THE EFFECTS OF MINDFULNESS AND KINDNESS MEDITATION ON TEACHER EMOTIONAL ABILITIES, COMPASSION, AND PROSOCIAL BEHAVIOR

by

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Abstract

Teachers evidence high rates of occupational burnout and turnover. Accordingly, the present study evaluated the feasibility and acceptability of digital meditation practices for teachers. The effects of digital meditation on teacher affect and mindfulness, and on teacher emotion skills, compassion, and prosocial behavior also were examined. Participants were K-12 teachers from Western Canada (N = 121). Teachers were randomly assigned to mindfulness meditation (MM), kindness meditation (KM), or music relaxation (MR; active control) training for six weeks (approximately one hour a week).

Teachers reported the digital MM, KM, and MR practices to be highly engaging, and moderately efficacious and enjoyable. Within-group increases in positive affect (d = 0.42) and mindfulness skills (ds = 0.42–0.45) were found in the MM group. Within-group reductions in negative affect (ds = -0.81, -0.83, -0.89) and stress (ds = -0.54, -0.49, -0.68) were found in the MM, KM, and MR groups, respectively. However, neither MM nor KM improved affect or mindfulness compared to the MR control. Thus, the within-group effects may (to some extent) reflect placebo effects and/or demand characteristics.

Regarding the effects of training on emotion skills, compassion, and prosocial behavior, only one finding was observed. KM increased prosocial behavior (decreased altruistic punishment; d = -0.56) compared to MM in an online economic game. From pre-test to post-test KM reduced, whereas MM iatrogenically heightened, altruistic punishment. Exploratory analyses were conducted that also tested the effects of training on emotion skills, compassion, and prosocial behavior in the female subsample (n = 96) (the male subsample was too small – n = 22 – to conduct gender-by-group interactions or male-only analyses). In female teachers, KM promoted prosocial behavior in online

economic games: KM versus the MR control increased altruistic giving (d = 0.57), and KM versus MM decreased altruistic punishment (d = -0.77).

Overall, the feasibility and acceptability of digital meditation (and music relaxation) practices for teachers was supported. However, the efficacy of digital meditations as tools to foster adaptive teacher social-emotional behavior received limited support. Studies utilizing more intensive interventions that employ longitudinal designs are needed to determine the social-emotional efficacy of digital meditation for educators.

Lay Summary

The present research examined the feasibility and efficacy of digital mindfulness meditation (MM) and kindness meditation (KM) trainings for K-12 teachers. Teachers reported the MM and KM practices, and the music relaxation (MR; control group) practices, to be highly engaging and somewhat effective and enjoyable. MM, KM, and MR all reduced teacher negative affect and stress over time as well. Further, KM decreased punishment behavior of unfair others (as predicted) in an economic game compared to MM, which increased punishment behavior (counter to prediction). KM also enhanced altruistic giving in (female) teachers in an economic game compared to the MR control. The results suggest that digital meditations may be feasible and acceptable tools for fostering healthy teacher social-emotional behavior. However, almost no differences were found between the digital meditation and music relaxation control trainings. Further study is needed to determine the efficacy of digital MM and KM practices for educators.

Preface

Approval for recruitment, administering the study interventions and measures online to teacher participants required expedited ethical approval from the University of British Columbia Behavioural Research Ethics Board (BREB; certificate number H16-00612). After receiving approval from BREB, ethical approval was obtained from the Vancouver School Board, Coquitlam School District, Burnaby School District, Surrey School District, and Sunshine Coast School District prior to recruitment and study participation efforts commencing.

Collaborators were the following individuals: Drs. Kimberly Schonert-Reichl, Patricia Jennings, and Bruno Zumbo, as well as graduate research assistant Jacqueline Maloney (M.A.). The collaborators provided guidance throughout the dissertation project, and Jacqueline Maloney provided some assistance with participant recruitment; however, the present researcher (James Lyman Floman) is otherwise solely responsible for the following work: the study design, participant recruitment, data collection, data analysis, and writing of the dissertation proposal and final dissertation document. At present, no publications arising from this work yet exist.

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Chapter 1: Introduction

> "I've come to a frightening conclusion that I am the decisive element in the classroom. It's my personal approach that creates the climate. It's my daily mood that makes the weather. As a teacher, I possess a tremendous power to make a child's life miserable or joyous. I can be a tool of torture or an instrument of inspiration. I can humiliate or heal. In all situations, it is my response that decides whether a crisis will be escalated or de-escalated and a child humanized or dehumanized" (Ginott, 1972, pp. 122).

Teaching is an emotionally demanding profession (Hargreaves, 1998, 2000, 2001; Schutz & Zembylas, 2009; Sutton & Wheatley, 2003). Teachers must navigate a complex set of job responsibilities (e.g., instruction, assessment, mentorship) and interpersonal relationships (e.g., teacher-student, teacher-parent, teacher-administrator interactions). Further, teachers are faced with the daily task of regulating their own as well as their students' cascading emotions (Fried, Mansfield, & Dobozy, 2015). Teachers thus report experiencing stress, anxiety, and anger on most school days (Frenzel, Becker-Kurz, Pekrun, & Goetz, 2015; Greenberg, Brown, & Abenavoli, 2016). Subsequently, a large portion of teachers across North America experience 'emotion regulation failure,' and report low job satisfaction and high occupational burnout compared to other human service professionals with about 30% to 50% teachers leaving the field in the first five years (Johnson et al., 2005; Montgomery & Rupp, 2005; Naylor & White, 2010; Travers, 2017). The downstream effects of chronic negative affect on teachers who remain in the profession are not well-understood. Yet, a plausible case can be made that their effects are deleterious to teacher psychological well-being, social relationships, and job performance (Hamre et al., 2013; Hamre & Pianta, 2005, 2006; Howard & Johnson, 2004; Jennings & Frank, 2015; Lupien, McEwen, Gunnar, & Heim, 2009; Schonert-Reichl, 2017; Travers, 2017). Although the study of teacher emotion is relatively new

(Fried et al., 2015), teacher positive and negative emotions may influence the direction of teacher behavior toward caring or hostile relationships with their students, and thereby, influence student social-emotional development and learning (Frenzel, Goetz, Lüdtke, Pekrun, & Sutton, 2009; Hagenauer, Hascher, & Volet, 2015; Jennings & Greenberg, 2009; Loman & Gunnar, 2010; Maldonado-Carreno & Votruba-Drzal, 2011; Milkie & Warner, 2011; Oberle & Schonert-Reichl, 2016; Roorda, Koomen, Spilt, & Oort, 2011).

As such, enhancing teachers' emotion skills (i.e., emotion awareness and emotion regulation) is a promising pathway to improve teacher burnout and job satisfaction, and also potentially to enhance personally and academically supportive relationships with their students (Brackett, Palomera, Mojsa-Kaja, Reyes, & Salovey, 2010; Jennings & Greenberg, 2009; Joseph & Newman, 2010; Roeser, Skinner, Beers, & Jennings, 2012; Skinner & Beers, 2016). For a theoretical model depicting how mindfulness meditation (MM)-based trainings in particular may be used to develop teachers' emotion skills and their prosocial qualities, see Figure 1 (below). In support of part of this model, emerging research suggests that MM-based trainings – which are typically intensive programs that include psychoeducation, discussion groups, and attention-centering meditations – improve teacher emotion awareness and regulation (Flook, Goldberg, Pinger, Bonus, & Davidson, 2013; Jennings et al., 2017; Kemeny et al., 2012; Roeser et al., 2013). However, the effects of meditation on teacher well-being (vs. stress) and positive (vs. negative) social characteristics, such as compassion and prosocial behavior, are scant (although evidence of the prosocial effects of meditation is emerging in other adult populations; e.g., Condon, Desbordes, Miller, & DeSteno, 2013; Weng et al., 2013, 2015). Further, virtually nothing is known regarding the independent effects of kindness

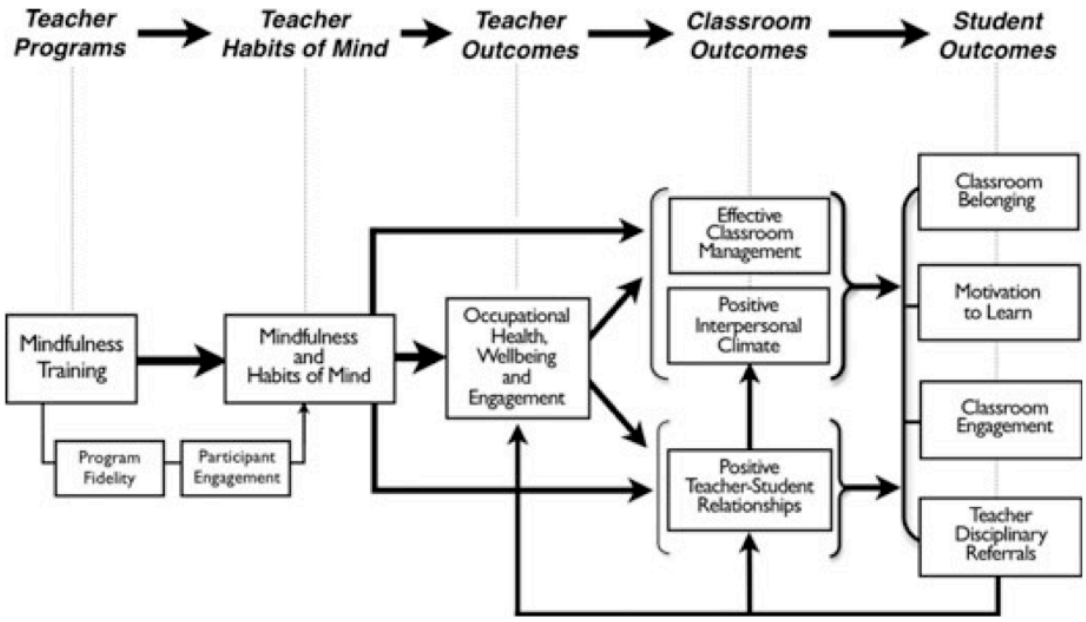

Figure 1. Theoretical model of mindfulness meditation-based training effects on teacher social-emotional and professional functioning, and consequently, the classroom environment as well as student performance and learning (Roeser et al., 2012; p. 171).

meditation (KM) on teachers, even though KM is designed to cultivate positive emotions, compassion, and prosocial tendencies in meditators (Kornfield, 2008, 2017; Salzberg, 2002). Overall, it is therefore important to highlight that the majority of research on teacher emotion and emotion skills has focused primarily on assuaging teacher stress and ill-being. Despite emerging theory and research that suggests teacher well-being, compassion, and prosocial behavior may be key to teacher-student relationships that support student learning and growth (e.g., Frenzel et al., 2009; Hamre & Pianta, 2005, 2006, 2010; Jennings & Greenberg, 2009; Roeser et al., 2012; Noddings, 2013), historically, there has been minimal emphasis on fostering adaptive social-emotional capacities in teachers (for exceptions see Jennings et al., 2017; Roeser et al., 2013). Accordingly, the present study aims to help fill this lacuna in the field.

Although limited, the growing 'contemplative education' literature suggests that MM and KM may be effective methods for improving teacher emotion skills, ill-being, and performance (Emerson et al., 2017; Jennings et al., 2017; Jennings & DeMauro, 2017; Roeser et al., 2012; Skinner & Beers, 2016). That said, what do MM and KM trainings consist of and how might they influence teachers compassion and prosocial behavior? Although more detailed answers to these questions are provided in the following sections, summary answers are provided here. In the central meditation component of most MM programs, meditators deliberately concentrate their attention on the present moment (often by focusing attention on the breath), while sustaining an accepting, nonjudgmental awareness toward all stimuli and experience (Kabat-Zinn, 2011; Kornfield, 2017). Broadening the field of present-centered, receptive attention via MM may heighten teacher attunement to various forms of student suffering in the classroom (see Halifax, 2012; Roeser et al., 2012). Further, MM may promote the effective regulation of negative emotions (e.g., anxiety, anger, personal distress) that impede prosocial teacher-student interactions by training teachers' capacity to perceive but not habitually react to pleasant and unpleasant emotions as they arise (see Batson, 2011; Skinner & Beers, 2016; Taylor et al., 2015; Vago & Silbersweig, 2012). In the central meditation component of most KM programs, practitioners visualize receiving and giving wishes for happiness and freedom from suffering to all beings, from self and loved ones to strangers and difficult others (Kornfield, 2008, 2017; Salzberg, 2002). By consciously generating positive emotions toward familiar and liked (i.e., ingroup) as well as unfamiliar and unliked (i.e., outgroup) individuals equally (e.g., helpful and challenging students), teacher compassion for all people may grow and thereby foster

prosocial teacher-student relationships (Bloom, 2017; Jennings et al., 2017; Jennings & Greenberg, 2009; Roeser et al., 2012). Accordingly, the primary hypotheses of this study are that MM and KM training versus an active control will enhance teacher emotional awareness and regulation, as well as compassion and prosocial behavior.

The effects of routine MM or KM training on teacher emotional abilities and compassion, and their subsequent effects on teacher prosocial behavior, however, are unclear. As noted, some studies employing rigorous designs and objective measures suggest that intensive meditation-based teacher programs enhance teacher emotional awareness and emotion regulation and may influence teacher well-being (Jennings et al, 2017; Kemeny et al., 2012; Roeser et al., 2013). However, much of the data in the field are derived from pilot studies or randomized controlled trials (RCTs) using passive control groups or self-report measures only, limiting their internal and ecological validity (Emerson et al., 2017; Jennings & DeMauro, 2017). Further, prior RCTs have employed meditation-based programs that combine MM and KM as well as psychoeducation and group discussions (Emerson et al., 2017; Jennings et al., 2017). Therefore, it is generally unknown whether meditation is indeed the 'active ingredient' in these trainings, or what the unique effects of MM vs. KM practices may be, given their distinct practice content, methods, and goals (Nash & Newberg, 2013; Singer et al., 2016). With that in mind, this study aimed to address these gaps in the field by comparing the effects of standalone MM and KM practices to music relaxation (an active control) on teacher emotion skills, compassion, and prosocial behavior using first-person and third-person measures.

Additionally, as they can be time and resource-intensive, in-person meditation-based trainings for teachers may provide limited scalability potential (see Wahbeh,

Svalina, & Oken, 2014). Consequently, in this study all trainings were delivered digitally. Digital trainings can be adapted to suit busy teacher schedules and allow teachers to engage in practices from virtually any location. At present, only a small number of RCTs of digital meditation's efficacy have been run (Spijkerman, Pots, & Bohlmeijer, 2016), and no such studies have been conducted with teachers. However, given the accessibility and scalability potential of digital meditation, this is a key issue in meditation and teacher research the present study addresses. As such, this study also tests the feasibility and acceptability of digital MM and KM trainings for teachers, and thus, aimed to contribute to an emerging literature evaluating online psychological interventions (Andersson, 2009; Proudfoot, 2013; Seligman, Steen, Park, & Peterson, 2005; Spijkerman et al., 2016).

Ultimately, knowledge of whether, and how, different digital meditation trainings influence teacher emotional skills, compassion, and prosocial behavior will help to: (i) substantiate simple-to-administer meditation trainings for educators (potentially increasing their accessibility, implementation, and scalability); (ii) elucidate mechanisms of meditation's prosocial effects, thereby helping to optimize training applications; and (iii) inform questions regarding the relative efficacy of training type (mindfulness vs. kindness meditation) based on a diverse set of outcomes (emotion skills vs. compassion vs. prosocial behavior), as assessed by self-report and behavioral measures (Singer et al., 2016). Insights from this research may also empower teachers to lead more equanimous and compassionate lives and to cultivate relationships that enrich student social-emotional development and learning (Davidson et al., 2012; Jennings & DeMauro, 2017; Jennings & Greenberg, 2009; Roeser et al., 2012; Schonert-Reichl, 2017).

Chapter 2: Background and Significance

In Chapter 2, a literature review is provided in seven sections that builds the case for the present study along with two sets of hypotheses. In Section 2.1, research on the emotional labor of teaching is examined along with the possible effects of emotional labor on teacher and student social-emotional well-being and performance. Also, a theoretical model is offered that guides the present research aims. In Section 2.2, definitions of MM and KM are provided as well as evidence of their salutary effects on emotion skills, compassion, and prosocial behavior in the general population. In Section 2.3, the limited (but growing) research on meditation-based trainings for teachers is reviewed, with a focus on the emotional and prosocial effects of the trainings. In Section 2.4, relevant data on the efficacy of digital meditation trainings is also considered. In Section 2.5, research on possible gender differences in the primary dependent variables and in response to meditation training is discussed. In Section 2.6, all sections are integrated and a synopsis of the gaps in prior research is offered to highlight the need for the present study. Finally, in Section 2.7, the three study aims are articulated along with two sets of hypotheses: a primary hypothesis set that was grounded in prior work, and a secondary hypothesis set that was exploratory and aimed to facilitate theory-building.

2.1 The Emotional Labor of Teaching, and Its Effects on Teachers and Students

Teachers navigate a multilayered and time-pressured set of professional duties and relationships, while acting as leading socialization and emotion regulation agents for their students (Eccles & Roeser, 2010; Fried et al., 2015; Hargreaves, 1998, 2000; Sutton & Harper, 2009). The responsibilities and interpersonal demands of teaching include: instructing unmotivated students, addressing student academic and behavioral difficulties,

adapting curricula to meet individual student needs and new requirements, preparing students for high-stakes testing, serving in leadership roles, conferencing with parents, and assisting colleagues and administrators with schoolwide and nationwide education issues (e.g., funding shortages; Collie, Perry, & Martin, 2017; Markow, Macia, & Lee, 2013; Richards, 2012). Teaching is thus among the most stressful professions in any industry (Johnson et al., 2005; Montgomery & Rupp, 2005; Travers, 2017). By a recent account, teaching (in the U.S.) was tied with nursing and eclipsed doctors as the profession with the most workers experiencing high stress: 46% (Gallup, 2014). In another U.S. survey, the percentage of teachers reporting 'great job stress' on most days rose from 36% in 1985 to 51% in 2012 (Markow et al., 2013). In particular, teachers indicate chronically experiencing anger, sadness, and anxiety in the classroom (Cross & Hong, 2012; Sutton & Wheatley, 2003). In a recent experiment, German elementary and secondary school teachers recorded their emotions before and after every class period for two weeks (Frenzel et al., 2015). Teachers reported feeling at least a moderate degree of anger in 41% of class periods and a moderate degree of anxiety in 22% of class periods.

Correspondingly, teachers across North America and Europe evidence high rates of occupational burnout (i.e., perceptions of emotional exhaustion and depersonalization, and feelings of low personal accomplishment; Hakanen, Bakker, & Schaufeli, 2006; Maslach, Schaufeli, & Leiter, 2001; Purvanova & Muros, 2010; Schaufeli, 2006; Travers, 2001, 2017). Teachers also report low job satisfaction and well-being across the U.S., U.K., Australia, and Canada compared to other human service professionals (Collie, Shapka, Perry, & Martin, 2015; Crossman & Harris, 2006; Howard & Johnson, 2004; Johnson et al., 2005; Renshaw, Long, & Cook, 2015; Schonfeld, 2001; Travers &

Cooper, 1993). In one survey, teacher job satisfaction in the U.S. dropped from its peak at 62% in 2008 to 39% in 2012: the largest decline since 1984 when the survey began (Markow et al., 2013). Unsurprisingly, burned out teachers with low job satisfaction may show elevated absenteeism (Bowers, 2004; Darr & Johns, 2008), and between about 30% to 50% of teachers leave the profession in the first five years internationally, often citing stress and burnout as leading factors (Canadian Teachers' Federation, 2011; Greenberg et al., 2016; Jalongo & Heider, 2006; Latham & Vogt, 2007; Wilhelm, Dewhurst-Savellis, & Parker, 2000). In the U.S., for example, about 90% of current teachers are replacements for teachers who left prior to retirement (Ingersoll, 2002; Ingersoll & May, 2012). Importantly, the data reveal that the emotional labor of teaching often exceeds the skills teachers possess to adaptively manage this load (Jennings & Greenberg, 2009).

The costs of teacher stress, burnout, and turnover are consequential. Overall, in the U.S., U.K., Australia, Canada, Germany, and beyond, teacher job stress and burnout are associated with: *mental health problems* (i.e., anxiety, depression, insomnia, somatization, low self-esteem; Ballou, 2012; Bowers, 2004; Howard & Johnson, 2004; Montgomery & Rupp, 2005; Schonfeld, 1992, 2001; Tang, Au, Schwarzer, & Schmitz, 2001; Tennant, 2001), *lower well-being* (less fulfillment in workload, organizational functioning, and student interactions; Collie et al., 2015, 2017), and *physical ill-being* (e.g., unhealthy patterns of the stress hormone cortisol; more smoking, drinking, binge eating, doctor visits, and sick days; Chambers, 1992; Chambers & Belcher, 1993; Johnson et al., 2005; Pruessner, Hellhammer, & Kirschbaum, 1999; Wilhelm et al., 2000). Also, the high rate of teacher turnover in the U.S., for instance, is estimated to cost

the economy over $7 billion per year, and it adversely impacts at-risk schools the most (National Commission on Teaching and America's Future, 2007).

In addition to the numerous costs for teachers and the economy, teacher stress and burnout adversely impact students. By impairing healthy teacher social-emotional behavior (Darr & Johns, 2008; Greenberg et al., 2016; Howard & Johnson, 2004; Lupiens et al., 2009), chronic stress and burnout may hamper teacher engagement and supportive teacher-student interactions, along with the delivery of instruction, thereby impeding student academic and personal development (Hamre & Pianta, 2005, 2006; Maldonado-Carreno & Votruba-Drzal, 2011; Pianta, Belsky, Vandergrift, Houts, & Morrison, 2008a; Roffey, 2012; Roorda et al., 2011; Spilt, Koomen, & Thijs, 2011; Wentzel, 1997, 1998, 2002). Students of stressed teachers, for example, have been found to experience more externalizing problems (e.g., arguing, impulsivity), interpersonal communication issues (e.g., expressing emotions, resolving conflicts), and internalizing problems (e.g., anxiety, low self-esteem; Milkie & Warner, 2011). Similarly, a recent study examining 'stress contagion' between teachers and students found teacher burnout predicted students' morning cortisol levels (Oberle & Schonert-Reichl, 2016). Burnout consisted of teachers' feelings of emotional exhaustion and depersonalization, and awakening cortisol is a physiological marker of autonomic stress regulation (Loman & Gunnar, 2010). Furthermore, the frequent expression of negative emotions and hostility by teachers predicts maladaptive behavior and academic problems in primary and secondary school students (Lewis, Romi, Katz, & Qui, 2008; Lewis, Romi, Qui, & Katz, 2005). Likewise, a study found that teachers with greater depressive symptoms in the winter were more likely to have students with poor math achievement in the spring, although only for

students who showed math deficiencies in the fall (McLean & Connor, 2015). This is noteworthy, as it suggests that students with the greatest learning difficulties may suffer the most when instructed by a teacher overcome by emotion dysregulation.

That said, 'emotion contagion' does not appear to be limited to negative affect and stress. Teachers who regularly express positive emotions, and effectively manage their negative emotions, can promote student engagement in even traditionally challenging subjects (e.g., mathematics; Frenzel et al. 2009), and they can also promote healthy student social-emotional behavior (Sutton & Harper, 2009; Wentzel, 2002) (for a review of teacher emotion research see Fried et al., 2015). In support of this point, a recent meta-analysis found that emotionally positive teacher-student relationships predicted student academic engagement (a medium-sized[1] effect, $r = .34$) as well as academic achievement (a small effect; $r = .16$; Roorda et al., 2011). Further, at the start of the school year, teacher personal accomplishment (a reverse scored dimension of teacher burnout; Maslach, et al., 2001), predicted fewer student externalizing behaviors and higher classroom quality later in the year, as measured by observations of teacher-student interactions (Hoglund, Klingle, & Hosan, 2015). Enhanced classroom quality, then in turn, predicted improved student social relationships and literary skills.

Taken together, the reviewed evidence indicates there is much at stake in how effectively the complex emotional labor of teaching is managed. Teachers ill-equipped to meet the social-emotional demands of the classroom are more likely to burnout and quit the profession, or experience mental and physical health problems if they remain. Additionally, at least some of these detrimental effects may be transmitted to their

[1] Approximate effect size conventions used throughout this dissertation are: $r = .10$ (small), $r = .30$ (medium), $r = .50$ (large); and $d = .20$ (small), $d = .50$ (medium), $d = .80$ (large) (Cohen, 1988).

students, potentially adversely influencing student mental health, social adjustment, and academic achievement. Some data also suggest that teachers' experiences of positive emotions may help to 'undo' the harmful effects of stress and negative emotions, and that they may promote well-being and job satisfaction in teachers and possibly academic engagement in students (e.g., Frenzel et al., 2009; Jennings & Greenberg, 2009; Roth, Assor, Kanat-Maymon, & Kaplan, 2007; see also Fredrickson, Mancuso, Branigan, & Tugade, 2000). Nevertheless, most research on teacher emotion has investigated how stress damages teacher, and to some extent student, health and performance. Far fewer studies examine the effects of teacher emotional resilience on adaptive teacher or student capacities, including compassion and prosocial behavior (see Fried et al., 2015; Jennings et al., 2017; Kemeny et al., 2012).

Bearing this in mind, to guide research in the field, Jennings and Greenberg (2009) developed what they call 'The Prosocial Classroom' model (see Figure 2, below). This model is based on a critical review of the literature regarding the role of teacher social-emotional skills in student social-emotional and academic development. In this model: *teacher social-emotional abilities and well-being influence healthy student-teacher relationships, effective classroom management, and efficacious social-emotional skills training implementation, which in turn foster a compassionate, socially connected classroom climate that subsequently improves student social-emotional maturation and academic success.* In their review, Jennings and Greenberg (2009) found significant (direct and indirect) support for their theory across a range of empirical studies and experimental designs. They also reviewed (the limited) evidence of social-emotional skills trainings for teachers, and highlighted two key 'proof-of-concept' studies. These

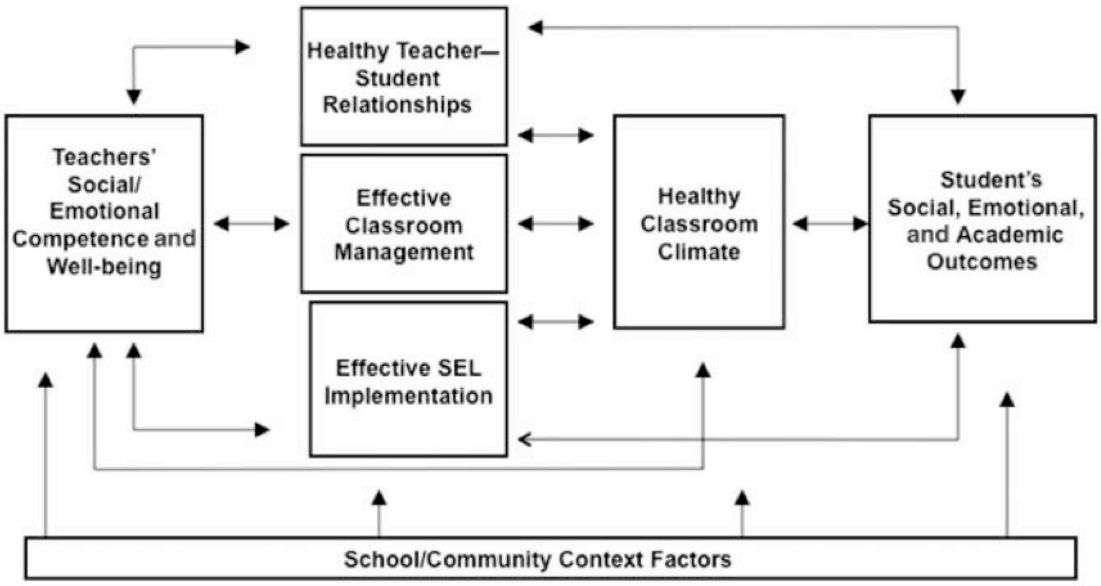

Figure 2. 'The Prosocial Classroom' model (Jennings & Greenberg, 2009; p .494).

studies are covered in a later section on meditation-based teacher programs (Section 2.3).

That said, in an innovative experiment, teachers attended an intensive (42-hour, 14-week) program designed to build knowledge, beliefs, and skills to better teach through interactions with students (Hamre et al., 2012). The core facets of the training – aligned with the Classroom Assessment Scoring System (CLASS; Pianta, La Paro, & Hamre, 2008) framework of teaching – focused on developing: emotional support (e.g., positive climate and teacher sensitivity), classroom organization (e.g., behavior management and productivity), and instructional support (e.g., quality of feedback and concept development). Compared to a treatment-as-usual control, teachers who received the intensive training showed enhanced emotional support (d = 0.41) and instructional support (d = 0.66) as rated by hypothesis-naïve observers. Utilizing rigorous a methodology, this study offers empirical support for The Prosocial Classroom model that guides the present study (see also Jennings et al., 2017). Thus, the Prosocial Classroom model provides an empirically-backed theoretical account for a primary assertion of the

field, and for a primary aim of this study: training teacher emotion skills and compassion may expand teachers' ability to engage effectively and prosocially in their daily work.

2.2 Mindfulness Meditation and Kindness Meditation: Definitions and Efficacy

To gain a clearer understanding of how mindfulness meditation (MM) and kindness meditation (KM) may be employed as methods for enhancing teacher emotion skills, compassion, and prosocial behavior, both practices are defined and context is provided on their modern secular forms. Additionally, research evaluating the efficacy of MM and KM trainings in non-teacher adults is reviewed. In this review, meditation trainings are denoted as 'mixed' when both MM and KM are included in the same training to distinguish combined from independent practice effects. Also, meditation trainings are denoted as a 'practice' (MM or KM practices only) or a 'program' (multi-faceted MM or KM-based interventions) to differentiate meditation practice-specific effects from program non-specific effects (e.g., psychoeducation, group discussion).

2.2.1 Mindfulness Meditation: Definition and Evidence of Efficacy

Mindfulness meditation is often employed in the West as a term that encompasses both focused attention (FA) and open monitoring (OM) meditations (Kabat-Zinn, 2011). Yet, for the most part, it is not a single technique sourced directly from classical Buddhist texts (Lutz, Dunne, & Davidson, 2007). Rather, MM typically integrates these two widely-practiced and adapted classical meditation styles: concentrative (*samatha*) meditation (FA) and insight (*vipassana*) meditation (OM) (Lutz, Slagter, Dunne, & Davidson, 2008). The former practice (FA) involves intensive and sustained single point-focused meditation that explicitly trains the acuity, flexibility, and stability of attention. The latter practice (OM) involves a non-reactive, non-elaborative, non-goal-directed

response to all (appetitive and aversive) sensations and perceptions (both internally and externally generated), and it is explicitly rooted in the body and basic somatic awareness (for more detailed descriptions of each practice see Lutz et al., 2007, 2008).

That said, FA and OM practices are combined in many modern Western psychology-based programs, and they are typically referred to as 'mindfulness-based interventions' (MBIs) (Cullen, 2011; Kabat-Zinn, 2003). Correspondingly, the most commonly employed definition of mindfulness in current scientific psychology is, "paying attention in a particular way: on purpose, in the present moment, and non-judgmentally" (see Kabat-Zinn, 2011; p. 291). The 'paying attention on purpose in the present moment' facet is principally derived from FA practices, and the paying attention 'in a particular way…and non-judgmentally' facet is principally derived from OM practices (Bishop et al., 2004; Lutz et al., 2008). The MM teacher trainings used in this study consisted of FA and OM practices (Kornfield, 2008, 2017), given that it is common to combine these meditations in traditional Buddhist and modern secular trainings.

It is important to note that the most taught and studied MBI to date is the Mindfulness-Based Stress Reduction program (MBSR; Kabat-Zinn, 2003, 2005). Since its creation, over 20,000 people have completed the MBSR program with MBSR-certified trainers through the Center for Mindfulness at the University of Massachusetts (www.umassmed.edu/cfm). Over 35 years ago, MBSR was designed to assuage suffering in chronic pain patients for whom modern medicine could provide no additional assistance (Kabat-Zinn, 2011). Similar to its early program structure, MBSR is typically run by a trained instructor as an 8-week in-person program with an average of 2.5 to 3.5 contact hours weekly. The program contains modules for: dedicated in-session meditation

practice (and 45min take-home audio-guided meditations to complete six times a week); yoga/mindful movement; a full (7.5hr) silent day meditation retreat; psychoeducational talks on stress, well-being, and mindfulness; sharing and compassionate communication exercises; reflective inquiry; and supportive group dialogues focusing on difficulties around stress and program practices (Kabat-Zinn, 2003, 2005). Although rooted in ancient Buddhist practices and philosophy, MBSR is secular. It should be underscored that MBSR often serves as the blueprint for secularized mindfulness and kindness meditation-based programs in the West, and it is often used in RCTs of meditation's efficacy (Chiesa & Serretti, 2009; de Vibe, Bjørndal, Tipton, Hammerstrøm, & Kowalski, 2012), including in many teacher studies (see Jennings & DeMauro, 2017).

For thousands of years people have practiced variations of MM (Lutz et al., 2007). In this time, among the many benefits purported by practitioners, two of the most central are: (1) a greater awareness of the causes and consequences of one's own and others' emotions (i.e., enhanced emotional awareness; see Lane et al., 1990), and (2) an enhanced ability to adaptively and non-reactively respond to both positive and negative emotional elicitors (i.e., enhanced emotion regulation; see Gross 1998, 2013) (Dalai Lama & Ekman, 2008; Ekman, Davidson, Ricard, & Wallace, 2005; Kornfield, 2008, 2017; Vago & Silbersweig, 2012; Wallace & Shapiro, 2006). Furthermore, an ultimate goal of training emotional-cognitive skills (at least in Tibetan meditative traditions) is the emergence of a global, non-discriminatory compassion, both in feeling and prosocial action (Dalai Lama & Ekman, 2008; Ekman et al., 2005; Kornfield, 2008, 2017).

Over the past few decades, although some evidence supports other meditations, such as transcendental meditation ('TM'; for a review see Orme-Johnson & Barnes,

2014), MM practices and MBIs have been used the most frequently to test the trainability of human social-emotional functioning, yielding a notable degree of efficacy across study designs and populations (for recent reviews see Chambers, Gullone, & Allen, 2009; Chiesa & Serretti, 2009; de Vibe et al., 2012; Desbordes et al., 2015; Eberth & Sedlmeier, 2012; Emerson et al., 2017; Hölzel et al., 2011; Sedlmeier et al., 2012). Indeed, a meta-analysis of MM's psychological effects in non-clinical, controlled studies was recently published (Eberth & Sedlmeier, 2012). The results suggest that the effects of pure MM practices are generally robust, although they may be smaller than the effects of mixed MM programs. The weighted mean effect sizes of pure MM practices vs. MBSR programs were: $\bar{r}$ = -.26 vs. $\bar{r}$ = -.30 (for lower anxiety); $\bar{r}$ = -.19 vs. $\bar{r}$ = -.32 (for lower negative affect); and $\bar{r}$ = .12 vs. $\bar{r}$ = .37 (for higher well-being), respectively. The meta-analysis also found that the mean effect size was inflated for pure MM practices and MBSR in *non*-RCT designs: $\bar{r}$ = .22 (MM) and $\bar{r}$ = .29 (MBSR) for RCTs versus $\bar{r}$ = .26 (MM) and r = .36 (MBSR) for non-RCTs (Eberth & Sedlmeier, 2012). Given that the present RCT uses pure MM practices, and compares them to an active control, the effects on social-emotional functioning are predicted to be small to moderate (Cohen, 1988).

In addition to these meta-analytic findings, a small set of studies has emerged that suggests MM increases compassion and prosocial behavior. These studies are briefly reviewed here. Using a prosocial economic game called the 'ultimatum game,' from pre-test to post-test, an 8-week MM program increased cooperative behavior (i.e., accepting less money for oneself for the 'good of the group'), but a progressive muscle relaxation (active control) program did not (Kirk et al., 2016). In another study, a 3-week mobile application-based MM practice increased compassionate behavior toward a stranger

versus a cognitive skills training control (Lim et al., 2015). Compassionate behavior was assessed by participants giving or not giving up their seat to a person (confederate) in a leg cast inside a waiting room, where there was nowhere to sit and no one would move.

Using a non-randomized pre-posttest design, a 9-week MBSR program increased self-reported compassion in Canadian healthcare employees (Moll, Frolic, & Key, 2015). Specifically, self-reported empathic concern, perspective taking, and personal distress improved (i.e., the emotional, cognitive, dysregulated facets of compassion, respectively) on the Interpersonal Reactivity Index (IRI; Davis, 1983). After three months of an intensive mixed MM retreat versus a waitlist control, another study found greater self-reported compassion on the IRI (Sahdra et al., 2011). Notably, the gains in compassion remained five months after the post-test assessment. These are the only known data regarding the longitudinal effects of meditation on compassion or prosocial behavior. The same study also found that MM participants were more likely to show facial expressions of sadness, and less likely to show expressions of disgust or anger, when watching videos of others suffering compared to controls, where the former served to indicate empathic concern and the latter served to indicate personal distress (Rosenberg et al., 2015).

Overall, prior research in non-teacher adults suggests that MM may be an effective method to enhance emotion skills, and also compassion and prosocial behavior, in chronically stressed teachers. That said, given the existing findings, why might MM practices enhance prosocial behavior specifically? Most of the theories and studies in the field focus on the role of MM practices in emotional skills and balance, or KM practices in compassion and prosocial behavior (Bankard, 2015; Eberth & Sedlmeier, 2012; Galante, Galante, Bekkers, & Gallacher, 2014; Hölzel et al., 2011). There is, however, an

exception. The 'Halifax Heuristic Model of Compassion' (HHMC; see Figure 3, below) proposes that 'compassionate wisdom' (i.e., compassion and prosocial behavior) is an enactive process that emerges when a range of domain-general cognitive, social-emotional, and somatic skills are cultivated via intentional MM training (Halifax, 2012; see also Vago, 2014). In other words, compassion and prosocial behavior are fundamentally 'emergent phenomena.' Therefore, it is more effective to foster them through the abilities and qualities that naturally give rise to them, rather than to target them directly (as would be the case in KM; Salzberg, 2002). According to the HHMC, if cultivated, the main abilities and qualities that allow compassionate wisdom to organically arise are: focused attention and balanced affect (the A/A Axis), wholesome intentions and insight into the true nature of oneself and reality (the I/I Axis), and embodiment of wisdom and ethical engagement with the world (the E/E Axis).

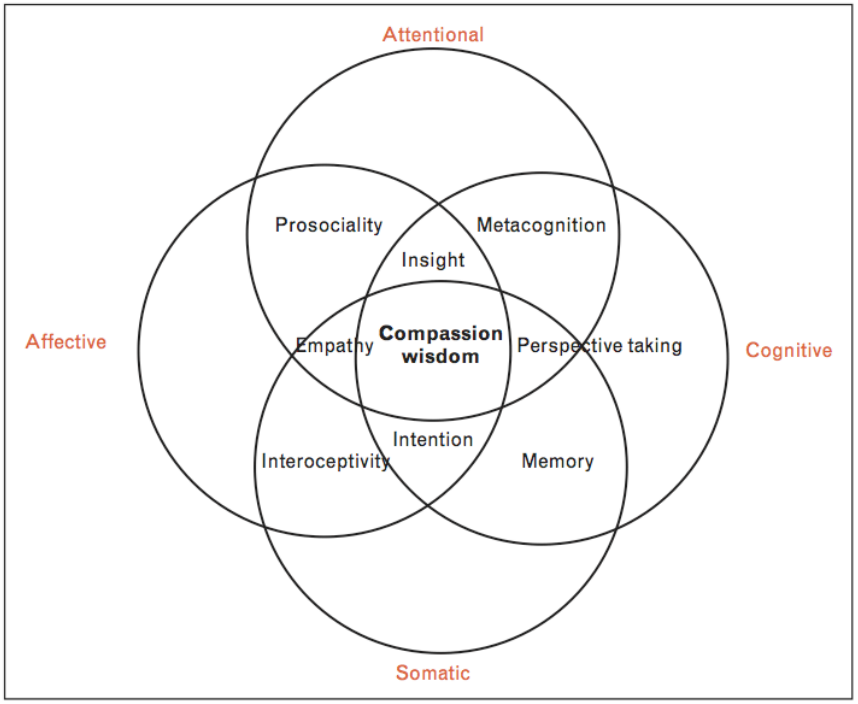

Figure 3. The 'Halifax Heuristic Model of Compassion' (Halifax, 2012; p. 7).

Although research suggests that MM does cultivate a number of these abilities and qualities, especially on the A/A Axis (Eberth & Sedlmeier; Sedlmeier et al., 2012; Tang, Holzel, & Posner, 2015), explicit links between MM-driven enhancements in prosocial behavior as mediated by changes in these abilities or qualities (e.g., emotion skills) are scant. In one of the only studies to test this question, an ability central to emotional awareness and regulation as well as compassion, known as 'empathic accuracy' (the ability to read emotions in faces), did not mediate MM-driven increases in compassionate behavior (Lim, Condon, & DeSteno, 2015). Accordingly, despite cogent theoretical accounts, the prosocial mechanisms of MM remain poorly understood.

2.2.2 Kindness Meditation: Definition and Evidence of Efficacy

There are two main forms of kindness meditation (KM) that have been the focus of scientific investigation: loving-kindness (*metta*) meditation (LKM) and compassion (*karuna*) meditation (CM) (Galante et al., 2014; Hofmann, Grossman, & Hinton, 2011; Jazaieri et al., 2013). LKM is a millennia-old, Buddhist-derived meditation practice that involves the intentional mental cultivation of positive emotions and well-wishing toward a range of familiar and unfamiliar individuals (Kornfield, 2008, 2017; Salzberg, 2002). Traditionally, meditators begin by directing positive feelings toward the self (because this is thought to be easier[2]), then toward close family and friends, next toward neighbors and acquaintances, and eventually toward neutral strangers and even disliked or offensive others ('enemies'). The purpose is to apply one's mental focus to wishing the well-being of an increasingly large circle of humanity, from the few in one's ingroup to the many in one's outgroup (e.g., see Bloom, 2017). The final stage of LKM is to generate a deep

[2] Modern Western models of KM practices are starting to change this order (e.g., Neff & Germer, 2012), because some evidence suggests that 'self-compassion' is more difficult to foster in 'self-focused' industrialized modern cultural contexts than other-oriented compassion (see Neff, 2003a).

wish for the happiness of all living beings. The successive expansion of good will and positive emotion toward others can occur within one practice session, as well as in successive phases over months or years (as each degree of expansion can be more difficult and may be practiced in stages; Kornfield, 2008, 2017; Salzberg, 2002).

Although similar to LKM, instead of wishing a growing circle of individuals positivity and well-being, in CM one wishes individuals peace and freedom from suffering. The distinction between LKM and CM is non-trivial because wishing someone happiness, and wishing someone liberation from pain, are dissociable intentions (Kornfield, 2008, 2017; Salzberg, 2002). Further, CM may be more 'emotionally challenging,' especially in distressed populations, because it involves the willingness to experience others' pain and suffering, whereas LKM may be less emotionally challenging because of its characteristic focus on positive emotions (e.g., see Frewen, Rogers, Flodrowski, & Lanius, 2015). That said, noting these differences, this study included both LKM and CM in the KM training group, as is often done in ancient and modern meditative traditions (Jazaieri et al., 2013; Kornfield, 2008, 2017). Even combined, LKM and CM practices may exert distinct social-emotional effects compared to MM practices and the active control group (Galante et al., 2014; Nash & Newberg, 2013).

