	1.278(.222)	
Focused Attention + Open Monitoring	626	6	.470	[−.005, .946]	80.875	.312*	.632	6	0	.470	.470	[−.005, .946]	.000	3.391(.028)	.210(.647)
Sample															
Student	2033	18	.484	[.306, .662]	75.052	.452	.521	15	3	.477	.378	[.179, .576]	.099	3.208(.007)	3.152(.207)
Community	909	10	.309	[.177, .441]	0.000	.278	.399	10	2	.386	.317	[.153, .482]	.068	.385(.714)	
Peer-Review Status															
Peer Reviewed	2363	24	.429	[.279, .579]	66.595	.401	.458								.001(.978)
Unpublished	737	5	.426	[.236, .616]	47.129	.337	.531								

Note. N = total sample size from all included samples; k = number of samples (i.e., number of effect sizes); g = weighted mean observed effect size; 95% CI(g) = 95% confidence interval of observed effect size; I^2 = percent of variance not attributed to random sampling error; g_{min} = lowest effect size after removing one study out of the meta-analysis at a time; g_{max}= highest effect size after removing one study out of the meta-analysis at a time; $k_{imputed}$ = number of trim-and-fill imputed effect sizes; g_{adj} = trim-and-fill adjusted observed effect size; 95% CI(g_{adj}) = 95% confidence interval of the adjusted effect size; Δg = change in observed effect size after trim-and-fill adjustment; $t(p)$ = statistical value and significance of Egger's regression; $Q(p)$ = statistical value and significance for mean difference in effect size between moderator groups. *95% confidence interval of summary effect size estimate includes zero after serially removing study.

Moderators of Mindfulness Intervention Effect

Figure 3 depicts between study heterogeneity in effect sizes, 63.7% of which was not attributable to sampling error (I^2 = 63.707). Planned moderator analyses were performed to explain this heterogeneity. Table 2 shows *Q* statistics and corresponding probabilities to test for statistically significant differences between categorical moderator levels. For moderation analyses, differences between subdistribution summary effect sizes were also compared alongside this inferential test; for inferential tests with a small number of studies, average effect sizes may provide better estimates of effect size magnitude (Hedges & Olkin, 1985).

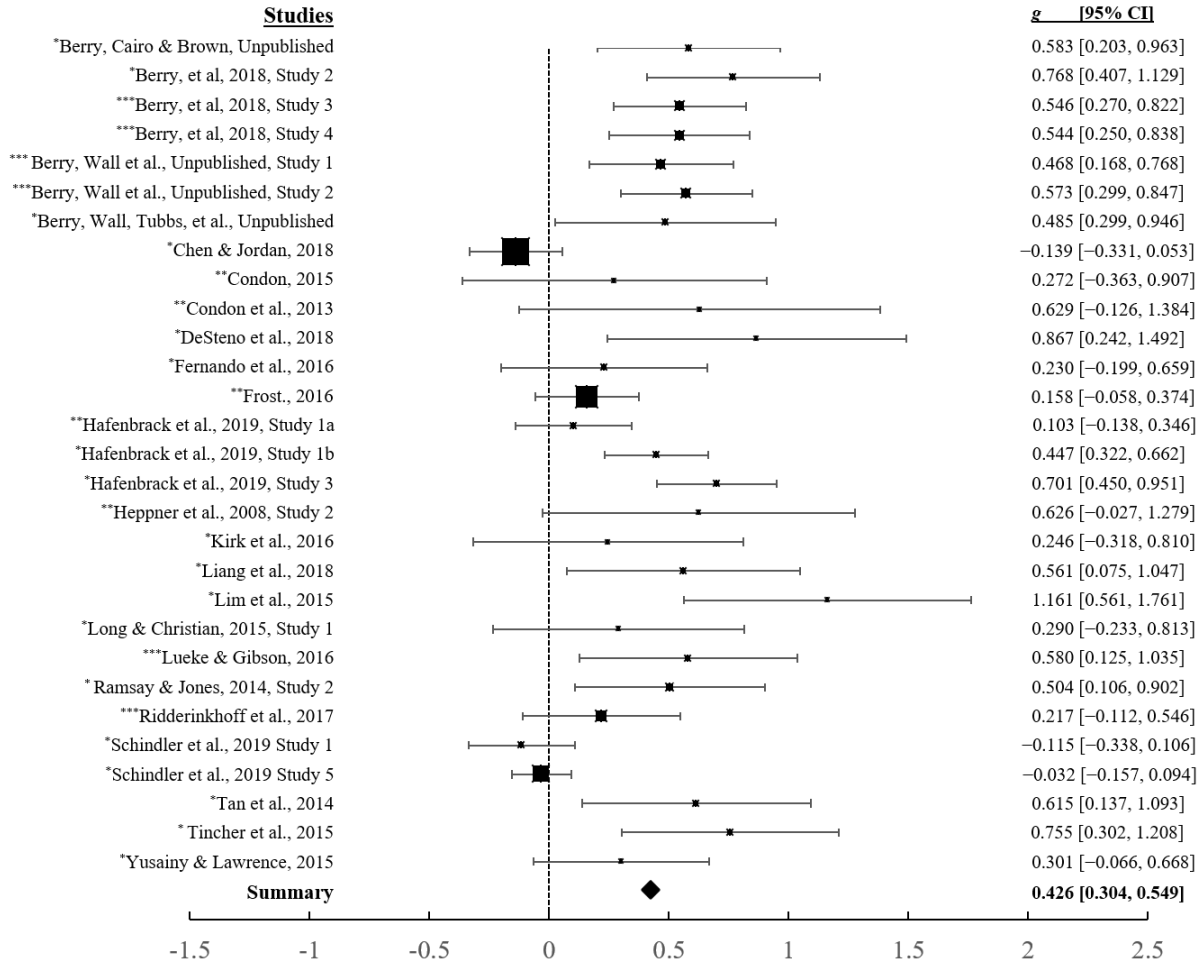

Figure 3. Forest plot of mean difference effect sizes (Hedge's *g*) and 95% confidence intervals of mindfulness training, relative to the average of active and inactive controls, on prosocial outcomes. *Note.* *mindfulness vs. active control, **mindfulness vs. inactive control, ***Mindfulness vs. average of active and inactive control.

Moderation by intervention duration and immediacy of prosocial probe. Analyses comparing the immediacy of the prosocial probe are presented alongside intervention duration analyses, as these findings are almost perfectly confounded. Only one study (Berry, Wall, Tubbs, et al., unpublished) measured prosocial behavior immediately after a multi-day intervention. No difference between multi-session and single-session meditation interventions was observed on prosocial behavior ($Q(1)$ .019, $p = .890$). Leave-one-out analyses showed that the 95% confidence interval of effect sizes of mindfulness training on prosocial behavior included zero—effect sizes were not reliable for multi-session interventions.

There was no difference between average effect sizes of studies measuring prosocial behavior immediately after mindfulness training or studies measuring prosocial behavior at least one day after training concluded ($Q(1) = .025$, $p = .875$). The average effect size of studies that measured prosocial behavior one day or more after training concluded were not robust to removing studies with extreme effect sizes. Because unpublished studies were removed from publication bias analyses, these two moderation analyses became perfectly confounded. Single session studies and those measuring prosociality immediately after training showed evidence of publication bias. There was also evidence of publication bias in studies using multi-session interventions and measuring prosocial behavior one day or more after training.

Moderation by outcome valence and type. Measuring prosocial behavior as increasing a prosocial act or reducing and antisocial act did not have a statistically significant influence on effect sizes ($Q(1) = 3.639$, $p = .056$). The summary effect sizes, however, were markedly different—effect sizes pertaining to increasing a prosocial act were lower than those concerning reducing an antisocial act. There was evidence of publication bias among studies measuring prosocial behaviors. A follow-up analysis compared the type of prosocial behavior which included five categories: compassionate helping, instrumental helping, generosity, reduction of prejudice, and reduction of retaliation. The effect sizes in the latter two categories were reverse coded to indicate a reduction in antisocial behavior. The effect sizes of mindfulness training on prosocial behavior appeared to be reliable only in studies that measured compassionate helping behaviors, as effect sizes concerning instrumental helping and generosity measures were lower and sensitive to leave-one-out analyses. There was evidence of publication bias in studies of that measured compassionate helping. Studies concerning reducing prejudice and reducing retaliation had reliable effect sizes that did not differ from those of studies measuring compassionate helping behavior. Although effect sizes in these two subdisitributions were robust to leave-one-out analyses, there was evidence of publication bias.

Moderation by facilitator biases. Kreplin et al. (2018) found that meditation effects on prosociality were limited to studies in which meditation facilitators were co-authors on the manuscript. We examined if this finding was supported among studies using mindfulness only mediation interventions. There was no evidence that heterogeneity among studies was explained by meditation facilitators appearing as co-authors on the manuscript or not ($Q(1) = 1.388$, $p = .239$). However, these summary effect sizes were quite different in magnitude; studies with

meditation facilitators appearing as co-authors showed higher effect sizes. There was evidence of publication bias in both subdistributions.