As with MM trainings, KM trainings are frequently delivered via 8-week, in-person, MBSR-type secular programs that draw on techniques from ancient Buddhist traditions. Example programs include Compassion Cultivation Training (CCT; see Jazaieri et al., 2013) and Cognitively-Based Compassion Training (CBCT; see Desbordes et al., 2012). One aspect of many KM-based programs to highlight, including CCT and CBCT, is that they typically incorporate MM practices. At least at the outset of the

training sessions, this is done to stabilize attention to prepare individuals to more fully engage in KM practices (e.g., see Jazaieri et al., 2013). Although it may be advantageous to create a program that integrates MM and KM to maximize salutary effects, scientifically, this approach obscures the distinctive effects of each practice. As MM and KM use different methods and have separate proximal goals, to delineate the effects of each meditation it is necessary to dissociate KM from MM in training. Yet, studies that test the differential effects of MM versus KM are rare (e.g., see Kok & Singer, 2017).

Historically, KM has been posited to produce a series of effects that include increased positive emotions, expanded compassion, and greater prosocial behavior (Kornfield, 2008, 2017; Salzberg, 2002). Compared to research on MM, the scientific study of KM is relatively new. However, a meta-analysis on KM was published that includes 22 studies of KM practice and program effects (Galante et al., 2014). Regarding its effects on emotional functioning, KM compared to passive control groups showed a moderate[3] reduction in depression (Hedges' g = -0.61), and a moderate increase in self-compassion (Hedges' g = 0.45; a facet of adaptive emotion regulation, as it interrupts cycles of critical self-referencing that perpetuate negative affect; Neff, 2003a; Neff & Germer, 2012). Compared to active controls, KM versus progressive muscle relaxation moderately increased positive affect (Hedges' g = 0.42). As KM explicitly focuses on up-regulating positive emotion, this finding is consistent with KM's proximal goals. Overall, the meta-analysis suggests that KM may foster adaptive emotional functioning in adults.

The meta-analysis also reviewed the effects of KM on other-oriented compassion and prosocial behavior (Galante et al., 2014). However, as the studies that examined

[3] Hedges' g is interpreted with the same effect size conventions as Cohen's d (Cohen, 1988) because above sample sizes of 20, the effect size estimates for both metrics converge (Field, 2013).

compassion or prosocial outcomes used heterogeneous comparison groups (e.g., passive controls, MM, cognitive reframing), a formal meta-analysis was not performed on those data. Also, a series of studies have been published since the meta-analysis was conducted. Therefore, to elucidate the current state of the field, the evidence of KM's effects on compassion and prosocial behavior is briefly reviewed here. To provide support for the present study, only studies using actively controlled experimental designs are discussed.

A 20-minute KM practice versus a progressive muscle relaxation control was found to increase self-reported compassion for others on a 'love of humanity' scale, which measures one's feelings of positive connection to and concern for others (Templeton, 2007). In another study, a one-day KM program versus an active memory control decreased negative affect and increased positive affect in response to videos of human suffering, a task-based measure of compassion (Klimecki, Leiberg, Lamm, & Singer, 2012). KM may have expanded acceptance of others' suffering, and thus shifted automatic responses to others' pain from one of aversion to one of positive emotionality.

In a longer-term study, a 4-week mixed digital KM program versus a light physical exercise control more than doubled the money participants donated to a charity (Galante, Bekkers, Mitchell, & Gallacher, 2016). However, the study was affected by 82% attrition, limiting generalizability of the results. Another study examined whether a 4-week mobile application-based KM practice versus two active controls influenced self-reported compassion (feelings of warmth and personal distress; Batson, 2011), and donation behavior toward others who were suffering (Ashar et al., 2016). The 'others' in the study were real individuals from a charity website about whom participants read biographies with photos (e.g., orphaned children, adults with cancer, homeless veterans).

KM expanded compassionate feelings versus the active controls, and although it did not increase donations, donations in one of the active controls declined compared to KM.

In addition, a few studies have examined how KM influences economic game behavior (Leiberg, Klimecki, & Singer, 2011; Weng et al., 2013, 2015). In one study, a one-day KM program versus a memory training control increased prosocial behavior toward strangers in a repeat-interaction online game (Leiberg et al., 2011). The game indexed prosociality by decisions to incur a financial cost to oneself to increase earnings of a team. Notably, this is the only known study to show that meditation can increase prosocial behavior in a pre-posttest actively controlled design. In another study, meditators paid to redistribute almost twice as much money from a selfish player to an unfairly treated player in an economic game (a blend of compassionate giving and altruistic punishment) after two weeks of digital KM versus a cognitive reframing control (Weng et al., 2013). In a different paper from the same study, digital KM versus the active control increased compassionate giving to an unfairly treated player in an economic game, but it did not affect punishment of a selfish player (Weng et al., 2015).

Overall, the meta-analysis (Galante et al., 2014) and recent RCTs suggest that KM can increase emotional balance, compassion, and prosocial behavior (Ashar et al., 2016; Galante et al., 2016; Leiberg et al., 2011; Weng et al., 2013, 2015). That said, why might KM enhance prosocial behavior specifically? The most parsimonious and empirically supported rationale for KM's effects on prosocial behavior is that KM may directly elevate compassion, and as a result, indirectly foster prosocial behavior (Bankard, 2015). As noted, a core proximal goal of KM is to up-regulate compassion for a wide range of others (Kornfield, 2008, 2017; Salzberg, 2002), and prior studies have

found that KM directly fosters compassion (Ashar et al., 2016; Klimecki et al., 2012; Weng et al., 2015). With that said, the 'compassion-altruism hypothesis' has been thoroughly investigated and ample support has been found across a range of empirical study designs (for detailed reviews see Batson, 2011; Gilbert, 2017). These data indicate that compassion is a reliable predictor of prosocial behavior, even when it comes at a cost to the self. Therefore, KM may foster prosocial behavior by increasing compassion. Yet, research showing that compassion mediates KM-driven rises in prosocial behavior is absent (despite recent explorations of this notion; Ashar et al., 2016; Weng et al., 2015).

2.3 Using Meditation Training to Enhance Teacher Emotional Awareness and Regulation, Compassion, and Prosocial Behavior

A relatively small but growing number of studies on meditation-based trainings for teachers have been published (for recent narrative and systematic reviews of the evidence see Emerson et al., 2017; Hwang, Bartlett, Greben, & Hand, 2017; Jennings & DeMauro, 2017). These studies are reviewed in the present section. The section is organized into two parts: 1) underpowered non-experimental studies are reviewed briefly, and 2) well-powered RCTs are reviewed in detail with a focus on two meditation-based teacher programs that have undergone rigorous evaluations: Cultivating Awareness and Resilience in Education (CARE; Jennings et al., 2013, 2017), and Stress Management and Relaxation Training in Education (SMART-in-Education or SMART; Cullen & Wallace, 2010; Benn, Akiva, Arel, & Roeser, 2012; Roeser et al., 2013).

2.3.1 Evidence of Meditation Training Efficacy in Teachers

A series of studies with underpowered (majority female) samples and non-experimental or quasi-experimental designs have been conducted (Anderson, Levinson,

Barker, & Kiewra, 1999; Beshai, McAlpine, Weare, & Kuyken, 2016; Flook, Goldberg, Pinger, Bonus, & Davidson, 2013; Franco, Mañas, Cangas, Moreno, & Gallego, 2010; Frank, Reibel, Broderick, Cantrell, & Metz, 2015; Gold et al., 2010; Harris, Jennings, Katz, Abenavoli, & Greenberg, 2016; Hülsheger, Alberts, Feinholdt, & Lang, 2013; Jennings, Snowberg, Coccia, & Greenberg, 2011; Poulin, Mackenzie, Soloway, & Karayolas, 2008; Taylor et al., 2015; Winzelberg & Luskin, 1999). These studies provide preliminary evidence for the feasibility and acceptability of meditation-based trainings for teachers. They also tentatively suggest that meditation programs may improve self-reports of teacher psychological and physical distress, depression, emotional exhaustion and depersonalization (facets of burnout), maladaptive emotion regulation, mindfulness, well-being, and life satisfaction, as well as behavioral measures of attention, emotion regulation, and student-teacher interactions. Yet, given the methodological limitations of these studies, causal inferences and generalizations of their findings are prohibited.

That said, one quasi-experimental study is highlighted because it is the only study to employ an active control group in teacher meditation training research. In this study, the effects of a 10-week MM program (n = 34) were compared to a psychomotor therapy active control (n = 34) (which included movement and music listening exercises; Franco et al., 2010). The MM program versus the active control reduced Spanish secondary school teachers' (57% female) psychological distress, including anxiety, depression, obsession-compulsion, interpersonal sensitivity, and hostility. All MM-driven attenuations in distress were sustained at a 4-month follow-up. Further, the effects were large: distress was reduced by about 30-50% in the MM group from pre-test to post-test and from pre-test to the 4-month follow-up. Although random assignment occurred

before pre-test assessments, possibly biasing self-reports, this study suggests meditation-based training may be associated with large and enduring effects on teacher emotion that are not confounded by placebo effects or demand characteristics (Boot et al., 2013).

Rigorous, better-powered RCTs of meditation-based programs for teachers also have been conducted, including studies evaluating two leading teacher programs (CARE and SMART). One study tested the effects of Cultivating Emotional Balance (CEB),[4] an intensive mixed MM program, on first-person and third-person measures of teacher (N = 82; 100% female) social-emotional behavior (Kemeny et al., 2012). After 8 weeks (42hrs) of training, CEB versus a waitlist control diminished self-reported negative affect, autonomic arousal (in response to a social stress task), and hostile behavior (as coded by trained observers during a real social interaction). Additionally, CEB compared to control teachers showed greater self-reported positive affect, emotional awareness (as measured by identifying emotions in facial images), and implicit compassion (as tested by a lexical reaction time measure). This study is important because it used both self-report and behavioral measures of emotion skills and compassion. Also, notably, the changes in negative affect, hostile behavior, emotional awareness, and implicit compassion persisted five months post-test. Also, a pilot with a self-selected group of 21 teachers from the same RCT suggested that the classroom climate (as rated by trained observers using the CLASS; Pianta et al., 2008b) was more 'productive' among CEB teachers versus controls (Jennings, Foltz, Snowberg, Sim, & Kemeny, 2011; Study 1).

A different set of studies evaluated the CARE program for teachers. Adapted for teachers from the CEB program, CARE integrates emotion skills instruction based on

[4] Cultivating Emotional Balance (CEB) was a precursor to the CARE program.

emotion science with MM and KM-based practices and group discussions. Thus, CARE is a mixed MM and KM program that typically requires 30 contact hours (six hours a day over five in-person training days plus phone coaching sessions; see Jennings et al., 2017). In a pilot RCT of CARE, CARE (n = 23) versus waitlist control (n = 27) teachers (89% female) showed reduced time urgency (feeling the need to rush; d = -0.42), and enhanced mindfulness on the observing (d = 0.73) and non-reacting facets (d = 0.73) of the Five Facet Mindfulness Questionnaire (FFMQ; Baer, Smith, Hopkins, Krietemeyer, & Toney, 2006) (Jennings et al., 2013). CARE versus control teachers also showed increased personal accomplishment (d = 0.40) (an inverse factor of burnout on the Maslach Burnout Inventory [MBI]; Maslach, Jackson, & Leiter, 1997). Emotion regulation was self-reported as well using the Emotion Regulation Questionnaire (ERQ; Gross & John, 2003). The ERQ assesses the tendency to reframe the cause or meaning of an emotional event to alter its affective impact, called 'cognitive reappraisal' (considered an adaptive emotion regulation strategy). Also, the ERQ assesses the tendency to repress the outward expression of emotional events one experiences, called 'expressive suppression' (considered a maladaptive emotion regulation strategy). Compared to the waitlist control, CARE increased cognitive reappraisal (d = 0.80) and decreased expressive suppression (d = -0.43). Additionally, CARE teachers reported perceptions of higher well-being (92%), and a greater ability to manage their classroom compassionately (77%) and to create and maintain supportive relationships with their students (83%).

In a recent cluster-RCT of CARE, a large sample (N = 224; 93% female) of K-5 teachers was recruited from 36 schools located in a high poverty region of New York City (Jennings et al., 2017). Teachers were randomly assigned within schools to CARE (n

= 118) versus a waitlist control (n = 106). A single measure of adaptive emotion regulation was created with the ERQ by reverse-scoring expressive suppression and combining it with reappraisal (Gross & John, 2003). Using this metric, CARE enhanced emotion regulation versus the control group (d = 0.35). Mindfulness was self-reported using scales that assessed intrapersonal and interpersonal teacher-specific mindfulness: the FFMQ (Baer et al., 2006) and Mindfulness in Teaching Scale (MTS; Frank, Jennings, & Greenberg, 2016). Both scales loaded onto a single mindfulness factor that CARE improved (d = 0.28) compared to the control group. Specifically, changes mostly occurred in the observing (d = 0.41) and non-judging (d = 0.21) facets of mindfulness.

In the cluster-RCT of CARE, psychological distress also was assessed, using the following six scales that loaded onto one latent factor: 1) the Patient Health Questionnaire Depression Scale (Kroenke et al., 2009); 2) the Generalized Anxiety Disorder Scale (Spitzer, Kroenke, Williams, & Löwe, 2006); 3) the negative affect subscale of the Positive and Negative Affect Schedule Short Form (PANAS; Thompson, 2007); 4) the Patient Reported Outcomes Measurement Information System Sleep Disturbance Questionnaire (Buysse et al., 2010); 5) the Emotional Exhaustion subscale of the MBI (Maslach et al., 1997); and 6) the Perceived Stress Scale (PSS; Cohen, Kamarck, & Mermelstein, 1983). CARE significantly assuaged psychological distress compared to the control group (d = -0.18). The changes mostly reflected improvements in teacher emotional exhaustion (d = -0.22) and sleep problems (d = -0.26).

Lastly, the quality of teachers' interactions with their students was assessed using the CLASS, an observational measure designed to assess three domains of teacher-student classroom interactions: Emotional Support, Classroom Organization, and

Instructional Support (Pianta et al., 2008). The Emotional Support domain is comprised of four dimensions: Positive Climate, Negative Climate, Teacher Sensitivity, and Regard for Student Perspectives. Classroom Organization is comprised of three dimensions: Behavior Management, Productivity, and Instructional Learning Formats. Instructional Support is comprised of three domains: Concept Development, Quality of Feedback, and Language Modeling. Compared to the control group, CARE significantly improved the Positive Climate ($d = 0.23$) and Teacher Sensitivity ($d = 0.23$) dimensions of Emotional Support, as well as the Productivity dimension of Classroom Organization ($d = 0.23$). Notably, compassionate and prosocial teacher-student interactions comprise a major portion of the Positive Climate and Teacher Sensitivity dimensions (Pianta et al., 2008). That said, this is the only study to show that meditation-based training for teachers can enhance compassionate and prosocial behavior between teachers and students.

Additionally, two RCTs of a different meditation-based program for teachers have been conducted. The program is called Stress Management and Relaxation Training (SMART; Cullen & Wallace, 2010). It is delivered over a 5-week period (36 contact hours), and contains didactic instruction and discussion, MM and KM practices, and homework assignments. Approximately 70% of SMART is comprised of MBSR, and the other 30% includes content and activities that focus on emotion theory and regulation, kindness and compassion, and the application of mindfulness to teaching. In the first RCT of SMART, a combination of teachers ($n = 35$) and parents ($n = 25$) of special needs children (91% female) were recruited from a school district in a small Midwestern city (Benn et al., 2012). Participants were randomized to SMART or a waitlist control. Stress was measured with the Perceived Stress Scale (PSS; Cohen et al., 1983), and negative

affect was assessed with the PANAS (Watson, Clark, & Tellgen, 1988). Anxiety was measured using the State subscale of the State–Trait Anxiety Inventory for Adults (Kendall, Finch, Auerbach, Hooke, & Mikulka, 1976), and depression was assessed using the Center for Epidemiological Studies Depression (CES-D) Scale (Radloff, 1977). Emotion regulation was measured via the Emotion Regulation at Work Self-Efficacy Scale (Roeser et al., 2011), and self-compassion was assessed using the Self-Compassion Scale (SCS; Neff, 2003b). Lastly, mindfulness was measured with the FFMQ (Baer et al., 2006), and empathy was assessed with the empathic concern IRI subscale (Davis, 1983).

SMART versus the waitlist control showed improvements in self-reported: stress (d = -0.40), anxiety (d = -0.52), depression (d = -0.51), negative affect (d = -0.36), emotion regulation (d = 0.55; just in the teacher sample), mindfulness (d = 0.52), self-compassion (d = 0.40), and empathic concern (d = 0.56). At a 2-month follow-up, all observed effects remained or grew in magnitude (except for the effect on depression). Furthermore, enhancements in mindfulness (but not self-compassion) at post-test mediated changes in stress, anxiety, and negative affect at the 2-month follow-up. This is one of the only studies to test the mechanisms underlying meditation-based program effects on teachers. The sample contained teachers as well as parents, however, making it difficult to disentangle training effects on teachers from training effects on parents.

Addressing this issue, in a second RCT of SMART, elementary and secondary school teachers (87% female) from the U.S. and Canada were randomly assigned to SMART (n = 54) or a waitlist control (n = 59) (Roeser et al., 2013). Occupational stress was measured with seven items from the Classroom Appraisal of Resources and Demands scale (CARD; Lambert, McCarthy, & Abbott-Shim, 2001), occupational

burnout was assessed using the MBI (Maslach et al., 1997), and occupational self-compassion was measured with a teacher-adapted version of the SCS (Neff, 2003b). Anxiety was measured with the State subscale of the STAI (Kendall et al., 1976), and depression was measured with the Beck Depression Inventory (Beck, Steer, & Brown, 1996). Physiological measures of stress also were administered: salivary cortisol (Pruessner, Hellhammer, & Kirschbaum, 1999) and resting heart rate (Fox et al., 2007). SMART teachers versus waitlist controls reported: enhanced mindfulness (d = 0.79) and teacher-specific self-compassion (d = 0.85), along with attenuated occupational stress (d = -0.57), burnout (d = -0.76), anxiety (d = -0.71), and depression (d = -1.06). Notably, these effects remained or grew at a 3-month follow-up. Further, post-test changes in mindfulness and self-compassion mediated SMART effects on stress, burnout, anxiety, and depression at the 3-month follow-up. Physiological stress markers were unchanged.

Lastly, the efficacy of SMART versus a waitlist control was further examined in a follow-up publication from the second RCT of SMART (Crane, Schonert-Reichl, & Roeser, 2016). Mindfulness was measured with the FFMQ (Baer et al., 2006), and job rumination at home was assessed with a subset of CARD items (Lambert et al., 2001). Mood at work and home was measured with items assessing what percentage of the time teachers were in a bad mood at work and home, and satisfaction with work and home life was assessed with two face-valid items created by the researchers. Compared to the control group, CARE: increased mindfulness (d = 0.79), reduced job rumination at home (d = -0.82), decreased bad moods at work (d = -0.59) and at home (d = -0.64), and increased satisfaction with work (d = 0.45) and with home life (d = 0.52). Notably, these effects were sustained at a 3-month follow-up assessment (except for satisfaction with

work or home life). Furthermore, improvements in mindfulness and rumination at post-test mediated reductions in bad moods at work and at home at the 3-month follow-up.

2.3.2 Evidence of Meditation Efficacy in Teachers: Summary and Critique

Noting the small but growing number of studies on the efficacy of meditation-based programs for teachers, the evidence is consistent (Emerson et al., 2017; Hwang et al., 2017; Jennings et al., 2017). Most studies find meditation-based teacher trainings reduce teacher stress and negative affect, and augment teacher emotion skills in the short-term and possibly in the long-term. Prior research also suggests that teacher programs may improve implicit compassion, reduce hostile social behavior, and expand prosocial classroom interactions. These results may be unreliable, confounded, and/or inflated because many studies were underpowered, non-actively controlled, and/or used largely self-reported outcomes (Jennings & DeMauro, 2017). Also, although meditation is theorized to be the active ingredient in teacher programs (e.g., Roeser et al., 2012), the independent effects of meditation on teachers are unknown because most studies evaluate multi-componential programs with non-meditation exercises. The potential for gender differences is unknown as well because about 85% of the teachers in prior research are female (Emerson et al., 2017; Jennings et al., 2017). Noting these limitations, the studies reviewed here, along with the reviewed MM and KM research, suggest that meditation may enhance emotion skills as well as compassion and prosocial behavior in teachers.

2.4 Digital Meditation Training: Utility for Teachers and Evidence of Efficacy

For meditation-based trainings to facilitate the development of teacher emotion skills, compassion, and prosocial behavior in a way that is scalable (i.e., can influence local and national educational systems), practices and programs have to be accessible.

'Accessible' carries numerous meanings in an intervention context and in the teacher context specifically (see Andersson & Titov, 2014). First, accessible means that the trainings do not require a large time investment. With the panoply of demands on teacher time and attention (e.g., Travers, 2017), shorter programs are more scalable. Second, accessible means flexible and transportable. Teachers may have pockets of free time during the day or night, but they may not be able to attend even an hour-long training outside of school on evenings or weekends. Offering trainings that can be integrated into teachers' daily lives may raise practice adherence and impact (e.g., Wolever et al., 2012).

Third, accessible means cheap or free. Trainings that are provided at a nominal fee may significantly expand the number of teachers who can try meditation. Fourth, and finally, accessible means understandable or 'user-friendly.' There are a lot of preconceptions of what meditation is or is not, and they all may not be accurate (e.g., see Grant, 2015; Jennings, 2016a). If teachers spend their most scarce resources (time and energy) to try meditation, and the practice instructions are dense or unrelatable, their interest may be lost. As such, trainings that make the process simple and easy to follow are fundamental. This is not to say that engaging in meditation should be simple and easy, but that the intentions and techniques of the practices should be clear and approachable. In support of these notions, a recent survey with a diverse sample found that of 500 respondents, 43% (the highest percentage) preferred online meditation training, and 38% preferred an individual format whereas 20% preferred a group format (Wahbeh et al., 2014). Some (11%) stated that they would refuse group training outright.

For all of these reasons, efforts across multiple fields in psychology to scale psychobehavioral interventions – including psychotherapy (Andersson, 2009; Andrews,

Cuijpers, Craske, McEvoy, & Titov, 2010) and positive psychology (see Bolier et al., 2013; Seligman et al., 2005) – are utilizing online platforms (see Proudfoot, 2013). Reviews of the literature indicate that online psychotherapeutic interventions can be as effective as in-person treatments (Andersson, 2009; Andrews et al., 2010; Barak, Hen, Boniel-Nissim, & Shapira, 2008). Likewise, a prior study compared an in-person to an online-based MM program, and found the program was equally effective at enhancing attention, and more effective at decreasing self-reported levels of chronic mental fatigue in traumatic brain injury and stroke patients (Johansson, Bjuhr, Karlsson, Karlsson, & Rönnbäck, 2015). Another study found commensurate effects between online versus in-person MM-based training on perceived stress, mindfulness, and physiological arousal (i.e., 'heart rate coherence') following an emotional trigger in working adults (Wolever et al., 2012). Notably, the effect on physiological arousal was greater, and the attrition rate was seven times lower, in the online (3.8%) versus the in-person training group (27.3%).

Moreover, a recent meta-analysis of digital meditation-based trainings (DMTs) extends this body of findings (Spijkerman, Pots, & Bohlmeijer, 2016; see also Cavanagh, Strauss, Forder, & Jones, 2014). Across 15 RCTs, DMTs produced small reductions in anxiety (Hedges' g = -0.22) and depression (Hedges' g = -0.29), and a moderate decrease in stress (Hedges' g = -0.51). DMTs also generated small gains in well-being (Hedges' g = 0.23) as well as mindfulness (Hedges' g = 0.32). Although this meta-analysis focuses on a mix of healthy and unhealthy adults, as well as MM-based programs and psychotherapies, it provides preliminary evidence that DMTs can enhance emotional functioning. Also, the DMT effect sizes were commensurate with effect sizes reported in recent meta-analyses on MM (Eberth & Sedlmeier, 2012) and KM (Galante et al., 2014).

Additionally, six studies (previously examined in the literature review on meditation; Section 2.2) found that different forms of DMT can enhance emotion skills as well as compassion and prosocial behavior (Ashar et al., 2016; Feldman et al., 2010; Galante et al., 2016; Lim et al., 2015; Weng et al., 2013, 2015). Specifically, audio-guided meditation practices versus active controls were found to decrease the association between negative affect and repetitive thought frequency (Feldman et al., 2010), as well as to increase compassion-related brain activity and behavior in third-party economic games (Weng et al., 2013, 2015). A website-based KM program was found to increase charitable donations versus an active control (though attrition was high; Galante et al., 2016). And finally, two different mobile application-based meditation trainings increased ecologically valid compassionate behavior toward a stranger in need (Lim et al., 2015), and may have increased real-world donations to suffering individuals (Ashar et al., 2016).

Taken together, prior work on non-meditative psychological interventions (e.g., Andrews et al., 2010), recent meta-analytic reviews of digital meditation studies (Cavanagh et al., 2014; Spijkerman et al., 2016), and six recent studies re-reviewed in this section (Ashar et al., 2016; Feldman et al., 2010; Galante et al., 2016; Lim et al., 2015; Weng et al., 2013, 2015), all suggest that DMTs may improve emotion skills, compassion, and prosocial behavior. A main limitation of prior research is that no studies examining DMTs have tested how particular forms of meditation (e.g., MM versus KM) may differentially influence social-emotional skills or behavior (see Singer et al., 2016). Moreover, an RCT evaluating the efficacy of DMTs for teachers has not been conducted.

2.5 Gender Differences in Emotion Skills, Compassion, and Prosocial Behavior, and in Response to Meditation Training

The evidence reviewed in the three previous sections (2.2–2.4) suggests that digital meditation trainings may plausibly enhance emotion skills, compassion, and prosocial behavior in teacher and non-teacher adults. That said, are there gender differences in emotional awareness and regulation, or in compassion and prosocial behavior that may interact with meditation training effects? Also, are there known gender differences in response to meditation training? This section briefly addresses these questions. First, evidence of gender differences in the primary study outcomes is reviewed, and then evidence of gender differences in response to meditation is reviewed.

Prior research has demonstrated that women score consistently and moderately higher on self-report and performance measures of intrapersonal and interpersonal emotional awareness (for reviews see Brody & Hall, 2008; Filkowski, Olsen, Duda, Wanger, & Sabatinelli, 2016; Joseph & Newman, 2010; see also Barrett, Lane, Sechrest, & Schwartz, 2000; Ciarrochi, Hynes, & Crittenden, 2005). Likewise, past studies suggest that women outperform men on intrapersonal and interpersonal emotion regulation tasks, though not necessarily on self-reports of emotion regulation (for a meta-analysis see Joseph & Newman, 2010). However, women report engaging in all emotion regulation strategies more often than men across the board, both those considered maladaptive (e.g., rumination, suppression, avoidance) and those considered adaptive (e.g., reappraisal, acceptance, problem solving; for a meta-analytic review see Tamres, Janicki, & Helgeson, 2002; see also Nolen-Hoeksema & Aldao, 2011). Further, prior work suggests that the strength of the association between maladaptive regulation strategies and

emotional health is greater than for adaptive regulation strategies (Aldao & Nolen-Hoeksema, 2010; Aldao, Nolen-Hoeksema, & Schweizer, 2010). As a result, although women may engage in adaptive strategies more often than men, and therefore perform better on emotion regulation tasks, women are still at increased risk for mental disorders characterized by emotion dysregulation (e.g., anxiety and depression; Kessler, Merikangas, & Wang, 2007; McLean & Anderson, 2009), as well as at increased risk for stress and emotional exhaustion at work (Klassen & Chiu, 2010; Purvanova & Muros, 2010). That is, possibly due to a tendency to engage in maladaptive emotion regulation strategies more frequently compared to men, rumination in particular, women may suffer from higher rates of stress, anxiety, and depression than men (for recent reviews see Aldao et al., 2010; Johnson & Whisman, 2013; Nolen-Hoeksema, 2012).

In regard to compassion, self-report scales consistently and task-based measures inconsistently suggest women may be more compassionate than men (for meta-analyses see Eisenberg & Lennon, 1983; Jaffe & Hyde, 2000; see also Davis, 1980; Rueckert, Branch, & Doan, 2011). In regard to prosocial behavior, women are more likely to behave prosocially on some indicators than men, including economic games (for a meta-analysis see Engel, 2011; see also Eckel & Grossman, 1998; Edele, Dziobek, & Keller, 2013), volunteering behavior (Einolf, 2011; Wilson, 2000; Wymer & Samu, 2002), and charitable donations (Andreoni, Brown, & Rischall, 2003; Mesch, Brown, Moore, & Hayat, 2011). Yet, there is variation in this pattern. In other contexts, women behave less prosocially than men (e.g., in anonymous interactions vs. bonded relationships, or when the personal cost of helping is low vs. high; for reviews see Andreoni & Vesterlund, 2001; Eagly & Crowley, 1986; Eckel & Grossman, 1996; Einolf, 2011; Wilson, 2000).

Given the body of data suggesting possible gender differences in emotion skills, compassion, and prosocial behavior, the extent to which meditation training influences these constructs may depend (in part) on gender. For example, if it is the case that women are more likely than men to experience stress, anxiety, or depression due to individual differences in emotion regulation (i.e., rumination specifically), then the extent to which MM or KM trainings alter emotion dysfunction via a reduction in rumination (e.g., see Piet & Hougaard, 2011) may modulate the effects of meditation on female more than on male emotional functioning. Or, for instance, if it is the case that women view compassion as a quality that is highly consistent with their social identities, but men view it as less consistent with their social identities (e.g., see Eagly & Crowley, 1986), then MM and KM may more effectively increase compassion in women versus men because women are more motivated to derive such benefits from their practice. Although these notions are speculative, they are provided to illustrate the value of incorporating gender in the design and analyses of meditation training studies: when males and females are treated as psychosocially uniform, the results may be masked or misrepresented.

In support of this notion, a recent study examined whether gender played a role in the efficacy of a meditation program (Rojiani, Santoyo, Rahrig, Roth, & Britton, 2017). In this non-experimental study, female versus male participants demonstrated decreased negative affect on the PANAS from pre-test to post-test (d = -0.19; Watson et al., 1988), whereas men showed a non-significant increase in negative affect (d = 0.12). Furthermore, compared to males, females showed enhanced mindfulness on the FFMQ (d = 0.24; all subscales showed the same pattern; Baer et al., 2006) and enhanced self-compassion on the SCS (d = 0.48; all subscales showed the same pattern; Neff, 2003b).

In contrast, a recent meta-analysis found gender was a moderator of the psychological effects of meditation, but women benefitted less from meditation than men (though the effect was small; r = -.09; Sedlmeier et al., 2012). Nevertheless, RCTs testing the effects of MM and KM versus a waitlist control (Condon et al., 2013), and MM versus an active control (Lim et al., 2015) did not find gender differences in compassionate behavior change (though compassion increased in the meditation versus the control groups).

Although research suggests there are gender differences in emotion skills, compassion, and prosocial behavior, and there may be gender differences in response to meditation training, the magnitude and even direction of these differences is unclear given the paucity of research on the subject (e.g., see Katz & Toner, 2013). The topic of gender differences in response to meditation training has gone virtually unstudied for decades, despite the exponential rise of research on MM (Black, 2014; Eberth & Sedlmeier 2012) and KM (Galante et al., 2014; Jazaieri et al., 2014). Given that about 75% of participants in non-teacher studies (Galante et al., 2014; Sedlmeier et al., 2012) and about 85% of participants in teacher studies (Emerson et al., 2017; Jennings et al., 2017) are female, assuming the existing data on meditation represent all genders is unjustified. Research that examines gender differences in meditation training is needed.

2.6 Gaps in Prior Research and The Need for The Present Study

Throughout Sections 2.1-2.5 in the literature review a case was developed, rooted in theory as well as evidence, that both MM and KM may enhance emotion skills, compassion, and prosocial behavior in teachers using a digital platform, and that these effects may vary in unknown ways based on gender. Although there is ample theoretical and empirical support for this case, the present study aims to advance the field by

addressing important gaps that remain in the literature. These lacunas are outlined together here, so that the potential contributions of this study are evident.

The first main limitation of prior research, particularly studies with teachers, is the almost exclusive use of passive or waitlist control groups. Critically, meditation studies cannot be double-blinded. Thus, the best way to control for placebo effects and demand characteristics is to use an active control training that generates highly similar expectations for change (Boot et al., 2013; Davidson & Kaszniak, 2015). Without an active control, any between-group differences may be due to differences across groups in expectations or demand characteristics rather than due to meditation practice. This strongly undermines causal claims of meditation's putative emotional or prosocial benefits. Accordingly, the present research utilizes music relaxation (MR) training as an active control group. The primary expectations for change in meditation are thought to be decreased stress and negative affect and increased relaxation and well-being (see McCoon et al., 2012). MR is one of the most common daily activities humans engage in for emotion regulation, and an established literature indicates certain kinds of music reduce stress and induce relaxation (see Chan, Wong, & Thayala, 2011; Koelsch & Jancke, 2015; Pelletier, 2004). However, cultivating present-moment attention and nonjudgmental awareness (in MM), or fostering prosocial emotions and compassion for oneself and others (in KM), are not inherent components of MR. Therefore, MR is matched on the major psychological expectancies of meditation, but not matched on what are considered to be the 'active ingredients' of MM or KM (Kornfield, 2008, 2017).

The second main limitation of prior research is that most studies with the general population, and especially with teachers, evaluate the efficacy of mixed MM and KM-

based multi-faceted programs. As such, it remains unclear the extent to which MM versus KM practices alone are responsible for improvements in emotion, compassion, and prosocial function, and furthermore, the extent to which non-specific program effects (e.g., psychoeducation, group discussions) drive salutary outcomes. Similarly, few studies (none with teachers) compare MM to KM in any training format on any outcomes (for exceptions see Condon et al., 2013; Desbordes et al., 2012; Feldman et al., 2010; Kok & Singer, 2017). Therefore, the unique effects of MM versus KM are virtually unknown despite their distinct methods and proximal goals (Kornfield, 2008, 2017; Nash & Newberg, 2013; Salzberg, 2002). This study assesses the influence of pure MM versus KM practices only (compared to MR) in an attempt to address these gaps in the field.

The third main limitation of prior research is that tests of the mechanisms underlying MM or KM's prosocial benefits are scant (e.g., see Ashar et al., 2016; Galante et al., 2016). Given the mounting evidence of meditation's prosocial behavioral efficacy in non-teacher adults (see Section 2.2 above), it is surprising that so few empirical investigations of their pathways have been conducted. Illuminating the underlying processes would facilitate a deeper understanding of how mental trainings may induce neuroplastic changes in compassion and prosocial behavior (Bryck & Fisher 2012; Vago & Silbersweig, 2012). Further, such insights may inform how to design and optimize meditation trainings for specific populations, such as classroom teachers (Roeser et al., 2012; Skinner & Beers, 2016). As such, noting that this is exploratory in nature given the limited evidence available, the present study tested the mechanisms of MM and KM training effects on prosocial behavior (see Sections 2.2 above and 2.7 below).

The fourth main limitation of prior research is that emotion skills are primarily indexed indirectly, particularly in teacher studies (Emerson et al., 2017), by increases in positive affect and decreases in negative affect. The present research uses first-person and third-person assessments of emotion skills, as well as self-reported positive and negative affect, to address this gap in the literature (see Chapter 3, below). Similarly, a small number of studies with the general population and no studies with teachers have tested the influence of meditation trainings on compassionate or prosocial behavior (one study measured the emotionally supportive quality of teacher-student interactions that includes prosocial behavior, but this was not clearly dissociable from other interpersonal behaviors; Jennings et al., 2017). Given that the end goal of MM and KM practices in many traditions and programs is to increase compassionate and prosocial *actions* in the world (Jennings et al., 2017; Kornfield, 2008, 2017; Roeser et al., 2013; Salzberg, 2002), this is a fundamental lacuna in the field. The present study aims to address this gap by using economic game behavior and charitable donations as third-person metrics of meditation's influence on teacher prosocial behavior (see Chapter 3 below).

The last main limitation of prior research is that evaluations of digital meditation trainings in the general population are new and not well-understood, although initial evidence supporting their efficacy is emerging (Spijkerman et al., 2016). Moreover, no known studies to date have investigated the efficacy of meditation-based trainings for K-12 teachers that are delivered completely and only digitally. Given the significant array of demands on teachers' time and energy, and the contribution of these demands to teacher stress and maladaptive social-emotional functioning (e.g., Greenberg et al., 2016; Schonert-Reichl, 2017; Travers, 2017), research that evaluates digital meditations for

teachers is needed. Accordingly, the present study is the first study to assess the efficacy of MM and KM trainings for K-12 teachers with a completely digital training design.

2.7 Present Study Aims and Study Hypotheses

Based on prior research, the present study served three specific aims. The first aim was to evaluate the feasibility and acceptability of digital meditation trainings for teachers. The second study aim was to establish the effectiveness of digital meditation trainings for teachers by investigating the extent to which they achieved core proximal goals of MM and KM practices (Kornfield, 2008, 2017). Specifically, core proximal goals of MM and KM trainings, which recent meta-analyses indicate each practice fosters, are increased positive affect and mindfulness and decreased negative affect and stress (Eberth & Sedlmeier, 2012; Emerson et al., 2017; Galante et al., 2014).

The third study aim was to test two primary hypotheses and two secondary hypotheses. The primary hypotheses were the focus of the present study and were more directly grounded in the existing literature. The secondary hypotheses were more exploratory in nature given the limited research that has explicitly examined the mechanisms of change in meditation training (Bankard, 2015; Emerson et al., 2017; Hölzel et al., 2011; Vago & Silbersweig, 2012) or the differential psychological effects of MM versus KM (e.g., see Desbordes et al., 2012; Kok & Singer, 2017). Gender also was incorporated as a factor into each hypothesis set, given the possible (though virtually unstudied) role that gender differences may play in determining responses to the meditation trainings (e.g., Burke et al., 2017; Rojiani et al., 2017; Sedlmeier et al., 2012).

The H1primary hypothesis was that: MM and KM group participants would show enhanced emotional awareness and regulation versus MR active control group

participants (Eberth & Sedlmeier, 2012; Emerson et al., 2017; Galante et al., 2014; Kemeny et al., 2012). The H2 primary hypothesis was that: MM and KM participants would show greater compassion and prosocial behavior than MR control participants (Jennings et al., 2017; Leiberg et al., 2011; Lim et al., 2015; Weng et al., 2013, 2015).

The H3 secondary hypothesis was that: MM and KM would both increase prosocial behavior versus the MR control, but the effects would be mediated by enhanced emotional awareness and regulation for MM (Batson, 2011; Eberth & Sedlmeier, 2012; Vago & Silbersweig, 2012), and enhanced compassion for KM (Bankard, 2015; Galante et al., 2014; Mascaro et al., 2015) (see Figure 4 below). Finally, the H4 secondary hypothesis was that: MM would improve emotion skills to a greater extent than KM, and KM would improve compassion and prosocial behavior to a greater extent than MM (Bankard, 2015; Desbordes et al., 2012; Eberth & Sedlmeier, 2012; Kok & Singer, 2017).

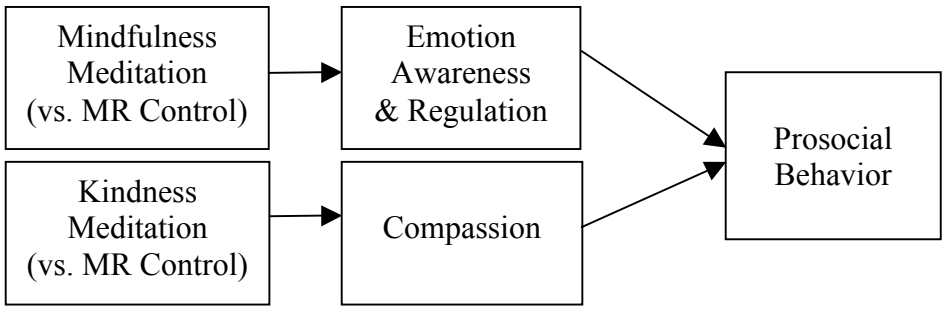

Figure 4. Logic model of MM versus KM effects on prosocial behavior (H3). Note that 'MR Control' means the music relaxation (active control) group.

Chapter 3: Methodology

3.1 Overview

The methodology for this study is organized in four sections. The first section details the participant recruitment and screening protocols, along with information about participant attrition and participant characteristics. The second section explains the study design and data collection procedures. The third section describes the content, instructor, dosage, and delivery of the mindfulness meditation (MM), kindness meditation (KM), and music relaxation (MR) trainings. Lastly, the fourth section specifies the self-report, performance tasks, and behavioral measures that multidimensionally operationalized the study variables: training feasibility and acceptability, positive and negative affect, stress, mindfulness, emotional awareness and regulation, compassion, and prosocial behavior.

3.2 Participants

3.2.1 Participant Recruitment and Screening

Classroom teachers were recruited from kindergarten to grade 12 public schools in five school districts in British Columbia (BC), Canada. Four school districts were from urban and suburban areas adjacent to a large city in BC. One school district was from a small southern coastal region in BC. The urban and suburban districts ranged in size from about 24,000 to 72,000 students enrolled, 49 to 120 schools per district, and about 2000 to 5500 teachers. The small coastal school district was comprised of 14 schools, 3300 students, and 500 employees (including teachers). To detect small to medium-sized effects at 80% power, as was predicted based on prior meditation research (Eberth & Sedlmeier, 2012; Galante et al., 2014; Weng et al., 2013, 2015), approximately 90 participants were needed (30 participants x 3 groups) (G*Power; Faul et al., 2007; also

see Moher et al., 1994, 2010).[5] To include gender in analytic models, this estimate accounted for the minimum power needed to detect gender-by-training interactions.[6] Also, based on prior online intervention research (Ashar et al., 2016; Galante et al., 2016; Mathieu, McGeechan, Barratt, & Herbert, 2013), upwards of 25% attrition was expected. As such, the recruitment target was 120 teachers (40 per group x 3 groups).

Prior to teacher recruitment, approval from the UBC Behavioural Research Ethics Board (BREB) and the research ethics committee from each of the five school districts was obtained. Full-time classroom teachers with minimal meditation and mindful movement experience were recruited. The recruitment strategy employed a multi-pronged, 8-month in-person and online professional network, paid advertising, and 'snowball sampling' approach. Prior to inclusion in the study, teachers interested in participation were first screened via a brief (15min) phone call to ensure they met study selection criteria (see Table 1 below for a description of the protocol), as well as to detail the commitments of the study (to prevent high rates of attrition common in online intervention research; Mathieu et al., 2013), and to facilitate study consent. Despite recruitment communications indicating only classroom teachers were eligible, many non-classroom teachers (e.g., counselors, librarians) responded. Also, many educators had a weekly meditation or mindful movement practice (especially yoga). In total, over the 8-month recruitment period, 255 phone interviews were conducted, and 121 teachers met the inclusion criteria and provided study consent (47.5% hit rate). Teachers who were ineligible to participate were given mindfulness and relaxation resources for self-care.

[5] Based on prior meditation research, using effect size conventions in social science (Cohen, 1988), small to medium-sized effects were estimated to be approximately $d = 0.35$ to $d = 0.50$.

[6] Upwards of 30 teachers per cell would have been preferable to conduct formal tests of gender-by-training interactions. However, 15 per cell was a reasonable estimate given the study timeframe and resources.

Table 1

Recruitment Interview and Screening Question Summary

	Screening Question	**Question Purpose/Justification**
Question 1	Are you a kindergarten to grade 12 classroom teacher?	Non-classroom teachers (e.g., librarians, resource teachers, school counselors), were excluded because of the different professional responsibilities and demands of those positions.
Question 2	Do you work at least 4 days a week at school (80% status)?	To match teachers on job responsibilities and social-emotional demands, teachers were required to be employed approximately 'full-time.'
Question 3	Will you have time to commit to participating in an 8-week-long study, from mid-October to mid-December of this school year? Total study time commitment is about 9 hours over the 8 weeks.	This question was critical to reduce attrition, which is likely higher when participants do not have a realistic understanding of the time commitment, especially among a time-pressured population, such as teachers.
Question 4	Do you currently practice meditation or mindful movement (e.g., yoga, tai chi) for one hour a week or more? Do you plan on doing so during the study? (Note: Teachers were only screened out for maintaining a personal practice, not teaching practices to their students.)	Individuals with more personal experience have demonstrated differences in how they engage in contemplative exercises and benefit from them, and they show changes in their cognitive-emotional capacities that may make them psychologically distinct (e.g., Brefczynski-Lewis et al., 2007; Lutz et al., 2008; McCall et al., 2014).
Question 5	Do you have access to a computer to take the study assessments, and a computer or mobile device to download the digital practices and listen to them with headphones/speakers?	As the entire study, the assessments and mental trainings, were delivered digitally, it was important to ensure that teachers had the necessary technology to fully participate in the study.
Question 6	Participants will be randomly assigned to one of three sets of mental trainings. Are you willing to receive any one of these trainings you are assigned?	Not all participants would receive the mindfulness meditations, although many teachers desired them. To reduce dropout, participants were told they may receive similar but different mental trainings known to enhance well-being.
Question 7	The goal of this study is to examine how meditation-based practices may improve teacher well-being, self-regulation, and compassion. Do you understand the purpose of this research?	Teachers were explained the purpose of the study, rather than concealing it, to: 1) increase teacher study buy-in; and 2) to prime expectations for the specific changes of interest across *all* groups (to reduce placebo and demand artifacts, which threaten the validity of RCT meditation research; Boot et al., 2013).

Efforts were made to recruit an equal number of female and male participants, and group randomization was stratified by gender (see below). This approach was taken to permit the statistical modeling of gender as a factor in training group differences, as there may be gender differences in emotion skills (e.g., Joseph & Newman, 2010; Nolen-Hoeksema, 2012), compassion (e.g., Eisenberg & Lennon, 1983; Jaffee & Hyde, 2000), and prosocial behavior (e.g., Eagly & Crowley, 1986; Einolf, 2011), and gender differences may interact with meditation training efficacy (Rojiani et al., 2017; Sedlmeier et al., 2012). Yet, achievement of this recruitment goal was unsuccessful. The final data set with full study completers (n = 109) was comprised of a disproportionate number of women (n = 87; 79.8%) versus men (n = 22; 20.2%) (no men dropped out of the study or failed to complete pre-test or post-test measures; see Section 3.2.2 below). As noted, a multi-pronged 8-month recruitment process was undertaken to obtain a sufficiently-powered and gender-balanced sample. However, teachers had limited availability to participate in the 8-week study, and their consent was made on the condition that the study would take place in October and November of 2016. Therefore, changing the study start date to recruit more male teachers would have caused a portion of teachers who had signed up to withdraw, leading to a counterproductive cycle of recruitment and drop-out.

3.2.2 Participant Flow

Of the 255 individuals interviewed, and the 121 participants who provided their consent, 118 completed the pre-test measures (prior to randomization).[7] That said, nine participants withdrew at different points after pre-test and randomization, resulting in 109 participants who completed the study (90% retention; see Figure 5 below). Study

[7] The three participants who consented, but withdrew prior to pre-test and randomization, indicated personal emergencies or time demands as reasons for withdrawal. Further data on them is unavailable.

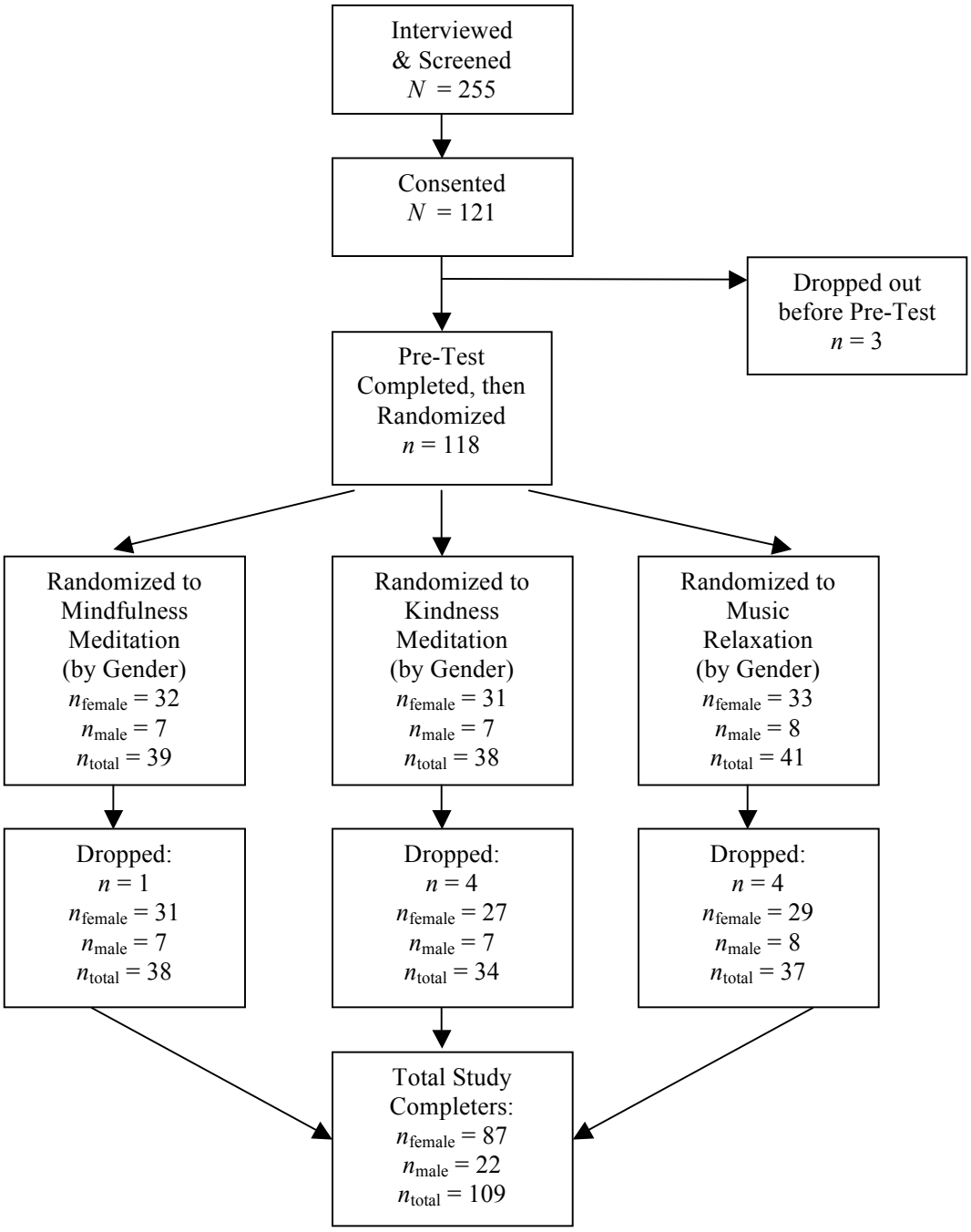

Figure 5. Participant flow chart from the recruitment screening interview to study completion. Randomization was conducted using the random number set generator at www.random.org. All drop-out participants throughout the 8-week study period were female (total attrition = 10%).

completers included 38 mindfulness meditation, 34 kindness meditation, and 37 music relaxation (active control) participants. Significant differences were not found for any

composite variables measured (at pre-test) between the nine non-completers and the 109 study completers, suggesting random assignment was sustained during the study.[8]

3.2.3 Participant Characteristics

The sample of study completers was on average 42.85 years old (*SD* = 8.39) (see Table 2 below for demographic statistics by gender and training group). Participants identified as: 79.2% Caucasian, 7.5% Asian, 6.6% 'other' race/ethnicity (e.g., Indo-Canadian), 4.7% mixed race/ethnicity, 0.9% Latin American, and 0.9% Arab or Persian. The distribution of annual household income was: 1.0% (25-50k), 17.3% (50-75k), 24% (75-100k), 35.6% (100-150k), 21.2% (150-250k), and 1.0% (250-500k). For teaching demographics, 76.8% of participants were elementary school teachers (grades K to 7), and 23.2% were secondary school teachers (grades 8 to 12).[9] This composition is consistent with prior meditation research in teachers (Emerson et al., 2017). For work status, 93.3% of participants taught five days a week and 6.7% taught four days a week.

3.2.4 Equivalence of Groups Following Randomization

Of the nine participants who withdrew after pre-test and randomization, all were female. This was reasonable as 81.4% of the sample at pre-test was female. A chi-square test was run to formally test if dropout varied by gender. It did not, χ^2 (1) = 2.23, p = .135. A chi-square analysis also was run to test if dropout varied by training group. It did not, χ^2 (2) = 2.14, p = .344. To assess whether the training groups were significantly different on any demographic or dependent variables in the completer sample at pre-test (which

[8] These analyses should be interpreted with caution given the large sample size difference between groups.

[9] These descriptive statistics represent 75.9% of the completer sample who reported grade taught (n = 82 of 108). One school district was organized into elementary school (K-5), middle school (6-8), and secondary school (9-12). This district represented 24.1% of the completer sample who reported grade taught (n = 26 of 108). In this district, 57.7%, 30.8%, and 11.5% were elementary, middle, and secondary school teachers.

Table 2

Summary of Demographic Statistics by Gender and Training Group Assignment

	Female Sample				Male Sample				Total Sample			
	Mind.	Kind.	Music.	Total	Mind.	Kind.	Music.	Total	Mind.	Kind.	Music.	Total
Participants (*n*)	31	27	29	87	7	7	8	22	38	34	37	109
Age (years)												
M	40.97[a]	42.26[b]	44.19[c]	42.42[d]	42.57	45.33[a]	45.71[b]	44.5[c]	41.28[a]	42.90[b]	44.52[c]	42.85[d]
SD	8.49	8.45	8.44	8.46	9.40	7.26	8.24	8.08	8.56	8.19	8.29	8.39
Race/Ethnicity (%)												
Caucasian	76.7[e]	72.0[f]	82.8[a]	77.4[g]	57.1	100	100	86.4	73.0[e]	78.1[f]	86.5[e]	79.2[g]
Asian	10.0	8.0	3.4	7.1	28.6	0	0	9.1	13.5	6.3	2.7	7.5
Other.	6.7	12.0	6.9	8.3	14.3	0	0	4.5	5.4	9.4	5.4	6.6
Mixed.	3.3	8.0	6.9	6.0	0	0	0	0	2.7	6.3	5.4	4.7
Latin American	3.3	0	0	1.2	0	0	0	0	2.7	0	0	0.9
Arab or Persian	0	0	0	0	0	0	0	0	2.7	0	0	0.9
Household Income. (%)												
0-25k	0[e]	0[b]	0[a]	0[h]	0	0	0	0	0[e]	0[h]	0[e]	0[i]
25-50k	0	4.3	0	1.2	0	0	0	0	0	3.3	0	1.0
50-75k	26.7	17.4	17.2	20.7	14.3	0	0	4.5	24.3	13.3	13.5	17.3
75-100k	13.3	43.5	13.8	22.0	28.6	57.1	12.5	31.8	16.2	46.7	13.5	24.0
100-150k	40.0	21.7	41.4	35.4	14.3	42.9	50	36.4	35.1	26.7	43.2	35.6
150-250k	20.0	8.7	27.6	19.5	42.9	0	37.5	27.3	24.3	6.7	29.7	21.2
250k-500k	0	4.3	0	1.2	0	0	0	0	0	3.3	0	1.0
Grade Taught (%)												
Elementary School (K-7)	88.0[f]	76.2[i]	70.0[j]	78.8[k]	83.3[a]	60.0[d]	60.0[d]	68.7[e]	87.1[j]	73.1[k]	68.0[l]	76.8[m]
Secondary School (8-12)	12.0	23.8	30.0	21.2	16.7	40.0	40.0	31.3	12.9	26.9	32.0	23.2
Work Status (%)												
Four Days Weekly	10.7[l]	7.7[c]	3.4[a]	7.2[m]	0	14.3	0	4.5	8.6[n]	9.1[c]	2.7[e]	6.7[0]
Five Days Weekly	89.3	92.3	96.6	92.8	100	85.7	100	95.5	91.4	90.9	97.3	93.3

Note. Mind. = mindfulness meditation group. Kind. = kindness meditation group. Music. = music relaxation group. Other. = other race or ethnicity (e.g., Indo-Canadian). Mixed. = mixed race or ethnicity. Household Income. = Annual household income. All percentages equal valid percent. Total variable percentages may not add to 100% due to rounding. Grade Taught statistics represent 75.9% of the completer sample who reported grade taught (n = 82 of 108); one district was not included in the table for Grade Taught because it's organized into elementary (K-5), middle (6-8), and secondary school (9-12), where 57.7%, 30.8%, and 11.5% were elementary, middle, and secondary school teachers (n = 26 of 108). All other statistics indicate the completer sample (n = 109). Exact sample sizes for each variable are below. Female sample sizes: [a] n = 29. [b] n = 23. [c] n = 26. [d] n = 78. [e] n = 30. [f] n = 25. [g] n = 84. [h] n = 82. [i] n = 21. [j] n = 20. [k] n = 66. [l] n = 28. [m] n = 83**.** Male sample sizes: n = 7 (MM), n = 7 (KM), n = 8 (MR), except [a] n = 6. [b] n = 7. [c] n = 20. [d] n = 5. [e] n = 16. Full sample sizes: [a] n = 36. [b] n = 29. [c] n = 33. [d] n = 98. [e] n = 37. [f] n = 32. [g] n = 106. [h] n = 30. [i] n = 104. [j] n = 25. [k] n = 21. [l] n = 20. [m] n = 66. [n] n = 35. [0] n = 105.

preceded randomization), chi-square tests were run for categorical variables (e.g., ethnicity/race) and ANOVAs with Tukey's HSD analyses were run for continuous variables (e.g., self-reported mindfulness). Significant differences at pre-test were found for self-reported emotional awareness, $F(2, 115) = 4.09$, $p = .019$. Post-hoc analyses indicated that emotional awareness at pre-test was higher in the mindfulness meditation group ($M = 4.44$, $SD = 0.58$) compared to the kindness meditation group ($M = 4.06$, $SD = 0.72$), $t(115) = 2.71$, $p = .011$, $d = 0.62$. Likewise, emotional awareness at pre-test was higher in the music relaxation group ($M = 4.37$, $SD = 0.56$) compared to the kindness meditation group, $t(115) = 2.18$, $p = .027$, $d = 0.49$. These results indicated the need to include self-reported emotional awareness as a covariate in all between-subjects analyses. Otherwise, group equivalence and random assignment were confirmed.

3.3 Study Procedure

The study was conducted in three phases. The first phase involved pre-test assessments. The second phase consisted of delivering a set of digital mental practices to participants for six weeks (average contact hours = 6.5). And, the third phase involved post-test assessments. A complete timeline of the study is presented in Figure 6 (below). After pre-test assessments, all participants were randomly assigned to the mindfulness

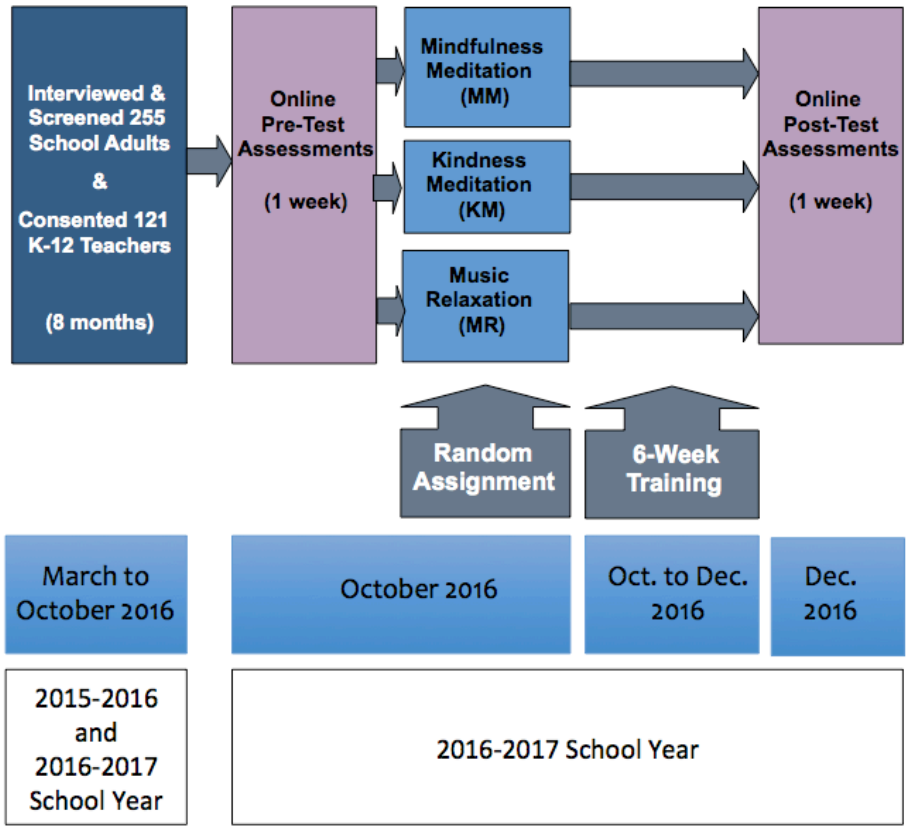

Figure 6. Timeline of the present study from the recruitment through post-test assessment phases. The 255 interviewed and screened individuals are referred to as 'school adults' because many of those who responded to the recruitment flyer were not teachers (e.g., school counselors, librarians). The final 121 individuals who consented to participate were K-12 classroom teachers working at 80-100% status.

meditation, kindness meditation, or music relaxation group as stratified by gender. Randomization was conducted by generating unique random subsets of the integers 1, 2, and 3 (1 = MM assignment, 2 = KM assignment, and 3 = MR assignment) independently for males and females (using a random number generator at www.random.org). As such, male and female participants were equally likely to be randomly assigned to the MM, KM, or MR training groups. The trainings were provided in a downloadable digital (MP3) format that could be used on computers, tablets, and mobile devices. This approach was used because it: (i) permitted a test of the effectiveness of digital meditation trainings for teachers; (ii) ensured delivered training fidelity; (iii) allowed

participants to seamlessly continue their practice post-study (see Cullen, 2011); and (iv) was accessible and affordable, potentially increasing adoption, commitment, and scalability of meditative practices in resource-constrained populations.

Pre-test and post-test assessments were collected using a multi-method (first-person and third-person measures) and multiple-levels-of-analysis (psychosocial) approach (explained in the Section 3.5 below). All measures and trainings were delivered online via FluidSurveys (www.fluidsurveys.com), a widely-used and reliable web environment for psychological research. At pre-test and post-test the same battery of assessments was administered (except for training feasibility and acceptability items, which were only administered at post-test). To prevent respondent fatigue and interaction effects between instruments, the pre-test and post-test assessments were split into two, 30-minute blocks (1 hour total). The assessments were completed on separate days during the same week at both pre-test and post-test. For their time and effort, participants each received $125 CAD after completing the post-test measures, plus potentially $0-6 more each, depending on how they behaved on the donation behavior measure (see Section 3.5.3.7 below). Participants also were given the study trainings assigned to the other groups, and a set of mindfulness and well-being resources for teachers and students, as additional study incentives following the completion of the post-test assessments.

3.4 Mental Trainings

3.4.1 Mental Training Content and Goals

The meditation trainings focused on present-centered, nonjudgmental awareness (in MM), and kindness and compassion cultivation (in KM). Table 3 (below) presents summaries of the methods and goals of each practice (this information is also detailed in-

Table 3

Summary of the 6-Week Digital Mental Trainings

	Proximal Training Goals	**Training Method**
Mindfulness Meditation (Kornfield, 2008, 2015, 2017; Vago & Silbersweig, 2012)	-Improved Mindfulness -Enhanced Attention and Executive Function -Greater Self-Awareness -Better Self-Regulation -Increased Emotional Balance	-Slow, deep breathing -Focus attention on breath -Redirect wandering mind to breath -Be kind in redirecting mind -Avoid deliberate thinking, or evaluating or elaborating on spontaneous thoughts or feelings -Stillness of body -Upright posture -Eyes closed or gaze cast downward
Kindness Meditation (Bankard, 2015; Kornfield, 2008, 2015, 2017)	-Enhanced Positive Emotions and Well-Being -Greater Connectedness to Others -Improved Emotional Resonance with Others -Increased Compassion for Self and Other Beings	-Focus on self, then loved ones, then strangers, then difficult others, then all beings -Send wishes of freedom from suffering and happiness to self, then loved ones, then strangers, then difficult others, then all beings -Redirect mind to persons of focus when it wanders -Be kind in redirecting mind -Stillness of body -Upright posture -Eyes closed or gaze cast downward
Music Relaxation (Pelletier, 2004)	-Physical Relaxation -Stress Reduction -Increased Calmness -Greater Positive Emotion and Psychological Well-Being	-Listening attentively to the recordings from start to finish -Stillness of body -Upright posture -Eyes closed or gaze cast downward

text). The practices were not teacher-specific. Active control group participants engaged in music relaxation practices to match MM and KM expectancies and demand characteristics (i.e., decreased stress or negative affect and increased calm or positive affect; see MacCoon et al., 2012), without explicitly fostering mindful attention and awareness (in MM) or kindness and compassion for oneself and others (in KM).

3.4.1.1 Mindfulness Meditation

Breath-focused meditations that train present-centered attention, and nonreactive, nonevaluative awareness toward all thoughts, feelings, and experiences served as the

focus of each MM practice (Kornfield, 2008, 2017; see Appendix B to access the MM practices). All MM trainings were taken from the 'Power of Awareness' (PoA) program (Kornfield & Brach, 2015). PoA is an online-based meditation course taught by Dr. Jack Kornfield and Tara Brach that was designed in collaboration with the Greater Good Science Center at UC-Berkeley, a world leader in research on well-being and compassion (www.greatergood.berkeley.edu). Only Dr. Kornfield's audio-guided meditations from the course were selected. The practices were truncated in a few places (i.e., periods of silence were shortened), so that training length across all training types was matched.

As such, MM practices of 8mins, 9mins, 20mins, and 23mins were provided to participants. The MM trainings recorded by Dr. Kornfield were designed specifically as support or 'booster' practices, and thus were not practices extracted from course instruction sessions. Although these specific practices had not been used in prior research, MM practices highly similar in structure and content have been used successfully in prior studies (Eberth & Sedlmeier, 2012), including studies on the prosocial effects of in-person (Condon et al., 2013) and digital (Lim et al., 2015) MM.

3.4.1.2 Kindness Meditation

In KM, practitioners visualize receiving and giving wishes for happiness (loving-kindness) and freedom from suffering (compassion) to all beings – from self and loved ones to strangers and difficult others (Kornfield, 2008, 2017; see the Appendix B to access the KM practices). By consciously generating positive emotions toward ingroup and outgroup members alike (e.g., helpful and challenging students), individuals' compassion for all beings may grow and motivate prosocially-driven behavior (Bankard, 2015; Salzberg, 2002). As with the MM trainings, participants followed audio-guided

KM trainings recorded by Dr. Kornfield. For KM, the practices were obtained from two separate meditation CDs released by Dr. Kornfield, and from the PoA online course (described above). The CDs were 'Guided Meditation: Six Essential Practices to Cultivate Love, Awareness, and Wisdom' (Kornfield, 2007), and 'Guided Meditations for Difficult Times' (Kornfield, 2010). The KM trainings were truncated in a few places (i.e., reducing the length of silences), so that training recordings across all groups were matched in length. Dr. Kornfield's KM practices were designed as self-administered, audio-guided trainings to cultivate greater kindness and compassion for oneself as well as others. As with MM, although prior research has not been conducted on these practices specifically, recent research using digital KM practices highly similar in structure and content was found to improve compassion and prosocial behavior in adults (Ashar et al., 2016; Galante et al., 2016; Weng et al., 2013, 2015).

3.4.1.3 Music Relaxation

In this study, drawing on prior work, music relaxation was employed as the active control training (see Appendix B to access the MR practices). Specifically, recordings of the following four classical music compositions by Wolfgang Amadeus Mozart served as the MR practices: 1) "Serenade No. 13 in G Major, 'Eine Kleine Nachtmusik', Kv. 525: Andante" (8mins); 2) "Sonata for Two Pianos in D Major, K. 448: Andante" (9mins); 3) "Clarinet Concerto in A, K. 622: Adagio" (20mins); and 4) "Concerto for Flute, Harp and Orchestra in C Major, K. 299: Andantino" (23mins). Recordings were obtained free of charge from Musopen, which is a non-profit organization, "focused on increasing access to music by creating free resources and educational materials" (https://musopen.org). The original recordings underwent minor edits to ensure the lengths of the MR practices

matched the durations of the MM and KM practices. Participants were instructed to find a comfortable and quiet place to sit, to close their eyes or lower their gaze, and to utilize headphones (if possible) to 'listen attentively' to the MR recordings.[10]

These four Mozart compositions were selected for a few reasons. First, across a range of study designs and populations, focused listening to classical music reliably attenuates negative emotions and increases positive emotions (Baumgartner, Esslen, & Jäncke, 2006; Chan et al., 2011; Harmat, Takács, & Bodizs, 2008; Kämpfe, Sedlmeier, & Renkewitz, 2010; Knight & Rickard, 2001; Koelsch & Jancke, 2015; Labbe, Schmidt, Babin, & Pharr, 2007; Pelletier, 2004; Siedliecki & Good, 2006). In this regard, MR is well-suited to match the documented salutary effects of MM (Eberth & Sedlmeier, 2012) and KM (Galante et al., 2014) on emotion. Second, although the 'Mozart effect' (i.e., the notion that simply listening to Mozart makes one more intelligent; Rauscher, Shaw, & Ky, 1993) has been debunked (Thompson, Schellenberg, & Husain, 2001), because of the putative Mozart effect, the influence of Mozart's music on psychological functioning is well-studied compared to the works of other composers (e.g., see Pietschnig, Voracek, & Formann, 2010). Furthermore, prior studies have found days-long and weeks-long interventions that involve listening to Mozart, including some of the pieces noted above, can induce relaxation and ameliorate stress and negative affect (Innes, Selfe, Khalsa, & Kandati, 2016; Moss et al., 2012; Smith & Joyce, 2004).

Third, out of the small number of studies that have used music as a control group in meditation research (e.g., Lavretsky et al., 2013; MacCoon et al., 2012; Pomykala et al., 2012; Wheeler & Lenick, 2015), a few recent studies have used Mozart pieces

[10] The same instructions were given to the MM and KM groups as well, to ensure all groups were matched on the basic instructions for engaging in the digital mental trainings.

specifically (Innes et al., 2016; Moss et al., 2012). Relying on this prior work increased the likelihood that the MR trainings would serve as effective active controls to MM and KM. Lastly, the pieces selected provided participants with variety. Given that personal preferences in music vary widely, it was important to afford some degree of choice in the kind of relaxing Mozart music available to participants to promote engagement. The 8min piece largely used string instruments, the 9min piece employed mostly piano, and the 20min and 23mins pieces used a combination of flute, harp, and full orchestration.

3.4.2 Mental Training Instructor and Facilitators

The same instructor with decades of meditation teaching experience and a doctorate in clinical psychology led the MM and KM trainings via digital recordings: Dr. Jack Kornfield. The same instructor was used because employing different instructors confounds meditation effects with the individual delivering them (i.e., 'teacher effects'). Dr. Kornfield was selected because he is recognized as one of the first modern teachers to introduce meditation practices to the West, he has led meditation trainings for over 40 years, and he has sold over a million books on cultivating equanimity and compassion (www.jackkornfield.com; Kornfield, 2008, 2017). Accordingly, Dr. Kornfield is among the top people in the world to understand and appreciate the subtle differences in how people may respond to meditation instruction in the East versus the West. Further, Dr. Kornfield also holds a PhD in Clinical Psychology. As such, Dr. Kornfield was uniquely poised to train Western beginners in Eastern-based meditation practices.

Additionally, all participants were offered the option to email or talk over the phone with a training facilitator (not Dr. Kornfield) during the training period. The purpose of the facilitator was twofold: 1) to provide answers to common questions that

were likely to arise for naïve meditation practitioners (e.g., 'Why can't I focus on my breath?' or 'When is the best time of day to practice?'); and 2) to provide a degree of social support for individuals beginning their new practice, as meditation can be a challenging practice to learn (e.g., Lindahl, Fisher, Cooper, Rosen, & Britton, 2017), and it appears that social reinforcement of learning mindfulness (and related skills) may increase practice success (e.g., see Eberth & Sedlmeier, 2012). Two people served as training facilitators. One facilitator was a yoga and meditation instructor with over a decade of teaching experience and personal practice. The other facilitator was a person with five years of experience teaching a compassion-based mindfulness program called 'Compassion Cultivation Training' (CCT; Jazaieri et al. 2013). Participants were reminded once a week during a weekly check-in that facilitators were available if they had practice questions. Nevertheless, no participants utilized a training facilitator. They chose to pursue the practices on their own. The reasons for this behavior are unknown.

3.4.3 Mental Training Timetable and Dosage

All MM, KM, or MR trainings were delivered on an 'incremental dose' timetable, and participants were asked to practice five days a week over the 6-week training period (see Jazaieri et al., 2013; Weng et al., 2013). For the incremental timetable, the practices were rolled out as follows: 8min practice (Week 1), 9min practice (Week 2), 20min practice (Week 3), and 23min practice (Week 4). Thus, participants started with just 8 minutes of basic training during the first week to familiarize themselves with the practices. Then, as the weeks went on, more in-depth trainings (up to 23-minutes long) were offered. This incremental approach better mimics the body of evidence in neuroscience regarding how humans seem to acquire new skills (e.g., Green & Bavelier,

2008), which is surprisingly not taken into account with many meditation trainings (for exceptions see Jazaieri et al., 2013 and Jennings, 2016b). The aim was that through incremental practice increases, the trainings would more effectively foster habit formation (e.g., Abraham & Michie, 2008), and they might reduce the strength or persistence of early aversive or iatrogenic experiences that deter further practice.

To support participant autonomy, participants were free to engage in any practice they had received, as long as they did so consistently, five days a week over the 6-week training period. As such, based on explicit instructions to the participants, the range for total recommended practice time was between 4 hours (engaging in only the 8min practice five days a week for six weeks) to 9 hours (engaging in the longest practice available each week five days a week for six weeks). This equaled, on average, approximately 6.5 hours of recommended training time over the 6-week training period. Although over a longer time span (six weeks vs. two weeks), this dosage is similar to the dosage assigned in recent studies that found digital meditation training enhanced compassion and prosocial behavior (Lim et al., 2015; Weng et al., 2013, 2015).

3.5 Measures

Table 4 (below) presents a summary of all measures used in this study, as organized by the three study aims. Detailed descriptions of these measures and their psychometric properties are provided in the following subsections. All measures were in part selected because of the ability to administer them in an online assessment battery.

3.5.1 First Study Aim: Digital Training Feasibility and Acceptability Measures

The first study aim was to evaluate the feasibility and acceptability of digital meditation trainings for K-12 classroom teachers. To this end, at post-test only,

Table 4

Summary of Dependent Variable Measures

Study Aim	**Theoretical Construct**	**Empirical Construct**
First Study Aim Evaluating the feasibility and acceptability of digital meditation for teachers.	Training Engagement, Efficacy, and Satisfaction	CARE Acceptability Questionnaire (4 items); Clarity and Compliance Questionnaire (2 items) [6 items]
Second Study Aim Testing the efficacy of digital meditation practices in achieving core practice goals.	Self-Rated Affect	modified Differential Emotions Scale (10 positive and 10 negative emotion word triads) [20 items]
	Self-Rated Stress	Based on modified Differential Emotions Scale & the Maslach Burnout Inventory for Educators (3 items)
	Self-Rated Mindfulness	Five Facet Mindfulness Questionnaire-Short Form (19 items)
Third Study Aim Testing the primary and secondary (exploratory) hypothesis sets (H1-H4).	Self-Rated Emotion Awareness	Perception of Emotion subscale, Assessing Emotions Scale (AES; 3 items); Perceiving Emotion subscale, Self-Rated Emotional Intelligence Scale (SREIS; 3 items); Difficulty Identifying Feelings subscale, Toronto Alexithymia Scale (6 items) [12 items]
	Self-Rated Emotion Regulation	Managing Own Emotions and Managing Other's Emotions subscales of AES (6 items); Managing Emotion and Social Management subscales of SREIS (6 items) [12 items]
	Emotion Awareness & Regulation Task	Emotion Skills Task: baseline affect, emotion induction video, judgment task (essay reading and grading) (at pre and post-test); and two-item emotion awareness and regulation probe (at post-test only)
	Self-Rated Compassion	Empathic Concern, Perspective Taking, and Personal Distress subscales of the Interpersonal Reactivity Index (21 items)
	Compassion Task	Multi-Faceted Empathy Test-CORE (responses to 20 images of suffering); emotional compassion facet only
	Prosocial Behavior	Dictator Game (participants keep money for themselves or give to anonymous other); Third-Party Game-Unfair Rounds (participants keep money for themselves or give it to anonymous other who is treated unfairly by an unknown third party); Third-Party Game-Fair Rounds (participants keep money for themselves or give it to anonymous other who is treated fairly by an unknown third party); Punishment Game (participants keep money for themselves or pay to take money away from an unknown third party who treats an anonymous other unfairly); Donation Behavior (participants give $0-6 to a local children's hospital or keep it for themselves) [5 measures total]

subjective reports of training engagement, efficacy, and satisfaction were assessed using six items. These six items were adapted from a program evaluation tool that has effectively assessed mindfulness-based training for teachers in a number of studies (i.e., the CARE Acceptability Questionnaire [CAQ]; Jennings et al., 2011, 2013, 2017), as well as from a two-item 'clarity and compliance questionnaire' (CAQ) from an audio-guided meditation training study (Feldman et al., 2010) (see Appendix C for the full measure). Detailed information regarding each item in the measure is provided below.

3.5.1.1 Training Engagement: Adherence and Practice-Induced Sleep

Training engagement was measured at post-test by asking participants how many times per week, on average, they engaged in their practices. Participants were also asked how many times per week, on average, they engaged in each particular practice (i.e., 8min, 9min, 20min, 23min), when a practice became available. Digital login records were not employed because it would have required participants to have internet access every time they used a practice, which would have reduced accessibility and possibly training adherence. Self-reported adherence has been used in a number of prior meditation RCTs (see Emerson et al., 2017; Galante et al., 2014; Sedlmeier et al., 2012). Training engagement was also (inversely) measured by asking how often teachers fell asleep during their practices on a 7-point scale from *never* to *always*. A feasibility concern was that, if many teachers used their practices in the morning or at night and the practices induced relaxation, they might fall asleep often and fail to accrue the psychosocial practice benefits (though sleep is a benefit too). The present researcher created this item.

3.5.1.2 Training Efficacy: Teaching Effectiveness, Stress, and Patience

Training efficacy was measured using two questions at post-test. First,

participants were asked how they thought the following outcomes may have changed due to their mental practices: 'teaching effectiveness,' 'teaching stress,' and 'teaching patience.' Participants responded on a 7-point bipolar scale ranging from *much worse* to *neither better nor worse* to *much better*. To measure perceived training efficacy, participants also were asked to rate the relative impact of the mental trainings on their job performance compared to other professional trainings. They responded using a 7-point, bipolar scale that ranged from *much lower* to *neither higher nor lower* to *much higher*.

3.5.1.3 Training Satisfaction: Practice Referrals and Enjoyment Motivation

Training satisfaction was measured with two questions at post-test. First, participants rated their willingness to recommend the practices to a colleague or friend. Ratings were made on a 5-point scale ranging from *not at all likely* to *highly likely*. Second, participants reported their degree of motivation to continue engaging in the practices during the 6-week training period, regardless of what motivated them when they began the study. Specifically, participants rated how motivating their 'enjoyment of the practices' was on a 5-point scale ranging from *not at all motivating* to *highly motivating*. Greater enjoyment motivation was used to index higher training satisfaction.

3.5.2 Second Study Aim: Affect and Mindfulness Measures

In this section, measures are described that assess self-reported positive and negative affect and stress, as well as four facets of mindfulness (i.e., observing, acting with awareness, non-reacting, and non-judging). These measures were included because salutary changes in affect and mindfulness are core proximal training goals of MM and KM (Kornfield, 2008, 2017), and they are outcomes that are reliably improved in meta-analyses on MM and KM (Eberth & Sedlmeier, 2012; Galante et al., 2016). As such, the

affect and mindfulness measures served as a 'manipulation check,' to test the extent to which the meditations in this study achieved core proximal goals and documented effects, prior to assessing their influence on emotion skills, compassion, and prosocial behavior.

3.5.2.1 Self-Reported Positive and Negative Affect

Positive affect and negative affect were measured using the validated Modified Differential Emotions Scale (mDES; Fredrickson, Tugade, Waugh, & Larkin, 2003; see Appendix D). The mDES assesses the frequency with which one has experienced 10 positive emotions (e.g., proud, interested) and 10 negative emotions (e.g., ashamed, scared) 'over the past 2-3 weeks.' This timeframe is thought to tap one's dispositional affect without being as subject to salient emotional memory biases as longer timeframes (Fredrickson, 2013). Each emotion is identified by three emotion synonyms. For example, gratitude is described with the words 'grateful, appreciative, thankful,' and anger is described with the words 'angry, irritated, annoyed.' Composite scores for positive and negative emotions were created by averaging across the 10 positive and 10 negative affect word triads. Responses ranged on a 6-point scale from *never* to *always*.

The subscales of the mDES were found to predict psychological resilience (Cohn, Fredrickson, Brown, Mikels, & Conway, 2009), and further, mediate post-traumatic depression and growth after the September 11th attacks in Americans (Fredrickson et al., 2003). Also, the positive affect subscale is sensitive to change due to meditation training, changes specifically which endured at a 2-week (Fredrickson et al., 2008) as well as a 15-month follow-up (Cohn & Fredrickson, 2010). The positive affect subscale showed high reliability at pre-test and post-test (α = .90 and .93). The negative affect subscale showed lower, but still good reliability at pre-test and post-test (α = .80 and .87). These

reliabilities are similar to those reported in prior work with the mDES (α = .94 for positive affect and α = .85 for negative affect; Fredrickson et al., 2008).

3.5.2.2 Self-Reported Stress

Based on the mDES (Fredrickson, 2013), a 3-item measure was used to assess psychological stress, given its importance in the present study and in teacher health and job performance (e.g., Greenberg et al., 2016). Three emotion word triads comprised the stress measure. These triads were: 'stressed, nervous, overwhelmed,' 'anxious, tense, on edge,' and 'drained, exhausted, burned out.' The stress scale was administered in random order within the positive and negative affect scales. A 6-point response scale ranging from *never* to *always* was employed. The first triad was taken directly from the mDES (Fredrickson, 2013). The present researcher created the second two triads based on the Emotional Exhaustion (EE) subscale of the Maslach Burnout Inventory-Educators' Survey (MBI; Maslach et al., 1997). The MBI is used widely in research on teacher stress and burnout given its ability to predict teacher social-emotional well-being (see McIntyre et al., 2017). Further, the MBI EE subscale was found to change in response to a meditation-based teacher program (Jennings et al., 2017). Given the hour-long assessment battery teachers completed, rather than using the full EE subscale, or another validated stress measure, the 3-item approach was adopted to assess teacher stress with economy and minimize respondent fatigue. In support of this approach, at baseline the stress scale correlated highly with the mDES negative affect subscale (r = .53, p = .000). Reliability of the stress measure was good at pre-test and post-test (α = .81 and .86). The reliabilities are consistent with prior work using the mDES (α = .85 for negative affect; Fredrickson et al., 2008), as well as the MBI EE subscale (α = .90; Maslach et al., 1996).

3.5.2.3 Self-Reported Mindfulness

The Five Facet Mindfulness Questionnaire-Short Form (FFMQ-SF) was utilized to measure mindfulness (Bohlmeijer, ten Klooster, Fledderus, Veehof, & Baer, 2011; see Appendix E for the full scale). The 'five facets' of the FFMQ-SF are: observing (e.g., 'I pay attention to physical experiences, such as the wind in my hair or sun on my face'), describing (e.g., 'I am good at finding words for my feelings'), acting with awareness (e.g., 'I find myself doing things without paying attention'), non-judging of inner experience ('non-judging'; e.g., 'I tell myself I shouldn't be thinking the way I'm thinking'), and non-reacting to inner experience ('non-reacting'; e.g., 'When I have distressing thoughts or images, I just notice them and let them go'). The 19-item measure was rated on a 7-point scale ranging from *never* to *always*.

The FFMQ was selected because it is a product of a factor analysis and conceptual synthesis of five prior mindfulness scales (Baer et al., 2006). Furthermore, it has been employed in numerous studies that show MM and KM can enhance mindfulness (Eberth & Sedlmeier, 2012; Emerson et al., 2017; Galante et al., 2014; Sedlmeier et al., 2012). Specifically, in multiple evaluations of two leading meditation-based programs for teachers (i.e., CARE and SMART), mindfulness was shown to change using the full FFMQ (Benn et al., 2012; Jennings et al., 2011, 2013, 2017; Roeser et al., 2013). The short-form was employed in the present study to reduce the cognitive demand of the assessment and respondent fatigue. The describing subscale was not included, as prior research on meditation-based training for teachers failed to find evidence that the describing subscale is responsive to change (Jennings et al., 2013, 2017).

Total scale reliability at both pre-test and post-test was good (α = .88 and .88). Subscale reliability at pre-test and post-test was also good: observing (α = .78 and α = .81), acting with awareness (α = .81 and .79), non-judging (α = .82 and .88), and non-reacting (α = .84 and .82). The reliabilities are largely consistent with those in prior research using the FFMQ-SF (observing α = .78; acting with awareness α = .86; non-judging α = .86; and non-reacting α = .73; Bohlmeijer et al., 2011).

3.5.3 Third Study Aim: Emotional Awareness and Regulation, Compassion, and Prosocial Behavior Measures

In this section, to address the third study aim, which was to test the primary and secondary set of hypotheses, measures are described that assess emotional awareness, emotion regulation, compassion, and prosocial behavior. Self-report, task-based, and behavioral operationalizations of these dependent variables were used to evaluate the present study hypotheses using both first-person and third-person measures.

3.5.3.1 Self-Reported Emotional Awareness

Self-reported emotional awareness was measured with items from three separate validated scales (detailed below) to comprehensively assess the construct (12 items in total; see Appendix F). This approach was taken because no known scale gauged awareness of one's own emotions (self-emotion awareness) and awareness of others' emotions (other-emotion awareness) as separate abilities. However, emotional awareness is a multi-faceted construct that includes awareness of one's own emotional state as well as the emotional states of others (Lane et al., 1990), which is likely particularly important for classroom teachers (see Hamre et al., 2013). Furthermore, emotional awareness can be conceptualized as a plastic emotional ability or an enduring psychological trait (see

Mayer, Salovey, & Caruso, 2008). As the present study examined how meditation could enhance emotional awareness in response to six weeks of training, the ability model of the construct was adopted. Therefore, three validated self-report scales were adapted and combined to create the present multi-faceted measure of emotional awareness ability.