It is possible that the potency of the mindfulness effects on prosocial behavior are explained by live facilitation—the physical presence of a trained facilitator. Although there were many types of non-live facilitated interventions (i.e., audio recording, smartphone application, reading prompts) to foster mindfulness, these methods were combined into one level of this moderator. There was no evidence of moderation by live facilitation ($Q(1) = .116$, $p = .734$). Publication bias was evident in the non-live facilitation subdistribution.

Moderation by control group types. In an initial analysis, control groups were categorized broadly as either active or inactive and compared. Because these levels of the moderator included overlapping samples, however, these subdistributions were analyzed in separate meta-analyses and statistical moderation analyses were not performed. Inconsistent with Kreplin et al. (2018), which showed that meditation interventions promoted prosociality only when compared to inactive controls, we found that mindfulness training produced higher prosocial behavior relative to active controls than compared to inactive controls. There was evidence of publication bias in the active control group subdistribution. Correcting for publication biases in studies using inactive controls produced the opposite results as in the primary moderation analyses—inactive control studies showed higher effect sizes than active controls.

We further parsed the type of active control used, as heterogeneity was high in studies implementing active controls ($I^2 = 69.080$). Effect sizes comparing mindfulness to control groups involving attentional control, relaxation, immersion, placebo, and broadband cognitive skills did not include zero. The relaxation and placebo effect sizes, however, were not robust to leave-one-

out analyses. It is noteworthy that there are only two samples in each of these subdistributions, and thus leave-one-out analyses merely indicate that one study showed a statistically significant effect while the other did not. Confidence intervals of effect sizes comparing mindfulness to mind-wandering and non-specific controls included zero. Although parsing control types did explain heterogeneity in studies implementing active controls ($Q(6) = 23.314$, $p = .001$), researchers should be cognizant of the relatively small number of samples in some subdistributions.

Moderation by intervention form. We intended to compare focused attention and open monitoring interventions, but only one study used an open monitoring intervention. Thus, we compared effect sizes of interventions using focused attention and focused attention plus open monitoring, which were not statistically different ($Q(1) = .210$, $p = .647$). The effect size of interventions combining focused attention and open monitoring may have not been reliable because they were confounded with multi-session interventions and measuring prosociality one day or more after training concluded. There was publication bias in studies that instantiated mindfulness via focused attention.

Moderation by sample type. Though not statistically different, student samples provided higher effect sizes than predominantly community samples[7] ($Q(1) = 3.152$, $p = .207$). There was publication bias in both subdistributions.

Moderation by peer-review status. Effect sizes of peer-reviewed studies and unpublished studies did not differ ($Q(1) = .001$, $p = .978$). Publication bias was not assessed for unpublished studies and are reported below for peer-reviewed studies.

[7] The Berry (2017) and Frost (2016) studies combined student and community samples.

Publication Bias

First, we constructed a metaregression with sample size and year of publication as predictors of mean difference effect sizes (Stanley & Jarret, 1989). The metaregression model revealed that year of publication and sample size provided better model fit than the intercept only model ($Q(2) = 20.01$, $p < .001$). Although year of publication was not related to effect size ($b = -.022$, SE(b) = .026, 95% CI(b) = [−.073, .029], $z = -.85$, $p = .394$), smaller sample sizes predicted larger effect sizes ($b = -.002$, SE(b) = .0005, 95% CI(b) = [−.003, −.001], $z = -3.76$, $p < .001$). The parameter estimate indicates that for every additional participant in a sample, the mean difference effect size decreased by .002. Second, Duval and Tweedie's (2000) trim-and-fill imputed six studies to correct for asymmetrical distribution in the funnel plot (see Table 2 and Figure 4). A 25% decrease in the reported effect size was observed, albeit the mean differences between mindfulness and controls were still greater than chance and in favor of mindfulness enhancing prosocial behavior. Third, significance tests of funnel plot asymmetry (Egger et al., 1997) provided additional evidence of publication bias. Together, these analyses lend support to the notion that in the published literature of experiments comparing mindfulness training to various controls on prosocial outcomes, small sample sizes produce larger effect sizes and large samples produce smaller effect sizes. Furthermore, small effect sizes are suppressed (i.e., not readily available) in the reviewed literature.

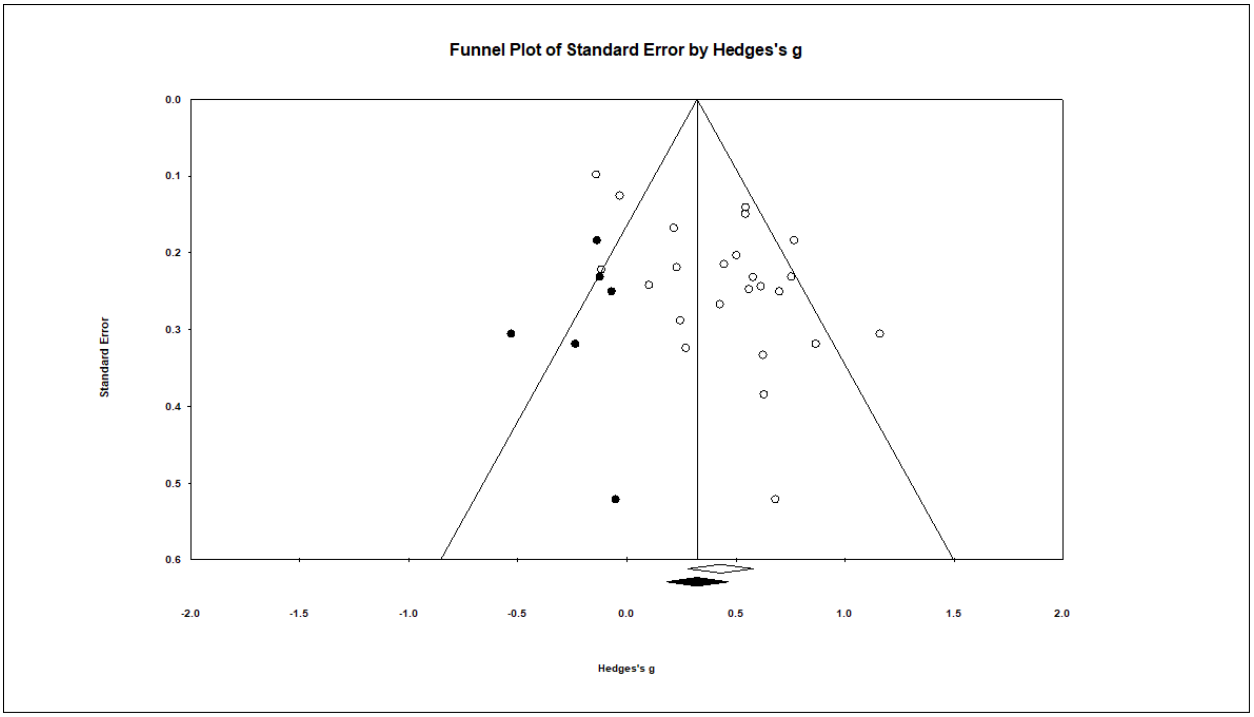

Figure 4. Funnel plot of trim-and-fill analyses for primary meta-analysis. Circles with no fill = published studies. Circles with black fill = imputed studies. Diamonds represent summary effect sizes.

***P*-curve analysis results**. Among the 29 samples used in this meta-analysis, 29 effects hypothesized in the original article that represented comparisons of mindfulness training to active controls only were submitted for *p*-curve analysis using the *p*-curve App (version 4.06, http://www.p-curve.com). Because multiple manuscripts reported two or more *p*-values, only the first reported *p*-value from each manuscript was submitted for *p*-curve analysis. Fourteen of these null results ($p > .05$) were excluded from the analysis, and thus the distribution 15 statistically significant *p*-values were analyzed. As shown in Figure 5, the effect sizes in the sampled studies of mindfulness interventions' effects on prosocial outcomes contain evidential value—the summary effects were not driven by selective reporting (half: $z = -2.680$, $p = .004$; full: $z = -2.310$, $p = .010$). Next, we compared the observed distribution of *p*-values to the *p*-

curve that would be expected if studies had an average of 33% power. The result from this test was not statistically significant (full: $z = -.012$, $p = .454$), and thus we fail to reject the null hypothesis that evidential value is absent in this set of studies. The average power of these studies was 31% (90% CI = [8%, 62%]). It is unlikely that any more than 62% of these studies would replicate, and it is most likely that 31% of them would replicate. The *p*-curve disclosure table (Table S1) and robustness *p*-curve analyses are shown in online supplemental material (Figure S1).