To measure self-emotional awareness, a set of items was extracted from the Difficulty in Identifying Feelings (DIF) subscale of the 20-item Toronto Alexithymia Scale (TAS-20; Bagby, Parker, & Taylor, 1994). Alexithymia is considered a trait versus ability. However, no ability scales of self-focused emotional awareness existed when this study was designed, and as noted below, the TAS-20 has been shown to change in response to meditation training (suggesting it may be a plastic construct). Six items that tap a dearth in emotional awareness (a facet of alexithymia) were assessed with the TAS-20 DIF subscale (e.g. 'I have feelings that I can't quite identify'). As all original items on the TAS-20 were negatively keyed, three items were changed from being negatively to positively keyed to reduce mindless automatic responding (i.e., the acquiescence bias; Lindwall et al., 2012). Also, two positively valenced items were created, modeled after the existing items, totaling 8 items overall (i.e., 'When I am glad, I know if I am excited, amused, or content' and 'I often don't know why I am happy'). This approach provided a more balanced assessment of emotional awareness, as all other items focused on negative emotions. The TAS-20 DIF subscale is significantly correlated with depression, anxiety, stress, and 'fears of compassion' (concerns with receiving compassion or giving compassion to oneself and others; Gilbert et al., 2012). The TAS-20 is sensitive to change as a result of meditation training (Arias, Justo, & Granados, 2010), and the DIF subscale has shown good reliability in prior work ($\alpha = .80$; Parker, Taylor, & Bagby, 2003).

To measure other-emotional awareness, a set of items was extracted from the Emotion Perception (EP) subscale of the Self-Rated Emotional Intelligence Scale (SREIS; Brackett et al., 2006), as well as from the Perception of Emotion (PoE) subscale of the Assessing Emotions Scale (AES; Schutte et al., 1998; Schutte, Malouff, & Bhullar, 2009). These scales were employed because they can be used to assess emotional awareness as an ability rather than a trait. Three of the four EP items on the SREIS were used (e.g., 'By looking at people's facial expressions, I recognize the emotions they are experiencing'). One item was dropped because of low construct validity ('I can tell when a person is lying to me by looking at his or her facial expression'). The SREIS predicts empathy, well-being, positive and negative affect, and life satisfaction (Brackett et al., 2006; Webb et al., 2013). It also predicts affective forecasting accuracy (prognosticating how one will feel in the future; Hoerger, Chapman, Epstein, & Duberstein, 2012). This suggests the SREIS' value in predicting emotion-based judgments, which aligns with the emotional awareness and regulation task used in this study (see Section 3.5.3.3 below). The SREIS has shown good reliability in prior work ($\alpha = .84$; Brackett et al., 2006).

There are five PoE items on the AES that tap other-emotion awareness (e.g., 'I can tell how people are feeling by listening to the tone of their voice'). Two items were dropped because of their high overlap with SREIS items. AES total scores predict mood repair after a negative emotion induction (Schutte, Malouff, Simunek, McKenley, & Hollander, 2002) and greater cooperative behavior in a Prisoner's Dilemma game (Schutte et al., 2001), which are similar to the behavioral measures used in this study to assess emotion skills and prosocial actions. AES scores also are higher in meditators than non-meditators and increase due to meditation practice (Chu, 2010). Studies using the

AES show that the PoE subscale has good reliability (mean α = .80; Schutte et al., 2009).

As each pool of items was taken from one of three different scales (i.e., TAS-20, SREIS, and AES), the instructions from the SREIS (Brackett et al., 2006) were adapted to suit all scale items. Response values ranged on a 7-point scale from *never* to *sometimes* to *always*. Participants were asked to rate each item in regard to 'how you are most of the time (in general).' In support of the validity of this newly adapted emotional awareness scale, at baseline the scale was negatively correlated with the mDES negative affect subscale (r = -.32, p = .000), and it was positively correlated with the mDES positive affect subscale (r = .46, p = .000). It was also moderately and positively correlated with all mindfulness subscales: observing (r = .38, p = .000), acting with awareness (r = .32, p = .000), non-judging (r = .31, p = .001), and non-reacting (r = .22, p = .019). Total scale reliability at pre-test and post-test was good to high (α = .89 and .91), and exceeded the reliabilities (see above) found in prior work using the TAS-20 (Parker et al., 2003), the SREIS (Brackett et al., 2006), and the AES (Shutte et al., 2009).

3.5.3.2 Self-Reported Emotion Regulation

To measure self-reported emotion regulation, items from two validated scales were employed (12 items in total; see Appendix G). Similar to emotional awareness, emotion regulation was assessed using items from the SREIS (Brackett et al., 2006) and the AES (Schutte et al., 1998, 2009). As with emotional awareness, these specific scales were chosen because they can be used to measure emotion regulation as a plastic ability versus a personality trait (see Mayer et al., 2008). Items from these scales were pooled to measure self-focused emotion regulation ('self-emotion regulation') – regulating one's own emotions to meet a goal or environmental demand – and other-focused emotion ('other-emotion regulation') – helping others to regulate emotions to meet a goal or

environmental demand. The approach was implemented because emotion regulation is a multifaceted construct that involves the regulation of one's own emotions as well as the emotions of others (see Brackett et al., 2006; Coan, 2011; Zaki & Williams, 2013), which is a significant distinction for classroom teachers (Sutton & Harper, 2009).

Accordingly, two of the four items on the Managing Emotion (ME; self-emotion regulation) subscale, and all four items on the Social Management (SM; other-emotion regulation) subscale of the SREIS were used. To preserve measurement economy, two items were dropped from the ME subscale given their clear redundancy with other items. An example item from the ME subscale is: 'I can handle difficult or stressful situations without getting too nervous.' Based on SREIS items, two additional self-emotion regulation items were created by the present researcher to better capture the multi-valenced nature of the construct (i.e., 'I can create or enhance positive moods in myself' and 'I don't know how to make myself feel better when I'm sad or down' [reverse-scored]). An example item from the SM subscale is: 'When someone I know is in a bad mood, I cannot help the person feel better' (reverse-scored). A few items on the ME and SM subscales were changed from positively to negatively keyed phrasing to reduce automatic responding (Lindwall et al., 2012). Also, some language was slightly adapted in a few items to better reflect emotion regulation ability versus knowledge (e.g., changed from: 'I know the strategies to create or enhance positive moods in other people' to 'I can create or enhance positive moods in other people'). The ME and SM subscales of the SREIS are positively associated with facets of affective forecasting accuracy (Hoerger et al., 2012), an emotion skill relevant to both emotional awareness and regulation (Dunn, Brackett, Ashton-James, Schneiderman, & Salovey, 2007). For more information on

SREIS reliability and validity see Section 3.5.3.1 (above).

Regarding items from the AES, the Managing Own Emotions (MWE; self-emotion regulation) and Managing Other's Emotions (MOE; other-emotion regulation) subscales are comprised of nine and eight items, respectively. Three items from the MWE and two items from the MOE were used. Reasons for not retaining the subscales in their entirety include item overlap with the SREIS subscales, measurement economy, and within-domain item homogeneity. An example item from the retained MWE subscale is: 'I have control over my emotions.' To provide a more balanced measure, the present researcher created one self-emotion regulation item based on the AES that was reverse-scored: 'I often feel overwhelmed by my emotions' (reverse-scored). An example item from the MOE subscale is: 'I cannot help other people feel better when they are sad or down' (reverse-scored). As with the SREIS, some AES items were slightly adapted so that an equal number of items were negatively and positively keyed. Likewise, language was adapted in a few items to convey emotion regulation ability versus knowledge (e.g., changed from: 'When I experience a positive emotion, I do not know how to make it last long' to 'When I experience a positive emotion, I cannot make it last long').

The AES does not correlate with an established measure of social desirability (Kirk, Schutte, & Hine, 2008). Further, the total AES and the emotion regulation subscale of the AES increase in response to eight weeks of meditation training (Chu, 2010). Both the MWE and MOE subscales of the AES have demonstrated good to acceptable internal consistencies as well (αs = .78 and .66, respectively; Schutte et al., 2009).

As emotion regulation items were taken from the SREIS and the AES, the instructions from the SREIS (Brackett et al., 2006) were adapted to suit all scale items.

Response values ranged on a 7-point scale from *never* to *sometimes* to *always.* Participants were asked to rate each item in regard to 'how you are most of the time (in general).' In support of the validity of this newly adapted emotion regulation scale, at baseline the scale was negatively correlated with the mDES negative affect subscale (r = -.45, p = .000) and the 3-item stress measure (r = -.27, p = .003), and it was positively correlated with the mDES positive affect subscale (r = .58, p = .000). The emotion regulation scale was also moderately correlated with all mindfulness subscales: observing (r = .40, p = .000), acting with awareness (r = .37, p = .000), non-judging (r = .52, p = .000), and non-reacting (r = .44, p = .000). Total scale reliabilities at pre-test and post-test were high (α = .90 and .91), and were better than the reliabilities reported in prior work (see above) on the SREIS (Brackett et al., 2006) and AES (Shutte et al., 2009).

3.5.3.3 Emotional Awareness and Regulation Task

Prior research has found that performance measures of emotion skills better predict social behavior than self-report measures because self-report measures may reflect inaccurate beliefs about one's abilities (e.g., Brackett et al., 2006). Thus, in addition to measuring emotional awareness and regulation via self-report, a complementary strategy is to exercise these skills during a performance task (e.g., see Mayer, Salovey, & Caruso, 2002). Accordingly, one of the most widely studied influences of emotion on cognition is 'emotion congruency.' This is the widespread phenomenon that positive emotions increase favorable, and negative emotions increase unfavorable judgments of people, places, and events (Forgas & Eich, 2012; Mayer, Gaschke, Braverman, & Evans, 1992). In a recent study, teachers were induced to feel sadness versus happiness via autobiographical recollection, and then they graded a real

student's essay on overall performance, spelling and punctuation, vocabulary, composition structure, and creativity (Brackett, Floman, Ashton-James, Cherkasskiy, & Salovey, 2013). Teachers in the sadness versus happiness condition evaluated the student essay more unfavorably. Also, when asked if they thought that their emotions affected their grading, 86% of teachers said that it did not, despite evidence that it did.

Correspondingly, one way to assess teacher emotional awareness and regulation skills in 'real-time' is to induce particular emotions, give teachers a complicated, but ambiguous decision-making task (such as grading a student essay), and then measure the extent to which emotion biases their judgment and the extent to which they are aware of this bias. It is important that the task require elaborate cognitive processing and is widely interpretable because it is under these conditions that emotions are more likely to color judgment (Forgas, & George, 2001). Those who show less bias are likely to be more emotionally aware and regulated than their counterparts (see McFarland, White, & Newth, 2003; Schwarz & Clore, 1983, 2003). Indeed, prior research indicates that greater emotional awareness and regulation reduce emotional bias in a range of judgments (e.g., Ciarrochi, Caputi, & Mayer, 2003; Seo & Barrett, 2007). To address the limitations of self-report measures of emotion skills (see Conte, 2005), the present study utilized just such an operationalization of emotional awareness and regulation. Furthermore, given that very few studies have assessed emotion skills behaviorally in meditation research with teachers (Emerson et al., 2017), and that this task had to be administered online, the implementation of a new emotion skills task was warranted.

With that in mind, the specific paradigm employed in the present study is described below (see Appendix H for the full procedure and assessment questions). First,

all participants self-reported their baseline affect on a 6-item, single emotion word scale (e.g., 'frustrated' and 'calm'). They responded using a 5-point scale ranging from *not at all* to *completely* to indicate how they were feeling 'right now.' Second, participants were induced to feel angry using a video depicting an ornery principal who has given up on her students (at pre-test), or a dispute between a teacher and her colleagues as its mediated by administrators (at post-test). Prior research has found that these videos effectively induce anger in teachers (Floman, Brackett, & Stern, 2016). Third, participants were given a different persuasive essay at pre-test and post-test written by a grade 6 student, and they were asked to evaluate each essay using following seven criteria: *student ability, overall quality, spelling and punctuation accuracy*, *vocabulary diversity, creativity, composition organization,* and *persuasiveness*. The essays were obtained from Thoughtful Learning, which is an organization that has been operated by educators for over 40 years and provides real K-12 student model essays for different proficiency classifications (based on the U.S. Common Core standards; https://k12.thoughtfullearning.com). The grading criteria were taken from a prior study that used a similar measurement paradigm in a non-training context (Brackett et al., 2013), with the addition of 'overall student ability' and 'persuasiveness.' Lastly, at post-test only, teachers rated the extent to which they were aware that their emotions may have influenced their grading, and the extent to which they may have regulated their emotions to prevent such an emotional influence.

To assess the validity of the new emotional awareness and regulation task, bivariate correlations were run between baseline task scores (using each standardized grading criterion as a single item in a composite measure) and baseline self-reports of affect, mindfulness, and emotional awareness and regulation. The emotion skills task was

correlated with self-reported positive affect on the mDES (r = .24, p = .021), observing on the FFMQ (r = .28, p = .003), and the newly adapted emotion regulation scale (r = .19, p = .044). Measure reliabilities (using each standardized grading criterion as a single item in a composite) at pre-test and post-test were acceptable to good (α = .78 and = .85).

3.5.3.4 Self-Reported Compassion

Self-reported compassion was measured using the Interpersonal Reactivity Index (IRI; Davis, 1980, 1983; see Appendix I for the full scale). Specifically, the IRI's empathic concern (EC), perspective taking (PT), and personal distress (PD) subscales were employed. EC is a measure of one's dispositional concern for others who are in need (i.e., the emotional resonance facet of compassion; Goetz et al., 2010). An example item is: 'I often have tender, concerned feelings for people less fortunate than me.' PT is a measure of the tendency to put oneself in other people's shoes (i.e., the cognitive understanding facet of compassion; Goetz et al., 2010). An example item is: 'Before criticizing somebody, I try to imagine how I would feel if I were in their place.' PD is a measure of dysregulated response tendencies toward others' suffering, such that people become consumed by their own psychological stress in response to the pain of others (see Batson, 2011). The fantasy subscale was not used, as it measures imaginative abilities more than compassionate qualities. The employed IRI was 21 items. A 5-point response scale was used ranging from *does not describe me very well* to *describes me very well*.

The IRI was selected because it provided a multifaceted assessment of compassion. Also, it specifically distinguishes between empathically concerned and emotionally regulated versus empathically concerned and emotionally dysregulated responses to the human suffering. These facets aligned with the hypothesis that MM

would increase prosocial behavior via its influence on emotional awareness and regulation (e.g., less personal distress), and KM would increase prosocial behavior via its influence on compassion (e.g., more empathic concern). Additionally, although it is a measure of trait compassion (one's dispositional compassion across time and contexts), MM-based training has been shown to improve the EC, PT, and PD subscales over only eight weeks (Moll et al., 2015). This suggests that the IRI may measure compassion as a malleable capacity. Indeed, a prior study found that a meditation-based teacher program enhanced the EC subscale as well (Benn et al., 2012). Also, prior studies testing the influence of digital KM on prosocial game behavior found that the EC subscale predicted compassionate giving and was inversely correlated with altruistic punishment in a set of prosocial economic games (which are two central prosocial measures in this study; Weng et al., 2013, 2015). At pre-test and post-test the reliability of the full scale (reverse-scoring PD) was acceptable (α = .75 and .78). The pre-test and post-test subscale reliabilities also were acceptable: EC (α = .74 and .69), PT (α = .80 and .81), and PD (α = .77 and .85). These reliabilities were generally commensurate with those found in prior research on the IRI (EC α = .72; PT α = .75; PD α = .78; Davis, 1980).

3.5.3.5 Compassion Task

Other-focused compassion is highly socially desirable (Fiske, Cuddy, & Glick, 2007). As such, compassion was measured using a behavioral task in addition to a self-report scale. The Multi-Faceted Empathy Test (MET) was employed (Dziobek et al., 2008). The MET is comprised of 23 image pairs drawn largely from an internationally normed and validated database (the International Affect Picture System; Lang, Bradley,

& Cuthbert, 1997). All images depict individuals in states of physical and/or emotional suffering at varying degrees of intensity and who vary on gender, age, and race/ethnicity.

An image-broadened (40 image pairs) and procedurally-streamed (20min-long) version of the MET – the MET-CORE – was employed in this study (Edele et al., 2013; see Appendix J for the measure). The MET-CORE was used because it provides stimuli with a wider emotional range, it requires half the time to complete, and it can be administered in an online battery (Edele et al., 2013). To preserve measurement economy, and minimize respondent fatigue and acquiescence bias (Lindwall et al., 2012), only the explicit emotional compassion facet of the MET-CORE was used. In this facet, participants were asked to indicate their degree of 'concern for' each person suffering in the task images, using a 6-point scale ranging from *not at all concerned* to *extremely concerned*. All MET-CORE images were presented to participants in randomized order.

A prior study found that emotional compassion measured by the MET-CORE strongly predicted prosocial economic game behavior (i.e., in the dictator game; $r = .53$; Edele et al., 2013). Dictator game behavior was employed as a primary measure of prosocial behavior in the present study (see Section 3.5.3.6 below). Further, prior research has effectively used behavioral assessments of compassion with similar paradigms that gauge participant responses to images or videos of others suffering in the context of meditation training (Klimecki et al., 2012; Desbordes et al., 2012; Rosenberg et al., 2015; Weng et al., 2013). That said, the present study is the first to employ the MET or MET-CORE in an RCT testing the effects of psychological training (in teachers or non-teacher adults). In support of the validity of the MET-CORE in this study, at baseline, scores on the emotional compassion task were moderately correlated with the

empathic concern (r = .33, p = .000) and perspective taking (r = .31, p = .001) subscales of the IRI. MET-CORE emotional compassion reliabilities at pre-test and post-test were high (α = .92 and = .93), which is consistent with prior work (α = .97; Edele et al., 2013).

3.5.3.6 Prosocial Game Behavior

As with compassion, self-report measures of prosocial behavior are highly subject to social desirability bias (e.g., Fiske et al., 2007). Also, given the inherently interactive nature of prosocial behavior, it is important to measure prosocial behavior in an interpersonal context. However, a limited number of prosocial behavior measures have been used in meditation RCTs and in the present study the measures had to be administered online. One widely-used approach is to employ iterations of interactive games that pit self-interest against prosocial motivations to discern 'true' prosocial behavior (Balliet, Mulder, & Van Lange, 2011; Van Lange, Joireman, Parks, & Van Dijk, 2013). A range of prosocial economic games have been designed to this end, and hundreds of prior studies have reliably administered them online (e.g., Engel, 2011; Rand, Dreber, Ellingsen, Fudenberg, & Nowak, 2009; Rand, Greene, & Nowak, 2012).

The most extensively studied prosocial economic game is the 'dictator game' (DG; Engel, 2011). The design of the DG is simple: participants are given a specific amount of funds and they are told that they can keep it all, or give some of it to another person playing the same game who has little to no funds. No further information is disclosed about the other player to prevent intergroup biases (e.g., Bloom, 2017). The amount of funds given is the index of prosocial behavior. Although questions have been raised regarding what the DG actually measures (Bardsley, 2008; List, 2007), in many cases it appears to tap (at least in part) genuine prosocial behavior (Engel, 2011). Indeed,

it has been found to positively correlate with donation behavior (Van Lange, Bekkers, Schuyt, & Vugt, 2007) and helping behavior (Franzen & Pointner, 2013). Also, a prior study employed the DG in the context of meditation training, and although it was not responsive to change, DG behavior was positively correlated with a different prosocial behavior measure that did increase due to meditation training (Leiberg et al., 2011). Accordingly, the DG was used in this research as a prosocial measure of 'altruistic giving.' In the present design, participants were given $3.00 CAD at pre-test and at post-test, and they were faced with the decision of how much of their funds to keep for themselves versus how much to transfer to another anonymous player (the possible range was $0.00 to $3.00 CAD; see Appendix K for more information on the prosocial economic game instructions and to see images of all four economic games played).

Prosocial behavior can take many forms (e.g., Keltner et al., 2014). Thus, to make generalizations regarding the extent to which (and how) meditation may increase teacher prosocial behavior, a multidimensional assessment was important. Therefore, to assess compassionate behavior as a subset of prosocial behavior (Batson, 2011), an economic game paradigm was taken from Weng et al. (2015), who assessed compassionate responding in a meditation training RCT. That is, a third-party economic game was used in which participants were given information about one player treating another player unfairly (TPG-Unfair; operationalized as giving less than 50% of their funds to the other; Engel, 2011). Again, no identifying information was given about the other players. The decision participants faced was what amount (from $0.00 to $3.00 CAD) they wished to transfer to the *unfairly treated* player. The amount of funds given to the player who was treated unfairly (versus kept for oneself) served as the index of 'compassionate giving.' A

prior study found that two weeks of digital KM training versus an active control increased compassionate giving using the same economic game (Weng et al., 2015).

In a third type of prosocial game, also taken from Weng et al. (2015), participants were told that one player treated another player *fairly* (TPG-Fair; operationalized as giving 50% or more of one's funds to another player; Engel, 2011). Participants had the choice to distribute $0.00 to $3.00 CAD to the fairly treated individual in the game or to keep it for themselves. This measure helped to conceal the purpose of the TPG-Unfair and punishment game (see below) because it was a third-party game that did not involve any unfair behavior. Also, this served as an additional measure of 'altruistic giving,' as there was no clear or compelling motivation to give to another player who was treated fairly in the game (at a personal cost to oneself), other than prosocial motivation.

Finally, the fourth prosocial economic game administered was a 'punishment game' (PG) taken from Weng et al. (2015) (see also Fehr & Gächter, 2002). The PG is similar to the TPG-Unfair paradigm. However, in the PG participants see one player treat another player unfairly (defined as giving less than 50% of one's funds to another player), and participants can spend their funds to *punish the unfair player* for this behavior. In this study, the unfair player kept over 90% of the funds ($2.75 out of $3.00 CAD). The amount spent by participants on punishment is the same amount they were told that the unfair player would lose (the possible range was $0.00 to $2.75 CAD). Given that punishment behavior comes at a personal cost to participants, and it is ostensibly enacted to correct unfair behavior directed at an anonymous other, it is considered an index of 'altruistic punishment' (Fehr & Gächter, 2002). However, as the present study was guided by The Prosocial Classroom model of education (Jennings &

Greenberg, 2009), PG behavior was treated as an *inverse* measure of prosocial behavior. That is because even punishment behavior deployed to increase fairness in the classroom may undermine student social-emotional well-being and learning (see Frenzel, Pekrun, & Goetz, 2007; Hamre et al., 2013; Jennings & Greenberg, 2009; Lewis et al., 2005, 2008). One prior study examined the influence of meditation training on altruistic punishment, and no effects were found (Weng et al., 2015). Yet, given its impact on student learning and well-being, a PG measure was employed to assess teacher altruistic punishment.

Testing effects and instrument sensitivity are methodological issues when using prosocial game paradigms in a pre-posttest design. Regarding testing effects, people may respond differently playing the game a second time regardless of training group because they have learned 'how it works,' or they simply repeat the same behavior from pre-test. Regarding instrument sensitivity, in the only known study to use a pre-posttest actively controlled design to test meditation's influence on prosocial game behavior, a DG was not responsive to KM training (Leiberg et al., 2011). Further, the results of a meta-analysis indicate that repeat interaction DGs, for example, show less altruistic giving as the rounds continue (for reasons that are unclear; Engel, 2011). However, few studies measure prosocial behavior at all in meditation or teacher research. Among those that have, many have employed prosocial games (Leiberg et al., 2011; McCall, Steinbeis, Ricard, & Singer, 2014; Weng et al., 2013, 2015). Further, as a post-test-only measure, prosocial economic games have been found to respond to digital meditation training versus active controls, they are correlated with self-reported compassion, and also they correlate with brain activity implicated in compassionate feelings and behavior (Weng et al., 2013, 2015). For these reasons, the prosocial economic games described above were

administered as primary prosocial behavior measures in the present study.

Moreover, to address the methodological issues faced by a pre-posttest design, the present researcher (in concert with H. Weng) created multiple rounds of the games for administration based on prior studies that have used repeated-measures approaches with economic games (see Koenings & Tranel, 2007; Sanfey, Rilling, Aronson, Nystrom, & Cohen, 2003). This approach was intended to make each game less familiar and learnable than one-shot versions of the games. Specifically, based on Weng et al. (2015), four rounds of the DG, three rounds of the TPG-Fair, two rounds of the TPG-Unfair, and one round of PG were created.[11] These rounds were played in randomized order to reduce testing and order effects, with the exception that the first the three games were always: one round of the TPG-Unfair, one round of the DG, and one round of the PG. This was done to ensure that the most 'process pure' responses were collected on participants' first exposure to the primary prosocial behavior measures. Again, this was important because although repeat administrations were needed to address the noted methodological concerns, they also can lead to reduced prosocial behavior (Engel, 2011).

Prior to playing the prosocial games, and during each game, participants were told the following: 1) they were playing for real money (to increase the felt stakes and costs of their behavior); 2) they were playing asynchronously with other people (to increase the ecological validity of the task), though they actually played a computer (see Kirk et al., 2016); 3), they were playing with new players in each game and that all games were independent of each other (to prevent reciprocity and retribution; Engel, 2011); and 4)

[11] To avoid respondent fatigue and preserve measure economy, the total prosocial game battery was kept to 10 rounds. DG rounds were identical. TPG-Unfair rounds had one player giving the other player $0.25 and $0.65 (out of $3.00; -50%). TPG-Fair rounds had one player giving another player $1.50, $1.80, and $2.25 (out of $3.00; =/+50%). PG rounds had one player giving another player $0.25 (out of $3.00; -50%).

only one of the games they played at pre-test and at post-test would be selected for payment (so their focus was on each game rather than their aggregate performance).

In partial support of the validity of the prosocial games used in the present study, at baseline, the empathic concern facet of compassion (using the IRI) was negatively correlated with altruistic punishment in the PG (r = -.28, p = .002). This finding supports the validity of the prosocial games (at least the PG) as measures of prosocial behavior because prior studies have shown empathic concern (using the IRI) is positively correlated with compassionate giving and negatively correlated with altruistic punishment in similar economic games (Weng et al., 2013, 2015). From this perspective, PG behavior appears to be an inverse measure of prosocial behavior as intended. Regarding internal consistency, at pre-test and post-test, reliability across all 10 game rounds was high (α = .92 and .96). At pre-test and post-test, the reliability of the DG rounds was also high (α = .93 and .95). The reliability of the TPG-Unfair rounds was acceptable to good (α = .74 and .90). And, the reliability of the TPG-Fair rounds was good to high (α = .87 and .94). The PG was only administered once, so an internal consistency was not calculated for it. The acceptable to high reliabilities within rounds of each game type permitted aggregating DG, TPG-Fair, and TPG-Unfair rounds into individual metrics of prosocial behavior.[12] As independent, multi-round, repeated-measures prosocial games are rare, and those that have been published do not provide internal reliabilities (e.g., Koenings & Tranel, 2007; Leiberg et al., 2011; Sanfey, et al., 2003), data to contextualize the reliability of the present prosocial games is unavailable.

[12] As games that tap altruistic punishment (PGs) are typically evaluated as distinct from games that measure compassionate or altruistic giving (e.g., Weng et al., 2015), and they are often administered once per study, the single PG round also served as an individual (inverse) metric of prosocial behavior.

3.5.3.7 Donation Behavior

The prosocial economic games are high on internal validity and experimental controllability (e.g., Engel, 2011), increasing the generalizability of the measures. However, changes in altruistic giving, compassionate giving, or altruistic punishment likely index idealized prosocial behavior. Additionally, given that this study examines whether digital meditation training can increase prosocial behavior in classroom teachers, it was critical to include a measure higher on external and ecological validity. More real world-relevant prosocial behavior, where greater social context was present and where the recipients of teachers' kindness were known to be children, were important factors in such a measure. Accordingly, charitable donations to a local children's hospital were assessed as an additional measure of teacher prosocial behavior.

At post-test, after completing all other measures, participants were informed that they had played the prosocial games not with other people, but with a computer. Afterward they were told they could keep the $6.00 CAD they were given from playing the games, or they could donate any portion of this money ($0.00 to $6.00 CAD) to the British Columbia Children's Hospital. Thus, donation behavior was measured using one behavioral metric of altruistic giving to a real population of children in need. In support of this approach, donation behavior was used in two recent digital meditation RCTs with pre-posttest designs and active control groups (Ashar et al., 2016; Galante et al., 2016).

Chapter 4: Results

4.1 Overview

The results of this study are reported in five sections. First, in lieu of presenting preliminary analyses that test for non-normality and assumption violations, a bootstrapping method is implemented, and missing data is also discussed. Second, to address the first study aim, results of feasibility and acceptability analyses that indicate participants' digital training engagement, perceived efficacy, and satisfaction are reported. Third, to address the second study aim, within-group and between-group analyses are presented that assess whether the digital meditations improved affect and mindfulness. Fourth, to address the third study aim, between-group analyses are reported that test two sets of hypotheses which guided this research. The primary hypotheses held that mindfulness meditation (MM) and kindness meditation (KM) versus music relaxation (MR) would improve emotional awareness and regulation (H1), and compassion and prosocial behavior in teachers (H2). The secondary (exploratory) hypotheses held that MM would increase prosocial behavior via change in emotional awareness and regulation, and KM would increase prosocial behavior via change in compassion (a test of mediation) (H3); and that MM would increase emotional awareness and regulation compared to KM, and KM would increase compassion and prosocial behavior compared to MM (a test of differential practice effects) (H4). Finally, all analyses conducted for the third study aim were run again for the female subsample, as an indirect method of exploring the role of gender in meditation training effects (given that an underpowered male sample prevented formal tests of gender by training interactions).

4.2 Preliminary Analyses

4.2.1 Assumptions and Outliers: Standard versus Robust Methods

Traditionally, prior to reporting descriptive or inferential statistics the data would be assessed first for influential data points (outliers), and then checked to ensure that it met the core assumptions of standard linear models and parametric tests (additivity and linearity, normal distribution, homogeneity of variance, and independence; Field, 2013). However, an increasingly utilized approach is to use 'robust methods' (Hayes, 2013; Wilcox, 2012). One of the most well-studied and commonly used robust methods is 'bootstrapping' (Efron & Tibshirani, 1993; Hayes, 2013). As a robust method, bootstrapping utilizes the sampled data in a given study as the normal distribution, rather than assuming an idealized normal distribution that infrequently occurs in social science (Wright, London, & Field, 2011). With the sampled data as the model distribution, anywhere from 1000 to 5000 subsets of the sampled data are randomly selected and iteratively replaced to estimate the properties of the sampling distribution. The observed sample effectively serves as the 'population' from which smaller samples are randomly taken (subsets of the observed data are called 'bootstrap resamples') and replaced to estimate model parameters (see Figure 7 below). An advantage of this approach is that it treats all participants, even those who may have 'extreme scores,' as valid members of the sampled population (save an objective reason to consider them as distinct[13]). Bootstrapping thus does not require transforming the data (e.g., 'winsorizing') or excluding valid participants (e.g., 'trimming') to 'fit a normal distribution,' particularly when using bias-corrected and accelerated (BCa) bootstrapping to correct for skewness.

[13] No cases were deemed invalid based on participants' membership in any objectively distinct group.

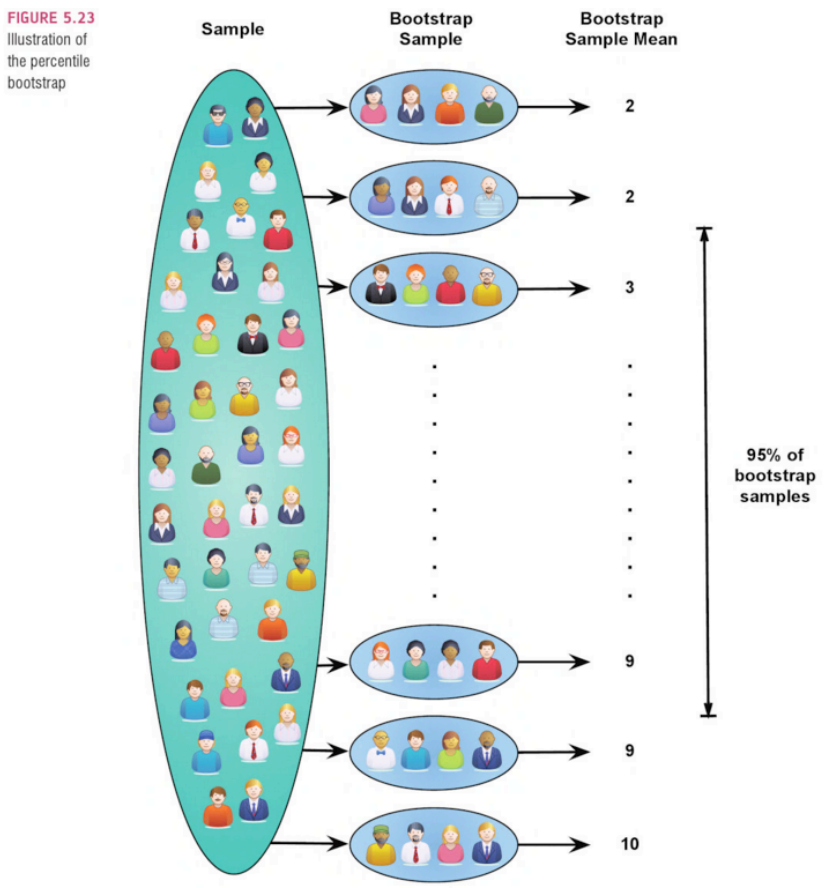

Figure 7. Conceptual illustration of the statistical procedure utilized in the robust method 'bootstrapping.' The sampled distrubtion serves effectively as the population from which subsamples are extracted, used to estimate parameters, and then are returned to the data pool (Field, 2013, p. 200).

Accordingly, as statistical science advances along with its application in social science, it is important to implement effective new analytic techniques, rather than relying on conventions for the sake of tradition (Cumming, 2014; Cumming & Calin-Jageman, 2017). With that in mind, all inferential analyses were run with parametric tests (i.e., correlations, paired samples *t*-tests, ANOVAs, ANCOVAs), but they employed the robust method of bootstrapping (Hayes, 2013). As a check, all inferential tests were conducted with standard *p*-values and BCa bootstrapped *p*-values, and no meaningful

differences in statistical significance were found between the two methods. Therefore, all findings reported in the Results present the bootstrapped *p*-values because this permitted the retention of the full study sample to maximize statistical power and generalizability.

4.2.2 Missing Data

For an online study, the participant retention rate was high (90% for the total sample).[14] A small set of participants did not complete all items at both pre-test and post-test assessment. However, partial responding was not substantial (total sample range = 8% to 11% missingness). These data were treated as missing completely at random, given that there were no marked differences in missing data between training groups (see Little, Jorgensen, Lang, & Moore, 2013). Accordingly, all analyses were run with the maximum number of participants available for each variable to maximize statistical power.

4.3 First Study Aim: Digital Training Feasibility and Acceptability

The first study aim was to evaluate the feasibility and acceptability of digital meditation trainings for K-12 classroom teachers. To this end, teachers' subjective experiences of the MM, KM, and MR trainings were surveyed at post-test. Specifically, teacher reports of training engagement, perceived efficacy, and satisfaction were collected. Data from these training assessments are presented below.

4.3.1 Training Engagement: Adherence and Practice-Induced Sleep

Based on recent studies on the prosocial effects of meditation, participants were instructed to practice at minimum once a day five days a week over the 6-week training period (see Weng et al., 2013, 2015). To support participant autonomy, whether

[14] Of the 121 participants who consented to participate in the study, 118 completed (at least) some pre-test measures and were randomly assigned to a training group. Of those 118, 109 completed (at least) some post-test measures. Thus, retention was defined by the number of participants who completed pre-test and post-test assessments along with the 6-week training period (even if they did not complete all items, which happened in some cases due to technical problems as well as a snowstorm during post-test assessment).

participants engaged in the 8min, 9min, 20min, or 23min practices was their decision. On average, across the training period, participants at post-test reported practicing MM and KM just over four times weekly, and practicing MR just over five times weekly (see Table 5 below).[15] Using this metric, training adherence was high in the MM (85.8%) and KM (88.4%) groups, and was perfect in the MR (control) group (103%).[16] ANOVAs indicated practice time differences were significant between the MM vs. MR and KM vs. MR groups, but not the MM vs. KM groups (see Table 6 below). Adherence was higher in the MR than in the MM or KM groups, but it was high across all groups. That said, average practice time varied from 1 to 5 sessions (MM), 2 to 6 sessions (KM), and 3 to 7 sessions (MR) a week (see Table 5), indicating a range of practice engagement.

Additionally, age and expectations for change have been found in prior work to predict levels of meditation engagement in naïve meditators (Delmonte, 1981, 1985). Yet, the bivariate correlations between average practice amount and age (r = .02, p = .88), and average practice amount and expectations for change in well-being (r = -.09, p = .40), self-regulation (r = .06, p = .55), and compassion (r = .08, p = .43) were not significant. Practice engagement was also gauged (inversely) by asking how frequently participants recalled falling asleep during their practices. On average, participants reported falling asleep 'very rarely' (on a 7-point scale from 'never' to 'always'; see Table 5). There were no differences between groups on practice-induced sleep (see Table

[15] Practice time also was assessed by how often participants (retrospectively) reported engaging in each of the four different practices as they became available. However, this approach proved less reliable as some participants reported difficulties in accurately responding to these questions, and notably fewer participants completed this measure, *n*s = 68 to 91. As such, 'average sessions per week' was used to index adherence.

[16] MR adherence exceeded 100% because (on average) it was practiced more often than the recommended dosage of five times per week.

Table 5

Training Engagement, Efficacy, and Satisfaction: Descriptive Statistics

	Mindfulness. *M* (*SD*), Range	Kindness. *M* (*SD*), Range	Music. *M* (*SD*), Range	Total Sample *M* (*SD*), Range
Training Engagement				
Weekly Practice.[a]	4.29 (1.09), 4.00	4.42 (1.15), 4.00	5.16 (0.99), 4.00	4.63 (1.13), 6.00
Practice Sleep.[a]	1.95 (1.82), 6.00	1.88 (1.56), 5.00	1.43 (1.59), 4.00	1.75 (1.66), 6.00
Training Efficacy				
Effectiveness.	4.57 (0.60), 2.00	4.73 (0.72), 3.00	4.59 (0.73), 2.00	4.63 (0.68), 3.00
Stress.	5.03 (0.65), 3.00	5.21 (0.82), 3.00	4.87 (0.82), 3.00	5.03 (0.77), 4.00
Patience.	5.08 (0.72), 3.00	5.27 (0.76), 3.00	4.89 (0.99), 3.00	5.08 (0.84), 3.00
Job Impact.	4.57 (1.07), 5.00	4.97 (1.13), 6.00	4.32 (1.51), 6.00	4.61 (1.27), 6.00
Training Satisfaction				
Recommend.	2.16 (1.07), 4.00	2.15 (1.06), 4.00	1.92 (1.26), 4.00	2.08 (1.13), 4.00
Enjoyment Motiv.	1.89 (0.88), 4.00	2.15 (0.91), 4.00	2.05 (1.13), 4.00	2.03 (0.98), 4.00

Note. Mindfulness. = mindfulness meditation group. Kindness. = kindness meditation group. Music. = music relaxation group. Weekly Practice. = average number of practice sessions engaged in weekly over the 6-week training period. Practice Sleep. = indicates how often on participants fell asleep during their practice on a 7-point scale. Effectiveness. = perceived improvements in teaching effectiveness due to the mental practices rated on 5-point bipolar scale. Stress. = perceived improvements in teaching stress due to the mental practices rated on 5-point bipolar scale. Patience. = perceived improvements in teaching patience due to the mental practices rated on 5-point bipolar scale. Job. Impact. = perceived impact of the mental practices on job performance compared to other professional trainings rated on 7-point bipolar scale. Recommend. = willingness to recommend the mental practices to a friend or colleague rated on 5-point scale. Enjoyment Motiv. = extent of motivation to continue engaging in the practices during the study period due to enjoyment rated on a 5-point scale. Range = the maximum score minus the minimum score reported (e.g., 4 – 1 = range of 3). All sample sizes are: n = 37 (MM), n = 33 (KM), n = 37 (MR), n = 107 (total) (with exceptions denoted below).

[a] n = 38 (MM), n = 33 (KM), n = 37 (MR), n = 108 (total).

6). This suggests that participants remained predominantly awake during their practices. Overall, given the high level of training engagement, initial support for the feasibility and acceptability of digital meditation (and music relaxation) for teachers was found.

4.3.2 Training Efficacy: Teaching Effectiveness, Stress, and Patience

To assess training efficacy, participants were first asked how they thought the following outcomes may have changed (from 'much worse' to 'much better') due to their mental practices: *teaching effectiveness*, *teaching stress*, and *teaching patience*. The means across groups (see Table 5 above) indicated that participants rated their teaching

Table 6

Effect of Training Group on Training Engagement, Efficacy, and Satisfaction: ANOVAs with Bootstrapped A Priori Contrasts

	Mindful. vs. Kind.				Mindful. vs. Music.				Kind. vs. Music.			
	SE	*t*	*p*	*d*	*SE*	*t*	*p*	*d*	*SE*	*t*	*p*	*d*
Training Engagement												
Weekly Practice. [a]	.26	-0.53	.623	--	.25	-3.52	.001**	-0.81	.26	-2.87	.009**	-0.69
Practice Sleep. [a]	.40	0.17	.878	--	.38	1.34	.201	--	.40	1.12	.260	--
Training Efficacy												
Effectiveness	.16	-0.98	.331	--	.16	-0.17	.865	--	.16	0.81	.419	--
Stress.	.18	-1.04	.298	--	.17	0.94	.354	--	.20	1.77	.082†	0.42
Patience.	.18	-1.08	.267	--	.20	0.94	.369	--	.21	1.81	.092†	0.43
Job Impact.	.26	-1.52	.134	--	.30	0.80	.426	--	.32	2.04	.052†	0.49
Training Satisfaction												
Recommend.	.27	0.04	.960	--	.26	0.92	.388	--	.27	0.86	.413	--
Enjoyment Motiv.	.21	-1.22	.213	--	.24	-0.69	.497	--	.24	0.40	.697	--

Note. Mindful. = mindfulness meditation group. Kind. = kindness meditation group. Music. = music relaxation group. Weekly Practice. = average number of practice sessions engaged in weekly over the 6-week training period. Practice Sleep. = how often on participants fell asleep during their practice. Effectiveness. = perceived improvements in teaching effectiveness due to the mental practices. Stress. = perceived improvements in teaching stress due to the mental practices. Patience. = perceived improvements in teaching patience due to the mental practices. Job Impact. = perceived impact of the mental practices on job performance compared to other professional trainings. Recommend. = willingness to recommend the mental practices to a friend or colleague. Enjoyment Motiv. = motivation to continue engaging in the practices during the study due to enjoyment. Blank spaces ("--") indicate no statistics to report. All *p*-values were generating using 1000 BCa bootstrap resamples. Effect size conventions for Cohen's d are as follows: small effect (d = 0.20), medium effect (d = 0.50), and large effect (d = 0.80) (Cohen, 1988). All sample sizes are: n = 37 (MM), n = 33 (KM), n = 37 (MR), n = 107 (total) (with exceptions denoted below).
[a] n = 38 (MM), n = 33 (KM), n = 37 (MR), n = 108 (total).
** $p < .01$. † $p < .10$.

effectiveness, stress, and patience as becoming 'a little better' due to the trainings. As a bipolar response scale was used, however, the means may not best elucidate the pattern of responses. To aid interpretability, an alternative approach is to examine these outcomes by percentage of response type given. Participants rated the effect of training on their teaching effectiveness at the following percentages: much worse (0.0%), somewhat worse (0.0%), a little worse (0.0%), neither better nor worse (47.7%), a little better (43%),

somewhat better (8.4%), and much better (0.9%).[17] The effect of training on teaching stress was rated as: much worse (0.0%), somewhat worse (0.0%), a little worse (0.9%), neither better nor worse (22.4%), a little better (52.3%), somewhat better (21.5%), and much better (2.8%). And, the effect of training on teaching patience was rated as: much worse (0.0%), somewhat worse (0.0%), a little worse (0.0%), neither better nor worse (26.2%), a little better (45.8%), somewhat better (22.4%), and much better (5.6%). Regarding training-specific effects, there were no significant differences between the MM, KM, and MR groups on any of the training efficacy factors (see Table 6 above). That said, there were trend-level[18] differences in perceived improvements in teaching stress and teaching patience in the KM vs. MR control group.