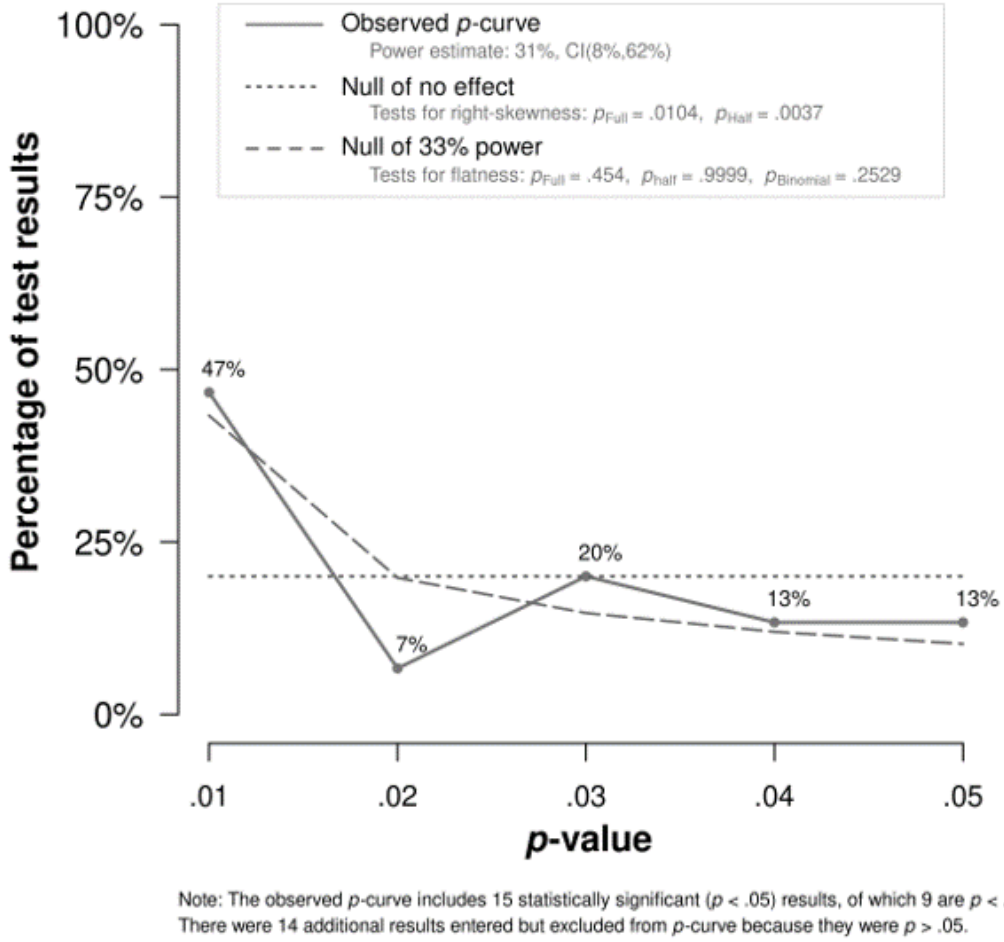

Figure 5. *p*-curves are depicted of the 29 *p*-values that were reported first in manuscripts comparing mindfulness to controls on prosocial outcomes. The dotted line shows the expected

distribution of *p*-values if there is no effect, and dashed line shows the expected distribution of *p*-values if the effect existed and studies averaged 33% power. Figures were generated by *p*-curve app 4.06.

Risk of bias analyses. The most prominent risks to bias in this research area concern a lack of adherence to open science practices. None of the studies were pre-registered and only 9% of researchers made data available in a public repository. Sixty-nine percent of studies made research materials available in supplemental materials or in the manuscript. Thirty-four percent of studies reported power-analyses.

Although 83% of studies used deception, 41% queried for participant suspicion about deception and 34% removed suspicious participants from analyses. Most researchers ruled out alternative explanations of the effect of mindfulness on prosociality using manipulation checks (76% of studies), however, 49% did not assess whether the mindfulness intervention used in the study increased state and/or trait mindfulness. All studies used valid indicators of prosocial behavior, and most kept experimenters (76%) and behavior coders (93%) blind to condition. Consistent with recommendations by Lutz and colleagues (2015), 93% of researchers specified the type of mindfulness training received; 64% leveraged control groups to isolate specific qualities of mindful attention. Only 28% of studies used inactive controls alongside active controls in studies that measured prosocial behavior using posttest only—important because the inactive control assists in ruling out the possibility that the active control reduced prosociality.

Discussion

Scientific research on the prosocial benefits of mindfulness has been a topic of recent debate in the sciences and humanities (see Condon, 2018; Monteiro et al., 2015 for reviews). At the heart of this debate is the question of whether secularized mindfulness interventions diverge from canonical derivations of contemplative practices, as they often lack grounding in ethical

frameworks (Bodhi, 2011; Montiero et al., 2015). Though there is merit to questioning the construct validity of secularized mindfulness interventions (Grossman, 2011), we have argued that incorporating ethical concepts into mindfulness training may compromise both the construct validity of the intervention and the internal validity of the inferences drawn from studies on the topic. As an extension of previous meta-analyses (Donald et al., 2018; Kreplin et al., 2018), this meta-analysis examined whether training in mindfulness that lacked explicit instruction in ethical concepts, relative to active and inactive controls, enhanced prosocial action. Quasi-experiments, correlational research, and studies with self-report outcomes of prosociality were excluded to reduce bias. We also sought to explain heterogeneity in effect sizes using theoretically relevant (Donald et al., 2018; Kreplin et al., 2018) and novel moderators. Mindfulness is a quality of receptive, careful attention to what is present (e.g., Anālayo, 2003) that exemplifies an empathic attentional set (Eisenberg, 1988; Goleman, 2013; Latané & Darley, 1970; Rogers, 1959)—a theorized promoter of prosocial behaviors. Thus, we predicted that training in mindfulness itself would increase prosocial action.

Across 29 samples, there was a range of small to medium standardized mean effect sizes of mindfulness training on overt acts of prosociality. Tests of moderation revealed that effect sizes were reliable in studies using single session interventions that measured prosocial behavior immediately after the intervention concluded. Effect sizes in studies using multi-session interventions which measured prosocial behavior one day or more after training concluded were not reliable to sensitivity analyses. The primary finding of this meta-analysis also appeared to be driven by studies measuring compassionate prosocial behaviors and behaviors concerning reduction of prejudice or retaliation. Effect sizes in studies measuring instrumental and generous helping behaviors were not reliable. Studies that included meditation intervention facilitators as

co-authors showed higher effect sizes than those that did not. This result was not attributable to potential expectancy biases introduced by live facilitators.

Studies using active controls found higher effect sizes of mindfulness on prosocial behavior than studies using inactive controls. This effect was reversed after imputing studies to control for asymmetry in the funnel plot. Though subdistributions had a relatively small number of studies, heterogeneity in effect sizes of studies comparing mindfulness to active controls was explained by parsing the type of control used. Effect sizes were reliable in studies comparing mindfulness to attention-based, immersion, and cognitive skills control groups. Subdistributions that compared mindfulness to relaxation and placebo controls each had two studies, and in both, the critical finding of one study was statistically significant and the other study was not. Effect sizes were stronger in studies using student samples than those using primarily community samples, but whether a study was published was not a predictor of effect size.

Tests of publication bias revealed extant selective reporting in the research record. Studies with smaller sample sizes produced the largest effect sizes of mindfulness on prosociality, and effect sizes were lower among studies with large sample sizes. Trim-and-fill analyses (Duval & Tweedie, 2000) showed that the effect size estimates of mindfulness on overt prosocial action could be as much as 25% lower than reported. *p*-curve analyses (Simmons et al., 2011; Simonsohn et al., 2014a; Simonsohn et al., 2014b) indicated that although there was publication bias in the research record of mindfulness effects on overt prosociality, the studies had evidential value—that is, the effects of mindfulness on overt prosociality were not completely attributable to selective reporting. Together, these results provide evidence that mindfulness itself can promote overt prosocial action and adds nuance to the ongoing debate about the fidelity of mindfulness practices for promoting prosocial action.

Implications for the Science of Secular Mindfulness Training on Prosocial Action

Despite concerns about the fidelity of secular mindfulness training, findings of this meta-analysis are consistent with mindfulness theory (e.g., Condon, 2018; Davidson & Harrington, 2002; DeSteno, 2015; Trautwein, Nanjo, & Schmidt, 2014) and recent meta-analytic reviews showing that mindfulness training facilitates prosocial action (Donald et al., 2018). Importantly, this meta-analysis extended previous meta-analyses on the topic by showing that mindfulness training without explicit ethics-based instruction can instantiate overt acts of prosociality. Several design features of this meta-analysis brought to relief important implications for the effects of mindfulness on overt prosociality.

First, the effects of mindful attention on overt prosociality is not limited to explicit training in ethical concepts. These results contradict theory (Montiero et al., 2015) and the results of Chen and Jordan (2018), which found that grounding mindfulness training in an ethical framework promoted higher prosocial action relative to mindfulness only practices—important because the effect size from this Chen and Jordan (2018) experiment was included in the present meta-analysis. Our results may diverge from this study because the control group in this study involved poetry analysis; instructions such as, "[t]hink about the author. What message is he trying to convey" and "[t]he more rich descriptions at the end like the images of the flowers that grow and…convey the author's affection for the woman he is going to see" may have inadvertently promoted perspective taking—a promoter of prosocial behavior (Batson, 2009, Batson et al., 1987). It is unclear how the study design isolates mindfulness and mindfulness training with explicit instructions in ethics, and thus, it is difficult to infer that ethics-based instructions are necessary to include alongside mindfulness training to promote prosocial behavior.