In addition to teaching effectiveness, stress, and patience, training efficacy was assessed by asking participants to rate the impact of the mental practices on their job performance relative to other professional trainings. On a scale ranging from 'much lower' to 'much higher,' participants on average rated the impact of their mental practices as 'a little higher' (see Table 5). However, again, the pattern of responses may be clearer upon examining the percentage of response type given. Participant responses indicated that relative to other programs, study trainings had a: much lower impact (2.8%), somewhat lower impact (5.6%), slightly lower impact (2.8%), neither higher nor lower impact (31.8%), slightly higher impact (36.4%), somewhat higher impact (15%), and much higher impact (5.6%). No group differences were found for perceived training impact on job performance (see Table 6). However, KM vs. MR control participants

[17] Percentages equal valid percent for the participants who completed these items (n = 107).

[18] Unless otherwise noted, defined as $p < .10$. Trend effects are noted in the Results, but are not interpreted.

reported a marginally[19] higher perceived impact of the trainings on their job performance.

Overall, 52.3%, 76.6%, and 73.8% of teachers reported that the practices made their teaching effectiveness, stress, and patience a little better to much better, respectively. The trainings improved perceived stress and patience among a large portion of teachers, and over half felt that it enhanced their teaching effectiveness. Similarly, 57% of teachers indicated that the trainings had a greater impact on their job performance than other professional trainings. Together, these results suggest the overall subjective training efficacy of the digital practices for teachers was moderate across all groups.

4.3.3 Training Satisfaction: Practice Referrals and Enjoyment Motivation

To assess training satisfaction, participants were asked about their willingness to recommend the practices to a colleague or friend (from 'not at all likely' to 'highly likely'). Across training groups, participants on average were 'a little likely' to recommend the practices to a colleague or friend (see Table 5 above). That said, participants' likelihood of recommending their practices by percentage was: not at all likely (9.3%), a little likely (22.4%), somewhat likely (29%), very likely (29.9%), and highly likely (9.3%). From this perspective, 68.2% of participants were somewhat to highly likely to suggest their practices to a colleague or friend. There were no significant between-group differences in practice referral likelihood (see Table 6 above).

Training satisfaction also was assessed by asking participants to report (retrospectively) on their motivation to continue engaging in the practices, regardless of their initial practice motivation. Specifically, participants rated how motivating their 'enjoyment of the practices' was from 'not at all motivating' to 'highly motivating.'

[19] 'Marginal' is another term for trend-level (or trending) effects, where $p < .10$ (unless otherwise noted).

Across groups, on average, participants rated practice enjoyment as 'a little motivating' (see Table 5). That said, participants reported enjoyment motivation at the following percentages: not at all motivating (6.5%), a little motivating (21.5%), somewhat motivating (39.3%), very motivating (28%), and highly motivating (4.7%). From this perspective, 72% of participants found their enjoyment of the practices somewhat to highly motivating in their continued practice engagement during the 6-week training period. There were no significant group differences in enjoyment motivation (see Table 6). Overall, across groups, teachers' level of digital training satisfaction was moderate.

4.3.4 Summary: Feasibility and Acceptability of Digital Mental Trainings

In summary, across all practices, training engagement was high, perceived training efficacy, even compared to prior teacher trainings was moderate, and teachers' training satisfaction was also moderate (see Emerson et al., 2017). Given these results, though the practices did not work for everyone, the data provide preliminary evidence in support of the feasibility and acceptability of self-administered digital meditation trainings for K-12 teachers. As there were no significant group differences on any training evaluation factors, other than for adherence (which was higher in the MR vs. MM and KM groups), the data also support the feasibility and acceptability of digital MR practices as potential tools to support teacher effectiveness and well-being.

4.4 Second Study Aim: Changes in Affect and Mindfulness

The second study aim was to examine whether the digital meditation trainings induced salutary changes in affect and mindfulness. To this end, both within-group and between-group analyses were conducted. It was possible that there would be no differences between groups because there were no significant changes in any of the

groups, or because there were similarly sized changes in all of the groups. Within-group analyses were run to distinguish the former from the latter pattern. For the same reason, within-group analyses helped to elucidate the extent to which the MR control was matched to the MM and KM groups. If matched on changes in affect over time specifically, this would increase confidence that any differences between MM and KM vs. MR were not due to differences in expectations for change, or in actual changes in affect, but were due to changes in the primary dependent variables (i.e., emotional awareness and regulation, compassion, and prosocial behavior) (see Boot et al., 2013). For within-group analyses, paired samples *t*-tests were run using BCa bootstrapped *p*-values to examine pre-test to post-test changes in affect and mindfulness. Effect sizes were indexed using Cohen's *d*. Cohen's *d* was calculated by subtracting the pre-test from the post-test mean of each affect and mindfulness variable, and then dividing by the pre-test standard deviation (Cohen, 1988), adjusting for the pre-posttest correlation of each variable (utilizing 'equation 8' in Morris & DeShon, 2002). Cohen's *d* was used because it permits the interpretation of the results in standard deviation units that can be compared across measures and studies. Effect size conventions for Cohen's *d* are as follows: small effect ($d = 0.20$), medium effect ($d = 0.50$), and large effect ($d = 0.80$) (Cohen, 1988). As the hypotheses these analyses were employed to test were rendered a priori (based on the literature), the threshold used for statistical significance was $p < .05$ (Field, 2013).

For between-group analyses, ANOVAs were run with a priori contrasts using BCa bootstrapped *p*-values to test the effect of training group on affect and mindfulness.[20] A

[20] As emotional awareness was higher in the MM vs. KM and MR vs. KM groups at pre-test, between-group analyses were run using ANCOVA as well, with emotion awareness as a covariate. The significance of the results did not change in these ANCOVA analyses, and thus only the ANOVA results are reported.

priori contrasts were used to test the practice-specific predictions regarding the influence of MM vs. MR and KM vs. MR on affect and mindfulness (i.e., MM and KM would improve affect and mindfulness vs. MR). All dependent variables entered into the ANOVAs were in the form of difference scores, yielded by subtracting pre-test from post-test scores. Difference scores were used because they permitted an unbiased estimate of change (independent of baseline values), as well as an intuitive understanding of the direction of change from pre-test to post-test due to training effects (Zumbo, 1999).[21] Positive difference scores signified an increase, negative difference scores signified a decrease, and close-to-zero difference scores signified no change in a given variable. For between-group effects, effect sizes were also generated using Cohen's *d*. In this case, Cohen's *d* was calculated utilizing 'equation 14' in Rosnow, Rosenthal, and Rubin (2000), and when group sample sizes were unequal, 'equation 11' was used to calculate the harmonic sample mean to adjust for the difference. All between-group analyses on affect and mindfulness used $p < .05$ as the statistical significance threshold (Field, 2013).

4.4.1 Effect of Time on Affect and Mindfulness

Affect was measured using three subscales. A negative affect and positive affect subscale were administered, along with a 3-item stress subscale. Across all groups, results from a paired samples *t*-test indicated that negative affect significantly decreased over time (see Table 7 for descriptive and Table 8 for inferential statistics below). Stress also declined from pre-test to post-test across groups. Positive affect significantly increased over time across groups as well. Based on convention, the changes in negative

[21] Although statistically comparable, repeated measures ANOVA was not employed because when significant differences are found utilizing this approach the direction of change is unclear, and thus the results cannot be used to test whether the expected pattern of effects has emerged (Tabachnick & Fidell, 2001; for a recent meditation-based RCT that used the same approach see Schonert-Reichl et al., 2015).

Table 7

Affect and Mindfulness: Descriptive Statistics

	Mindfulness. *M* (*SD*), Range	Kindness. *M* (*SD*), Range	Music. *M* (*SD*), Range	Total Sample *M* (*SD*), Range
Negative Affect				
Pre-Test	1.49 (0.62)	1.46 (0.53)	1.42 (0.51)	1.45 (0.55)
Post-Test	1.09 (0.51)	1.03 (0.56)	1.05 (0.57)	1.06 (0.54)
Difference	-0.43 (0.53), 2.43	-0.43 (0.52), 2.30	-0.35 (0.39), 1.90	-0.40 (0.48), 2.58
Stress				
Pre-Test	2.90 (0.91)	2.94 (0.96)	2.80 (0.83)	2.88 (0.89)
Post-Test	2.55 (0.96)	2.36 (0.94)	2.30 (0.87)	2.40 (0.92)
Difference	-0.39 (0.73), 3.00	-0.57 (1.16), 4.33	-0.49 (0.71), 3.33	-0.48 (0.88), 4.33
Positive Affect				
Pre-Test	2.55 (0.66)	2.43 (0.81)	2.55 (0.69)	2.51 (0.72)
Post-Test	2.78 (0.81)	2.68 (0.77)	2.53 (0.67)	2.66 (0.75)
Difference	0.26 (0.63), 2.40	0.22 (0.71), 3.50	-0.01 (0.66), 3.60	0.15 (0.67), 3.70
Mindful. Aw.				
Pre-Test	3.21 (0.82)	3.03 (0.96)	3.30 (0.62)	3.18 (0.81)
Post-Test	3.37 (0.71)	3.31 (0.92)	3.46 (0.67)	3.38 (0.77)
Difference	0.22 (0.54), 2.40	0.19 (0.66), 3.00	0.12 (0.71), 3.40	0.18 (0.64), 3.60
Mindful. NR.				
Pre-Test	2.68 (0.90)	2.89 (1.01)	2.86 (0.84)	2.81 (0.91)
Post-Test	2.89 (0.96)	3.08 (1.00)	3.20 (0.75)	3.05 (0.91)
Difference	0.24 (0.85), 3.60	0.20 (0.91), 3.80	0.29 (0.93), 4.00	0.24 (0.89), 4.60
Mindful. NJ.				
Pre-Test	3.25 (1.18)	3.12 (0.90)	3.20 (0.80)	3.19 (0.96)
Post-Test	3.40 (1.11)	3.45 (1.09)	3.60 (1.02)	3.49 (1.07)
Difference	0.24 (0.81), 4.00	0.28 (1.02), 4.60	0.38 (0.97), 4.60	0.30 (0.93), 5.00
Mindful. Ob.				
Pre-Test	4.00 (0.95)	3.71 (1.19)	3.92 (0.94)	3.88 (1.03)
Post-Test	4.20 (0.91)	3.76 (1.19)	3.91 (0.92)	3.97 (1.02)
Difference	0.24 (0.53), 2.25	0.11 (0.71), 2.75	0.03 (0.73), 3.25	0.13 (0.66), 3.25

Note. Mindfulness. = mindfulness meditation group. Kindness. = kindness meditation group. Music. = music relaxation group. Mindful. Aw. = mindfulness-awareness subscale. Mindful. NR. = mindfulness-non-reactivity subscale. Mindful. NJ. = mindfulness-non-judgment subscale. Mindful. Ob. = mindfulness-observing subscale. Difference = difference score (simple difference between pre-test and post-test score). Difference scores may diverge from simply subtracting the pre-test from the post-test scores in the table because difference scores reflect changes in pre-test and post-test scores for participants who provided data at both time points. Range = the maximum score minus the minimum score reported (e.g., 4 – 1 = range of 3). All pre-test sample sizes are: n = 39 (MM), n = 38 (KM), n = 41 (MR), n = 118 (total). All post-test and difference score sample sizes are: n = 37 (MM), n = 34 (KM), n = 37 (MR), n = 108 (total).

Table 8

Effect of Time on Affect and Mindfulness: Paired Samples Bootstrapped T-Tests

	Mindfulness.			Kindness.			Music.			Total Sample		
	t	*p*	*d*	*t*	*p*	*d*	*t*	*p*	*d*	*t*	*p*	*d*
Negative Aff.	-4.88	.001**	-0.81	-4.82	.002**	-0.83	-5.41	.001**	-0.89	-8.66	.001**	-0.84
Stress	-3.22	.006**	-0.54	-2.86	.007**	-0.49	-4.14	.001**	-0.68	-5.66	.001**	-0.55
Positive Aff.	2.55	.023*	0.42	1.76	.088†	0.30	-0.10	.922	--	2.38	.015*	0.24
Mind. Aw.	2.50	.021*	0.42	1.66	.106	--	1.00	.321	--	2.87	.009**	0.27
Mindful. NR.	1.71	.082†	0.28	1.28	.220	--	1.92	.060†	0.32	2.86	.007**	0.28
Mindful. NJ.	1.79	.089†	0.30	1.62	.116	--	2.35	.033*	0.39	3.35	.002**	0.33
Mindful. Ob.	2.75	.016*	0.45	0.89	.382	--	0.22	.795	--	1.97	.035*	0.18

Note. Mindfulness. = mindfulness meditation group. Kindness. = kindness meditation group. Music. = music relaxation group. Negative Aff. = negative affect. Positive Aff. = positive affect. Mindful. Aw. = mindfulness-awareness subscale. Mindful. NR. = mindfulness-non-reactivity subscale. Mindful. NJ. = mindfulness-non-judgment subscale. Mindful. Ob. = mindfulness-observing subscale. Blank spaces ("--") indicate no statistics to report. All *p*-values were generating using 1000 BCa bootstrap resamples. Sample size for all analyses: $n = 37$ (MM), $n = 34$ (KM), $n = 37$ (MR), $n = 108$ (total sample).
** $p < .01$. * $p < .05$. † $p < .10$.

affect over time across groups were large, the changes in stress were medium, and the changes in positive affect were small.

To test if these pre-posttest changes occurred within each training group alone, paired samples *t*-tests were run for each group (see Table 8). For MM participants, negative affect and stress significantly decreased. Positive affect significantly increased over time. For KM participants, negative affect and stress significantly declined over time. Positive affect trended toward an increase. For MR participants, negative affect and stress also decreased over time. Positive affect did not change. Notably, the effect sizes of MM, KM, and MR on negative affect were large, and bigger than the mean effect sizes reported in recent meditation and music meta-analyses (Emerson et al., 2017; Galante et al., 2014; Pelletier, 2004; Sedlmeier, et al., 2012). The salutary effects of all trainings on

stress were approximately medium, though they were slightly bigger for MR. And, the significant increase in positive affect in the MM group was small to medium.

Mindfulness was assessed using four factors from a multifaceted scale: acting with awareness, non-reacting, non-judging, and observing. The results indicated that all facets of mindfulness significantly increased from pre-test to post-test across all groups. These effects were small, and were smaller than the medium effect sizes reported in prior studies on MM and KM training (Eberth & Sedlmeier, 2012; Galante et al., 2014).

To test if these pre-posttest changes occurred within each training group, paired samples *t*-tests were run for each group (see Table 8). In the MM group, acting with awareness and observing significantly increased over time, whereas non-reacting and non-judging only marginally increased. The two significant effects were small to medium in size. In the KM group, there were no significant changes in any of the four facets of mindfulness. In the MR group, there were no changes in acting with awareness or observing, and non-reacting only marginally increased. However, non-judging significantly increased over time in the MR group. This effect was conventionally small.

4.4.2 Effect of Training Group on Affect and Mindfulness

Between-group ANOVAs using a priori contrasts indicated that changes in negative affect, stress, and positive affect did not vary significantly between any training group (see Table 7 above for descriptive statistics and Table 9 below for inferential statistics), though the MM group showed marginally higher positive affect compared to the MR control. For negative affect and stress, this may be explained by the presence of large improvements in all groups over time. For positive affect, this may be explained by a general absence of significant or sizeable changes within training groups over time.

Table 9

Effect of Training on Affect and Mindfulness: ANOVAs with Bootstrapped A Priori Contrasts

	Mindful. vs. Kind.				Mindful. vs. Music.				Kind. vs. Music.			
	SE	*t*	*p*	*d*	*SE*	*t*	*p*	*d*	*SE*	*t*	*p*	*d*
Negative Aff.	.12	0.22	.841	--	.12	-0.43	.686	--	.12	-0.64	.518	--
Stress	.23	0.78	.443	--	.17	0.59	.558	--	.23	-0.36	.699	--
Positive Aff.	.16	0.31	.759	--	.16	1.77	.066†	0.41	.16	1.42	.166	--
Mindful. Aw.	.15	0.22	.814	--	.15	0.71	.507	--	.15	0.47	.647	--
Mindful. NR.	.21	0.18	.854	--	.21	-0.26	.784	--	.21	-0.43	.684	--
Mindful. NJ.	.22	-0.20	.834	--	.22	-0.64	.516	--	.22	-0.42	.707	--
Mindful. Ob.	.16	0.84	.355	--	.15	1.39	.163	--	.16	0.51	.613	--

Note. Mindful. = mindfulness meditation group. Kind. = kindness meditation group. Music. = music relaxation group. Mindful. Aw. = mindfulness-awareness subscale. Mindful. NR. = mindfulness-non-reactivity subscale. Mindful. NJ. = mindfulness-non-judgment subscale. Mindful. Ob. = mindfulness-observing subscale. Results report simple ANOVA analyses with a priori contrasts using difference scores as the dependent variables. All *p*-values were generating using 1000 BCa bootstrap resamples. Blank spaces ("--") indicate no statistics to report. Sample size for all analyses: $n = 37$ (MM), $n = 34$ (KM), $n = 37$ (MR), $n = 108$ (total sample).

† $p < .10$.

Regarding between-group differences on the four facets of mindfulness, the ANOVA results also were not significant for any group comparisons (see Table 7 for descriptive statistics and Table 9 for inferential statistics above). These results are overall consistent with the within-group analyses that indicated mindfulness was largely unchanged from pre-test to post-test in each group (with the exception of MM).

4.4.3 Summary: Changes in Affect and Mindfulness

Consistent with the second study aim, each training group attenuated negative affect and stress from pre-test to post-test. That is, all trainings reduced negative affect, showing large effects with MR producing the biggest effects. For stress, the effects were moderate for MM and KM, but were moderate to large for MR. Counter to the second study aim, MM and KM did not improve negative affect and stress vs. the MR active control, although this may be due to the sizeable salutary changes in negative affect and

stress in all groups. Additionally, MM may have increased positive affect as well as two facets of mindfulness (acting with awareness and observing) from pre-test to post-test, whereas KM and MR largely did not increase positive affect or mindfulness over time. Neither MM nor KM improved positive affect or mindfulness compared to the MR active control. As such, in terms of negative affect and stress reduction, the results suggest that the digital MM and KM trainings were effective in teachers, as well as that the MR group served as a well-matched active control. MM also may have exerted a small to moderate enhancing effect on teacher positive affect and mindfulness. Yet, no significant differential effects on affect and mindfulness emerged for any training group comparison.

4.5 Third Study Aim: Changes in Emotion Skills, Compassion, and Prosocial Behavior

The third study aim was to test two hypothesis sets. The primary hypothesis set was that MM vs. MR and KM vs. MR would enhance emotion awareness and regulation (H1), as well as compassion and prosocial behavior (H2). The secondary hypothesis set was that MM would increase prosocial behavior via improved emotional awareness and regulation, and KM would increase prosocial behavior via greater compassion (H3); and MM would increase emotional awareness and regulation more than KM, and KM would increase compassion and prosocial behavior more than MM (H4). To test both sets of hypotheses, ANOVAs using a priori contrasts with BCa bootstrapped *p*-vales were conducted. A priori contrasts vs. omnibus effects tests were utilized because of the training-specific comparisons articulated in the hypotheses.[22] Difference scores (i.e., change from pre-test to post-test) for emotion skills, compassion, and prosocial behavior were entered into ANOVAs as the dependent variables. Difference scores were used

[22] ANCOVAs were also run with emotional awareness as a covariate, as it was higher in the MM vs. KM and MR vs. KM groups at pre-test. The ANCOVAs yielded negligible differences in levels of statistical significance and effect sizes. Thus, ANOVAs with contrasts are reported to maximize statistical power.

because they provided results indicating the specific direction of change in group differences, which permitted a clear interpretation of whether the results supported the hypotheses (see Schonert-Reichl et al., 2015; Tabachnick & Fidell, 2001; Zumbo, 1999).[23] The threshold for statistical significance was $p < .05$ given the theoretically and empirically grounded a priori hypotheses these analyses tested (Field, 2013).

4.5.1 Effect of Training Group on Expected and Perceived Changes

Expectations for change (at pre-test) and perceptions of change (at post-test) in well-being, self-regulation, and compassion were measured to assess if all training groups were matched on the primary study expectancies and demand characteristics (MacCoon et al., 2012). If matched, this would increase the strength of causal inferences regarding the differential effects of MM and KM training vs. the MR active control (Boot et al., 2013; Davidson & Kaszniak, 2015), as well as MM vs. KM training against one another. As such, before testing the two hypothesis sets, two omnibus ANOVAs were run to examine whether there were between-group differences in expected (at pre-test) or perceived (at post-test) changes in well-being, self-regulation, and compassion. Omnibus ANOVA analyses indicated that expectations for changes in well-being, $F(2, 114) = 0.34$, $p = .715$, self-regulation, $F(2, 114) = 0.27$, $p = .765$, and compassion, $F(2, 115) = 1.29$, $p = .278$, did not vary by group at pre-test. Likewise, omnibus ANOVA analyses indicated that perceived changes in well-being, $F(2, 105) = 0.10$, $p = .905$, self-regulation, $F(2, 105) = 1.42$, $p = .248$, and compassion, $F(2, 105) = 0.11$, $p = .893$, were not different between groups at post-test. Accordingly, the data suggest that at pre-test and post-test all groups were largely matched on expectations and perceived changes in

[23] See footnote 15 above.

the primary dependent variables (i.e., emotion skills, compassion, and prosocial behavior). This helps to rule out placebo effects and demand characteristics that often limit causal inferences in RCTs of meditation training (Davidson & Kaszniak, 2015).

4.5.2 Effect of Training on Emotional Awareness and Regulation

A primary hypothesis of this research was that MM and KM would increase emotional awareness and regulation vs. the MR control (H1). A secondary hypothesis was that MM would increase emotional awareness and regulation vs. KM (H4). As such, a series of ANOVAs with a priori contrasts were run for self-reported emotional awareness and emotion regulation. However, changes in self-reported emotional awareness and regulation did not vary significantly between training groups (see Table 10 for descriptive statistics and Table 11 for inferential statistics below).

Task-based emotional awareness and emotion regulation were measured (inversely) via the presence of negative emotional bias on an ecologically valid judgment task. Specifically, after undergoing a negative emotion induction teachers were given a real grade 6 student essay and were asked to evaluate it on seven different criteria: overall quality, student ability, creativity, spelling/punctuation accuracy, composition organization, language diversity, and persuasiveness (see Brackett et al., 2013). It was predicted that participants in the MM and KM groups would show comparatively *less* unfavorable evaluations of the student essay (i.e., less emotional bias) than the MR control (H1), and that the MM group would show less bias than the KM group (H4).

State affect was measured just prior to the negative emotion induction and grading task to test for baseline differences in state affect. ANOVAs indicated that neither state positive affect, $F(2, 114) = 1.81$, $p = .168$, nor state negative affect, $F(2, 114) = 1.03$, $p =$

Table 10

Self-Reported Emotional Awareness and Regulation: Descriptive Statistics

	Mindfulness. *M* (*SD*), Range	Kindness. *M* (*SD*), Range	Music. *M* (*SD*), Range	Total Sample *M* (*SD*), Range
Emotion Aware.				
Pre-Test	4.44 (0.58)	4.06 (0.72)	4.37 (0.56)	4.29 (0.64)
Post-Test	4.51 (0.58)	4.16 (0.78)	4.36 (0.53)	4.34 (0.64)
Difference	0.07 (0.43), 1.75	0.04 (0.55), 2.08	0.04 (0.39), 1.67	0.05 (0.46), 2.08
Emotion Reg.				
Pre-Test	3.85 (0.73)	3.68 (0.86)	3.85 (0.54)	3.79 (0.71)
Post-Test	4.02 (0.71)	3.78 (0.75)	3.93 (0.67)	3.91 (0.71)
Difference	0.18 (0.48), 1.92	0.07 (0.51), 2.00	0.08 (0.48), 1.86	0.11 (0.49), 2.08

Note. Mindfulness. = mindfulness meditation group. Kindness. = kindness meditation group. Music. = music relaxation group. Emotion Aware. = emotional awareness. Emotion Reg. = emotion regulation. Difference = difference score (simple difference between pre-test and post-test score). Difference scores may diverge from simply subtracting the pre-test from the post-test scores in the table because difference scores reflect changes in pre-test and post-test scores for participants who provided data at both time points. Range = the maximum score minus the minimum score reported (e.g., 4 – 1 = range of 3). All pre-test sample sizes are: n = 39 (MM), n = 38 (KM), n = 41 (MR), n = 118 (total). All post-test and difference score sample sizes are: n = 37 (MM), n = 34 (KM), n = 37 (MR), n = 108 (total).

Table 11

Effect of Training on Self-Reported Emotional Awareness and Regulation: ANOVAs with Bootstrapped A Priori Contrasts

	Mindful vs. Kind.			Mindful. vs. Music.			Kind. vs. Music.		
	SE	*t*	*p*	*SE*	*t*	*p*	*SE*	*t*	*p*
Emotion Aware.	.11	0.31	.790	.11	0.27	.771	.11	-0.05	.952
Emotion Reg.	.12	0.99	.348	.11	0.92	.358	.12	-0.09	.934

Note. Mindful. = mindfulness meditation group. Kind. = kindness meditation group. Music. = music relaxation group. Emotion Aware. = emotional awareness. Emotion Reg. = emotion regulation. Results report simple ANOVA analyses with a priori contrasts using difference scores as the dependent variables. All p-values were generating using 1000 BCa bootstrap resamples. Sample size for all analyses: n = 37 (MM), n = 34 (KM), n = 37 (MR), n = 108 (total sample).

.360, were different between groups at pre-test. Likewise, neither state positive affect, $F(2, 104) = 0.07$, $p = .935$, nor state negative affect, $F(2, 105) = 0.94$, $p = .394$, were different between groups at post-test. Therefore, all groups were affectively equivalent

before completing the emotion induction and grading task at pre-test and post-test. [24]

As there were no group differences in state affect, seven separate ANCOVAs were run with mean essay grades on each of the seven grading criteria individually (at post-test) as dependent variables, and mean essay grades on each of the seven criteria (at pre-test) as covariates (to control for baseline task behavior). There were no significant between-group differences on any of the seven grading criteria for the post-test student essay (see Table 12 for descriptive and Table 13 for inferential statistics below).

To further assess emotional awareness, at post-test only (to avoiding revealing the task purpose), participants responded to a two-item awareness probe. They were asked: 1) if they thought their emotions may have affected their essay grading, and 2) if they did anything to manage their emotions to reduce emotional influence on essay grading. The training groups did not differ significantly on the two awareness probes, $F(2, 103) = 0.31$, $p = .733$ (question 1), and $F(2, 103) = 0.11$, $p = .899$ (question 2). Prior research dichotomized these awareness probes (Brackett et al., 2013). Accordingly, responses were dichotomized into either 'unaware of bias' or 'aware of bias' (question 1), and 'didn't attempt to manage emotions' or 'did attempt to manage emotions' (question 2). Chi-square analyses showed no group differences for emotion awareness, $\chi^2 (2) = 2.59$, $p = .274$, or for emotion regulation, $\chi^2 (2) = 1.33$, $p = .515$. That said, overall awareness of and attempts to regulate emotional bias were slightly higher in the MM (75.7% and 64.9%) and KM groups (69.7% and 63.6%) than in the MR group (58.3% and 52.8%), respectively. MM and KM participants reported greater levels of emotional awareness

[24] State affect was not measured again directly after the negative emotion induction at pre-test or post-test. Not including a manipulation check was intentional because self-reporting one's affective state after an emotion induction may reduce or eliminate the impact of 'incidental emotion' on judgment (e.g., see McFarland et al., 2003; Schwarz & Clore, 1983, 2003). Also, the same negative emotion induction videos were found to be effective in a sample of educators in prior research (Floman, Brackett, & Stern, 2016).

Table 12

Student Essay Grades (Task-Based Emotional Awareness and Regulation): Descriptive Statistics

	Mindfulness. *M* (*SD*)	Kindness. *M* (*SD*)	Music. *M* (*SD*)	Total Sample *M* (*SD*)
Overall Quality				
Pre-Test	3.65 (0.75)[a]	3.87 (1.00)[a]	3.76 (0.93)[a]	3.76 (0.90)[b]
Post-Test	2.95 (0.87)[c]	3.15 (1.06)[d]	3.08 (0.97)[e]	3.06 (0.96)[f]
Student Ability				
Pre-Test	3.19 (0.81)[a]	3.16 (0.83)[a]	3.19 (1.13)[a]	3.18 (0.93)[b]
Post-Test	2.74 (0.86)[c]	2.88 (1.08)[d]	2.92 (0.91)[e]	2.84 (0.93)[f]
Creativity				
Pre-Test	2.84 (0.80)[a]	3.16 (0.69)[a]	2.95 (0.78)[a]	2.98 (0.76)[b]
Post-Test	2.62 (0.79)[g]	2.67 (0.85)[d]	2.64 (0.83)[e]	2.64 (0.82)[h]
Accuracy				
Pre-Test	2.51 (0.69)[a]	2.68 (0.75)[a]	2.51 (0.69)[a]	2.57 (0.71)[b]
Post-Test	2.49 (0.84)[g]	2.76 (0.79)[d]	2.61 (0.65)[e]	2.61 (0.76)[h]
Organization				
Pre-Test	3.24 (0.80)[a]	3.65 (0.63)[a]	3.41 (0.69)[a]	3.43 (0.72)[b]
Post-Test	3.08 (0.64)[g]	3.24 (0.97)[d]	3.31 (0.79)[e]	3.21 (0.80)[h]
Language				
Pre-Test	2.78 (0.72)[e]	2.92 (0.76)[a]	2.97 (0.69)[a]	2.89 (0.72)[i]
Post-Test	2.39 (0.69)[e]	2.42 (0.90)[d]	2.42 (0.81)[e]	2.41 (0.79)[j]
Persuasiveness				
Pre-Test	3.30 (0.66)[e]	3.58 (0.65)[d]	3.39 (0.69)[e]	3.42 (0.67)[j]
Post-Test	2.94 (0.72)[a]	3.21 (0.99)[e]	3.06 (0.79)[e]	3.04 (0.83)[k]

Note. Mindfulness. = mindfulness meditation group. Kindness. = kindness meditation group. Music. = music relaxation group. Accuracy = spelling and punctuation accuracy. Organization = composition organization. Language = diversity of language. All values represent essay grades of two separate student essays evaluated at pre-test and post-test on each of the 7 grading criteria on the emotional awareness and regulation task. Overall Quality and Student Ability were rated on 6-point scales; all other criteria were rated on 5-point scales.

[a] $n = 37$. [b] $n = 111$. [c] $n = 38$. [d] $n = 33$. [e] $n = 36$. [f] $n = 107$. [g] $n = 37$. [h] $n = 106$. [i] $n = 110$. [j] $n = 105$. [k] $n = 109$.

and regulation, and yet evidenced no differences in their emotion skills compared to MR participants, revealing a discrepancy between participant perceptions and behavior.

Overall, the hypotheses that MM and KM would increase emotional awareness and emotion regulation vs. the MR control (H1), and that MM would increase emotional awareness and regulation compared to KM (H4) were not supported by the results.

Table 13

Effect of Training on Post-Test Student Essay Grades, Controlling for Pre-Test Student Essay Grades (Task-Based Emotion Awareness and Regulation): ANCOVAs with LSD Post-Hoc Tests

	Mindful. vs. Kind.			Mindful. vs. Music.			Kind. vs. Music.		
	SE	*t*	*p*	*SE*	*t*	*p*	*SE*	*t*	*p*
Overall Quality[a]	.20	0.42	.686	.25	-0.54	.573	.25	-0.11	.934
Student Ability[a]	.24	0.48	.660	.21	0.99	.285	.25	0.49	.630
Creativity[b]	.19	-0.51	.616	.19	-0.45	.647	.20	0.06	.951
Accuracy[b]	.20	1.27	.253	.19	0.91	.358	.18	0.37	.711
Organization[b]	.20	-0.46	.676	.16	0.67	.455	.21	-1.10	.350
Language[c]	.19	-0.14	.910	.17	0.04	.964	.20	0.17	.859
Persuasiveness[c]	.21	0.42	.690	.18	0.25	.765	.22	-0.17	.883

Note. Mindful. = mindfulness meditation group. Kind. = kindness meditation group. Music. = music relaxation group. Accuracy = spelling and punctuation accuracy. Organization = composition organization. Language = diversity of language. Results report ANCOVA analyses including each pre-test grading criterion as a control variable with each post-test grading criterion as the dependent variable (e.g., overall quality at pre-test as the control variable, and overall quality at post-test as the dependent variable). All *p*-values were generating using 1000 BCa bootstrap resamples.

[a] n = 38 (MM), 33 (KM), 36 (MR), 107 (total sample). [b] n = 37 (MM), 33 (KM), 36 (MR), 106 (total sample). [c] n = 36 (MM), 33 (KM), 36 (MR), 105 (total sample).

4.5.3 Effect of Training on Compassion

A primary hypothesis of this research was that MM and KM would increase compassion vs. MR (H1). A secondary hypothesis was that KM would increase compassion vs. MM (H4). As such, in this section, between-group differences in self-reported and task-based compassion were tested using ANOVAs with a priori contrasts. Self-reported compassion was measured using a scale with three facets: empathic concern (emotional compassion), perspective taking (cognitive compassion), and personal distress (emotionally dysregulated responses to compassion-eliciting scenarios) (Davis, 1983). No significant differences between training groups were found for the three compassion subscales, although MM vs. KM marginally increased perspective taking (see Table 14 for descriptive statistics and Table 15 for inferential statistics below).

Table 14

Compassion: Descriptive Statistics

	Mindfulness. *M* (*SD*), Range	Kindness. *M* (*SD*), Range	Music. *M* (*SD*), Range	Total Sample *M* (*SD*), Range
Compassion EC.				
Pre-Test	4.24 (0.57)	4.09 (0.46)	4.09 (0.62)	4.14 (0.55)
Post-Test	4.19 (0.48)	4.13 (0.55)	3.98 (0.55)	4.10 (0.53)
Difference	-0.02 (0.40), 1.71	0.01 (0.50), 2.00	-0.10 (0.42), 1.71	-0.04 (0.44), 2.00
Compassion PT.				
Pre-Test	3.62 (0.64)	3.64 (0.68)	3.65 (0.67)	3.64 (0.66)
Post-Test	3.66 (0.73)	3.60 (0.60)	3.58 (0.57)	3.61 (0.63)
Difference	0.10 (0.42), 1.57	-0.09 (0.51), 2.14	-0.04 (0.52), 2.00	-0.01 (0.49), 2.14
Compassion PD.				
Pre-Test	2.59 (0.70)	2.60 (0.73)	2.36 (0.60)	2.51 (0.68)
Post-Test	2.60 (0.73)	2.58 (0.81)	2.17 (0.65)	2.44 (0.75)
Difference	-0.01 (0.40), 1.86	-0.01 (0.61), 2.71	-0.12 (0.55), 2.57	-0.05 (0.52), 3.14
Compassion Task				
Pre-Test	2.63 (0.81)	2.59 (0.82)	2.51 (0.55)	2.58 (0.73)
Post-Test	2.65 (0.65)	2.75 (0.82)	2.43 (0.79)	2.61 (0.76)
Difference	0.09 (0.63), 3.27	0.11 (0.50), 2.14	-0.06 (0.62), 2.95	0.04 (0.59), 3.35

Note. Mindfulness. = mindfulness meditation group. Kindness. = kindness meditation group. Music. = music relaxation group. Difference = difference score (simple difference between pre-test and post-test score). Compassion EC. = self-reported compassion-empathic concern subscale. Compassion PT. = self-reported compassion-perspective taking subscale. Compassion PD. = self-reported compassion-personal distress subscale. Difference scores may diverge from simply subtracting the pre-test from the post-test scores in the table because difference scores reflect changes in pre-test and post-test scores for participants who provided data at both time points. Range = the maximum score minus the minimum score reported (e.g., 4 – 1 = range of 3). All pre-test sample sizes are: n = 39 (MM), n = 38 (KM), n = 41 (MR), n = 118 (total). All post-test and difference score sample sizes are: n = 37 (MM), n = 34 (KM), n = 37 (MR), n = 108 (total).

Table 15

Effect of Training on Compassion: ANOVAs with Bootstrapped A Priori Contrasts

	Mindful. vs. Kind.				Mindful. vs. Music.				Kind. vs. Music.			
	SE	*t*	*p*	*d*	*SE*	*t*	*p*	*d*	*SE*	*t*	*p*	*d*
Compassion EC.	.11	-0.27	.776	--	.10	0.79	.364	--	.11	1.05	.324	--
Compassion PT.	.12	1.59	.097†	0.38	.11	1.20	.201	--	.12	-0.41	.676	--
Compassion PD.	.13	-0.05	.959	--	.12	0.89	.297	--	.13	0.92	.409	--
Compassion Task	.14	-0.21	.809	--	.14	1.06	.324	--	.14	1.25	.180	--

Note. Mindful. = mindfulness meditation group. Kind. = kindness meditation group. Music. = music relaxation group. Compassion EC. = self-reported compassion-empathic concern subscale. Compassion PT. = self-reported compassion-perspective taking subscale. Compassion PD. = self-reported compassion-

personal distress subscale. Compassion Task = the MET-CORE affective image response task. Results report ANOVA analyses with a priori contrasts using difference scores as the dependent variables. All *p*-values were generating using 1000 BCa bootstrap resamples. Group sample sizes for all analyses: $n = 37$ (MM), $n = 34$ (KM), $n = 37$ (MR), $n = 108$ (total sample).
† $p < .10$.

Task-based compassion was indexed by participants' responses to a series of affectively charged photos of human suffering (Edele et al., 2013). Using this measure, counter to prediction, the results indicated that there were no differences in compassion between the MM, KM, and MR groups (see Table 14 for descriptive statistics and Table 15 for inferential statistics above). Consequently, the results do not support the predictions that MM and KM would increase compassion vs. the MR active control (H2), or that KM training would increase compassion more than MM training (H4).

4.5.4 Effect of Training on Prosocial Behavior

A primary hypothesis of this research was that MM and KM would enhance prosocial behavior vs. MR (H1). A secondary hypothesis was the KM would enhance prosocial behavior vs. MM (H4). Prosocial behavior was assessed using five metrics: 1) dictator game behavior (DG; i.e., altruistic giving); 2) third-party game behavior in unfair trials (TPG-Unfair; i.e., compassionate giving); 3) third-party game behavior in fair trials (TPG-Fair; i.e., altruistic giving); 4) punishment game behavior (PG; i.e., altruistic punishment); and 5) donation behavior (donating to a children's hospital) (see prosocial behavior descriptives in Table 16 below). The assessments indexed prosocial behavior via a distribution of resources to unknown others at a personal cost to the self. Difference scores on the DG, TPG-unfair, TPG-fair, and PG served as the dependent variables in a series of ANOVAs with a priori contrasts. Donation behavior was only assessed at post-test, so difference scores could not be computed for it. Also, as noted below, a ceiling

Table 16

Prosocial Behavior: Descriptive Statistics

	Mindfulness. *M* (*SD*), Range	Kindness. *M* (*SD*), Range	Music. *M* (*SD*), Range	Total Sample *M* (*SD*), Range
Altruistic Giving (Dictator Game)				
Pre-Test	$1.18 (0.64)	$1.10 (0.61)	$1.29 (0.62)	$1.19 (0.62)
Post-Test	$1.33 (0.68)	$1.21 (0.63)	$1.20 (0.68)	$1.25 (0.66)
Difference	$0.15 (0.73), 3.85	$0.11 (0.47), 2.75	-$0.11 (0.62), 3.06	$0.05 (0.63), 4.54
Compass. Giving (TPG-Unfair)				
Pre-Test	$1.13 (0.59)	$1.07 (0.64)	$1.18 (0.66)	$1.13 (0.63)
Post-Test	$1.26 (0.64)	$1.08 (0.67)	$1.11 (0.67)	$1.15 (0.66)
Difference	$0.12 (0.56), 3.00	-$0.03 (0.63), 2.88	-$0.09 (0.64), 3.13	$0.00 (0.61), 4.13
Altruistic Giving (TPG-Fair)				
Pre-Test	$0.98 (0.63)	$0.90 (0.58)	$1.01 (0.70)	$0.96 (0.64)
Post-Test	$1.00 (0.78)	$1.01 (0.67)	$0.95 (0.77)	$0.99 (0.74)
Difference	$0.01 (0.83), 4.18	$0.14 (0.51), 2.75	-$0.06 (0.60), 2.68	$0.03 (0.66), 4.50
Altruistic Punish. (Punish. Game)				
Pre-Test	$0.39 (0.63)	$0.49 (0.63)	$0.48 (0.65)	$0.45 (0.63)
Post-Test	$0.59 (0.77)	$0.25 (0.36)	$0.49 (0.65)	$0.45 (0.63)
Difference	$0.20 (0.77), 4.25	-$0.24 (0.62), 3.50	-$0.01 (0.92), 5.25	-$0.01 (0.80), 5.25
Donation.				
Post-Test	$5.65 (1.38), 6.00	$5.71 (1.22), 6.00	$5.84 (0.99), 6.00	$5.73 (1.20), 6.00

Note. Mindfulness. = mindfulness meditation group. Kindness. = kindness meditation group. Music. = music relaxation group. Difference = difference score (simple difference between pre-test and post-test score). Compass. Giving = compassionate giving. Altruistic Punish. = altruistic punishment. Punish. Game = punishment game. TPG-Unfair = unfair trials of the third-party game. TPG-Fair = fair trials of the third-party game. Difference scores may diverge from simply subtracting the pre-test from the post-test scores in the table because difference scores reflect changes in pre-test and post-test scores for participants who provided data at both time points. All state compassion responses were given on a 6-point scale. All prosocial games were played for $0.00 to $3.00 CAD, except the Punishment Game that was played for $0.00 to $2.75 CAD. Donation behavior was only measured at post-test on a $0.00 to $6.00 CAD scale. Range = the maximum score minus the minimum score reported (e.g., 4 – 1 = range of 3). Prosocial game pre-test sample sizes are: n = 39 (MM), n = 38 (KM), n = 41 (MR), n = 118 (total). Prosocial game post-test and difference score sample sizes are: n = 38 (MM), n = 34 (KM), n = 37 (MR), n = 109 (total). Donation behavior sample sizes are: n = 37 (MM), n = 34 (KM), n = 37 (MR), n = 108 (total).

effect was observed, and so ANOVAs could not be conducted for post-test donation behavior. Instead, a chi-square analysis was run on dichotomized responses.

4.5.4.1 Prosocial Game Analyses: Dictator Game (DG)

The DG results largely do not support the hypothesis that MM and KM training would increase prosocial behavior vs. the MR control (H2). Neither MM nor KM training significantly elevated altruistic giving vs. MR, though MM and KM training marginally increased giving vs. MR (see Table 17 below). There also were no significant differences between the MM and KM groups in altruistic giving behavior (counter to H4).

Table 17

Effect of Training on Prosocial Behavior: ANOVAs with Bootstrapped A Priori Contrasts

	Mindful. vs. Kind.				Mindful. vs. Music.				Kind. vs. Music.			
	SE	*t*	*p*	*d*	*SE*	*t*	*p*	*d*	*SE*	*t*	*p*	*d*
Altruistic Giving (Dictator Game)	.15	0.31	.753	--	.14	1.80	.084†	0.42	.15	1.45	.086†	0.34
Compass. Giving (TPG-Unfair)	.14	1.08	.287	--	.14	1.52	.126	--	.15	0.40	.707	--
Altruistic Giving (TPG-Fair)	.16	-0.84	.452	--	.15	0.44	.690	--	.16	1.26	.154	--
Altruistic Punish. (Punish. Game)	.19	2.39	.009**	0.56	.18	1.13	.321	--	.19	-1.27	.199	--

Note. Mindful. = mindfulness meditation group. Kind. = kindness meditation group. Music. = music relaxation group. Compass. Giving = compassionate giving. Altruistic Punish. = altruistic punishment. TPG-Unfair = third-party game unfair trials. TPG-Fair = third-party game fair trials. Punish. Game = punishment game. Results are from simple ANOVA analyses with a priori contrasts using difference scores as the dependent variables. All *p*-values were generating using 1000 BCa bootstrap resamples. Blank spaces ("--") indicate no statistics to report. Sample size for all analyses: $n = 38$ (MM), $n = 34$ (KM), $n = 37$ (MR), $n = 109$ (total sample).
** $p < .01$. † $p < .10$.