Kreplin and colleagues (2018) called for greater methodological rigor in this research area—namely, reducing experimenter and social desirability biases. Donald and colleagues (2018) attempted to resolve this issue in a subsequent meta-analysis by comparing mindfulness only training to mindfulness training including explicit ethics-based language. They also compared studies using self-report and overt indicators of prosocial behavior. As mentioned in the introduction, however, these analyses did not rule out these biases. We attempted to eliminate these biases by excluding studies that incorporated explicit ethics-based instructions alongside mindfulness training. These exclusion criteria in the present meta-analysis offer a second important implication for research on the effects of mindfulness training on prosociality. Relative to the meta-analysis by Donald and colleagues, these controls bolstered the internal validity of the claims made in the present work. Indeed, the differences in the questions raised by each meta-analysis not only yielded unique search criteria, but only 28% of the sampled studies in the two meta-analyses were the same. Moreover, the present meta-analysis yielded smaller effect sizes (g = .426) than the previous work on the topic (d = .510), and when correcting for publication bias, our meta-analysis indicated that this difference in effect sizes may be quite significant (g = .322).

Third, effect sizes of mindfulness training on prosocial action were lower when comparing mindfulness training to active controls, relative to inactive controls after controlling for asymmetry in the funnel plot. Furthermore, studies including meditation facilitators as study co-authors produced higher effect sizes than studies that did not. These findings are consistent with those of the Kreplin et al (2018) meta-analysis. Unlike the Kreplin et al. meta-analysis, confidence intervals of effect sizes in studies using active controls and studies that did not include meditation facilitators as co-authors did not include zero. This means that although

sensitive to these research design features, the effect of mindfulness training on prosociality was not fully dependent upon them. Inflated effect sizes among studies including facilitators as co-authors, however, were not solely attributable to biases associated with live facilitation, as effect sizes of studies using a live facilitator did not differ from those using other types of facilitation.

In sum, the findings of this meta-analysis are consistent with previous research suggesting that secular forms of mindfulness training that do not include ethical concepts in didactic instruction promote prosocial behaviors (e.g., Berry et al., 2018; Condon et al., 2013; Lim et al., 2015). Additional moderator analyses reveal important caveats to this claim. Prosocial outcomes were less affected by mindfulness training than outcomes concerning reduction of antisocial behavior. Thirty-eight percent of studies measuring prosocial behavior, however, used instrumental or generosity-related helping behavior outcomes, which based on our findings may not be mutable to training in mindfulness.

The issue of publication bias is also concerning and need further consideration. Most notably, larger sample sizes generated the smallest effect sizes. For every participant added to a sample, effect sizes dropped by .002. This would indicate that if a study had 100 participants and an effect size of .40, a study with 50 participants would have an effect size of .30. Thus, it could be inferred that if all study sample sizes were sufficiently large, the summary effect size estimates of this meta-analysis would be unreliable. This publication bias finding may have been driven by the fact that studies with the largest sample sizes (i.e., Chen & Jordan, 2018; Frost, 2017; Schindler et al., 2019) used generosity-related prosocial outcomes.

Is mindfulness an empathic attentional set? We argued that phenomenological features of mindfulness were consistent with cross-disciplinary conceptualizations of the empathic attentional set (e.g., Barrett-Lennard, 1981; Batson, 2009; Latané & Darley, 1970; Rogers,

1959), attention paid to others in need in which one does not confuse the other's suffering with one's own. We suggested that as a mental *capacity* that varies in the population (e.g., Brown & Ryan, 2003) and can be bolstered situationally (Heppner & Shirk, 2018), deploying interventions that bolster or integrate mindfulness may be efficacious in promoting prosociality. More specifically, we posit that mindfulness may promote prosociality by attenuating the influence of automaticity on behavior (Lueke & Gibson, 2015) and/or by dampening self-related cognition (see Berry & Brown, 2017 for review). Two results in the present meta-analysis indirectly support this theoretical framework.

First, effect sizes of this meta-analysis were only reliable among studies using single-session interventions that probed for prosociality immediately after training concluded. Effect sizes from studies that used multi-session interventions and measured prosociality days after training concluded were not reliable. We are tentative to make this inference, but this finding may lend support to the contention that it is a mindful quality of attention itself that promotes prosocial action, and not the multifaceted and nonspecific factors that are included in secular mindfulness training. Studies implementing brief mindfulness interventions (e.g., Berry et al., 2018; Ridderinkoff et al., 2017; Tan et al., 2014) most often induce a mindful state immediately before measuring prosocial action. Long term intervention studies (e.g., Chen & Jordan, 2018; Condon et al., 2013; Lim et al., 2015) less often make mindfulness salient prior to measuring study outcomes (but see Berry, 2017). It is possible that in long-term intervention studies prosocial action is not captured when one is abiding in a mindful state. Sedlmeier et al. (2018) suggest, however, that the stronger effects sizes commonly realized among short-term mindfulness training studies could be due to placebo effects. We find this explanation of the results in the present meta-analysis less likely, as the effects of mindfulness training on

prosociality were robust to use of various types of controls. Importantly, in a subdistribution we refer to as placebo controls, mindfulness training showed reliable effect sizes. One study compared mindfulness training to a well-validated Health Enhancement Program (MacCoon et al., 2012) and another used sham mindfulness meditation as a control group (Zeidan et al., 2015). Expectancy biases associated with thinking that one is meditating cannot be fully ruled out, but we encourage more research using these placebo controls.

Second, mindfulness training was most effective at increasing compassionate (but not instrumental or generosity-related) helping behavior. There is ambiguity about what motivated compassionate behaviors in the sampled studies, (see Batson, 2009 for review), but compassionate acts are generally defined as those ameliorating others' suffering (Batson et al., 1987; Goetz et al., 2010; Tomasello, 2009). Across disciplines it has been theorized that an empathic attentional set can catalyze concern for others in need (Latané & Darley, 1970; Rogers, 1959, 1975; Schuster, 1979; Singer, Critchley, & Preuschoff, 2009). Thus, it could be that mindfulness training promotes prosocial behavior in circumstances in which ameliorating the suffering of others is the primary goal. Consistent with this, mindfulness trainees were generally less harmful to others who had harmed them. Also related to this theoretical framework and previous research (e.g., Lueke & Gibson, 2015, 2016), is the finding that mindfulness reduces prejudiced prosocial responding.

Limitations and Future Directions

One observation that we made while completing this meta-analysis was the careful rigor put into designing control groups. Risk of bias analyses revealed that many of the studies in this meta-analysis implemented active control groups designed to isolate qualities of attention and other factors in mindfulness that may promote prosociality (e.g., Ridderinkhof et al., 2017).

Active control conditions included focused attention-based controls (e.g., Berry et al., 2018; Lueke & Gibson, 2016; Yusainy & Lawrence, 2015), relaxation (Berry et al., 2018; Ridderinkhof et al., 2017), mind-wandering (Liang et al., 2018; Long & Christian, 2015; Ridderinkhof et al., 2017), cognitive skills (DeSteno et al., 2018, Lim et al., 2015), immersion (Tan et al., 2014; Tincher et al., 2015), and sham mindfulness meditation (Berry, Wall, Tubbs et al., unpublished). However, few studies (e.g., Lueke & Gibson, 2016) used two control groups, one active and the other inactive. Studies on prosocial action typically require between subjects designs in which prosociality is assessed only once, as participants may become aware that the researcher is studying prosociality if repeated measures are taken. The use of two control groups allows researchers to rule out the possibility that the active control is reducing prosociality rather than mindfulness increasing it. As this area matures, researchers should be cognizant of how control groups assist in specifying a mindful quality of attention (e.g., Lutz et al., 2015) and how they separate the effects of mindfulness from non-specific factors (e.g., Davidson, 2010).

Alongside this noted methodological rigor, there are abundant possibilities for improving the research of mindfulness on prosociality. As we have noted, an analysis of publication bias revealed that the largest effect sizes were realized among smaller samples. It is difficult to pinpoint the cause of this finding, but one can conclude that the total population of studies are not represented in this meta-analysis. Risk of bias analyses also showed that although most researchers reported enough information to replicate the study, no studies were pre-registered and only two had posted data in a public repository. Furthermore, few studies conducted power analyses, and although deception was used in this research, few studies queried for suspicion about deception and excluded suspicious participants from analyses. Although these findings give pause in making firm claims about the precision of effect size estimates on studies of

mindfulness and prosociality, recent efforts have been made by the Mind and Life Institute to promote a culture of open science on mindfulness and other contemplative practices. The private institution now expects grantees to follow the Guidelines for Transparency and Openness Promotion in Science (Nosek et al., 2015) on all funding mechanisms—the largest funding mechanism is dedicated to prosocial outgrowths of contemplative practice. Pre-registration of hypotheses, methodology, and analysis plans, as well as sharing data and research materials will be essential to assessing methodological rigor in attempting to falsify that mindfulness facilitates prosociality.