4.5.4.2 Prosocial Game Analyses: Third-Party Games (TPGs)

Different from DG behavior, the TPG-Unfair trials measured how compassionately one responded to a player who was treated unfairly by another in the games (unfair behavior was defined as giving less than 50% of one's money to the other; Engel, 2011). Compassionate giving was indexed by how much of their money

participants gave to the unfairly treated player. For the TPG-Unfair, all tests were non-significant (see Table 16 for descriptive and Table 17 for inferential statistics above).

In the TPG-Fair trials, participants were informed that one player treated another player fairly in the game (gave them 50% or more of their money; Engel, 2011), and then participants could give their own money to the fairly treated player. Altruistic giving was indexed by how much of their money participants transferred to the fairly treated player. For the TPG-Fair, the results of all group comparisons were again non-significant (see Table 16 for descriptive and Table 17 for inferential statistics above).

The results do not support the primary hypothesis that MM and KM would increase prosocial behavior vs. the MR active control (H2). Also, no differential effects were found between KM vs. MM training on TPG behavior, counter to the secondary hypothesis that KM would enhance prosocial behavior as compared to MM (H4).

4.5.4.3 Prosocial Game Analyses: Punishment Game (PG)

Similar to the TPG-Unfair, the PG placed participants in a third-party context where they were informed that one player treated another player unfairly (defined as giving less than 50% of one's money to the other; Engel, 2011). However, instead of assessing how compassionately participants responded to the unfairly treated player, the PG assessed how much of their own money participants paid to punish (take away money from) the player who acted unfairly (i.e., altruistic punishment; Weng et al., 2015).

Counter to prediction (H1), ANOVAs with a priori contrasts indicated that neither MM nor KM decreased altruistic punishment compared to the MR active control group (see Table 16 for descriptive statistics and Table 17 for the inferential statistics above). However, consistent with prediction (H4), the analyses revealed a significant difference

between MM and KM on altruistic punishment. KM training reduced altruistic punishment (increased prosocial behavior) vs. MM training, which paradoxically heightened altruistic punishment (see Figure 8 below). This effect was medium in size.

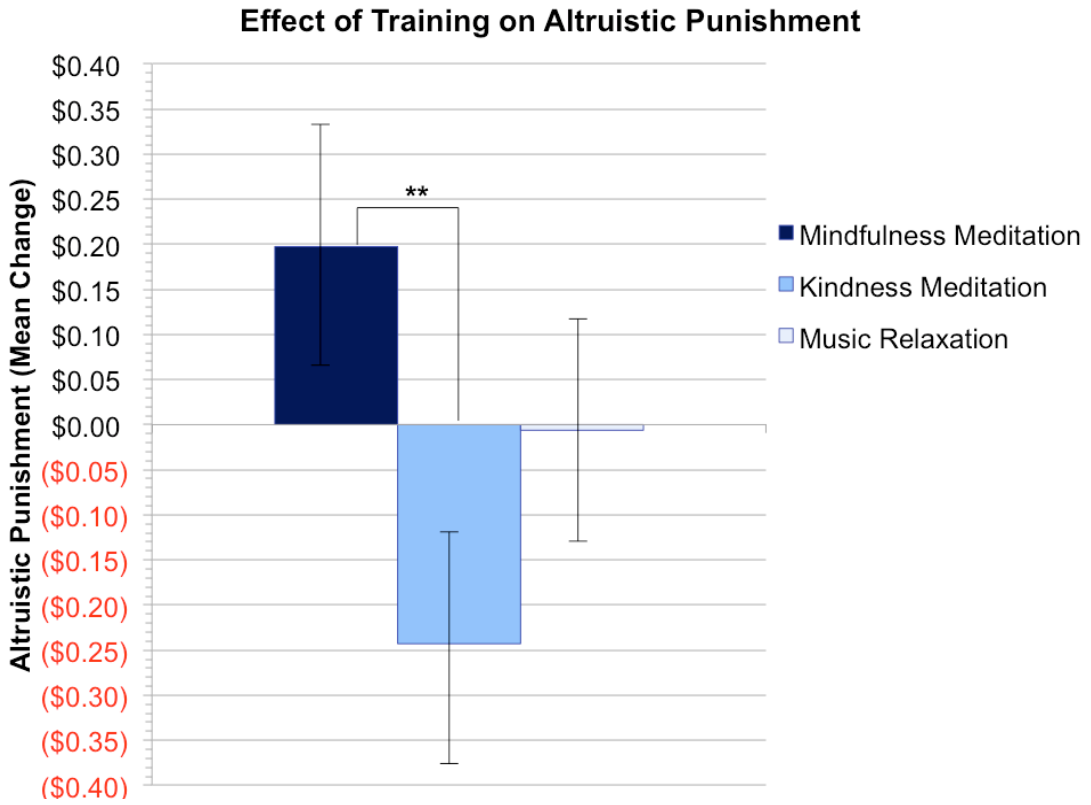

Figure 8. Results depict ANOVA findings from testing the effect of training group on mean changes in altruistic punishment in the punishment game (PG). Responses were given on a $0.00 to $2.75 CAD scale. Sample sizes: n = 38 (MM), n = 34 (KM), n = 37 (MR), n = 109 (total sample). The asterisk (**) indicates a significant difference between groups ($p < .01$). Error bars = +/- 1 SE.

The PG results run counter to the prediction that MM vs. MR and KM vs. MR would increase prosocial behavior (i.e., reduce altruistic punishment) (H1). However, the results support the prediction that KM would increase prosocial behavior vs. MM (H4).

4.5.4.4 Donation Behavior Analyses

At the post-test only, a single measure of donation behavior was administered. After teachers were told the prosocial games they played were with a computer (not other

people), and that the $6.00 CAD they were given to use in the games would be added to their incentive cheque, they were asked if they wanted to donate any portion of their funds to a local children's hospital (possible range was $0.00 to $6.00 CAD).

Examining the frequencies of donation amounts revealed that only six participants, out of the 108 who provided donation behavior responses, did *not* donate the full $6.00 CAD to the children's hospital (see Table 16 above). As such, a ceiling effect was observed. Of the six people who did not donate the full amount, three were in the MM group, two were in the KM group, and one was in the MR group. Given the virtual absence of variance in donation behavior, ANOVA analyses were forgone (Field, 2013). Nevertheless, responses were dichotomized into two groups: gave $6.00 CAD (donated full amount) or gave less than $6.00 CAD (kept some of the 'bonus' money for oneself). A 2 x 2 chi-square test was run crossing group with dichotomized donation behavior. The result of this analysis was not significant, χ^2 (2) = 1.04, p = .594. Counter to prediction (H2, H4), there were no training group differences in charitable donation behavior.

4.5.5 Mediation Analyses: Mechanisms of Prosocial Behavior Change

A secondary (exploratory) hypothesis proposed that different processes would mediate the effects of MM and KM training on prosocial behavior (H3). For MM training, it was hypothesized that enhanced emotional awareness and regulation would mediate increases in prosocial behavior. For KM training, it was hypothesized that enhanced compassion would mediate increases in prosocial behavior. Given that MM and KM training did not significantly improve teacher emotion skills, compassion, or prosocial behavior compared to the MR active control training, analyses with emotion skills and compassion as mediators of prosocial behavior change were omitted.

4.5.6 Summary: Third Study Aim Results

For the third study aim, the analyses reported in this section tested whether the primary hypotheses and secondary hypotheses were supported. Regarding the primary hypothesis H1, emotional awareness and regulation did not improve in the MM or KM groups compared to the MR active control group. Regarding the primary hypothesis H2, no differences were found between MM versus MR or KM versus MR in compassion or prosocial behavior. Regarding the exploratory hypothesis H3, changes in emotional awareness and regulation (in MM) or in compassion (in KM) were not tested as mediators of increases in prosocial behavior due to the lack of changes in emotion skills, compassion, and prosocial behavior. Regarding the exploratory hypothesis H4, MM did not enhance emotional awareness and regulation more than KM. However, partial support for the fourth hypothesis was found. KM significantly reduced altruistic punishment in the PG economic game versus MM, which counter to prediction increased altruistic punishment. Overall, minimal support was found for the third study aim.

4.6 Third Study Aim Results for the Female Subsample

The effects of the mental trainings were hypothesized to interact with gender, given known gender differences in emotion skills, compassion, and prosocial behavior, as well as preliminary evidence of gender differences in response to meditation. Yet, the goal of recruiting an equal number of male and female teachers was not achieved. Specifically, 96 participants identified as female (n_{MM} = 32, n_{KM} = 31, n_{MR} = 33), whereas 22 participants identified as male (n_{MM} = 7, n_{KM} = 7, n_{MR} = 8). Thus, given the small cell sizes, neither gender by group interactions nor group differences in the male-only sample could be reliably modeled and interpreted (see Field, 2013). For the sake of

completeness, descriptive and inferential statistics for the male sample are reported in Appendix A. That said, in lieu of formally testing gender differences in response to training, this section reports exploratory analyses for the third study aim in the female-only subsample (see demographic statistics for the female sample in Table 2 above).

Given that group assignment was stratified by gender (i.e., male and female participants were equally likely to be in the MM, KM, or MR groups), randomization was preserved in the female sample analyses. Also, a majority of teachers in Canada and in British Columbia are female: approximately 70% (World Bank, 2017) and 74% (BC Public Schools, 2017). As such, the female sample analyses carry significance for the population of study interest. Similarly, prior studies on meditation training for teachers (Emerson et al., 2017; Jennings et al., 2017) and other adults (Galante et al., 2014; Sedlmeier et al., 2012) have employed about 75% to 85% female samples.

Importantly, however, these analyses are to be considered exploratory and should be interpreted with caution. Strong causal inferences should be tempered because approximately 20% of the full sample was removed in the female sample analyses, and thus the findings may not meet strict standards of RCT evidence (which the full sample analyses did meet; see Moher et al., 2010). As directional a priori predictions were not made regarding gender differences, or for female participants specifically, a more conservative statistical threshold ($p < .01$) was used to protect against Type I error (see Benjamin et al., 2017). ANOVAs with contrasts were conducted using BCa bootstrapped *p*-values comparing the effects of MM, KM, and MR training on emotion skills, compassion, and prosocial behavior. Effect sizes were indexed using Cohen's *d*.

4.6.1 Effect of Training on Expected and Perceived Changes (Female Sample)

Expectations for change (at pre-test) and perceptions of change (at post-test) in well-being, self-regulation, and compassion were assessed to test if all mental trainings were matched on the primary study expectancies and demand characteristics (MacCoon et al., 2012). Omnibus ANOVAs indicated that expectations for changes in well-being, $F(2, 92) = 0.13$, $p = .876$, self-regulation, $F(2, 92) = 0.12$, $p = .889$, and compassion, $F(2, 93) = 0.64$, $p = .528$, did not vary by group at pre-test. Likewise, omnibus ANOVAs indicated that perceived changes in well-being, $F(2, 85) = 0.22$, $p = .805$, self-regulation, $F(2, 85) = 0.14$, $p = .873$, and compassion, $F(2, 85) = 0.51$, $p = .605$, were not different between groups at post-test. Thus, the female sample was chiefly matched across groups on expectations and perceived changes in the main dependent variables at pre-test and post-test, reducing the possibility of placebo effects and demand characteristic biases.

4.6.2 Effect of Training on Emotion Skills (Female Sample)

A primary study hypothesis was that MM and KM would enhance emotional awareness and regulation vs. MR (H1). A secondary hypothesis was that MM would enhance emotional awareness and regulation vs. KM (H4). ANOVAs with contrasts were run for self-reported emotional awareness and regulation. As with the full sample, changes in self-reported emotional awareness and regulation were not different between groups (see Table 18 for descriptive and Table 19 for inferential statistics below).

Emotional awareness and regulation were measured behaviorally by assessing the extent of negative emotional bias in teachers' grading of a student essay directly following a negative emotion induction (see Brackett et al., 2013). State positive and negative affect were measured just prior to the behavioral task as control variables.

Table 18

Self-Reported Emotional Awareness and Regulation: Descriptive Statistics (Female Sample)

	Mindfulness. *M* (*SD*), Range	Kindness. *M* (*SD*), Range	Music. *M* (*SD*), Range	Total Sample *M* (*SD*), Range
Emotion Aware.				
Pre-Test	4.42 (0.56)	4.12 (0.71)	4.34 (0.60)	4.30 (0.63)
Post-Test	4.50 (0.54)	4.31 (0.64)	4.33 (0.56)	4.38 (0.58)
Difference	0.09 (0.40), 1.50	0.10 (0.53), 2.00	0.03 (0.41), 1.67	0.07 (0.44), 2.08
Emotion Reg.				
Pre-Test	3.87 (0.78)	3.71 (0.86)	3.86 (0.59)	3.82 (0.75)
Post-Test	4.04 (0.70)	3.81 (0.79)	3.91 (0.68)	3.92 (0.72)
Difference	0.20 (0.46), 1.92	0.04 (0.52), 2.00	0.06 (0.48), 1.86	0.10 (0.48), 2.08

Note. Mindfulness. = mindfulness meditation group. Kindness. = kindness meditation group. Music. = music relaxation group. Emotion Aware. = emotional awareness. Emotion Reg. = emotion regulation. Difference = difference score (simple difference between pre-test and post-test score). Difference scores may diverge from simply subtracting the pre-test from the post-test scores in the table because difference scores reflect changes in pre-test and post-test scores for participants who provided data at both time points. Range = the maximum score minus the minimum score reported (e.g., 4 – 1 = range of 3). These data represent the female subsample. All pre-test sample sizes are: n = 32 (MM), n = 31 (KM), n = 33 (MR), n = 96 (total). All post-test and difference score sample sizes are: n = 30 (MM), n = 27 (KM), n = 31 (MR), n = 88 (total).

Table 19

Effect of Training on Self-Reported Emotional Awareness and Regulation: ANOVAs with Bootstrapped Contrasts (Female Sample)

	Mindful vs. Kind.			Mindful. vs. Music.			Kind. vs. Music.		
	SE	*t*	*p*	*SE*	*t*	*p*	*SE*	*t*	*p*
Emotion Aware.	.12	-0.10	.923	.11	0.56	.539	.12	0.65	.532
Emotion Reg.	.12	1.23	.238	.12	1.07	.290	.13	-0.20	.842

Note. Mindful. = mindfulness meditation group. Kind. = kindness meditation group. Music. = music relaxation group. Emotion Aware. = emotional awareness. Emotion Reg. = emotion regulation. Results report simple ANOVA analyses with contrasts using difference scores as the dependent variables. All p-values were generating using 1000 BCa bootstrap resamples. These data represent the female subsample. Sample size for all analyses: n = 30 (MM), n = 27 (KM), n = 31 (MR), n = 88 (total sample).

Omnibus ANOVAs indicated that neither state positive affect, $F(2, 92) = 1.62$, $p = .203$, nor state negative affect, $F(2, 92) = 0.48$, $p = .621$, were different between groups at pre-test. Likewise, neither state positive affect, $F(2, 83) = 0.30$, $p = .740$, nor state negative affect, $F(2, 84) = 0.97$, $p = .383$, were different between groups at post-test. As such, a

seven ANCOVA analyses were conducted for each of the seven grading criteria (controlling for pre-test grading scores only; see Table 20 for descriptive and Table 21 for inferential statistics below). Counter to prediction (H1, H4), as with the full sample, between-group differences in emotional awareness and regulation were not found.

Table 20

Student Essay Grades (Task-Based Emotional Awareness and Regulation): Descriptive Statistics (Female Sample)

	Mindfulness. *M* (*SD*)	Kindness. *M* (*SD*)	Music. *M* (*SD*)	Total Sample *M* (*SD*)
Overall Quality				
Pre-Test	3.65 (0.76)[a]	3.87 (0.90)[b]	3.73 (0.94)[b]	3.75 (0.86)[c]
Post-Test	3.00 (0.89)[a]	3.04 (1.04)[d]	2.93 (0.98)[e]	2.99 (0.96)[f]
Student Ability				
Pre-Test	3.13 (0.81)[a]	3.17 (0.79)[b]	3.30 (1.09)[b]	3.20 (0.90)[c]
Post-Test	2.81 (0.91)[a]	2.81 (1.06)[d]	2.82 (0.91)[e]	2.81 (0.95)[f]
Creativity				
Pre-Test	2.77 (0.76)[a]	3.17 (0.70)[b]	2.83 (0.79)[b]	2.92 (0.76)[c]
Post-Test	2.67 (0.84)[b]	2.62 (0.94)[d]	2.61 (0.92)[e]	2.63 (0.89)[g]
Accuracy				
Pre-Test	2.52 (0.72)[a]	2.77 (0.73)[b]	2.50 (0.73)[b]	2.59 (0.73)[c]
Post-Test	2.67 (0.76)[b]	2.77 (0.86)[d]	2.50 (0.64)[e]	2.64 (0.76)[g]
Organization				
Pre-Test	3.26 (0.82)[a]	3.60 (0.62)[b]	3.37 (0.72)[b]	3.41 (0.73)[c]
Post-Test	3.13 (0.63)[b]	3.15 (1.00)[d]	3.25 (0.84)[e]	3.18 (0.82)[g]
Language				
Pre-Test	2.73 (0.74)[b]	2.87 (0.78)[b]	2.93 (0.69)[b]	2.84 (0.73)[h]
Post-Test	2.41 (0.73)[i]	2.27 (0.87)[d]	2.39 (0.79)[e]	2.36 (0.79)[j]
Persuasiveness				
Pre-Test	3.29 (0.64)[a]	3.55 (0.69)[i]	3.35 (0.72)[i]	3.39 (0.69)[k]
Post-Test	3.00 (0.74)[b]	2.92 (0.98)[d]	3.00 (0.82)[e]	2.98 (0.84)[g]

Note. Mindfulness. = mindfulness meditation group. Kindness. = kindness meditation group. Music. = music relaxation group. Accuracy = spelling and punctuation accuracy. Organization = composition organization. Language = diversity of language. All values represent essay grades of two separate student essays evaluated at pre-test and post-test on each of the 7 grading criteria on the emotional awareness and regulation task. Overall Quality and Student Ability were rated on 6-point scales; all other criteria were rated on 5-point scales. These data represent the female subsample.

[a] $n = 31$. [b] $n = 30$. [c] $n = 91$. [d] $n = 26$. [e] $n = 28$. [f] $n = 85$. [g] $n = 84$. [h] $n = 90$. [i] $n = 29$. [j] $n = 83$. [k] $n = 89$.

Table 21

Effect of Training on Post-Test Student Essay Grades, Controlling for Pre-Test Student Essay Grades (Task-Based Emotion Awareness and Regulation): ANCOVAs with LSD Post-Hoc Tests (Female Sample)

	Mindful. vs. Kind.			Mindful. vs. Music.			Kind. vs. Music.		
	SE	*t*	*p*	*SE*	*t*	*p*	*SE*	*t*	*p*
Overall Quality[a]	.26	-0.21	.849	.24	-0.07	.939	.28	0.14	.917
Student Ability[a]	.27	-0.13	.914	.24	0.26	.797	.28	0.37	.716
Creativity[b]	.22	-1.17	.261	.22	-0.78	.405	.24	0.40	.719
Accuracy[b]	.22	0.25	.828	.19	-0.45	.612	.21	-0.68	.509
Organization[b]	.22	-0.75	.491	.19	0.29	.758	.26	1.01	.392
Language[c]	.22	-0.86	.398	.21	-0.09	.926	.23	0.76	.461
Persuasiveness[d]	.22	-0.83	.403	.21	-0.16	.866	.25	0.65	.563

Note. Mindful. = mindfulness meditation group. Kind. = kindness meditation group. Music. = music relaxation group. Accuracy = spelling and punctuation accuracy. Organization = composition organization. Language = diversity of language. Results report ANCOVA analyses including each pre-test grading criterion as a control variable with each post-test grading criterion as the dependent variable (e.g., overall quality at pre-test as the control variable, and overall quality at post-test as the dependent variable). All *p*-values were generating using 1000 BCa bootstrap resamples. These data represent the female subsample. [a] *n* = 30 (MM), 25 (KM), 26 (MR), 81 (total sample). [b] *n* = 29 (MM), 25 (KM), 26 (MR), 80 (total sample). [c] *n* = 27 (MM), 25 (KM), 26 (MR), 78 (total sample). [d] *n* = 29 (MM), 24 (KM), 25 (MR), 78 (total sample).

4.6.3 Effect of Training on Compassion (Female Sample)

A primary hypothesis of this study was that MM and KM would enhance compassion vs. MR (H1). A secondary hypothesis was the KM would enhance compassion vs. MM (H4). Self-rated compassion was assessed using a scale containing three facets: empathic concern (emotional compassion), perspective taking (cognitive compassion), and personal distress (emotionally dysregulated reactions to compassion-eliciting events) (Davis, 1983). ANOVAs with contrasts revealed no differences between training groups on the three compassion subscales (see Table 22 for descriptive and Table 23 for inferential statistics below). Also, counter to prediction, there were no significant differences in behavior on the image-based compassion task for any training group comparisons (see Table 22 for descriptive statistics and Table 23 for inferential

Table 22

Compassion: Descriptive Statistics (Female Sample)

	Mindfulness. *M* (*SD*), Range	Kindness. *M* (*SD*), Range	Music. *M* (*SD*), Range	Total Sample *M* (*SD*), Range
Compassion EC.				
Pre-Test	4.29 (0.57)	4.04 (0.46)	4.07 (0.64)	4.13 (0.57)
Post-Test	4.21 (0.48)	4.13 (0.46)	3.98 (0.53)	4.10 (0.50)
Difference	-0.03 (0.40), 1.71	0.06 (0.50), 2.00	-0.09 (0.45), 1.71	-0.02 (0.45), 2.00
Compassion PT.				
Pre-Test	3.63 (0.61)	3.61 (0.67)	3.57 (0.72)	3.61 (0.66)
Post-Test	3.72 (0.66)	3.61 (0.47)	3.55 (0.57)	3.63 (0.57)
Difference	0.16 (0.43), 1.57	-0.05 (0.52), 1.71	-0.01 (0.51), 2.00	0.04 (0.49), 2.14
Compassion PD.				
Pre-Test	2.61 (0.73)	2.63 (0.75)	2.39 (0.58)	2.54 (0.69)
Post-Test	2.59 (0.77)	2.64 (0.87)	2.15 (0.64)	2.45 (0.79)
Difference	-0.05 (0.42), 1.86	0.04 (0.53), 2.00	-0.19 (0.56), 2.57	-0.07 (0.51), 2.57
Compassion Task				
Pre-Test	2.64 (0.79)	2.57 (0.89)	2.53 (0.57)	2.58 (0.75)
Post-Test	2.61 (0.69)	2.75 (0.91)	2.47 (0.78)	2.60 (0.79)
Difference	0.05 (0.59), 2.95	0.13 (0.48), 1.94	-0.04 (0.64), 2.95	0.04 (0.57), 3.35

Note. Mindfulness. = mindfulness meditation group. Kindness. = kindness meditation group. Music. = music relaxation group. Difference = difference score (simple difference between pre-test and post-test score). Compassion EC. = self-reported compassion-empathic concern subscale. Compassion PT. = self-reported compassion-perspective taking subscale. Compassion PD. = self-reported compassion-personal distress subscale. Difference scores may diverge from simply subtracting the pre-test from the post-test scores in the table because difference scores reflect changes in pre-test and post-test scores for participants who provided data at both time points. Range = the maximum score minus the minimum score reported (e.g., 4 – 1 = range of 3). These data represent the female subsample. All pre-test sample sizes are: $n = 32$ (MM), $n = 31$ (KM), $n = 33$ (MR), $n = 96$ (total). All post-test and difference score sample sizes are: $n = 30$ (MM), $n = 27$ (KM), $n = 31$ (MR), $n = 88$ (total).

Table 23

Effect of Training on Compassion: ANOVAs with Bootstrapped Contrasts (Female Sample)

	Mindful. vs. Kind.			Mindful. vs. Music.			Kind. vs. Music.		
	SE	*t*	*p*	*SE*	*t*	*p*	*SE*	*t*	*p*
Compassion EC.	.12	-0.78	.453	.12	0.51	.586	.12	1.29	.226
Compassion PT.	.13	1.61	.114	.13	1.30	.198	.13	-0.36	.739
Compassion PD.	.13	-0.66	.473	.13	1.09	.268	.13	1.73	.097
Compassion Task	.15	-0.49	.603	.15	0.61	.580	.15	1.09	.260

Note. Mindful. = mindfulness meditation group. Kind. = kindness meditation group. Music. = music relaxation group. Compassion EC. = self-reported compassion-empathic concern subscale. Compassion PT. = self-reported compassion-perspective taking subscale. Compassion PD. = self-reported compassion-

personal distress subscale. Compassion Task = the MET-CORE affective image response task. Results report ANOVA analyses with contrasts using difference scores as the dependent variables. All *p*-values were generating using 1000 BCa bootstrap resamples. These data represent the female subsample. Group sample sizes for all analyses: *n* = 30 (MM), *n* = 27 (KM), *n* = 31 (MR), *n* = 88 (total sample).

statistics). As with the full sample, the female sample results are inconsistent with the hypotheses that MM and KM would increase compassion vs. the MR control (H2), and that KM would increase compassion to a greater extent than MM practices (H4).

4.6.4 Effect of Training on Prosocial Behavior (Female Sample)

A primary study hypothesis was that MM and KM training would increase prosocial behavior vs. MR (H1). A secondary hypothesis was the KM would increase prosocial behavior vs. MM (H4). Prosocial behavior was measured employing five metrics: 1) dictator game behavior (DG; i.e., altruistic giving); 2) third-party game behavior in unfair trials (TPG-Unfair; i.e., compassionate giving); 3) third-party game behavior in fair trials (TPG-Fair; i.e., altruistic giving); 4) punishment game behavior (PG; i.e., altruistic punishment); and 5) donation behavior (donating to a local children's hospital) (see prosocial behavior descriptive statistics in Table 24 below).

4.6.4.1 Prosocial Game Analyses: Dictator Game (DG)

Counter to prediction (H2), MM vs. MR did not significantly increase altruistic giving in the female sample (see Table 25 below). However, the effect was marginal using a conservative statistical threshold. Further, consistent with prediction (H2), KM vs. MR did significantly increase altruistic giving in the DG in the female sample, despite this effect not emerging in the full sample (see Figure 9 below). This is a medium-sized effect. Counter to prediction (H4), there were no differences between the MM and KM groups in altruistic giving. Expectations for changes in compassion at pre-test, and perceived changes in compassion at post-test, did not differ by group (see Section 4.6.1

Table 24

Prosocial Behavior: Descriptive Statistics (Female Sample)

	Mindfulness. *M* (*SD*), Range	Kindness. *M* (*SD*), Range	Music. *M* (*SD*), Range	Total Sample *M* (*SD*), Range
Altruistic Giving (Dictator Game)				
Pre-Test	$1.16 (0.67)	$1.14 (0.55)	$1.32 (0.53)	$1.21 (0.59)
Post-Test	$1.33 (0.75)	$1.30 (0.59)	$1.13 (0.62)	$1.26 (0.66)
Difference	$0.17 (0.79), 3.85	$0.15 (0.51), 2.75	-$0.21 (0.50), 2.38	$0.04 (0.64), 4.54
Compass. Giving (TPG-Unfair)				
Pre-Test	$1.14 (0.63)	$1.08 (0.59)	$1.21 (0.57)	$1.15 (0.59)
Post-Test	$1.26 (0.71)	$1.20 (0.63)	$1.06 (0.60)	$1.17 (0.65)
Difference	$0.11 (0.60), 3.00	$0.07 (0.60), 2.75	-$0.18 (0.64), 3.08	$0.00 (0.62), 4.13
Altruistic Giving (TPG-Fair)				
Pre-Test	$0.94 (0.66)	$0.97 (0.57)	$1.03 (0.62)	$0.98 (0.61)
Post-Test	$1.01 (0.85)	$1.13 (0.68)	$0.90 (0.69)	$1.01 (0.74)
Difference	$0.06 (0.87), 4.18	$0.19 (0.54), 2.75	-$0.13 (0.62), 2.68	$0.04 (0.70), 4.50
Altruistic Punish. (Punish. Game)				
Pre-Test	$0.33 (0.63)	$0.56 (0.65)	$0.52 (0.67)	$0.47 (0.65)
Post-Test	$0.58 (0.82)	$0.24 (0.34)	$0.38 (0.48)	$0.41 (0.61)
Difference	$0.24 (0.74), 3.50	-$0.33 (0.66), 3.50	-$0.17 (0.82), 4.00	-$0.08 (0.78), 5.25
Donation.				
Post-Test	$5.77 (1.10), 6.00	$5.63 (1.36), 6.00	$5.81 (1.08), 6.00	$5.74 (1.17), 6.00

Note. Mindfulness. = mindfulness meditation group. Kindness. = kindness meditation group. Music. = music relaxation group. Difference = difference score (simple difference between pre-test and post-test score). Compass. Giving = compassionate giving. Altruistic Punish. = altruistic punishment. Punish. Game = punishment game. TPG-Unfair = unfair trials of the third-party game. TPG-Fair = fair trials of the third-party game. Difference scores may diverge from simply subtracting the pre-test from the post-test scores in the table because difference scores reflect changes in pre-test and post-test scores for participants who provided data at both time points. All state compassion responses were given on a 6-point scale. All prosocial games were played for $0.00 to $3.00 CAD, except the Punishment Game that was played for $0.00 to $2.75 CAD. Donation behavior was only measured at post-test on a $0.00 to $6.00 CAD scale. Range = the maximum score minus the minimum score reported (e.g., 4 – 1 = range of 3). These data represent the female subsample. Prosocial game pre-test sample sizes are: *n* = 32 (MM), *n* = 31 (KM), *n* = 33 (MR), *n* = 96 (total). Prosocial game post-test and difference score sample sizes are: *n* = 31 (MM), *n* = 27 (KM), *n* = 29 (MR), *n* = 87 (total). Donation behavior sample sizes are: *n* = 30 (MM), *n* = 27 (KM), *n* = 31 (MR), *n* = 88 (total).

above). Thus, the significant (KM vs. MR) and marginal (MM vs. MR) increases in altruistic giving are less likely to be confounded by placebo effects or demand biases.

Table 25

Effect of Training on Prosocial Behavior: ANOVAs with Bootstrapped Contrasts (Female Sample)

	Mindful. vs. Kind.				Mindful. vs. Music.				Kind. vs. Music.			
	SE	*t*	*p*	*d*	*SE*	*t*	*p*	*d*	*SE*	*t*	*p*	*d*
Altruistic Giving (Dictator Game)	.16	0.16	.877	--	.16	2.38	.037†	0.62	.17	2.15	.010*	0.57
Compass. Giving (TPG-Unfair)	.16	0.23	.799	--	.16	1.83	.070	--	.15	1.54	.143	--
Altruistic Giving (TPG-Fair)	.18	-0.69	.498	--	.18	1.03	.369	--	.19	1.67	.049†	0.45
Altruistic Punish. (Punish. Game)	.20	2.93	.003*	0.77	.19	2.15	.038†	0.56	.20	-0.81	.428	--

Note. Mindful. = mindfulness meditation group. Kind. = kindness meditation group. Music. = music relaxation group. Compass. Giving = compassionate giving. Altruistic Punish. = altruistic punishment. TPG-Unfair = third-party game unfair trials. TPG-Fair = third-party game fair trials. Punish. Game = punishment game. Results are from ANOVA analyses with contrasts using difference scores as the dependent variables. All *p*-values were generating using 1000 BCa bootstrap resamples. Given the exploratory nature of the female subsample analyses, a conservative threshold for statistical significance was used (see below). Blank spaces ("--") indicate no statistics to report. These data represent the female subsample. Sample size for all analyses: $n = 31$ (MM), $n = 27$ (KM), $n = 29$ (MR), $n = 87$ (total sample). * $p < .01$. † $p < .05$.

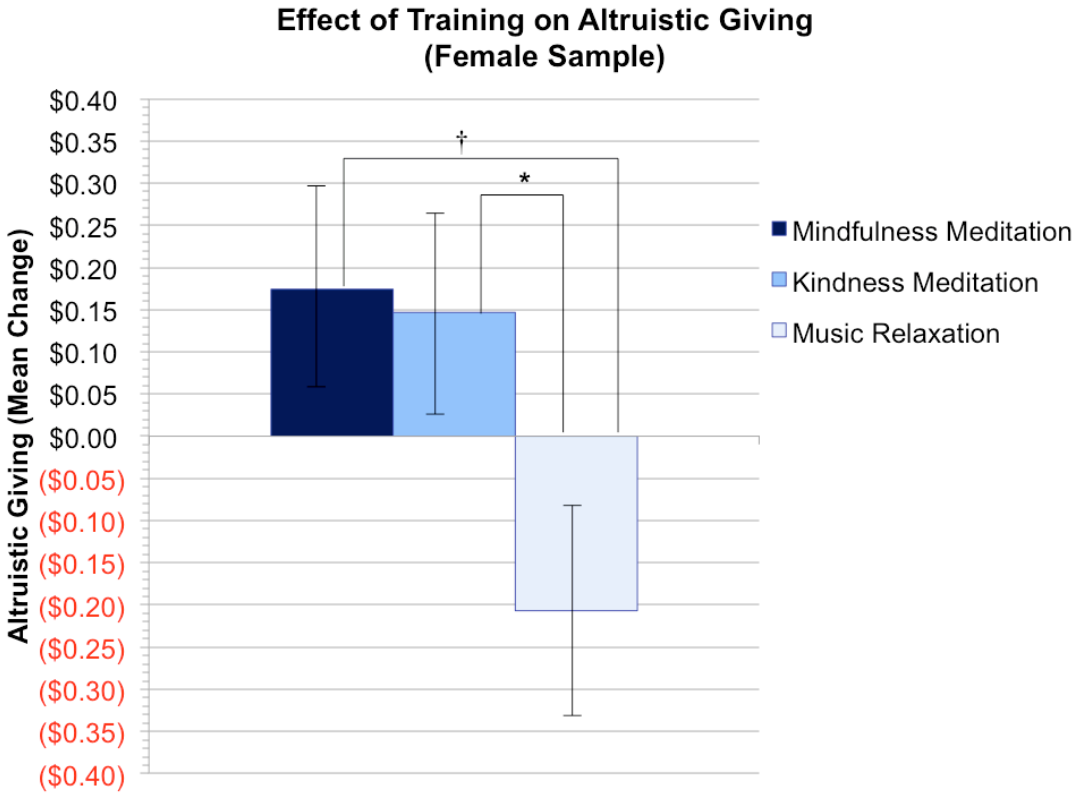

Figure 9. Results depict ANOVA findings from testing the effect of training group on mean changes in altruistic giving in the dictator game (DG). Responses were given on a $0.00 to $3.00 CAD scale. Sample sizes: n = 31 (MM), n = 27 (KM), n = 29 (MR), n = 87 (total female sample). Given the exploratory nature of the female subsample analyses, a more conservative threshold for statistical significance was used. The asterisk (*) indicates a significant difference between groups ($p < .01$). The staff (†) indicates a trend-level difference between groups ($p < .05$). Error bars = +/- 1 SE.

4.6.4.2 Prosocial Game Analyses: Third-Party Games (TPGs)

The TPG-Unfair trials assessed how compassionately participants responded to a player who was treated unfairly by another player in the game (unfair behavior was defined as giving less than 50% of one's money to the other; Engel, 2011). Changes in compassionate giving were not significantly different for any training group comparison (see Table 24 for descriptive statistics and Table 25 for inferential statistics above).

In the TPG-Fair trials, participants were informed that one player treated another player fairly in the game (defined as giving 50% or more of their money; Engel, 2011), and they were provided with a chance to give resources to the fairly treated player. Altruistic giving was marginally higher in the KM vs. MR control group; however, no significant between-group differences emerged in altruistic giving in the TPG-Fair games (see Table 24 for descriptive and Table 25 for inferential statistics above).

These female sample results principally do not support the primary hypothesis that MM and KM would elevate prosocial behavior vs. the MR control (H2). Likewise, counter to the secondary hypothesis that KM would increase prosocial behavior vs. MM (H4), significant differences in TPG behavior were not found between the KM vs. MM groups. The null results in the female sample are consistent with those in the full sample.

4.6.4.3 Prosocial Game Analyses: Punishment Game (PG)

The PG placed participants in a third-party context in which they were informed

that one player treated another player unfairly (defined as giving less than 50% of one's money to the other; Engel, 2011). The PG assessed how much of their own fiscal resources participants spent to punish (take away money from) the player who behaved unfairly in the economic game (i.e., altruistic punishment; Weng et al., 2015).

Inconsistent with prediction (H1), ANOVAs with contrasts indicated that neither MM nor KM decreased altruistic punishment vs. MR in female participants (see Table 24 for descriptive and Table 25 for inferential statistics above). Further, counter to prediction (H1), MM vs. MR showed a trend-level increase in female altruistic punishment. However, consistent with prediction (H4), the results indicated a significant difference between the effects of MM and KM on female altruistic punishment: KM reduced altruistic punishment (i.e., increased prosocial behavior) vs. MM, which paradoxically augmented altruistic punishment (see Figure 10 below). The effect was moderate to large.

Figure 10. Results depict ANOVA findings from testing the effect of training group on mean changes in altruistic punishment in the punishment game (PG). Responses were given on a $0.00 to $2.75 CAD scale. Sample sizes: n = 31 (MM), n = 27 (KM), n = 29 (MR), n = 87 (total female sample). Given the exploratory nature of the female subsample analyses, conservative thresholds for statistical significance were used. The asterisk (*) indicates a significant difference between groups ($p < .01$). The staff (†) indicates a trend-level difference between groups ($p < .05$). Error bars = +/- 1 SE.

4.6.4.4 Donation Behavior Analyses

Donation behavior was assessed at post-test using a single measure of charitable giving. Specifically, teachers were asked if they wished to donate any portion of the $6.00 CAD they would receive from playing the prosocial economic games (described above) to a local children's hospital (possible range was $0.00 to $6.00 CAD).

Inspecting the frequencies of donation amounts indicated that only five female participants, out of the 88 who provided charitable behavior responses, did *not* donate the full $6.00 CAD to the hospital (see Table 24 above). As with the total sample, there was a ceiling effect. Of the five participants who did not donate the entire amount, two were in the MM group, two were in the KM group, and one was in the MR group. Given the lack of variance in donation behavior, ANOVA analyses were forgone (see Field, 2013). However, responses were dichotomized: gave $6.00 CAD (donated entire amount) or gave less than $6.00 CAD (kept some of the 'bonus' money for oneself). A 2 x 2 chi-square analysis was conducted testing group against dichotomized donation behavior. The result was non-significant, χ^2 (2) = 0.55, p = .758. Counter to prediction (H2, H4), charitable donation behavior did not vary between training groups in the female sample.

4.6.5 Mediation: Mechanisms of Prosocial Behavior Change (Female Sample)

A secondary (exploratory) hypothesis of this study was that the effects of meditation training on prosocial behavior would be mediated by emotional awareness and

regulation for MM, and by compassion for KM (H3). However, significant increases in emotional awareness and regulation as well as compassion were not observed in the female sample. Accordingly, as with the full study sample, analyses with emotion skills and compassion as mediators of change in prosocial behavior were omitted.

4.6.6 Summary: Third Study Aim Results (Female Sample)

To further address the third study aim, the analyses reported in this section tested the primary and secondary hypothesis sets in the female subsample. As with the full sample, no between-group differences in emotion skills or compassion were found. These results are counter to the primary and secondary hypotheses that MM and KM would increase emotion skills (H1) and compassion (H2) compared to the MR active control, as well as that MM would increase emotions skills more than KM, and KM would increase compassion more than MM (H4). That said, consistent with prediction (H2), KM significantly elevated altruistic giving in the dictator game compared to MR. This was a medium-sized effect based on convention. Notably, this was the only significant effect that emerged in the female sample that did not emerge in the full sample. Lastly, partially consistent with prediction (H4), KM significantly decreased altruistic punishment in the PG compared to MM, although it was not predicted that MM would increase altruistic punishment. Given the exploratory nature of the female sample results, firm causal inferences regarding the effects of MM and KM on female teachers should be tempered.

Chapter 5: Discussion

5.1 Overview

The purpose of the present study was to examine whether digital meditation training could help to promote teacher well-being and attenuate deleterious rates of teacher stress and burnout by enhancing teacher emotion skills, compassion, and prosocial behavior (Jennings & Greenberg, 2009; McIntyre et al., 2017; Roeser et al., 2012; Schonert-Reichl, 2017). Accordingly, a randomized controlled trial was conducted to assess the feasibility, acceptability, and efficacy of digital mindfulness meditation (MM) and kindness meditation (KM) training in teachers compared to a music relaxation (MR) active control group. In the following sections, organized by the three study aims, the results are interpreted, including discussions of why the observed and unobserved effects converged and diverged from the study aims and hypotheses, and how the findings varied between the full study sample and the female subsample. The scientific significance and educational implications of the findings are also considered.

5.2 First Study Aim: Digital Training Feasibility and Acceptability

The first study aim was to investigate the feasibility and acceptability of digital meditation training for classroom teachers. To this end, participants were asked a series of questions at post-test, such as how often they meditated, how much they felt the trainings impacted their job performance, and how likely they were to recommend their practices to friend. Teachers' responses to these questions and their implications for online psychological interventions and teacher training programs are considered below.

5.2.1 Training Engagement: Adherence and Practice-Induced Sleep

A reliable behavioral indicator of self-guided training engagement is practice

adherence (see Eysenbach, 2005; Galante et al., 2016). The suggested dosage was five days a week, and on average, participants in the MM and KM training groups reported engaging in their trainings just over four times a week. Thus, meditation practice adherence was high. If participants found the meditations inaccessible or impractical, their adherence likely would have been lower. Also, as many teachers reported practicing in the evening, and the practices can induce relaxation, practice-induced sleep was a feasibility concern (e.g., Lim et al., 2015). Yet, on average, teachers reported falling asleep 'very rarely' during all practices. As such, the results provide the first evidence supporting the feasibility and acceptability of digital MM and KM trainings for teachers.

The average practice amount was about four sessions a week for MM and KM, whereas it was about five sessions a week for MR. These group differences in practice time were significant and constituted large effects. Interestingly, this suggests that participants were more likely to practice focused music listening than they were to meditate in the present study. Why was this the case? As all trainings were matched in length and delivered as audio-guided practices, differences in practice structure or format do not explain participant behavior. One reason for this result may be that MM and KM practices are more cognitively and emotionally demanding than MR practices (e.g., see Ashar et al., 2016). In MM individuals sit and face their own internal thought patterns for extended periods of time, and are asked to focus on a particular object of attention while minimizing mind wandering and goal-directed cognition (Kornfield, 2008, 2017). In KM individuals sit and actively up-regulate happiness and compassion for themselves, their family and friends, strangers, difficult others, and eventually all living beings (Kornfield, 2008, 2017). Focusing on relaxing music with one's eyes closed may not require the

same degree psychological effort as MM or KM, and yet it still may induce temporary calmness (Pelletier, 2004). Although unknown, it is also possible that participants played their MR practices in the background while they were doing other activities, as 'background music' is nearly ubiquitous in modern societies and is commonly employed to regulate daily affect (Schäfer, Sedlmeier, Städtler, & Huron, 2013). In contrast, participants may have been less likely to play 'background meditation' to the same extent while they were engaged in other activities, given the mental concentration required to follow the guided meditations. Regardless of the reason, the results also provide the first evidence of the feasibility and acceptability of digital MR practices for teachers.