Although we infer that by removing studies with ethics-based instruction from the present meta-analysis that the observed increases in overt prosocial action are attributable to a mindful quality of attention itself, it is still difficult to determine that it is mindfulness promoting prosociality. For example, in studies with live facilitators, non-specific factors like the facilitator's posture and group leadership experience/skills (see Condon, 2018 for review) could model and promote prosociality. It is promising, however, that effect sizes for studies in which mindfulness was trained by some means other than a live facilitator, were comparable to the effect sizes of studies that used live facilitators. It is noteworthy that few studies measured state mindfulness as a manipulation check to ensure that participants were indeed abiding in a mindful state while engaging in prosocial behavior. State mindfulness resulting from single-session interventions administered via audio recordings or read prompts is likely ephemeral, but these intervention platforms could be leveraged in future research, as they may afford greater control of non-specific factors that promote prosociality. Because using manipulation checks may add time between the conclusion of mindfulness training and measurement of prosociality, it will be important for researchers to describe accurately the duration in which meditation naïve

participants can remain mindful during these short interventions. With such knowledge we could begin to make firmer claims about the role of a mindful state in promoting prosocial behavior without using manipulation checks.

It is important to clarify that we do not mean to imply that grounding mindfulness practices in ethics is not a viable question for scientific inquiry. The challenge for this research question is ruling out the possibility that ethics-based instructions cue participants to the study aims and lead to behaviors that corroborate experimenters' hypotheses. As Condon (2018) has suggested, taking an interactionist approach, in which pre-existing dispositions are examined as moderators of the prosocial outgrowths of mindfulness training, is one avenue toward understanding whether mindfulness is reliant on ethical concepts. Another way to test the incremental effects, if any, of ethics-based language alongside mindfulness would be to isolate the apparent demand for prosociality (e.g., Williams et al., 2018). For example, Ashar et al., (2016) leveraged an oxytocin placebo, telling participants that it would enhance their compassion. The researchers compared this control to compassion meditation training.

How mindfulness promotes prosociality is still not understood. Berry and colleagues (2018) found that empathic concern (also called compassion; Batson, 2009) mediated the relation between brief mindfulness training and helping behavior toward strangers. The researchers admit, however, that it could not be determined if mindfulness was reducing the accessibility of self-related cognitions (Brown et al., 2016) that hinder prosocial action (Fennis, 2011), or if it promotes prosocial action by increasing careful attention toward others (Trautwein et al., 2014). Further research is needed to parse these phenomenological states; perhaps neuroscientific (e.g., Ashar, Andrews-Hanna, Dimidjian, & Wager, 2017; Lamm, Batson, & Decety, 2007) and/or ecological momentary assessment (Berry, 2017) strategies could be studied alongside overt

prosocial action to better understand the intrapsychic features of mindfulness that promote prosocial action. As mentioned, the use of active control groups may assist in isolating the effects of mindfulness on prosociality.

Prosocial action is parochial in intergroup interactions (Bloom, 2017; Cikara & Van Bavel, 2014). Specifically, when in need, ingroup members are often shown more empathy and help relative to outgroup members (Cuddy, Fiske, & Glick, 2007; Saucier, Miller, & Doucet, 2005). The four unpublished studies in this manuscript, show that brief (Berry, Wall, et al., unpublished; Frost, 2016) and multi-day mindfulness trainings (Berry, Wall, Tubbs, et al., unpublished) promote prosocial action toward racial outgroup members. These studies lack a condition, however, in which prosociality toward an ingroup member is measured (but see Frost, 2016). Thus, mindfulness could be increasing prosociality as a main effect, but not reducing the gap in helping behavior that favors ingroup members. While this work shows promise, future work should examine how best to implement mindfulness training to promote lasting increases in interracial prosociality, and if mindfulness mitigates the parochial nature of intergroup prosociality.

Conclusion

This meta-analysis extends previous work on the positive interpersonal outcomes of mindfulness training (see Donald et al., 2018 for review) by showing that mindfulness training without explicit ethics-based concepts enhances overt outcomes of prosocial action. These findings deepen our understanding of the attentional bases of prosociality and call for additional theory and research on how and in which circumstances mindfulness confers prosociality.

References

Anālayo, B. (2003). *Sattipatthana: The direct path to realization*. Birmingham, England: Windhorse.

Anderson, C. A., & Huesmann, L. R. (2007). Human aggression: A social cognitive view. In M. A. Hogg, & J. Cooper (Eds.) *The Sage handbook of social psychology: Concise student edition*, 1 (pp. 3–23). Thousand Oaks, CA: Sage Publications

Ashar, Y. K., Andrews-Hanna, J. R., Dimidjian, S., & Wager, T. D. (2017). Empathetic care and distress: Predictive brain markers and dissociable brain systems. *Neuron, 94*(6), 1263-1273. https://doi.org/10.1016/j.neuron.2017.05.014

Baer, R. A. (2009). Self-focused attention and mechanisms of change in mindfulness-based treatment. *Cognitive Behaviour Therapy, 38*(S1), 15-20. https://doi.org/10.1080/16506070902980703

Bargh, J. A. & Chartrand, T. L. (1999). The unbearable automaticity of being. *American Psychologist, 54*, 462-479. https://doi.org/10.1037/0003-066X.54.7.462

Barrett-Lennard, G. T. (1981). The empathy cycle: Refinement of a nuclear concept. *Journal of Counseling Psychology, 28*, 91-100. https://doi.org/10.1037/0022-0167.28.2.91

Batson, C. D. (2009). These things called empathy: Eight related but distinct phenomena. In J. Decety & W. Ickes (Eds.), *Social Neuroscience. The social Neuroscience of Empathy* (pp. 3-15). Cambridge, MA: MIT Press. https://doi.org/10.7551/mitpress/9780262012973.003.0002

Batson, C. D., Early, S., & Salvarani, G. (1997). Perspective taking: Imagining how another feels versus imaging how you would feel. Personality and Social Psychology Bulletin, 23, 751–758. http://dx.doi .org/10.1177/0146167297237008

Batson, C. D., Fultz, J., & Schoenrade, P. A. (1987). Distress and empathy: Two qualitatively distinct vicarious emotions with different motivational consequences. *Journal of Personality, 55,* 19-39. https://doi.org/10.1111/j.1467-6494.1987.tb00426.x

Batson, C. D., O'Quinn, K., Fultz, J., Vanderplas, M., & Isen, A. (1983). Influence of self-report distress and empathy on egoistic versus altruistic motivation to help. *Journal of Personality and Social Psychology, 45,* 706-718.

Berry, D. R. (2017). Bridging the empathy gap: Effects of brief mindfulness training on helping outgroup members in need. (Doctoral dissertation) Retrieved from Virginia Commonwealth University Scholars Compass.

Berry, D. R. (2019, March 5). Does mindfulness training without ethics-laden instructions promote prosocial behaviors? A meta-analysis. https://doi.org/10.17605/OSF.IO/MSP6T

Berry, D. R., & Brown, K. W. (2017). Reducing separateness with presence: How mindfulness catalyzes intergroup prosociality. In J. Karremans & E. Papies (Eds.). *Mindfulness in Social Psychology*. Hove, UK: Psychology Press.

*Berry, D. R., Cairo, A. H., & Brown, K. W., *Brief mindfulness instruction increases prosocial helping of a dissimilar other.* Unpublished Manuscript

*Berry, D. R., Cairo, A. H., Goodman, R. J., Quaglia, J. T., Green, J. D., & Brown, K. W. (2018). Mindfulness increases prosocial responses toward ostracized strangers through empathic concern. *Journal of Experimental Psychology: General, 147,* 93-112. http://dx.doi.org/10.1037/xge0000392

*Berry, D. R., Wall, C. S. J., Cairo, A. H., Plonski, P. E., & Brown, K. W. *Brief mindfulness instruction increases prosocial helping of an ostracized racial outgroup member.* Unpublished Manuscript

*Berry, D. R., Wall, C. S. J., Tubbs, J. D., Zeidan, F, & Brown, K. W. *Mindfulness increases real-world helping behavior toward racial outgroup members in need.* Unpublished Manuscript

Black, D. S. (2010). Mindfulness research guide: A new paradigm for managing empirical health information. *Mindfulness, 1,* 174-176.

Bloom, P. (2017). *Against empathy: The case for rational compassion*. NY: Random House.

Bodhi, B. (2011). What does mindfulness really mean? A canonical perspective. *Contemporary Buddhism, 12*, 19-39. https://doi.org/10.1080/14639947.2011.564813

Borenstein, M., Hedges, L. V., Higgins, J. P., & Rothstein, H. R. (2011). *Introduction to meta-analysis*. John Wiley & Sons.