A challenge online psychological interventions face is maintaining training engagement and adherence, as social commitments and cues that reinforce training behavior are often absent (Andersson, 2009; Mathieu et al., 2013; Proudfoot, 2013). A recent study examining the influence of an online KM program on well-being and prosocial behavior reported 82% attrition (Galante et al., 2016). The authors note that one reason attrition was so high was virtually anyone could self-register for the study and begin training right away. Their method maximized the accessibility of the trainings and possibly broadened the pool of participants. However, participants may have been less committed to the KM practices. The present research implemented a different protocol.

In this study, the target population of teachers was recruited mostly via online channels (i.e., emails and digital newsletters), and then potential participants were interviewed for 15 minutes before obtaining their consent. Although this process was primarily run to screen out non-classroom teachers and experienced meditators, it also informed participants of the specific study responsibilities and timeline. Participants were

asked if they could commit to study participation, and they were also told that the purpose of the study was to promote their well-being and ultimately their colleagues' and students' well-being. Additionally, each week participants were emailed for a 'weekly check-in' that served as a social support to facilitate participant habit formation and to monitor practice problems (e.g., Lim et al., 2015). A recent online study found that email check-ins can produce a more than twofold increase in meditation practice adherence (Galante et al., 2016). Therefore, the recruitment procedure paired with the weekly check-ins may have increased training commitment and engagement. As a commonly reported stressor for teachers is time scarcity (Travers, 2017), and limited time is a main reason for meditation study attrition (see Galante et al., 2016), it is noteworthy that most teachers were highly engaged in their practices over the study period. As such, this research provides a contribution to digital intervention and teacher training research by highlighting effective protocols for maximizing training adherence and engagement.

5.2.2 Training Efficacy: Teaching Effectiveness, Stress, and Patience

Teacher perceptions of MM, KM, and MR training efficacy were also assessed. Across groups, 52.3%, 76.6%, and 73.8% of teachers reported that the practices made their teaching effectiveness, stress, and patience a little better to much better, respectively. Furthermore, over half (57%) of teachers indicated that the trainings had a greater impact on their job performance than other professional training programs. Given that the trainings were brief (8 to 23 minutes long) and self-administered digitally, it is notable that many teachers found their practices efficacious. Teacher mindfulness programs often provide face-to-face social support and group meditation practices that may increase training engagement and efficacy (see Emerson et al., 2017). Indeed, a

recent meta-analysis found that the average effect size across psychological outcomes for MM-based programs (which contain social support and group meditation) compared to MM practices was: $r = .31$ vs. $r = .25$ (Eberth & Sedlmeier, 2012). Therefore, the present findings indicate that digital meditation training can be seen as effective for classroom teachers, even when the training takes place outside of a traditional supportive social context with group-based practices. As there were no training group differences in perceived training efficacy, further evidence was provided that MR may serve as an effective intervention for teachers. That said, it is noteworthy that teachers reported practicing MM and KM significantly less often than MR, and yet the perceived benefits in teaching effectiveness, stress, and patience were equivalent across groups.

Teachers face an array of stressors that can trigger a burnout cascade of ill-being and ineffective teaching that adversely impact student health and learning, and which is associated with 30-50% turnover in the first five years of teaching (e.g., Gallup, 2014; Greenberg et al., 2016; Jennings & Greenberg, 2009; Lewis et al., 2005, 2008; Milke & Warner, 2011; Oberle & Schonert-Reichl, 2016). The detrimental effects of ignoring the emotional demands of teaching on teachers, students, and the economy are hard to overstate. However, providing teachers with training that develops their emotional and prosocial capacities may buttress teacher resilience and effectiveness amidst their emotional labor (Hamre et al., 2012; Jennings et al., 2017; Roeser et al., 2013; Skinner & Beers, 2016). In support of this point, although the current research often lacks rigorous RCT designs, a recent systematic review found that meditation-based trainings are moderately effective at improving emotion regulation and reducing stress in teachers, as well as promoting teacher mindfulness and self-compassion (Emerson et al., 2017). In

this regard, the present study contributes to a growing body of research examining accessible and efficacious approaches for enhancing teacher social-emotional skills and job performance (Jennings & DeMauro, 2017; Schonert-Reichl, 2017). From the perspective of the teachers themselves, just six weeks of digital MM, KM, and MR training were successful at moderately improving teaching effectiveness, stress, and patience. Given that digital meditation and music relaxation practices are easily accessible, transportable, and cost-effective, their scalability potential is high.

5.2.3 Training Satisfaction: Practice Referrals and Enjoyment Motivation

Training satisfaction was assessed by asking teachers the likelihood that they would recommend their practices to a friend or colleague, and also by asking the extent to which teachers engaged in their practices during the study because they enjoyed them. Across groups, the results indicated that 68.2% of teachers were somewhat to highly likely to suggest their practices to a colleague or friend, and 72% of teachers reported that their enjoyment of the practices was somewhat to highly motivating in their training engagement. Notably, the digital meditation and music practices were perceived as about equally enjoyable. Given that the practices were digital and self-directed, it is significant that teacher satisfaction with the trainings was moderate. Without any expert guidance from a live meditation instructor or social support from others engaging in the practices, it was largely unknown how teachers would respond. No prior studies have used self-administered or digital meditation trainings with teachers (Emerson et al., 2017). As such, this study advances research on digital meditation (Spijkerman et al., 2016) and teacher training (e.g., Schonert-Reichl, 2017) by suggesting that novices practicing on their own may find digital MM, KM, and MR to be moderately satisfying. This finding is consistent

with a survey that found a large portion of adults in North America preferred online meditation trainings that they could engage in independently (Wahbeh et al., 2014).

5.2.4 Summary: Digital Training Feasibility and Acceptability

Overall, the results provide preliminary support for the feasibility and acceptability of digital meditation trainings for K-12 teachers. As this was the first RCT evaluating digital meditation for teachers (see Emerson et al., 2017), it was unclear whether teachers would engage in their assigned practices routinely enough to yield benefits. That said, the necessary dosage and intensity required to produce meaningful psychological changes is unclear and is debated in the meditation literature (see Sedlmeier et al., 2012). Therefore, even if teachers did engage in the practices often, it was unknown whether they would perceive any value in their efforts. Furthermore, given that MM and KM can be cognitively and emotionally demanding, and to some extent exert paradoxical effects on naïve practitioners (e.g., Lindahl et al., 2017), teachers may not have felt the practices were worth their limited time and mental energy (Travers, 2017). Nonetheless, despite many potential barriers to feasibility and acceptability, the majority of teachers practiced MM, KM, and MR four to five times a week, and they reported the practices to be moderately efficacious and satisfying. In this regard, the present study provides evidence that supports a digital approach to enhancing teacher well-being and job performance via meditation-based training (Emerson et al., 2017; Jennings et al., 2017). However, these findings are preliminary. Longitudinal RCTs with objective measures of training engagement and efficacy are needed prior to the adoption of digital meditations into teacher training programs and education systems.

5.3 Second Study Aim: Changes in Affect and Mindfulness

The second study aim was to test the effectiveness of digital MM and KM trainings in achieving core proximal practice goals with teachers (Kornfield, 2008, 2017). The core practice goals were increased positive affect and mindfulness, and decreased negative affect and stress. Meta-analyses indicate that MM and KM reliably improve affect and mindfulness (Eberth & Sedlmeier, 2012; Galante et al., 2014). Accordingly, the second study aim also provided information regarding the comparability of the present study findings to the field. That said, an MR active control was employed that is known to induce positive affect and reduce negative affect (Pelletier, 2004). As such, it was possible that differences between the meditations and the control would not emerge because all practices were ineffective, or because all practices were effective. Thus, both within-group and between-group analyses were run to test whether the digital meditations enhanced affect and mindfulness. Findings from these analyses are discussed below.

5.3.1 Changes in Affect

Significant changes in positive affect did not emerge within or between groups with one exception: Positive affect increased from pre-test to post-test in the MM group. Prior studies have found that MM training enhances positive affect (Eberth & Sedlmeier, 2012; Sedlmeier et al., 2012). However, this is the first study to show digital MM-only training can increase positive emotions in teachers (one prior study found that a multi-faceted in-person meditation program enhanced teacher positive affect; Kemeny et al., 2012). The effect size was between small and medium, which is similar to the medium effect size reported in recent meditation meta-analyses (Eberth & Sedlmeier, 2012; Sedlmeier et al., 2012). As these are within-group results, it is possible that they are (at

least partially) due to a placebo effect, measurement effect, and/or event effect. Yet, positive affect did not significantly increase in the KM or MR groups, and all groups were matched on expectations for and perceived changes in well-being. As such, it is not likely that these confounds drove the change in positive affect in the MM group alone. Although the gains in positive affect were greater in the MM than in the KM group, and the MR group showed no change, the difference between groups was not significant. The differential effects of MM, KM, and MR on teacher positive affect require further study.

To the extent that MM enhanced positive affect in teachers, there are a number of implications (see Fried et al., 2015). Positive emotions may confer value to chronically stressed teachers because positive emotions can 'undo' the detrimental effects of negative emotions on psychological and physiological functioning (e.g., facilitate cardiovascular recovery after facing a social stressor; Fredrickson et al., 2000; Lyubomirsky, King, & Diener, 2005). Further, while they are unfolding, positive emotions can expand an individual's 'thought-action repertoire' in ways that lead to an increase in adaptive biopsychosocial resources (e.g., creative solutions to complex problems, forming meaningful relationships, improved physiological markers of autonomic regulation; Fredrickson, 2013; Lyubomirsky et al., 2005). Also, 'positive emotion contagion' can occur between teachers and students. Thus, more frequent positive emotions in teachers may lead to more frequent positive emotions in students (e.g., enthusiasm), and thereby promote student engagement (Frenzel et al., 2009; Pekrun, 2006). Lastly, positive emotions are primary motivators of prosocial cognitions and behaviors (Floman, 2015; Ma, Tunney, & Ferguson, 2017; Mikulincer & Shaver, 2010), which are central to effective teaching and student personal and academic development (e.g., Hamre et al.,

2013; Jennings & Greenberg, 2009). To date, the field has focused on the intrapersonal sequelae of teacher negative affect and stress. Understanding how the effects of meditation on teacher positive affect exert a downstream influence on student well-being and performance is an important area for future research (e.g., see Jennings et al., 2017).

Additionally, within-group analyses indicated that MM and KM training significantly decreased negative affect. The effect sizes were large based on convention and recent meditation meta-analyses (Eberth & Sedlmeier, 2012; Galante et al., 2014; see also Emerson et al., 2017). MR was effective at reducing negative affect from pre-test to post-test as well. It produced large improvements compared to the effect sizes reported in a meta-analysis on music and affect (Pelletier, 2004). Likewise, stress was significantly assuaged in the MM, KM, and MR groups from pre-test to post-test. The effects were medium in all groups. As such, the results indicate that all trainings improved teacher negative affect and stress, and that MR served as a well-matched active control training. The present study is the first to suggest that digital MM and KM practices alone – independent of a full training program with group activities and psychoeducation – can be delivered digitally and attenuate negative affect and stress in teachers facing emotional labor (e.g., Greenberg et al., 2016). Although MM and KM training did not improve affect compared to MR, the sizeable improvements in each group across time may explain these null findings (e.g., see Feldman et al., 2010; MacCoon et al., 2012). However, given that all training groups improved, and that all groups were matched on expectations for and perceived changes in well-being and self-regulation, the changes in negative affect and stress may (at least in part) reflect a shared placebo effect, measurement effect, and/or event effect in all groups (Boot et al., 2013).

To the extent that the digital meditation and music practices assuaged negative affect and stress, such reductions may help to disrupt teacher burnout cascades that lead to psychological and physical health problems, as well as undermine supportive teacher-student relationships and teaching effectiveness (Greenberg et al., 2016; Hamre et al., 2013; Jennings & Greenberg, 2009; McIntyre et al. 2017). Given the toll that chronic stress takes on teachers and their students, accessible and transportable methods for improving teacher affect may confer a wide range of benefits, such as reducing current increases in teacher attrition (Greenberg et al., 2016) and declines in job satisfaction (Markow et al., 2013), and improving student personal and academic development (e.g., Hoglund et al., 2015). Yet, as this is the only study to evaluate digital meditation (and music relaxation) as a means to promote teacher well-being, research is needed that examines whether these effects are robust and durable, and if so, their long-term trajectories (e.g., see Franco et al., 2010; Kemeny et al., 2012; Roeser et al., 2013).

5.3.2 Changes in Mindfulness

Upon examining changes in mindfulness, three significant within-group effects emerged. Two effects occurred in the MM group and one effect occurred in the MR group. Specifically, MM moderately increased the observing and acting with awareness facets of mindfulness. Observing is one's perceptual and experiential attunement to subtleties in the environment (Baer et al., 2006). Acting with awareness is one's ability to navigate the tasks of daily life with present-centered, focused attention (Baer et al., 2006). Prior research also found that a meditation-based teacher program (CARE) moderately enhanced mindful observing in teachers (Jennings et al., 2013, 2017). However, previous studies have not found that meditation enhances acting with

awareness in teachers. While concentrating on a focal object, the core proximal goals of MM practices include observing present-moment phenomenological experience and waves of external stimulation as they affect the mind and body (Kornfield, 2008, 2017). By routinely paying attention to patterns of fluctuating internal states and outward stimulation, MM may heighten the accuracy of one's observations of the world and the presentness of one's awareness in daily life (Baer et al., 2006). Thus, improvements in observing and acting with awareness are congruent with the core proximal goals of MM.

That said, why did MM augment observing and acting with awareness from pre-test to post-test, but not non-judging and non-reacting? Observing and acting with awareness may be the first skills to develop. They are basic components of the MM practice itself, and they both primarily involve attentional and awareness abilities (e.g., focusing on present-moment experience, and noticing the influence of the breath on the nostrils; see Good et al., 2016). As more regulatory and cognitively elaborate facets of mindfulness, non-judging and non-reacting may require more extensive training to enhance compared to other mindfulness skills (see Baer et al., 2006; Good et al., 2016; Kornfield, 2008, 2017). However, whether this was the case is unknown. Studies are needed that examine the rates at which particular mindfulness abilities develop over time as a result of MM and KM practice (e.g., Singer et al., 2016), especially with teachers. Improvements in teacher mindfulness due to meditation training have been consistently reported in prior research (Emerson et al., 2017; Jennings & DeMauro, 2017). However, it is unclear which facets of mindfulness are mostly likely to improve at what stages of practice in teachers, and how increases in teacher mindfulness track with changes in other domains of teacher well-being and performance (see Roeser et al., 2013).

Bearing this in mind, why did MM not significantly increase any facets of mindfulness compared to KM or the MR control? Difference scores showed small increases in mindfulness in all groups from pre-test to post-test, and the larger increases in the MM group were not big enough to yield a differential effect. Given the paucity of studies comparing MM to KM trainings in all populations, and MM to active controls in teachers, research is needed that tests whether more intense doses of MM over longer periods of time would yield differences in any facets of mindfulness for MM compared to KM or a control. As there were no between-group effects, all within-group changes in mindfulness may (at least in part) represent placebo and/or demand characteristic effects.

To the extent that six weeks of MM training enhanced teachers' capacities to mindfully observe and act with awareness, there are educational implications to consider. Noticing subtle changes in student behavior, mental health, and learning difficulties, for example, require acute observation and attentional abilities (see Hamre et al., 2013; Skinner & Beers, 2016). Further, convergent theoretical models drawn from the fields of meditation (Halifax, 2012; see Figure 4 in Chapter 2 above), and teacher training (Roeser et al., 2012; see Figure 1 in Chapter 1 above), hold that increased awareness of others' needs and well-being is essential to the spontaneous emergence of compassion and prosocial behavior (see also Jennings & Greenberg, 2009; Vago & Silbersweig, 2012). Accordingly, increases in teacher observing and acting with awareness may contribute to teacher compassionate and prosocial actions as downstream effects. Indirect support for this point was found in a recent study that showed a meditation-based program increased teacher mindfulness (including the observing facet), as well as prosocial teacher-student interactions (Jennings et al., 2017). Future research should test whether changes in

teacher mindfulness formally mediate changes in teacher compassion and prosocial behavior. Moreover, future research is needed that examines whether improvements in teacher mindfulness mediate changes in student well-being and learning.

The final significant within-group change in mindfulness was, unexpectedly, that non-judgmental awareness showed a small increase in the MR group. Non-judgmental awareness is the ability to attend to positively and negatively valenced mental contents without consciously assessing their value ('goodness' or 'badness') or cognitively elaborating on them (Baer et al., 2006). Non-judgmental awareness is not a focus of MR, and yet to varying degrees it is an explicit goal of MM and KM (Kornfield, 2008, 2017; Salzberg, 2002). There are no studies to the present researcher's knowledge that demonstrate focused music listening can increase non-judgmental awareness. Although 'flow states' are reported in experts and novices playing music (e.g., Bakker, 2005), such states are more rarely reported when simply listening to music (e.g., Diaz, 2013). Further, though partially overlapping, flow and non-judgmental awareness are distinct states (Reid, 2011): flow is defined by 'non-self-aware skilled action' (Nakamura & Csikszentmihalyi, 2014), whereas non-judgmental awareness is defined by 'self-aware skilled being' (Baer et al., 2006). More research is required to better understand how MR may (at least temporarily) alter non-judgmental awareness. To the degree that MR did increase teachers' non-judgmental awareness, it may aid their ability to attend to challenging emotions elicited by internal thought patterns or disruptive student behavior, for example, without fixating on them or their meaning (Baer et al., 2006; Skinner & Beers, 2016). Further, enhanced non-judgmental awareness may improve teachers' ability

to adaptively process and resolve emotionally charged situations at school or at home, which may reduce teacher burnout cascades (see Jennings & DeMauro, 2017).

It is important to note that critiques have been leveled at the mindfulness construct as measured by a self-report methods (Chiesa, 2012; Lutz et al., 2015; see also Baer, 2011). Self-reported mindfulness may suffer from low construct validity for multiple reasons. In addition to the well-known issue of social desirability, individuals may perceive that they are more (or less) mindful than they actually are, and meditation training may alter the understanding of item meaning and item ratings. In regard to the latter point, those who become more mindful through meditation practice may become more aware of how mindless they are, leading to lower versus higher self-reported mindfulness. Accordingly, it can be difficult to interpret what self-rated mindfulness scores mean in the context of meditation training (Davidson & Kaszniak, 2015; Lutz et al., 2015). Employing behavioral measures of mindfulness in MM and KM research will help address these issues (Levinson, Stoll, Kindy, Merry, & Davidson, 2014).

5.3.3 Summary: Changes in Affect and Mindfulness

Overall, the results provide partial support for the second study aim. To some extent, digital MM and KM improved affect and mindfulness in teachers. MM was found to moderately increase positive affect, all trainings produced large to moderate reductions in negative affect and stress, and MM led to small enhancements in mindful observing and acting with awareness. These data suggest that digital MM and KM training may be used in teachers to partially achieve core goals of MM and KM practices (Kornfield, 2008, 2017). They also suggest that self-administered digital meditations may work as well as other formats of meditation training in teachers for certain outcomes (Emerson et

al., 2017). This is notable because standalone digital trainings carry scalability potential that traditional face-to-face programs do not provide. Given the numerous adverse effects of chronic stress on teacher well-being and performance (e.g., Greenberg et al., 2016), these findings offer preliminary support for digital meditations (and also possibly music relaxation practices) as tools for fostering teacher resilience. However, to rule out potential placebo, measurement, and event effects that can confound within-group changes, more research is needed to investigate whether the within-group effects for digital MM, KM, and MR found here are observed compared to a control group.

5.4 Third Study Aim: Changes in Emotion Skills, Compassion, and Prosocial Behavior

The third aim of the present study was to test two sets of hypotheses: a primary hypothesis set that was firmly rooted in prior research and a secondary hypothesis set that was more exploratory. The primary hypotheses proposed that MM versus MR, and KM versus MR would enhance teacher emotional awareness and regulation, as well as compassion and prosocial behavior. The secondary hypotheses proposed that increases in prosocial behavior would be mediated by changes in emotional awareness and regulation for MM participants, and by increases in compassion for KM participants; and that MM would increase emotional awareness and regulation more than KM, whereas KM would increase compassion and prosocial behavior more than MM. Results from tests of these hypotheses are discussed below along with their scientific and educational implications.

5.4.1 Changes in Emotional Awareness and Regulation

Counter to prediction, there were no significant between-group differences in self-reported emotional awareness or emotion regulation. These results suggest that no changes in emotion skills occurred. However, as a body of data suggests that MM and KM enhance emotional functioning and possibly emotion skills (Eberth & Sedlmeier,

2012; Emerson et al., 2017; Galante et al., 2014; Sedlmeier et al., 2012), this null result also may be due to instrument insensitivity. Prior studies using the TAS-20 (Parker et al., 2003) and the AES (Schutte et al., 1998, 2009) have found meditation-driven improvements in emotional awareness and regulation (Arias et al., 2010; Chu, 2010). Yet, the emotional awareness and regulation scales used in this study also included items from the SREIS (Brackett et al., 2006), which has not been used in meditation research. Further, none of the items from the three scales were previously used with teachers. The TAS-20, AES, and SREIS were integrated to provide a comprehensive assessment of teachers' intrapersonal and interpersonal emotion skills (e.g., see Zaki & Williams, 2013). Nonetheless, the adapted emotional awareness and regulation measures may have been unable to detect meditation-induced changes in teacher emotion skills.

It is also important to note that emotional awareness and regulation are difficult constructs to validly measure via self-report (e.g., see Conte, 2005). This is particularly true in response to meditation training. People may not have accurate knowledge of their emotional awareness or regulation skills, especially if they have low baseline self-awareness and meta-cognitive awareness. However, higher self-awareness and meta-cognitive awareness as driven by meditation training may lead individuals to note more instances in which they are emotionally unaware and/or unregulated, and therefore, to self-report lower emotional awareness and regulation even though improvements have occurred (e.g., see Chiesa, 2012; Davidson & Kaszniak, 2015). As such, the meaning of self-reported emotion skills in the context of meditation training is unclear. For these reasons, behavioral measures of emotional awareness and regulation provide greater construct validity than self-report measurement approaches (e.g., Brackett et al., 2006).

Accordingly, emotional awareness and regulation also were assessed using a behavioral task adapted from prior research with teachers (Brackett et al., 2013). In this paradigm, at pre-test and post-test, teachers were shown provocative videos of school conflicts to induce frustration, then they were asked to grade a real student's essay using subjective and ambiguous criteria. Greater emotional awareness and regulation are known to reduce the influence of 'incidental affect' on a range of human judgments among experts and novices (MacFarland et al., 2003; Schwarz & Clore, 1983, 2003). Therefore, by maximizing the potential for negative emotional bias on the grading task, a lower incidence of unfavorable grading in the MM and KM groups versus the MR active control was used to index improvements in teacher emotional awareness and regulation.

Counter to prediction, there were no significant between-group differences on the emotion skills task. That said, at post-test participants were asked two questions: 1) whether they were aware the videos they watched may have induced emotions that affected their grading, and 2) if they were aware of a potential bias, whether they attempted to regulate their emotions. Dichotomized responses indicated that awareness of and attempts to manage emotional bias were slightly higher in the MM (75.7% and 64.9%) and KM groups (69.7% and 63.6%) than in the MR group (58.3% and 52.8%). Despite no group differences in emotional bias, MM and KM participants reported higher emotional awareness and regulation than MR participants, suggesting a discord between participants' perceived and actual emotion skills. This occurred even though all groups were matched on expected and perceived changes in well-being and self-regulation.

Why did MM and KM not decrease negative emotional bias in teacher grading compared to MR? Other than the possibility that no changes in emotion skills occurred,

there are two plausible, non-mutually exclusive explanations. First, the emotional awareness and regulation task was newly adapted from a prior study to suit the pre-posttest design of the present study (Brackett et al., 2013). Obtaining an online behavioral measure of emotion skills that had been validated in teachers and in the context of meditation training proved difficult (see Emerson et al., 2017). As such, the specific emotion skills task employed had not been tested in teachers, or in a pre-posttest RCT design. Thus, the measure was potentially insensitive to change in the present study. Second, it is more difficult to detect, especially small to medium-sized effects, in actively controlled RCTs with behavioral outcome measures (e.g., see Eberth & Sedlmeier, 2012; Galante et al., 2014). The present study employed precisely this design. Therefore, as a large portion of studies on meditation and emotion skills in teachers (Emerson et al., 2017; Jennings et al., 2017; Kemeny et al., 2012) and non-teachers (Eberth & Sedlmeier, 2012; Galante et al., 2014; Sedlmeier et al., 2012) have used waitlist controls with self-report measures, the rigorous design used here may (partially) explain why changes in emotions skills found in prior studies were not observed. A waitlist and actively controlled RCT with behavioral assessments validated in teachers is needed to test whether digital meditation trainings can enhance teacher emotion skills.

5.4.2 Changes in Compassion

Compassion was assessed using a multifaceted scale that measured empathic concern (emotional compassion), perspective taking (cognitive compassion), and personal distress (dysregulated responses to others' suffering; Davis, 1983). Counter to a primary hypothesis, neither MM nor KM increased compassion compared to the MR control group. Likewise, counter to a secondary hypothesis, KM did not enhance

compassion compared to MM. The trainings appear to have exerted no effect on self-rated compassion. However, prior work suggests that meditation can increase self-report and behavioral measures of compassion in teachers (Benn et al., 2012; Jennings et al., 2017; Kemeny et al., 2012) and non-teachers (Condon et al., 2013; Leiberg et al., 2011; Lim et al., 2015; Weng et al., 2013, 2015). What was different in the present study, compared to prior studies, that might explain this discrepancy? To equalize expectations for change across groups, during the recruitment and training periods all participants were informed that their trainings were shown to increase compassion. This procedure was effective, as expected and perceived changes in compassion were not different between groups at pre-test and post-test. To the present researcher's knowledge, no prior meditation studies have primed participants across training groups to expect increased compassion. Therefore, to the extent that meditation training enhanced compassion in prior studies as a result of placebo effects and/or demand characteristics, the null results found here may be due to the psychologically well-matched training groups.

That said, the construct validity of self-reporting compassion may be reduced by social desirability (see Fiske et al., 2007). As such, compassion was measured using a behavioral task as well. Participants were asked to indicate their degree of concern for suffering individuals depicted in a series of images. No between-group differences were found on the compassion task. These results also are counter to the primary hypothesis that MM and KM would increase compassion versus the MR control, and counter to the secondary hypothesis that KM would increase compassion versus MM.

The absence of changes on the compassion task may indicate that MM and KM do not enhance compassion, though it is to varying degrees a goal of MM and KM

(Kornfield, 2008, 2017). However, given the studies suggesting that MM and KM increase compassion noted above (e.g., Jennings et al., 2017; Weng et al., 2013, 2015), it may be that the measure employed was not responsive to meditation training. The MET-CORE (Edele et al., 2013) was used because, in addition to giving money to others who are treated unfairly or in need, compassion is often assessed in meditation research by asking participants to respond to images of human suffering (Desbordes et al., 2012; Klimecki et al., 2012; Rosenberg et al., 2015; Weng et al., 2013). The MET-CORE includes images from a database that prior meditation studies on compassion have used (Weng et al., 2013), and the MET-CORE predicts prosocial behavior in the dictator game (Edele et al., 2013). Yet, the MET-CORE had not been used in meditation research or with teachers. Therefore, the compassion measure may not have been sensitive to meditation training, and/or it may not have captured variance in teacher compassion.

Additionally, past studies examining the effects of MM and KM on compassion have used image-response tasks assessing compassion via fMRI activity (Desbordes et al., 2012; Klimecki et al., 2012; Weng et al., 2013), or via facial expressions in response to the stimuli (Rosenberg et al., 2015). As such, the effects of meditation on these compassion tasks have been largely indexed by changes in involuntary brain or body function, not by explicit feelings toward people in the images, as was employed here. Thus, changes in image-based compassion behavior found in prior meditation studies may operate outside of conscious awareness. If so, this would explain the null result. That said, the limited ecological validity of current compassion tasks makes it difficult to effectively employ them in samples with high daily compassion demands, including teachers. More research is needed that measures compassion as a reaction to realistic

stimuli of others' suffering (versus static images) in the context of meditation training to advance the field (e.g., Rosenberg et al., 2015). With such measures, future research can better determine whether digital meditation training can enhance compassion in teachers.

5.4.3 Changes in Prosocial Behavior

Prosocial behavior was operationalized via economic games as well as charitable donations. Based on game theory research, prosocial economic games are measures high on internal validity and generalizability (Engel, 2011; Rand & Nowak, 2013). The games indexed three different forms of prosocial behavior: 1) *altruistic giving* (dictator game [DG]; and third-party game-fair rounds [TPG-Fair]; Engel, 2011), 2) *compassionate giving* (third-party game-unfair rounds [TPG-Unfair]; Weng et al., 2013, 2015), and 3) *altruistic punishment* (punishment game [PG]; Fehr & Gächter, 2002; Weng et al., 2015). All prosocial games assessed participants' allocation of fiscal resources to anonymous others at a personal cost to themselves. Prosocial behavior also was measured using a more ecologically valid, population-relevant measure of teachers' financial donations to a local children's hospital. A primary study hypothesis was that MM and KM training would enhance all forms of prosocial behavior versus the MR active control, and a secondary hypothesis was that KM would increase all forms of prosocial behavior versus MM. Results from tests of these hypotheses and their implications are discussed below.

5.4.3.1 Changes in Altruistic Giving (Dictator Game)

Consistent with prediction, the KM trainings significantly increased (and the MM trainings marginally increased) altruistic giving in the DG compared to the MR control. However, this medium-sized effect did not emerge in the full study sample analyses, rather, it only emerged in the exploratory female subsample analyses. Difference scores

indicate that KM (and MM) participants increased in altruistic giving from pre-test to post-test, whereas MR participants declined in altruistic giving. The DG literature suggests that people give less money in DG paradigms each time they play (Engel, 2011), and so this pattern was expected in the control group. Reasons for the tendency to become less generous in repeated DGs are unclear. However, it is possible that initial decisions are more spontaneous and intuitive, but subsequent decisions are more planned and calculated, and the former process permits more prosocial responses than the latter (Rand, Greene, & Nowak, 2012). Notably, meditation training-driven gains in altruistic giving in the DG worked against this prosocial decline, and yet increases were still found.

The present results extend previous work examining the effects of meditation on DG behavior. Only one prior study examined the influence of meditation on DG behavior and no effects were found (Leiberg et al., 2011). Thus, the present findings provide the first evidence that meditation may increase altruistic giving in the canonical DG. Given that this effect only emerged in the female sample, however, more research with male and female meditators is needed to determine the robustness of the results. Further, although randomization was stratified by gender, the female sample analyses reflect an approximate 20% loss in sample size, and so causal inferences should be tempered.

Why were enhancements in altruistic giving only found in the female sample? The emergence of increased prosocial behavior in the female sample, despite reduced statistical power and the use of a more conservative statistical threshold, suggests that there were: 1) preexisting gender differences which accounted for unique changes in altruistic giving; and/or 2) male and female participants responded differently to the trainings which interacted with their effect on altruistic giving. There were no gender

differences on any prosocial games at pre-test,[25] largely ruling out the former explanation. However, the latter explanation may be valid. As noted, in female participants KM (and MM) moderately increased and MR moderately decreased altruistic giving. Yet, in male participants difference scores indicated KM and MM had almost no effect, whereas MR had a moderate to large enhancing effect on altruistic giving (see Table 28 in Appendix A). This distinct pattern of changes for female and male participants may explain the disparate results for the full sample versus the female sample. That said, the small male sample prevented analytic modeling of gender by group interactions. Future research with gender-balanced samples is needed to formally test whether, and how, the effects of meditation on teacher prosocial behavior vary by gender.

Notably, this is also the first study to suggest that digital meditation training may enhance altruistic giving in a group for whom prosociality is particularly influential: female teachers (Hamre et al., 2013; Jennings & Greenberg, 2009; Wentzel, 1997, 1998, 2002). Only two prior studies have tested the influence of meditation-based training on compassionate or prosocial behavior in teachers (Jennings et al., 2017; Kemeny et al., 2012). These studies (using majority female samples) found meditation-based programs increased implicit compassion (Kemeny et al., 2012) and emotionally supportive teacher-student interactions (Jennings et al., 2017). That said, prior studies did not utilize an active control group, and so it is possible that expectancy effects or demand characteristics drove some of their observed changes (Boot et al., 2013). Also, past studies employed multi-faceted programs that included psychoeducation, group discussions, and a mixture of MM and KM practices. Therefore, this is the only study to

[25] DG altruistic giving: $t(116) = 0.55$, $p = .585$. TPG-Unfair compassionate giving: $t(116) = 0.48$, $p = .636$. TPG-Fair compassionate giving: $t(116) = 0.50$, $p = .620$. PG altruistic punishment: $t(116) = 0.62$, $p = .534$.

suggest that meditation (i.e., KM) on its own, may increase prosocial behavior in (female) teachers, and thereby, that meditation practices may be active ingredients in meditation-based teacher programs as theorized (Roeser et al., 2012).

Teachers serve as the 'regulatory resource hub' in the classroom, and thus their social-emotional behavior sets the tone for student academic performance and interpersonal behavior (Coan & Sbarra, 2015; Skinner & Beers, 2016). As students spend about one quarter of their waking lives in school (Hamre & Pianta, 2010), repeated exposure to callous and emotionally dysregulated teaching can trigger physiological and behavioral maladaptation in students (e.g., Frenzel et al., 2007; Lewis et al., 2005; Milke & Warner, 2011; Oberle & Schonert-Reichl, 2016). However, evidence is emerging that teacher prosocial behavior can exert salubrious effects which may counter student social-emotional and academic difficulties (e.g., Hamre & Pianta, 2005, 2006; Jennings et al., 2017; Schonert-Reichl, 2017). Consequently, as posited by The Prosocial Classroom model, expanding teacher prosocial behavior via meditation training may also cultivate healthier and more productive classrooms (Jennings & Greenberg, 2009). A recent study found that a meditation-based program for teachers altered the 'emotional climate' of the classroom, increasing both emotionally supportive and productive teacher-student interactions (Jennings et al., 2017). In addition to improving teacher performance and teacher-student relationships, behaving prosocially carries an array of health benefits. These benefits include reducing risk of a heart attack and depression to increasing happiness, social connectedness, and life satisfaction (Piliavin & Siegl, 2007; Post, 2005; Sin & Lyubomirsky, 2009; Willigen, 2000; Weinstein & Ryan, 2010). To the extent that

meditation practices foster more frequent prosocial behavior, they also may yield a range of salutary effects on teacher physical and psychological well-being.

5.4.3.2 Changes in Altruistic and Compassionate Giving (Third-Party Games)

Counter to a primary hypothesis that MM and KM would increase altruistic giving (TPG-Fair) and compassionate giving (TPG-Unfair) compared to MR, no group differences were found. Similarly, counter to a secondary hypothesis that KM would increase TPG altruistic and compassionate giving compared to MM, no group differences were observed. Prior studies have not found meditation can increase altruistic giving in a TPG-Fair paradigm, and these game rounds were primarily included to obfuscate the purpose of the TPG-Unfair and PG rounds. That said, prior studies have shown digital KM increases compassionate giving in a prosocial game (Weng et al., 2013, 2015). Why were these effects not replicated in the present study? Compassion was assessed via a self-report scale, an image-based task, and a prosocial economic game. No changes appeared on any compassion measures. As such, the mental trainings seem to have exerted no influence on teacher compassion for reasons (articulated above in Section 5.4.2) that include: priming expectations for increased compassion across training groups, limited variance in teacher compassion, and/or instrument insensitivity to changes in teacher compassion. To better understand the prosocial effects of meditation on teachers, more research is needed that explicitly measures teacher compassion as a primary outcome in RCTs of meditation training. To date, only two other studies have examined the effect of meditation on teacher compassion (Benn et al., 2012; Kemeny et al., 2012).

5.4.3.3 Changes in Altruistic Punishment (Punishment Game)

An inverse index of prosocial behavior administered in this study was altruistic

punishment. When teachers were informed that one person treated another person unfairly in an economic game, how much did they spend of their own funds to punish the unfair person, and did meditation training change their behavior? Counter to prediction, MM and KM did not reduce altruistic punishment versus the MR control group. Consistent with prediction, KM decreased altruistic punishment (i.e., increased prosocial behavior) compared to MM, which paradoxically heightened altruistic punishment. The size of this effect was medium in the full sample and large in the female sample.

Why did KM reduce altruistic punishment? In KM one explicitly cultivates feeling positive emotions and compassion for others, from family members and friends to strangers and individuals one finds difficult or offensive (Salzberg, 2002). As a result, when playing the PG, KM participants may have more effectively up-regulated positive emotions and/or felt more compassion even for the unfair player (Bankard, 2015; Galante et al., 2014). In support of this notion, baseline empathic concern (the emotional resonance facet of compassion; Davis, 1983) was moderately and negatively correlated with altruistic punishment. Further, forgiveness directed inwardly toward the self and outwardly toward others is practiced as a form of compassion in KM (Kornfield, 2008, 2017). Also, a prior study found that KM versus an active control diminished punishment harshness ratings assigned to moral transgressors in a series of vignettes (Templeton, 2007). Therefore, in the context of the punishment game, it is possible that increases in both empathic concern and forgiveness dampened punishment behavior. Nevertheless, more research is needed on meditation and altruistic punishment that empirically tests this notion, as self-reported empathic concern did not respond to meditation training in the present study and forgiveness was not measured.

Why did MM heighten altruistic punishment? Prosocial economic games, such as the DG, TPGs, and PG, gauge people's sensitivity to equity norms as well as their prosociality (Bolton, Katok, & Zwick, 1998; Engel, 2011). However, this may be particularly true of PGs, where participants learn one person treats another person unfairly, and then they are given the opportunity to punish this unfair person at a personal cost. Furthermore, MM may increase sensitivity to fairness norms, as part of a larger broadening of self-awareness and growing attunement to the social-moral environment (Halifax, 2012; Roeser et al., 2014). Indeed, a prior study found that MBSR versus a waitlist control increased participants scores on a moral reasoning test (Shapiro, Jazaieri, & Goldin, 2012). This test (in part) assessed the extent to which participants were aware of and valued tradeoffs between individual interests and the collective good. If similar changes in moral cognition occurred in this study due to MM, then participants engaging in MM may have become more reactive to violations of fairness norms, and therefore more readily punished 'transgressors' of these norms. Yet, more research on meditation and altruistic punishment (especially in teachers) is needed to investigate this possibility.

The present study is the first to demonstrate that meditation training can influence altruistic punishment, with KM decreasing and MM increasing this behavior. A prior study combined compassionate giving and altruistic punishment into a single 'redistribution' metric, and found two weeks of KM versus an active control increased the redistribution of funds from an unfair person to an unfairly treated person (Weng et al., 2013). As the two measures were conflated, it was unclear whether greater spending indicated increased compassionate giving and/or altruistic punishment. In a follow-up study, the two measures were separated and KM increased compassionate giving versus

an active control, but no effects were found on altruistic punishment (Weng et al., 2015). Also, one non-experimental study found that long-term KM practitioners only engaged in altruistic punishment when others were treated unfairly, but not when they were treated unfairly (McCall et al., 2014). Noting the limitations of prior work, the present findings extend the literature on the plasticity of prosociality to altruistic punishment.

Moreover, this is the first study to test and find that meditation training can alter altruistic punishment in teachers. Punishment behavior is a daily feature of teaching that plays a consequential role in teaching effectiveness and in student well-being and academic success (Frenzel et al., 2007; Hamre et al., 2007, 2013; Jennings & Greenberg, 2009; Lewis et al., 2005, 2008). For example, teachers who rely frequently on punishment for classroom discipline (even in service of fairness norms), especially when students do not understand its justification or are not included in disciplinary decisions, create greater disengagement and hostility in their students (Lewis et al., 2005, 2008). In turn, conflictual teacher-student relationships can impair teacher health and self-efficacy, creating a burnout cascade (McIntyre et al., 2017). Accordingly, the findings in the present study extend the efficacy of meditation for teachers beyond stress reduction to the attenuation of punishment behavior (Emerson et al., 2017; Jennings et al., 2017; Kemeny et al., 2012; Roeser et al., 2012). However, a salutary effect only emerged for KM, whereas MM increased teachers' punishment of others who behaved unfairly. This points to the possibility of paradoxical effects induced by MM training in novices (Lindahl et al., 2017), at least for self-administered practices that are removed from the values and community within which the practices were originally developed (Greenberg & Mitra, 2015). In future studies, more careful consideration should be given to whom and how

MM training is administered, particularly for naïve practitioners in a professional context. More MM and KM training research with teachers is needed where there is explicit monitoring of unintended or iatrogenic practice effects, so that these effects can be better understood and addressed to improve training efficacy.

5.4.3.4 Changes in Donation Behavior

Prosocial behavior was also measured at post-test by asking participants how much money they wished to donate to a local children's hospital (possible range = $0.00 to $6.00 CAD). Meditation training did not influence charitable donations because close to all participants donated the maximum amount in the full sample and the female subsample. Why was there a ceiling effect on donation behavior, but not on prosocial game behavior? The opportunity to donate to a children's hospital may have strongly elicited teacher prosocial behavior because it is highly consistent with teachers' professional identity and social value orientation (see Keltner et al., 2014; Van Lange et al., 2007). Given that teachers choose to work with youth every day, and many teachers see their work as a calling rather than a job, teachers' willingness to help children in need may be atypically high (e.g., Hargreaves 1998, 2000). The prosocial economic games, for their limits in external validity, removed considerations of personal social preferences toward the imagined beneficiaries (Bloom, 2017), as no information was provided about the other players. Therefore, the prosocial games were unlikely to produce ceiling effects. That said, it is important for future research to explore the extent to which meditation training in teachers, as well as adults more generally, influences prosocial behavior outside of controlled experimental paradigms to examine their real-world relevance and efficacy (e.g., see Ashar et al., 2016; Galante et al., 2016; Jennings et al., 2017).

5.4.4 Mechanisms of Prosocial Behavior Change

A secondary hypothesis of the present research proposed potential mechanisms underlying changes in prosocial behavior caused by meditation. Based on prior research, it was predicted that MM would increase prosocial behavior via improved emotional awareness and regulation (Eberth & Sedlmeier, 2012; Vago & Silbersweig, 2012), whereas KM would increase prosocial behavior via improved compassion (Bankard, 2015; Batson, 2011). However, as no changes in emotion skills or compassion were found, all mediational analyses were forgone. As more studies discover that meditation-based practices can enhance prosocial behavior in adults (e.g., Condon et al., 2013; Leiberg et al., 2011; Lim et al., 2015; Weng et al., 2013, 2015), and in some cases in teachers specifically (Jennings et al., 2017; Kemeny et al., 2012), unpacking the processes underlying these changes is important. Understanding the mechanisms of change is valuable as a method for studying the architecture and malleability of prosociality (Ashar et al., 2016; Gilbert, 2017; Mascaro et al., 2015). Moreover, it is valuable as a way to design trainings for applied populations whose prosocial capacities are consequential, such as classroom teachers (e.g., Jennings & Greenberg, 2009; Schonert-Reichl, 2017). In absence of such data, it will remain unclear how neuroplasticity may be leveraged to expand the capacity to care, against self-interest, for others outside of one's in-group, which by some accounts is the fundamental sociocultural challenge of the present day (Bloom, 2017; Dalai Lama, 2009; Greene, 2013). Also, in absence of such data, it will remain unclear what the proximal targets of psychological trainings should be (e.g., emotion skills, compassion, ethical values, moral reasoning) to substantively and sustainably foster teacher prosocial behavior.