Bornemann, B., Herbert, B. M., Mehling, W. E., & Singer, T. (2015). Differential changes in self-reported aspects of interoceptive awareness through 3 months of contemplative training. *Frontiers in Psychology, 5,* 1504. https://doi.org/10.3389/fpsyg.2014.01504

Brockwell, S. E., & Gordon, I. R. (2001). A comparison of statistical methods for meta-analysis. *Statistics in Medicine, 20,* 825-840. https://doi.org/10.1002/sim.650

Brewer, M. B. (2000). Research design and issues of validity. In H. T. Reis & C. M. Judd (Eds). Handbook of research: methods in social and personality psychology (pp. 3-39). New York, NY: Cambridge University Press

Brown, K. W., Creswell, J. D., & Ryan, R. M. (2015). *Handbook of Mindfulness: Theory, Research, and Practice.* New York, NY: The Guilford Press.

Brown, K. W. & Ryan, R. M. (2003). The benefits of being present: Mindfulness and its role in psychological well-being. *Journal of Personality and Social Psychology, 84*(4), 822-848. http://dx.doi.org.ezproxy.csusm.edu/10.1037/0022-3514.84.4.822

Brown, K. W., Ryan, R. M., & Creswell, J. D. (2007). Mindfulness: Theoretical foundations and evidence for its salutary effects. *Psychological Inquiry*, *18*(4), 211-237. https://doi.org/10.1080/10478400701598298

Burnstein, E., Crandall, C., & Kitayama, S. (1994). Some neo-Darwinian decision rules for altruism: Weighing cues for inclusive fitness as a function of the biological importance of the decision. *Journal of Personality and Social Psychology, 67*(5), 773-789.

Campbell, D. T. (1957). Factors relevant to the validity of experiments in social settings. Psychological Bulletin, 54(4), 297-312

*Chen, S. & Jordan, C. H. (2018). Incorporating ethics into brief mindfulness practice: Effects on well-being and prosocial behavior. Mindfulness. https://doi.org/10.1007/s12671-018-0915-2

Cialdini, R. B., Schaller, M., Houlihan, D., Arps, K., Fultz, J., & Beaman, A. L. (1987). Empathy-based helping: Is it selflessly or selfishly motivated? *Journal of Personality and Social Psychology, 52*, 749-758. http://dx.doi.org/10.1037/0022-3514.52.4.749

Cikara, M., & Van Bavel, J. J. (2014). The neuroscience of intergroup relations: An integrative review. *Perspectives on Psychological Science, 9*(3), 245-274. doi: 10.1177/1745691614527464

Cohen, J. (1992). A power primer. *Psychological Bulletin*, *112*, 155-159.

*Condon, P. (2015). Cultivating compassion: The effects of compassion- and mindfulness-based meditation on pro-social mental states and behavior. Doctoral dissertation) Retrieved from http://ezproxy.csusm.edu/login?url=https://search.proquest.com/docview/1692289105?accountid=10363

*Condon, P. (2019). Meditation in context: Factors that facilitate prosocial behavior. *Current*

Opinion in Psychology. https://doi.org/10.1016/j.copsyc.2018.09.011

Condon, P., Desbordes, G., Miller, W. B., & DeSteno, D. (2013). Meditation increases compassionate responses to suffering. *Psychological Science, 24*, 2125-2127. https://doi.org/10.1177/0956797613485603

Cuddy, A. J., Fiske, S. T., & Glick, P. (2007). The BIAS map: behaviors from intergroup affect and stereotypes. *Journal of Personality and Social Psychology*, *92*(4), 631. doi: 10.1037/0022-3514.92.4.63

Darley, J. M., & Batson, C. D. (1973). " From Jerusalem to Jericho": A study of situational and dispositional variables in helping behavior. *Journal of Personality and Social Psychology*, *27*(1), 100-108.

Darley, J. M., & Latané, B. (1968). Bystander intervention in emergencies: Diffusion of responsibility. *Journal of Personality and Social Psychology*, *8*, 377-383.

Davidson, R. J. (2010). Empirical explorations of mindfulness: Conceptual and methodological conundrums. *Emotion, 10*, 8-11. https://doi.org/10.1037/a0018480

Davidson, R. J. & Harrington, A. (2002). *Visions of compassion: Western scientists and Tibetan Buddhists examine human nature.* New York, NY: Oxford University Press

Decety, J., & Jackson, P. L. (2004). The functional architecture of human empathy. *Behavioral and Cognitive Neuroscience Reviews*, *3*(2), 71-100. https://doi.org/10.1177/1534582304267187

DeSteno, D. (2015). Compassion and altruism: How our minds determine who is worthy of help. *Current Opinion in Behavioral Sciences, 3*, 80-83. https://doi.org/10.1016/j.cobeha.2015.002

*DeSteno, D., Lim, D., Duong, F., & Condon, P. (2018). Meditation inhibits aggressive

responses to provocations. *Mindfulness, 9*, 1117-1122. http://dx.doi.org/10.1007/s12671-017-0847-2

Donald, J. N., Sahdra, B. K., Van Zanden, B., Duineveld, J. J., Atkins, P. W. B., Marshall, S. L., Ciarrochi, J. (2018). Does your mindfulness benefit others? A systematic review and meta-analysis of the link between mindfulness and prosocial behavior. *British Journal of Psychology, 110*, 101-125. https://doi.org/10.1111/bjop.12338

Dunne, J. D. (2015) Buddhist Styles of Mindfulness: A Heuristic Approach. In B. Ostafin, M. Robinson, & B. Meier (Eds.) *Handbook of Mindfulness and Self-Regulation*. New York, NY: Springer. https://doi.org/10.1007/978-1-4939-2263-5_18

Duval, S., & Tweedie, R. (2000). A nonparametric "trim and fill" method of accounting for publication bias in meta-analysis. *Journal of the American Statistical Association*, *95*, 89-98. https://doi.org/10.1080/01621459.2000.10473905

Egger, M., Smith, G. D., Schneider, M., & Minder, C. (1997). Bias in meta-analysis detected by a simple, graphical test. *BMJ*, *315*, 629-634. https://doi.org/10.1136/bmj.315.7109.629

Egger, M., Zellweger-Zähner, T., Schneider, M., Junker, C., Lengeler, C., & Antes, G. (1997). Language bias in randomised controlled trials published in English and German. *The Lancet*, *350* 326-329. https://doi.org/10.1016/S0140-6736(97)02419-7

Eisenberg, N. (1988). The development of prosocial and aggressive behavior. In M. H. Bornstein & M. E. Lamb (Eds.), *Developmental Psychology: An Advanced Textbook* (pp. 461-495). Hillsdale, NJ, US: Lawrence Erlbaum Associates, Inc.

Ekman, P., Davidson, R. J., Ricard, M., & Alan Wallace, B. (2005). Buddhist and psychological

perspectives on emotions and well-being. *Current Directions in Psychological Science, 14,* 59-63. https://doi.org/10.1111/j.0963-7214.2005.00335.x

Farb, N. A., Segal, Z. V., Mayberg, H., Bean, J., McKeon, D., Fatima, Z., & Anderson, A. K. (2007). Attending to the present: mindfulness meditation reveals distinct neural modes of self-reference. *Social Cognitive and Affective Neuroscience, 2*(4), 313-322. https://doi.org/10.1093/scan/nsm030

Farb, N. A., Segal, Z. V., & Anderson, A. K. (2012). Mindfulness meditation training alters cortical representations of interoceptive attention. *Social Cognitive and Affective Neuroscience, 8*(1), 15-26. https://doi.org/10.1093/scan/nss066

Fennis, B. M. (2011). Can't get over me: Ego depletion attenuates prosocial effects of perspective taking. *European Journal of Social Psychology, 41,* 580-585. https://doi.org/10.1002/ejsp.828

*Fernando, A. T., Skinner, K., & Consedine, N. S. (2017). Increasing compassion in medical decision-making: can a brief mindfulness intervention help? *Mindfulness, 8,* 276-285.

Fredrickson, B. L., Cohn, M. A., Coffey, K. A., Pek, J., & Finkel, S. M. (2008). Open hearts build lives: positive emotions, induced through loving-kindness meditation, build consequential personal resources. *Journal of Personality and Social Psychology, 95,* 1045-1062. http://dx.doi.org/10.1037/a0013262

*Frost, K. (2017). Calming Meditation Increases Altruism, Decreases Parochialism. *bioRxiv*, 060616.

Goetz, J. L., Keltner, D., & Simon-Thomas, E. (2010). Compassion: An evolutionary analysis and empirical review. *Psychological Bulletin, 136,* 351-374. https://doi.org/10.1037/a0018807

Goleman, D. (2013). *Focus: The hidden driver of excellence*. New York, NY: HarperCollins Publishing.

Greenwald, A. G., McGhee, D. E., & Schwartz, J. L. (1998). Measuring individual differences in implicit cognition: the implicit association test. *Journal of Personality and Social Psychology, 74*, 1464-1480.

Grossman, P. (2011). Defining mindfulness by how poorly I think I pay attention during everyday awareness and other intractable problems for psychology's (re)invention of mindfulness: Comment on Brown et al. (2011). *Psychological Assessment, 23*, 1034-1040. http://dx.doi.org/10.1037/a0022713

*Hafenbrack, A. C., Cameron, L. D., Spreitzer, G. M., Zhang, C., Noval, L. J., & Shaffakat, S. (2019). Helping People by Being in the Present: Mindfulness Increases Prosocial Behavior. *Organizational Behavior and Human Decision Processes*. https://doi.org/10.1016/j.obhdp.2019.08.005

Hedges, L. V. (1981). Distribution theory for Glass's estimator of effect size and related estimators. *Journal of Educational Statistics, 6*, 107-128.

Hedges, L. V., & Vevea, J. L. (1998). Fixed-and random-effects models in meta-analysis. *Psychological Methods, 3*, 486-504. https://doi.org/10.1037/1082-989X.3.4.486

*Heppner, W. L., Kernis, M. H., Lakey, C. E., Campbell, W. K., Goldman, B. M., Davis, P. J., & Cascio, E. V. (2008). Mindfulness as a means of reducing aggressive behavior: Dispositional and situational evidence. *Aggressive Behavior, 34*, 486-496. https://doi.org/10.1002/ab.20258

Heppner, W. L., & Shirk, S. D. (2018). Mindful moments: A review of brief, low-intensity

mindfulness meditation and induced mindful states. *Social and Personality Psychology Compass, 12,* e12424.

Higgins, J. P., & Green, S. (Eds.). (2008). *Cochrane handbook for systematic reviews of Interventions* (Vol. 5). Chichester, England: Wiley-Blackwell.

Hofmann, S. G., Grossman, P., & Hinton, D. E. (2011). Loving-kindness and compassion meditation: Potential for psychological interventions. *Clinical Psychology Review, 31,* 1126-1132. https://doi.org/10.1016/j.cpr.2011.07.003

Hölzel, B. K., Lazar, S. W., Gard, T., Schuman-Olivier, Z., Vago, D. R., & Ott, U. (2011). How does mindfulness meditation work? Proposing mechanisms of action from a conceptual and neural perspective. *Perspectives on psychological science*, *6*(6), 537-559.

Ioannidis, J. P., & Trikalinos, T. A. (2005). Early extreme contradictory estimates may appear in published research: The Proteus phenomenon in molecular genetics research and randomized trials. *Journal of Clinical Epidemiology, 58,* 543-549. https://doi.org/10.1016/j.jclinepi.2004.10.019

Kabat-Zinn, J. (2003). Mindfulness-based stress reduction (MBSR). *Constructivism in the Human Sciences, 8*(2), 73-107.

Kang, Y., Gruber, J. & Gray, J. R. (2013). Mindfulness and de-automatization. *Emotion Review, 5*, 192-201. https://doi.org/10.1177/1754073912451629

Karremans, J. C. & Papies, E. K. (2017). *Mindfulness in social psychology.* New York, NY: Routledge.

Killingsworth, M. A., & Gilbert, D. T. (2010). A wandering mind is an unhappy mind. *Science, 330*(6006), 932-932. doi: 10.1126/science.1192439

*Kirk, U., Gu, X., Sharp, C., Hula, A., Fonagy, P., & Montague, P. R. (2016). Mindfulness

training increases cooperative decision making in economic exchanges: Evidence from fMRI. *NeuroImage, 138,* 274-283. https://doi.org/10.1016/j.neuroimage.2016.05.075

Kreplin, U., Farias, M., & Brazil, I. A. (2018). The limited prosocial effects of meditation: A systematic review and meta-analysis. *Scientific Reports, 8,* 2403. https://doi.org/10.1038/s41598-018-20299-z

Lamm, C., Batson, C. D., & Decety, J. (2007). The neural substrate of human empathy: Effects of perspective-taking and cognitive appraisal. *Journal of Cognitive Neuroscience, 19,* 42-58. https://doi.org/10.1162/jocn.2007.19.1.42

Latané, B. & Darley, J. M. (1970). T*he unresponsive bystander: Why doesn't he help?* Appleton-Century-Crofts.

Latané, B., & Rodin, J. (1969). A lady in distress: Inhibiting effects of friends and strangers on bystander intervention. *Journal of Experimental Social Psychology, 5,* 189-202.

*Liang, L. H., Brown, D. J., Ferris, D. L., Hanig, S., Lian, H., & Keeping, L. M. (2018). The dimensions and mechanisms of mindfulness in regulating aggressive behaviors. *Journal of Applied Psychology, 103,* 281-299. http://dx.doi.org/10.1037/apl0000283

Light, R., and Pillemer, D. (1984), *Summing up: The science of reviewing research.* Cambridge, MA: Harvard University Press

*Lim, D., Condon, P., & DeSteno, D. (2015). Mindfulness and compassion: An examination of mechanism and scalability. *PLoS ONE, 10*: e0118221. https://doi/org/10.1371/journal.pone.0118221

Lindsay, E. K., & Creswell, J. D. (2014). Helping the self help others: Self-affirmation increases self-compassion and pro-social behaviors. *Frontiers in Psychology, 5,* 421. https://doi.org/10.3389/fpsyg.2014.00421

Lippelt, D. P., Hommel, B., & Colzato, L. S. (2014). Focused attention, open monitoring and loving kindness meditation: effects on attention, conflict monitoring, and creativity–A review. *Frontiers in Psychology*, *5*, 1083. https://doi.org/10.3389/fpsyg.2014.01083

*Long, E. C., & Christian, M. S. (2015). Mindfulness buffers retaliatory responses to injustice: A regulatory approach. *Journal of Applied Psychology*, *100*, 1409-1422. http://dx.doi.org/10.1037/apl0000019

Luberto, C. M., Wasson, R. S., Kraemer, K. M., Sears, R. W., Hueber, C., & Cotton, S. (2017). Feasibility, acceptability, and preliminary effectiveness of a 4-week mindfulness-based cognitive therapy protocol for hospital employees. *Mindfulness, 8*, 1522-1531. https://doi.org/10.1007/s12671-017-0718-x

Lueke, A., & Gibson, B. (2015). Mindfulness meditation reduces implicit age and race bias: The role of reduced automaticity of responding. *Social Psychological and Personality Science*, *6*(3), 284-291. doi: 10.1177/1948550614559651

*Lueke, A., & Gibson, B. (2016). Brief mindfulness meditation reduces discrimination. *Psychology of Consciousness: Theory, Research, and Practice*, *3*, 34-44. http://dx.doi.org/10.1037/cns0000081

Lutz, A., Jha, A. P., Dunne, J. D., & Saron, C. D. (2015). Investigating the phenomenological matrix of mindfulness-related practices from a neurocognitive perspective. *American Psychologist*, *70*(7), 632-658. https://doi.org/10.1037/a0039585

MacCoon, D. G., Imel, Z. E., Rosenkranz, M. A., Sheftel, J. G., Weng, H. Y., Sullivan, J. C., ... & Lutz, A. (2012). The validation of an active control intervention for Mindfulness Based Stress Reduction (MBSR). *Behaviour Research and Therapy, 50*(1), 3-12. doi:10.1016/j.brat.2011.10.011

Mascaro, J. S., Rilling, J. K., Tenzin Negi, L., & Raison, C. L. (2012). Compassion meditation enhances empathic accuracy and related neural activity. *Social Cognitive and Affective Neuroscience*, *8*, 48-55. https://doi.org/10.1093/scan/nss095

Moher, D., Liberati, A., Tetzlaff, J., & Altman, D. G. (2009). Preferred reporting items for systematic reviews and meta-analyses: the PRISMA statement. *Annals of internal medicine*, *151*, 264-269.

Monteiro, L. M., Musten, R. F., & Compson, J. (2015). Traditional and contemporary mindfulness: finding the middle path in the tangle of concerns. *Mindfulness*, *6*, 1-13.

Neff, K. D. (2003). The development and validation of a scale to measure self-compassion. *Self and Identity*, *2*, 223-250. https://doi.org/10.1080/15298860309027

Olendski, A. (2005) The roots of mindfulness. In C. K. Germer, R. D. Siegel, & P. R, Fulton (Eds.) *Mindfulness and psychotherapy* (pp. 241-261). New York: Guilford Press.

Patsopoulos, N. A., Evangelou, E., & Ioannidis, J. P. (2008). Sensitivity of between-study heterogeneity in meta-analysis: proposed metrics and empirical evaluation. *International Journal of Epidemiology*, *37*, 1148-1157. https://doi.org/10.1093/ije/dyn065

Penner, L. A., Dovidio, J. F., Piliavin, J. A., & Schroeder, D. A. (2005). Prosocial behavior: Multilevel perspectives. *Annual Review of Psychology*, *56*, 365-392. doi: 10.1146/annurev.psych.56.091103.070141 https://doi.org/10.1017/S0140525X02000018

Quaglia, J. T., Braun, S. E., Freeman, S. P., McDaniel, M. A., & Brown, K. W. (2016). Meta-analytic evidence for effects of mindfulness training on dimensions of self-reported dispositional mindfulness. *Psychological Assessment*, *28*, 80-8183. http://dx.doi.org/10.1037/pas0000268

Quintana, D., & Tiebel, J. (2018, August 2). How to calculate statistical power for your meta-analysis. Retrieved from osf.io/5c7uz

*Ramsey, A. T., & Jones, E. E. (2015). Minding the interpersonal gap: Mindfulness-based interventions in the prevention of ostracism. *Consciousness and Cognition, 31*, 24-34. https://doi.org/10.1016/j.concog.2014.10.003

*Ridderinkhof, A., de Bruin, E. I., Brummelman, E., & Bögels, S. M. (2017). Does mindfulness meditation increase empathy? An experiment. *Self and Identity, 16*, 251-269. https://doi.org/10.1080/15298868.2016.1269667

Rogers, C. R. (1959). A theory of therapy, personality, and interpersonal relationships, as developed in the client-centered framework. In S. Koch (Eds.), *Psychology: A Study of a Science: Vol. 3* (pp. 184-256). New York, NY: McGraw-Hill.

Rogers, C. R. (1975). Empathic: An unappreciated way of being. *The Counseling Psychologist, 5*, 2-10. https://doi.org/10.1177/001100007500500202

Rosenberg, E. L., Zanesco, A. P., King, B. G., Aichele, S. R., Jacobs, T. L., Bridwell, D. A., ... & Lavy, S. (2015). Intensive meditation training influences emotional responses to suffering. *Emotion, 15*, 775-790. http://dx.doi.org/10.1037/emo0000080

Rosenthal, R. (1979). The file drawer problem and tolerance for null results. *Psychological Bulletin, 86*, 638-641. http://dx.doi.org/10.1037/0033-2909.86.3.638

Rothstein, H. R., Sutton, A. J., & Borenstein, M. (Eds.). (2006). *Publication bias in meta-analysis: Prevention, assessment and adjustments*. Chichester, England: John Wiley & Sons.

Salzberg, S. (2002). *Loving-kindness: The revolutionary art of happiness*. Boston: Shambala Publications.

Saucier, D. A., Miller, C. T., & Doucet, N. (2005). Differences in helping whites and blacks: A meta-analysis. *Personality and Social Psychology Review, 9*(1), 2-16. https://doi.org/10.1207/s15327957pspr0901_1

Sedlmeier, P., Losse, C., & Quasten, L. C. (2018). Psychological effects of meditation for healthy practitioners: an update. *Mindfulness, 9*(2), 371-387.

*Schindler, S., Pfattheicher, S., & Reinhard, M. A. (2019). Potential negative consequences of mindfulness in the moral domain. *European Journal of Social Psychology, 49*, 1055-1069. https://doi.org/10.1002/ejsp.2570

Schulz, K. F., Altman, D. G., & Moher, D. (2010). CONSORT 2010 statement: updated guidelines for reporting parallel group randomised trials. *BMC Medicine, 8*(1), 18. doi: 10.1186/1741-7015-8-18

Schuster, R. (1979). Empathy and mindfulness. *The Journal of Humanistic Psychology, 19*, 71-77.

Simmons, J. P., Nelson, L. D., & Simonsohn, U. (2011). False-positive psychology: Undisclosed flexibility in data collection and analysis allows presenting anything as significant. *Psychological Science, 22*, 1359-1366. https://doi.org/10.1177/0956797611417632

Simonsohn, U., Nelson, L. D., & Simmons, J. P. (2014a). p-curve and effect size: Correcting for publication bias using only significant results. *Perspectives on Psychological Science, 9*, 666-681. https://doi.org/10.1177/1745691614553988

Simonsohn, U., Nelson, L. D., & Simmons, J. P. (2014). P-curve: a key to the file-drawer. *Journal of Experimental Psychology: General, 143*, 534-547. http://dx.doi.org/10.1037/a0033242

Singer, T., Critchley, H. D., & Preuschoff, K. (2009). A common role of insula in feelings, empathy and uncertainty. *Trends in Cognitive Sciences, 13,* 334-340. https://doi.org/10.1016/j.tics.2009.05.001

Sterne, J. A., & Egger, M. (2005). Regression methods to detect publication and other bias in meta-analysis. In H. R. Rothstein, A. J. Sutton, & M. Borenstein (Eds.) *Publication bias in meta-analysis: Prevention, assessment and adjustments*, (pp. 99-110). Chichester, England: John Wiley & Sons.

*Tan, L. B. G., Lo, B. C. Y., & Macrae, C. N. (2014). Brief mindfulness meditation improves mental state attribution and empathizing. *PLoS ONE 9*: e110510.

*Tincher, M. M., Lebois, L. A., & Barsalou, L. W. (2016). Mindful attention reduces linguistic intergroup bias. *Mindfulness, 7*(2), 349-360.

Toi, M., & Batson, C. D. (1982). More evidence that empathy is a source of altruistic motivation. *Journal of Personality and Social Psychology, 43*, 281-292. http://dx.doi.org/10.1037/0022-3514.43.2.281

Tomasello, M. (2009). *The cultural origins of human cognition*. Harvard university press.

Trautwein, F. M. Nanjo, J. R., & Schmidt, S. (2014). Meditation effects in the social domain: self-other connectedness as a general mechanism? In *Meditation-Neuroscientific Approaches and Philosophical Implications* (pp. 175-198). Springer International Publishing.

Valentine, J. C., Pigott, T. D., & Rothstein, H. R. (2010). How many studies do you need? A primer on statistical power for meta-analysis. *Journal of Educational and Behavioral Statistics, 35,* 215-247. https://doi.org/10.3102/1076998609346961

Van Doesum, N. J., Van Lange, D. A., & Van Lange, P. A. (2013). Social mindfulness: Skill and

will to navigate the social world. *Journal of Personality and Social Psychology, 105,* 86-103. https://doi.org/10.1037/a0032540

Varela, F. J. & Depraz, N. (2003). Imagining: Embodiment, phenomenology, transformation. In *Buddhism & science: Breaking new ground* (pp. 195-230). New York, NY: Columbia University Press.

Wallace, B. A. (1999). The Buddhist tradition of Samatha: Methods for refining and examining consciousness. *Journal of Consciousness Studies, 6,* 175-187.

Williams, P. B., Poljacik, G., Decety, J., & Nusbaum, H. C. (2018). Loving-kindness language exposure leads to changes in sensitivity in imagined pain. *The Journal of Positive Psychology, 13,* 429-433. https://doi.org/10.1080/17439760.2017.1315648

*Yusainy, C., & Lawrence, C. (2015). Brief mindfulness induction could reduce aggression after depletion. *Consciousness and Cognition, 33,* 125-134. https://doi.org/10.1016/j.concog.2014.12.008

Zaki, J. (2014). Empathy: A motivated account. *Psychological Bulletin, 140,* 1608-1647. http://dx.doi.org/10.1037/a0037679

Zeidan, F., Emerson, N. M., Farris, S. R., Ray, J. N., Jung, Y., McHaffie, J. G., & Coghill, R. C. (2015). Mindfulness meditation-based pain relief employs different neural mechanisms than placebo and sham mindfulness meditation-induced analgesia. *Journal of Neuroscience*, *35*(46), 15307-15325.

Figure Captions

Figure 1. Graphic depiction of moderator analyses performed in a previous meta-analysis by Donald and colleagues (2018).

Figure 2. Flow chart of literature search results and study selection for meta-analysis.

Figure 3. Forest plot of mean difference effect sizes (Hedge's *g*) and 95% confidence intervals of mindfulness training, relative to the average of active and inactive controls, on prosocial outcomes. *Note.* *mindfulness vs. active control, **mindfulness vs. inactive control, ***Mindfulness vs. average of active and inactive control.

Figure 4. Funnel plot of trim-and-fill analyses for primary meta-analysis. Circles with no fill = published studies. Circles with black fill = imputed studies. Diamonds represent summary effect sizes.

Figure 5. *p*-curve of 29 *p*-values that were reported first in manuscripts comparing mindfulness to controls on prosocial outcomes. The dotted line shows the expected distribution of *p*-values if there is no effect, and dashed line shows the expected distribution of *p*-values if the effect existed and studies averaged 33% power. Figures were generated by *p*-curve app 4.06.

Figure S1. *p*-curve "robustness" of the eight *p*-values reported second in each manuscript. The five *p*-values (bottom left) reported first and three *p*-values reported second (bottom right) in manuscripts that compared mindfulness to inactive controls on prosocial outcomes. Solid lines

indicate the distribution of statistically significant *p*-values. The dotted line shows the expected distribution of *p*-values if there is no effect, and dashed line shows the expected distribution of *p*-values if the effect existed and studies averaged 33% power. Figures were generated by *p*-curve app 4.06.