5.4.5 Summary: Changes in Emotion Skills, Compassion, & Prosocial Behavior

The third aim of the present study was to test two hypothesis sets. The primary hypothesis set predicted that MM and KM compared to MR would enhance emotional awareness and regulation, as well as compassion and prosocial behavior. Counter to prediction, MM and KM did not increase emotion skills or compassion compared to MR (in the full sample or in the female subsample). These null results highlight the need to develop behavioral measures of emotion skills and compassion that are ecologically valid and which respond to mental training in teacher and non-teacher populations. Although MM did not significantly increase prosocial behavior versus MR, KM increased altruistic giving in the dictator game versus MR, consistent with prediction. This effect was not found in the full study sample, but was found only in the female subsample. These data are the first to suggest that meditation training may enhance teacher prosocial behavior compared to an active control group. Specifically, the results tentatively suggest that digital KM training may be a scalable tool for female teacher prosocial development.

The secondary (exploratory) hypothesis set predicted that greater prosocial behavior would be mediated by enhanced emotional awareness and regulation in the MM group, and increased compassion in the KM group; and that MM would enhance emotional awareness and regulation more than KM, whereas KM would increase compassion and prosocial behavior more than MM. Counter to prediction, neither emotion skills nor compassion mediated changes in prosocial behavior, as there were no changes in either set of mediators. The prosocial mechanisms of meditation remain virtually unknown and are an important direction for future research (see Ashar et al., 2016; Lim et al., 2015). MM versus KM exerted no effects on emotion skills and

compassion. Yet, the meditations exerted divergent effects on prosocial game behavior. Consistent with prediction, KM reduced altruistic punishment in the punishment game, whereas MM heightened altruistic punishment (indicating decreased prosocial behavior). The salutary effects of KM compared to the paradoxical effects of MM should be an intensive focus of future research as they remain poorly understood (Lindahl et al., 2017), especially among classroom teachers (see Emerson et al., 2017). That is to say, although MM and KM are among the most studied mental practices (Eberth & Sedlmeier, 2012; Galante et al., 2014), the differential effects of MM versus KM training on social-emotional functioning are principally unknown (see Kok & Singer, 2017).

5.5 Study Limitations and Directions for Future Research

The present research has limitations to consider. An effort was made to recruit an equal number of male and female teachers. However, this goal was not achieved. Although random assignment was stratified by gender, the cell sizes were too small to formally test for gender by training interactions or to interpret the results for the male subsample. Accordingly, along with reporting and interpreting the results for the full study sample, the results were reported and interpreted for the female subsample in an attempt to explore gender differences in response to meditation training. That said, women versus men report higher levels of stress (Klassen & Chiu, 2010; Matud, 2004) and emotional exhaustion (a facet of burnout; Purvanova & Muros, 2010), and women report employing adaptive and maladaptive emotion regulation strategies more often than men (Nolen-Hoeksema & Aldao, 2011; Tamres et al., 2002), which may increase their stress, anxiety, and depression (Aldao et al., 2010; Johnson & Whisman, 2013; Nolen-Hoeksema, 2012). Also, there is a complex set of gender differences in compassion

(Eisenberg & Lennon, 1983; Jaffe & Hyde, 2000) and prosocial behavior (Eagly & Crowley, 1986; Einolf, 2011), including differences in self-reported compassion (Davis, 1980), prosocial economic game behavior (Engel, 2011), and donation behavior (Mesch et al., 2011). Importantly, these gender differences may have interacted with the efficacy of the meditation trainings in teachers (Rojiani et al., 2017; Sedlmeier et al., 2012). However, gender differences could not be modeled analytically. Thus, it is possible such gender differences played a role in the pattern of findings in ways that remain unknown.

As teachers in North America are approximately 75% female and 25% male (World Bank, 2017), and a large portion of studies on meditation employ majority female samples (see Eberth & Sedlmeier, 2012; Emerson et al., 2017; Galante et al., 2014, Sedlmeier et al., 2012), the relevance of the study findings to the population of interest and their comparability to prior research is high. Yet, as the sample was comprised of teachers from British Columbia, which has a unique education policy and climate, the sample may not necessarily represent teachers in Canada or North America. Also, this was the first study to suggest meditation may influence altruistic giving (in the dictator game) and altruistic punishment (in any paradigm). Therefore, it is unclear if these findings would apply to a community sample in North America or adults globally (Henrich, Heine, & Norenzayan, 2010). To test the robustness of these results, future research should use diverse professional and community samples that are international.

A different limitation is that this study did not utilize a completely random sampling of the teacher population, limiting the generalizability of the findings. To prevent high dropout and poor training adherence, teachers with an interest in improving their well-being, self-regulation, and compassion were recruited. Although such intrinsic

motivation can significantly influence the efficacy of psychological interventions (e.g., Lyubomirsky, Dickerhoof, Boehm, & Sheldon, 2011), extrinsic motivation was also tapped, as teachers were each given $125 CAD for study participation along with a set of mindfulness and well-being resources. Further, all participants with a regular weekly meditation, yoga, or mindful movement practice were excluded. Removing all individuals with any potential interest in psychological self-improvement, however, was likely to be exceedingly difficult. Anyone who signed-up for the study was already likely to be different from those in the general population. Participating in a study on meditation suggests a level of well-being-enhancement motivation that may interact with training effects (Lyubomirksy et al., 2011). This is a limitation that affects all studies on meditation training. Expectations for and perceived changes in well-being, self-regulation, and compassion were all measured. However, primary motivations for participating in the study were not assessed. Future research could provide a detailed list and open-ended assessment of study motivations to evaluate the extent of selection bias.

Another limitation of this research was that practice time was not objectively measured via digital login records (to maximize practice accessibility). Rather, practice time was retrospectively self-reported by participants. It is possible that social desirability or recall biases reduced the accuracy of reported practice amounts. On the whole, average practice time reported across groups was between four and five sessions per week over the 6-week training period. Given that five sessions a week was the suggested practice amount for participants, demand characteristics may have increased the likelihood that participants reported such practice levels. However, this result could reflect high adherence just as well as it could reflect demand characteristic-driven reporting. As self-

reported practice time was a central metric of the feasibility and acceptability of the digital meditation (and music relaxation) practices for teachers, it is important for future research to obtain objective reports of training adherence to minimize reporting biases. Also, practice time data only was obtained regarding how many times per week participants engaged in a practice, but the length of practices engaged in, and total amount of contact hours per participant remain unknown. As participants were given a choice to use a practice that was between 8 and 23 minutes in length, having more accurate practice time data would have permitted precise dose-response analyses.

An additional limitation of this research is that the financial stakes in the prosocial behavior measures were small. That is, at most, the prosocial games were played for $0.00 to $3.00 CAD at pre-test and post-test ($6.00 CAD total).[26] Likewise, the total money at stake in the donation behavior measure was $6.00 CAD. The 'face validity' of changes in quantities of dollars, and in this case, significant differences were on the order of cents (i.e., about $0.25 to $0.50 CAD), may be low. However, game theory paradigms typically utilize between $5 and $10 stakes (e.g., Engel, 2011), and recent studies using prosocial games in the context of meditation have employed similar amounts (Weng et al., 2013, 2015). Thus, the amounts used in this study do not notably diverge from prior research. Also, in a recent meta-analysis of dictator game behavior, the amount of money in the game did not meaningfully affect participant behavior (with the exception that behavior may change for stakes over $100; Engel, 2011). Further, another study tested whether $1.00 stakes altered prosocial behavior in four canonical economic games (Amir & Rand, 2012). Participant behavior in lower stakes games administered online was not

[26] For the punishment game, the stakes were $0.00 to $2.75 CAD.

meaningfully different from participant behavior in higher stakes games administered in the lab. Accordingly, the total amount at stake may not be as consequential as it seems.

One more limitation of the present study was that prosocial behavior was assessed via prosocial economic games and donation behavior, which both used money as the behavioral index. Prosocial behavior can take many forms other than financial generosity, including volunteering one's time, sharing one's knowledge or skills, or introducing people to one another (e.g., Grant, 2013; Keltner et al., 2014). Sharing time, knowledge, and social networks are all meaningful ways in which people help each another. The present study employed fiscal resources as the prosocial metric because there is an established literature on prosocial games (e.g., Balliet et al., 2011; Engel, 2011) and on charitable giving (e.g., Bekkers, & Wiepking, 2011). These literatures suggest that prosocial games and charitable donations are reliable assessments of prosociality, and further, that they are correlated with other forms of prosocial behavior, such as volunteering (e.g., Einolf, 2011; Van Lange et al., 2007). Nevertheless, it is unclear how well prosocial game behavior predicts teacher prosocial behavior at school. Meditation studies using measures of prosocial behavior that are higher on ecological validity and involve dynamic social interactions (e.g., classroom observations of student-teacher interactions) are needed (e.g., see Jennings et al., 2017). Studies of this kind will provide valuable information for designing effective training programs for teachers.

A final limitation of the present research was that a set of measures used to assess the primary dependent variables was adapted and/or had never been employed before in a meditation RCT or teacher study. This issue particularly affected the measurement of emotion skills and compassion. Regarding emotion skills, three different self-report

measures were adapted and combined to comprehensively assess the intrapersonal and interpersonal facets of emotional awareness and regulation (Lane et al., 1990; Zaki & Williams, 2013). Emotion skills have been measured behaviorally in teacher meditation studies rarely (Emerson et al., 2017; Kemeny et al. 2012), and in the broader field of MM and KM research infrequently (Eberth & Sedlmeier, 2012; Galante et al., 2014; Sedlmeier et al., 2012). Thus, based on prior research (Brackett et al., 2013), an ecologically valid task was created that assessed teachers' ability to face a school-related trigger and grade a real student essay without emotional bias. Nevertheless, both the self-report and task-based measures of emotional awareness and regulation failed to respond to training. As such, it remains unclear whether the digital trainings had no influence on teacher emotion skills, or whether the measures simply did not detect training effects.

Limited choices were available to measure compassion using a behavioral task that was not an economic game, and that could be administered in an online assessment. Most meditation studies that assess compassion have employed a task which asks participants to respond to images of human suffering (e.g., Desbordes et al., 2012; Klimecki et al., 2012; Rosenberg et al., 2015). As such, this paradigm was adopted here. Yet, the specific measure, the MET-CORE (Edele et al., 2013), had not been used in meditation research before or with teachers, and it did not respond to training. Again, it is unknown whether compassion did not change due to the trainings' lack of efficacy or because the MET-CORE was insensitive to change. This is an issue of Type II error. Overall, for both teachers and non-teachers, there is a critical need for ecologically valid behavioral measures of emotion skills and compassion which have been demonstrated to change in response to psychological training including meditation training.

5.6 Conclusion

The first study aim was to evaluate the feasibility and acceptability of digital meditation trainings for teachers. As teachers indicated engaging in the digital MM and KM practices about four times a week, and they reported their practices to be moderately effective and enjoyable, preliminary support for the first study aim was found. Teachers indicated engaging in the digital MR (active control) practices about five times a week, and they also reported their practices to be moderately effective and enjoyable. Thus, the feasibility and acceptability of digital MR practices for teachers was supported as well.

The second study aim was to examine whether digital MM and KM could improve teacher affect and mindfulness, given the millennia-old traditions and empirical literature suggesting these are core practice outcomes. As MM showed within-group increases in positive affect and two facets of mindfulness, and MM and KM showed within-group decreases in negative affect and stress, some support for the second study aim was found. However, changes in affect and mindfulness did not differ between training groups. The MR control practices also showed within-group reductions in negative affect and stress. As such, the observed changes in affect and mindfulness may (at least partially) reflect placebo effects and/or demand characteristics.

The third study aim was to test hypotheses regarding the effects of digital meditation on teacher emotion skills, compassion, and prosocial behavior. Counter to prediction, no changes in emotion skills or compassion were found. However, KM increased prosocial behavior (decreased altruistic punishment; consistent with prediction) compared to MM, which decreased prosocial behavior (increased altruistic punishment; counter to prediction). This is the first study to suggest that KM and MM may exert

divergent effects on social-moral behavior. Consistent with prediction, KM also increased prosocial behavior (altruistic giving) compared to MR in a female subsample analysis. That said, overall, the results provide minimal support for the third study aim.

In summary, evidence of the feasibility and acceptability of digital mindfulness and kindness meditation trainings for K-12 teachers was found. Evidence of the efficacy of digital mindfulness and kindness meditation for teachers, however, particularly compared to the music relaxation control group, was limited. The causal effects of digital meditation on adaptive social-emotional functioning in teachers remain open avenues for future investigation. Prior to their adoption in school systems, longitudinal studies are needed that employ more intensive interventions and validated behavioral measures to elucidate the salutary (and possibly iatrogenic) effects of digital meditations on educators.

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Appendices

Appendix A: Difference Scores by Gender, and Male Sample Descriptive and Inferential Statistics

In this section, difference scores for the dependent variables in the third study aim are reported by gender, including emotional awareness and regulation, compassion, and prosocial behavior.[27] Second, all of the descriptive and inferential statistics conducted with the full study sample (for the third study aim) are reported for the male sample (see Table 2 above for male demographic statistics). That said, it is important to note that the reliability and generalizability of these results is highly limited given the underpowered sample: $n = 7$ (MM), $n = 7$ (KM), $n = 8$ (MR), and $n = 22$ (total male sample).[28] These statistics are presented here for the sake of completeness, and to confer full transparency in the data analytic process. To the extent that these results are interpreted, they should be interpreted with extreme caution. The data are presented in tables on the following pages in consecutive order, continuing from the tables presented in the Results section above.

[27] The emotional awareness and regulation task (i.e., the student essay grading task) did not permit the use of difference scores because the emotion induction and student essay at pre-test and post-test were not identical (even though the assessment paradigm was the same). The descriptive statistics for the male participants on the emotional awareness and regulation task can be found in Table 31 (below).

[28] In some analyses, the sample size was even smaller due to missing data.

Table 26

Emotional Awareness and Regulation Difference Scores By Gender and Training Group

	Mindfulness. *M* (*SD*), Range	Kindness. *M* (*SD*), Range	Music. *M* (*SD*), Range	All Groups *M* (*SD*), Range
Emotion Aware.				
Male	-0.01 (0.58), 1.67[a]	-0.21 (0.60), 1.83[a]	0.13 (0.28), 0.67[b]	-0.04 (0.51), 1.83[c]
Female	0.05 (0.38), 1.50[d]	0.10 (0.54), 2.00[e]	0.03 (0.42), 1.67[d]	0.06 (0.44), 2.08[f]
Emotion Reg.				
Male	0.12 (0.59), 1.58[a]	0.18 (0.49), 1.58[a]	0.15 (0.53), 1.33[b]	0.15 (0.51), 1.67[c]
Female	0.15 (0.44), 1.92[d]	0.06 (0.54), 2.00[e]	0.11 (0.48), 1.86[d]	0.11 (0.48), 2.08[f]

Note. Mindfulness. = mindfulness meditation group. Kindness. = kindness meditation group. Music. = music relaxation group. Emotion Aware. = emotional awareness. Emotion Reg. = emotion regulation. Difference score = simple difference between pre-test and post-test score. Range = the maximum score minus the minimum score reported (e.g., 4 – 1 = range of 3).

[a] $n = 7$. [b] $n = 6$. [c] $n = 20$. [d] $n = 28$. [e] $n = 23$. [f] $n = 79$.

Table 27

Compassion Difference Scores By Gender and Training Group

	Mindfulness. *M* (*SD*), Range	Kindness. *M* (*SD*), Range	Music. *M* (*SD*), Range	All Groups *M* (*D*), Range
Compassion EC.				
Male	0.04 (0.41), 1.14[a]	-0.18 (0.49), 1.29[a]	-0.14 (0.31), 0.86[b]	-0.09 (0.41), 1.43[c]
Female	-0.03 (0.41), 1.71[d]	0.13 (0.48), 1.71[e]	-0.06 (0.43), 1.71[d]	0.01 (0.44), 1.86[f]
Compassion PT.				
Male	-0.16 (0.27), 0.71[a]	-0.23 (0.49), 1.43[a]	-0.21 (0.54), 1.43[b]	-0.20 (0.42), 1.86[c]
Female	0.12 (0.42), 1.57[d]	-0.06 (0.54), 1.71[e]	-0.06 (0.50), 2.00[d]	0.01 (0.49), 2.14[f]
Compassion PD.				
Male	0.14 (0.22), 0.57[a]	-0.18 (0.89), 2.71[a]	0.24 (0.37), 1.00[b]	0.06 (0.58), 2.71[c]
Female	-0.02 (0.41), 1.86[d]	-0.01 (0.52), 2.00[e]	-0.19 (0.59), 2.57[d]	-0.08 (0.51), 2.57[f]
Compassion Task				
Male	0.23 (0.80), 2.32[a]	0.07 (0.62), 1.90[a]	-0.17 (0.54), 1.45[b]	0.06 (0.65), 2.32[c]
Female	0.04 (0.59), 2.95[d]	0.12 (0.49), 1.94[e]	-0.06 (0.65), 2.95[d]	0.03 (0.58), 3.35[f]

Note. Mindfulness. = mindfulness meditation group. Kindness. = kindness meditation group. Music. = music relaxation group. Compassion EC. = self-reported compassion-empathic concern subscale. Compassion PT. = self-reported compassion-perspective taking subscale. Compassion PD. = self-reported compassion-personal distress subscale. Difference score = simple difference between pre-test and post-test score. Range = the maximum score minus the minimum score reported (e.g., 4 – 1 = range of 3).
[a] $n = 7$. [b] $n = 6$. [c] $n = 20$. [d] $n = 28$. [e] $n = 23$. [f] $n = 79$.

Table 28

Prosocial Behavior Difference Scores by Gender and Training Group

	Mindfulness. *M* (*SD*), Range	Kindness. *M* (*SD*), Range	Music. *M* (*SD*), Range	All Groups *M* (*SD*), Range
Altruistic Giving (Dictator Game)				
Male	\$0.06 (0.45), 1.19[a]	-\$0.04 (0.28), 0.82[a]	\$0.27 (0.88), 3.00[b]	\$0.10 (0.60), 3.00[c]
Female	\$0.18 (0.81), 3.85[d]	\$0.15 (0.52), 2.75[e]	-\$0.21 (0.50), 2.38[d]	\$0.03 (0.64), 4.54[f]
Compassion. Giving (TPG-Unfair)				
Male	\$0.19 (0.37), 0.93[a]	-\$0.43 (0.63), 1.54[a]	\$0.24 (0.54), 1.75[b]	\$0.01 (0.59), 2.38[c]
Female	\$0.11 (0.62), 3.00[d]	\$0.08 (0.61), 2.75[e]	-\$0.18 (0.64), 3.08[d]	\$0.00 (0.63), 4.13[f]
Altruistic Giving (TPG-Fair)				
Male	-\$0.22 (0.59), 1.43[a]	-\$0.04 (0.31), 0.85[a]	\$0.19 (0.50), 1.42[b]	-\$0.01 (0.50), 1.77[c]
Female	\$0.09 (0.87), 4.00[d]	\$0.20 (0.55), 2.75[e]	-\$0.13 (0.62), 2.68[d]	\$0.05 (0.70), 4.50[f]
Altruistic Punish. (Punish. Game)				
Male	\$0.00 (0.91), 2.75[a]	\$0.11 (0.24), 0.75[a]	\$0.59 (1.05), 3.25[b]	\$0.25 (0.83), 4.25[c]
Female	\$0.26 (0.76), 3.50[d]	-\$0.31 (0.66), 3.50[e]	-\$0.17 (0.82), 4.00[d]	-\$0.07 (0.78), 5.25[f]
Donation Behavior				
Male	\$5.14 (0.27), 6.00[a]	\$6.00 (0.00), 0.00[a]	\$6.00 (0.00), 0.00[g]	\$5.70 (1.34), 6.00[h]
Female	\$5.75 (1.14), 6.00[i]	\$5.57 (1.47), 6.00[j]	\$6.00 (0.00), 0.00[i]	\$5.79 (1.05), 6.00[k]

Note. Mindfulness. = mindfulness meditation group. Kindness. = kindness meditation group. Music. = music relaxation group. Compassion. Giving = compassionate giving. TPG-Unfair = unfair trials of the third-party game. TPG-Fair = fair trials of the third-party game. Altruistic Punish. = altruistic punishment. Punish. Game = punishment game. Difference score = simple difference between pre-test and post-test score. All state compassion responses were given on a 6-point scale. All prosocial games were played for \$0.00 to \$3.00 CAD, except the punishment game that was played for \$0.00 to \$2.75 CAD. Donation behavior was only measured at post-test on a \$0.00 to \$6.00 CAD scale. Range = the maximum score minus the minimum score reported (e.g., 4 – 1 = range of 3).

[a] $n = 7$. [b] $n = 8$. [c] $n = 22$. [d] $n = 29$. [e] $n = 26$. [f] $n = 84$. [g] $n = 6$. [h] $n = 20$. [i] $n = 28$. [j] $n = 23$. [k] $n = 79$.

Table 29

Self-Reported Emotional Awareness and Regulation: Descriptive Statistics (Male Sample)

	Mindfulness. *M* (*SD*), Range	Kindness. *M* (*SD*), Range	Music. *M* (*SD*), Range	Total Sample *M* (*SD*), Range
Emotion Aware.				
Pre-Test	4.57 (0.69), 1.75[a]	3.80 (0.76), 1.92[a]	4.47 (0.30), 0.92[b]	4.29 (0.67), 2.67[c]
Post-Test	4.56 (0.77), 2.33[a]	3.58 (1.02), 3.00[a]	4.50 (0.28), 0.75[d]	4.20 (0.87), 3.83[e]
Difference	-0.01 (0.58), 1.67[a]	-0.21 (0.60), 1.83[a]	0.13 (0.28), 0.67[d]	-0.04 (0.51), 1.83[e]
Emotion Reg.				
Pre-Test	3.79 (0.41), 1.17[a]	3.51 (0.89), 2.17[a]	3.79 (0.28), 0.83[b]	3.70 (0.56), 2.17[c]
Post-Test	3.91 (0.81), 2.67[a]	3.69 (0.61), 2.00[a]	4.01 (0.64), 1.67[d]	3.86 (0.67), 2.67[e]
Difference	0.12 (0.59), 1.58[a]	0.18 (0.49), 1.58[a]	0.15 (0.53), 1.33[d]	0.15 (0.51), 1.67[e]

Note. Mindfulness. = mindfulness meditation group. Kindness. = kindness meditation group. Music. = music relaxation group. Emotion Aware. = emotional awareness. Emotion Reg. = emotion regulation. Difference = difference score (simple difference between pre-test and post-test score). Difference scores may diverge from simply subtracting the pre-test from the post-test scores in the table because difference scores reflect changes in pre-test and post-test scores for participants who provided data at both time points. Range = the maximum score minus the minimum score reported (e.g., 4 – 1 = range of 3). These data represent the male subsample.

[a] $n = 7$. [b] $n = 8$. [c] $n = 22$. [d] $n = 6$ [e] $n = 20$.

Table 30

Effect of Training on Self-Reported Emotion Awareness and Regulation: ANOVAs with Bootstrapped Contrasts (Male Sample)

	Mindful. vs. Kind.			Mindful. vs. Music.			Kind. vs. Music.		
	SE	*t*	*p*	*SE*	*t*	*p*	*SE*	*t*	*p*
Emotion Aware.	.28	0.73	.477	.29	-0.47	.643	.29	-1.17	.258
Emotion Reg.	.29	-0.21	.838	.30	-0.10	.921	.30	0.10	.922

Note. Mindful. = mindfulness meditation group. Kind. = kindness meditation group. Music. = music relaxation group. Emotion Aware. = emotional awareness. Emotion Reg. = emotion regulation. Results report ANOVA analyses with contrasts using difference scores as the dependent variables. All *p*-values were generating using 1000 bootstrap resamples. These data represent the male subsample. The sample size for all analyses is: $n = 7$ (MM), $n = 7$ (KM), $n = 6$ (MR), $n = 20$ (total sample).

Table 31

Student Essay Grades (Task-Based Emotion Awareness and Regulation) Descriptive Statistics (Male Sample)

	Mindfulness. *M* (*SD*), Range	Kindness. *M* (*SD*), Range	Music. *M* (*SD*), Range	Total Sample *M* (*SD*), Range
Overall Quality				
Pre-Test	3.67 (0.82), 2.00[a]	3.86 (1.46), 4.00[b]	3.86 (0.90), 2.00[b]	3.80 (1.06), 4.00[c]
Post-Test	2.71 (0.76), 2.00[b]	3.57 (1.13), 3.00[b]	3.63 (0.74), 2.00[d]	3.32 (0.95), 3.00[e]
Student Ability				
Pre-Test	3.50 (0.84), 2.00[a]	3.14 (1.07), 3.00[b]	2.71 (1.25), 4.00[b]	3.10 (1.07), 4.00[c]
Post-Test	2.43 (0.53), 1.00[b]	3.14 (1.22), 3.00[b]	3.25 (0.89), 2.00[d]	2.96 (0.95), 3.00[e]
Creativity				
Pre-Test	3.17 (0.98), 3.00[a]	3.14 (0.69), 2.00[b]	3.43 (0.54), 1.00[b]	3.25 (0.72), 3.00[c]
Post-Test	2.43 (0.53), 1.00[b]	2.86 (0.38), 1.00[b]	2.75 (0.46), 1.00[d]	2.68 (0.48), 1.00[e]
Accuracy				
Pre-Test	2.50 (0.55), 1.00[a]	2.29 (0.76), 2.00[b]	2.57 (0.53), 1.00[b]	2.45 (0.61), 2.00[c]
Post-Test	1.71 (0.76), 2.00[b]	2.71 (0.49), 1.00[b]	3.00 (0.53), 2.00[d]	2.50 (0.80), 3.00[e]
Organization				
Pre-Test	3.17 (0.75), 2.00[a]	3.86 (0.69), 2.00[b]	3.57 (0.53), 1.00[b]	3.55 (0.69), 3.00[c]
Post-Test	2.86 (0.69), 2.00[b]	3.57 (0.79), 2.00[b]	3.50 (0.53), 1.00[d]	3.32 (0.72), 2.00[e]
Language				
Pre-Test	3.00 (0.63), 2.00[a]	3.14 (0.69), 2.00[b]	3.14 (0.69), 2.00[b]	3.10 (0.64), 2.00[c]
Post-Test	2.29 (0.49), 1.00[b]	3.00 (8.17), 2.00[b]	2.50 (0.93), 3.00[d]	2.59 (0.80), 3.00[e]
Persuasiveness				
Pre-Test	3.33 (0.82), 2.00[a]	3.71 (0.49), 1.00[b]	3.57 (0.53), 1.00[b]	3.55 (0.61), 2.00[c]
Post-Test	2.67 (0.52), 1.00[a]	3.86 (0.69), 2.00[b]	3.25 (0.71), 2.00[d]	3.29 (0.78), 3.00[f]

Note. Mindfulness. = mindfulness meditation group. Kindness. = kindness meditation group. Music. = music relaxation group. Overall Quality and Student Ability were rated on 6-point scales; all other criteria were rated on 5-point scales. Accuracy = spelling and punctuation accuracy. Organization = composition organization. Language = diversity of language. All values indicate essay grades on each of the 7 rating criteria from the emotional awareness and regulation task measure. Range = the maximum score minus the minimum score reported (e.g., 4 – 1 = range of 3). These data represent the male subsample.
[a] $n = 6$. [b] $n = 7$. [c] $n = 20$. [d] $n = 8$. [e] $n = 22$. [f] $n = .21$.

Table 32

Effect of Training on Post-Test Student Essay Grades, Controlling for Pre-Test Student Essay Grades (Task-Based Emotion Awareness and Regulation): ANCOVAs with LSD Post-Hoc Tests (Male Sample)

	Mindful. vs. Kind.				Mindful. vs. Music.				Kind. vs. Music			
	SE	*t*	*p*	*d*	*SE*	*t*	*p*	*d*	*SE*	*t*	*p*	*d*
Overall Quality[a]	.47	-1.42	.175	--	.47	-1.42	.175	--	.45	0.00	1.00	--
Student Ability[a]	.50	-1.58	.135	--	.52	-2.12	.050†	-1.28	.48	0.65	.532	--
Creativity[b]	.26	-1.98	.065	-1.20	.27	-1.46	.164	--	.26	0.52	.613	--
Accuracy[b]	.36	-3.09	.007*	-1.87	.35	-3.74	.002*	-2.26	.35	-0.63	.536	--
Organization[b]	.37	-0.71	.488	--	.35	-1.11	.282	--	.33	0.38	.708	--
Language[c]	.33	-1.64	.120	--	.33	-0.36	.726	--	.32	1.34	.198	--
Persuasiveness[d]	.38	-3.01	.008*	-1.82	.37	1.20	.247	--	.35	1.97	.067	1.14

Note. Mindful. = mindfulness meditation group. Kind. = kindness meditation group. Music. = music relaxation (MR) group. Accuracy = spelling and punctuation accuracy. Organization = composition organization. Language = diversity of language. Blank spaces ("--") indicate no statistics to report. Results report ANCOVA analyses including each pre-test grading criterion as a control variable with each post-test grading criterion as the dependent variable. All *p*-values were generating using 1000 bootstrap resamples. Given the exploratory nature of the male subsample analyses, a conservative threshold for statistical significance was used (see below). These data represent the male subsample. The subgroup sample size for all analyses is: $n = 6$ (MM), $n = 7$ (KM), $n = 7$ (MR), $n = 20$ (total sample).
* $p < .01$. † $p < .05$.

Table 33

Compassion Descriptive Statistics (Male Sample)

	Mindfulness. *M* (*SD*), Range	Kindness. *M* (*SD*), Range	Music. *M* (*SD*), Range	Total Sample *M* (*D*), Range
Compassion EC.				
Pre-Test	4.04 (0.56), 1.86[a]	4.33 (0.43), 1.14[a]	4.21 (0.55), 1.57[b]	4.20 (0.51), 1.86[d]
Post-Test	4.08 (0.51), 1.29[a]	4.14 (0.84), 2.00[a]	4.02 (0.65), 1.71[c]	4.09 (0.65), 2.00[e]
Difference	0.04 (0.41), 1.14[a]	-0.18 (0.49), 1.29[a]	-0.14 (0.31), 0.86[c]	-0.09 (0.41), 1.43[e]
Compassion PT.				
Pre-Test	3.57 (0.84), 2.57[a]	3.78 (0.75), 2.29[a]	3.96 (0.18), 0.57[b]	3.78 (0.63), 2.71[d]
Post-Test	3.41 (1.00), 3.00[a]	3.55 (1.00), 2.86[a]	3.76 (0.59), 1.43[c]	3.56 (0.86), 3.00[e]
Difference	-0.16 (0.27), 0.71[a]	-0.23 (0.49), 1.43[a]	-0.21 (0.54), 1.43[c]	-0.20 (0.42), 1.86[e]
Compassion PD.				
Pre-Test	2.49 (0.62), 1.86[a]	2.49 (0.63), 1.86[a]	2.26 (0.71), 1.71[b]	2.41 (0.64), 2.29[d]
Post-Test	2.63 (0.51), 1.57[a]	2.31 (0.43), 1.14[a]	2.25 (0.77), 2.00[c]	2.41 (0.57), 2.14[e]
Difference	0.14 (0.22), 0.57[a]	-0.18 (0.89), 2.71[a]	0.24 (0.37), 1.00[c]	0.06 (0.58), 2.71[e]
Compassion Task				
Pre-Test	2.58 (0.94), 2.45[a]	2.70 (0.41), 1.20[a]	2.45 (0.51), 1.40[b]	2.57 (0.63), 2.45[d]
Post-Test	2.81 (0.42), 1.30[a]	2.77 (0.39), 1.15[a]	2.25 (0.86), 2.30[c]	2.63 (0.60), 2.50[e]
Difference	0.23 (0.80), 2.32[a]	0.07 (0.62), 1.90[a]	-0.17 (0.54), 1.45[c]	0.06 (0.65), 2.32[e]

Note. Mindfulness. = mindfulness meditation group. Kindness. = kindness meditation group. Music. = music relaxation group. Difference = difference score (simple difference between pre-test and post-test score). Compassion EC. = self-reported compassion-empathic concern subscale. Compassion PT. = self-reported compassion-perspective taking subscale. Compassion PD. = self-reported compassion-personal distress subscale. Difference scores may diverge from simply subtracting the pre-test from the post-test scores in the table because difference scores reflect changes in pre-test and post-test scores for participants who provided data at both time points. Range = the maximum score minus the minimum score reported (e.g., 4 – 1 = range of 3). These data represent the male subsample.

[a] $n = 7$. [b] $n = 8$. [c] $n = 6$ [d] $n = 22$. [e] $n = 20$.

Table 34

Effect of Training on Compassion: ANOVAs with Bootstrapped Contrasts (Male Sample)

	Mindful. vs. Kind.			Mindful. vs. Music.			Kind. vs. Music.		
	SE	*t*	*p*	*SE*	*t*	*p*	*SE*	*t*	*p*
Compassion EC.	.22	1.00	.328	.23	0.79	.439	.23	-0.18	.862
Compassion PT.	.24	0.26	.799	.25	0.21	.839	.25	-0.04	.968
Compassion PD.	.35	0.94	.379	.17	-0.55	.595	.37	-1.14	.285
Compassion Task	.36	0.45	.656	.37	1.01	.297	.37	0.64	.530

Note. Mindful. = mindfulness meditation group. Kind. = kindness meditation group. Music. = music relaxation group. Compassion EC. = self-reported compassion-empathic concern subscale. Compassion PT. = self-reported compassion-perspective taking subscale. Compassion PD. = self-reported compassion-personal distress subscale. Compassion Task = the MET-CORE affective image response task. Results report ANOVA analyses with contrasts using difference scores as the dependent variables. All *p*-values were generating using 1000 bootstrap resamples. These data represent the male subsample. Group sample sizes for all analyses: $n = 7$ (MM), $n = 7$ (KM), $n = 6$ (MR), $n = 20$ (total sample).

Table 35

Prosocial Behavior Descriptive Statistics (Male Sample)

	Mindfulness. *M* (*SD*), Range	Kindness. *M* (*SD*), Range	Music. *M* (*SD*), Range	Total Sample *M* (*SD*), Range
Altruistic Giving (Dictator Game)				
Pre-Test	$1.26 (0.48), 1.31	$0.88 (0.83), 1.88	$1.19 (0.95), 3.00	$1.11 (0.77), 3.00
Post-Test	$1.32 (0.23), 0.50	$0.84 (0.69), 1.50	$1.45 (0.86), 3.00	$1.22 (0.69), 3.00
Difference	$0.06 (0.45), 1.19	-$0.04 (0.28), 0.82	$0.27 (0.88), 3.00	$0.10 (0.60), 3.00
Compassion. Giving (TPG-Unfair)				
Pre-Test	$1.11 (0.42), 1.13	$1.03 (0.88), 2.13	$1.05 (0.98), 3.00	$1.06 (0.77), 3.00
Post-Test	$1.30 (0.18), 0.50	$0.60 (0.63), 1.44	$1.30 (0.89), 3.00	$1.08 (0.71), 3.00
Difference	$0.19 (0.37), 0.93	-$0.43 (0.63), 1.54	$0.24 (0.54), 1.75	$0.01 (0.59), 2.38
Altruistic Giving (TPG-Fair)				
Pre-Test	$1.18 (0.41), 1.07	$0.60 (0.56), 1.33	$0.93 (1.02), 3.00	$0.90 (0.73), 3.00
Post-Test	$0.96 (0.39), 0.93	$0.57 (0.46), 1.15	$1.11 (1.06), 3.00	$0.89 (0.73), 3.00
Difference	-$0.22 (0.59), 1.43	-$0.04 (0.31), 0.85	$0.19 (0.50), 1.42	-$0.01 (0.50), 1.77
Altruistic Punish. (Punish. Game)				
Pre-Test	$0.64 (0.57), 1.50	$0.18 (0.37), 1.00	$0.31 (0.59), 1.50	$0.38 (0.54), 1.50
Post-Test	$0.64 (0.50), 1.25	$0.29 (0.47), 1.25	$0.91 (0.98), 2.75	$0.63 (0.72), 2.75
Difference	$0.00 (0.91), 2.75	$0.11 (0.24), 0.75	$0.59 (1.05), 3.25	$0.25 (0.83), 4.25
Donation Behavior				
Post-Test	$5.14 (0.27), 6.00	$6.00 (0.00), 0.00	$6.00 (0.00), 0.00	$5.70 (1.34), 6.00

Note. Mindfulness. = mindfulness meditation group. Kindness. = kindness meditation group. Music. = music relaxation group. Compassion. Giving = compassionate giving. TPG-Unfair = unfair trials of the third-party game. TPG-Fair = fair trials of the third-party game. Altruistic Punish. = altruistic punishment. Punish. Game = punishment game. Difference = difference score (simple difference between pre-test and post-test score). Difference scores may diverge from simply subtracting the pre-test from the post-test scores in the table because difference scores reflect changes in pre-test and post-test scores for participants who provided data at both time points. All state compassion responses were given on a 6-point scale. All prosocial games were played for $0.00 to $3.00 CAD, except the punishment game which was played for $0.00 to $2.75 CAD. Donation behavior was only measured at post-test on a $0.00 to $6.00 CAD scale. Range = the maximum score minus the minimum score reported (e.g., 4 – 1 = range of 3). These data represent the male subsample. Sample size for all statistics: n = 7 (MM), n = 7 (KM), n = 8 (MR), n = 22 (total sample).

Table 36

Effect of Training on Prosocial Behavior: ANOVAs with Bootstrapped Contrasts (Male Sample)

	Mindful. vs. Kind.				Mindful. vs. Music.				Kind. vs. Music.			
	SE	*t*	*p*	*d*	*SE*	*t*	*p*	*d*	*SE*	*t*	*p*	*d*
Altruistic Giving (Dictator Game)	.33	0.33	.747	--	.32	-0.64	.528	--	.32	-0.98	.339	--
Compass. Giving (TPG-Unfair)	.28	2.20	.041†	1.27	.27	-0.20	.843	--	.27	-2.47	.023†	-1.37
Altruistic Giving (TPG-Fair)	.26	-0.71	.486	--	.25	-1.63	.119	--	.25	-0.90	.380	--
Altruistic Punish. (Punish. Game)	.44	-0.24	.814	--	.43	-1.38	.183	--	.43	-1.14	.270	--

Note. Mindful. = mindfulness meditation group. Kind. = kindness meditation group. Music. = music relaxation group. Compass. Giving = compassionate giving. TPG-Unfair = third-party game unfair trials. TPG-Fair = third-party game fair trials. Altruistic Punish. = altruistic punishment. Punish. Game = punishment game. Blank spaces ("--") indicate no statistics to report. Results are from ANOVA analyses with contrasts using difference scores as the dependent variables. All *p*-values were generating using 1000 bootstrap resamples. Given the exploratory nature of the male subsample analyses, a conservative threshold for statistical significance was used (see below). These data represent the male subsample. Sample size for all analyses: $n = 7$ (MM), $n = 7$ (KM), $n = 8$ (MR), $n = 22$ (total sample).

† $p < .05$.

Appendix B: Digital Mental Trainings

All of the digital mindfulness meditations, kindness meditation, and music relaxation control practices used in this study, along with basic practice instructions, can be found here (permanently):
https://www.dropbox.com/sh/5whkku2hjwqyvl9/AAAnIVrURbOuJWbZh8ylJD9ca?dl=0

Appendix C: Training Feasibility and Acceptability Measure

Instructions: On average, how many times per week did you engage in a mindfulness or relaxation session during the 6-week training period? Please answer with your actual (not desired) behavior.

- ○ One Time Per Week
- ○ Two Times Per Week
- ○ Three Times Per Week
- ○ Four Times Per Week
- ○ Five Times Per Week
- ○ Six Times Per Week
- ○ Seven Times Per Week
- ○ More Than Seven Times Per Week (please specify the number)

_____

Instructions: On average, how many times per week did you engage in each specific practice during the 6-week training period? Please use the dropdown menu to answer with your actual (not desired) behavior.

8min Practice	○	1 Time
9min Practice	○	2 Times
20min Practice	○	3 Times
23min Practice	○	4 Times
	○	5 Times
	○	6 Times
	○	7 Times
	○	8 Times

- ○ 9 Times
- ○ 10 Times
- ○ 11 Times
- ○ 12 Times
- ○ 13 Times
- ○ 14 Times
- ○ 15 Times
- ○ 16 Times
- ○ 17 Times
- ○ 18 Times
- ○ 19 Times
- ○ 20+ Times

Instructions: During the 6-week training period, how often do you recall falling asleep during your practices?

- ○ Never
- ○ Very Rarely
- ○ Rarely
- ○ Sometimes
- ○ Often
- ○ Very Often
- ○ Always

Instructions: Please rate how you think the items listed below have changed for you due to your mental practices.

	Much Worse	Somewhat Worse	Little Worse	Neither Better Nor Worse	A Little Better	Somewhat Better	Much Better
Teaching Effectiveness	○	○	○	○	○	○	○
Teaching Stress	○	○	○	○	○	○	○
Teaching Patience	○	○	○	○	○	○	○

Instructions: Please rate the perceived impact of your mental practices on your job performance as compared to other professional training programs.

- ○ Much Lower
- ○ Somewhat Lower
- ○ A Little Lower
- ○ Neither Higher Nor Lower
- ○ A Little Higher
- ○ Somewhat Higher
- ○ Much Higher

Instructions: Please rate your likeliness to recommend your mental practices to a colleague or friend, using the scale below.

- ○ Not At All Likely
- ○ A Little Likely
- ○ Somewhat Likely
- ○ Very Likely

○ Highly Likely

Instructions: Regardless of why you signed up for this study, what kept you engaged in your practices each week? Please rate the reason from most to least motivating using the response options below.

	Not at All Motivating	A Little Motivating	Somewhat Motivating	Very Motivating	Highly Motivating
Enjoyment of The Practices	○	○	○	○	○

Appendix D: Positive Affect and Negative Affect Measure:
modified Differential Emotions Scale (mDES; Fredrickson et al., 2003)

Instructions: Please think about how you feel **in general (most of the time)**. Using the scale below, indicate how often you experience each of the following feelings.

0 = Never

1 = Rarely

2 = Some of the time

3 = Often

4 = Most of the time

1. How often do you feel **amused**, **fun-loving**, or **silly**?

2. How often do you feel **angry**, **irritated**, or **annoyed**?

3. How often do you feel **ashamed**, **humiliated**, or **disgraced**?

4. How often do you feel **awe**, **wonder**, or **amazement**?

5. How often do you feel **contemptuous**, **scornful**, or **disdainful**

6. How often do you feel **disgust**, **distaste**, or **revulsion**?

7. How often do you feel **embarrassed**, **self-conscious**, or **blushing**?

8. How often do you feel **grateful**, **appreciative**, or **thankful**?

9. How often do you feel **guilty**, **repentant**, or **blameworthy**?

10. How often do you feel **hate**, **distrust**, or **suspicion**?

11. How often do you feel **hopeful**, **optimistic**, or **encouraged**?

12. How often do you feel **inspired**, **uplifted**, or **elevated**?

13. How often do you feel **interested**, **alert**, or **curious**?

14. How often do you feel **joyful**, **glad**, or **happy**?

15. How often do you feel **love**, **closeness**, or **trust**?

16. How often do you feel **proud**, **confident**, or **self-assured**?

17. How often do you feel **sad**, **downhearted**, or **unhappy**?

18. How often do you feel **scared**, **fearful**, or **afraid**?

19. How often do you feel **serene**, **content**, or **peaceful**?

20. How often do you feel **stressed**, **nervous**, or **overwhelmed**?

Note. Positive affect (PA) items: 1, 4, 8, 11, 12, 13, 14, 15, 16, 19. Negative affect (NA) items: 2, 3, 5, 6, 7, 9, 10, 17, 18, 20.

Appendix E: Self-Report Mindfulness Measure:
Five Facet Mindfulness Questionnaire-Short Form (FFMQ-SF; Bohlmeijer et al., 2011)

Instructions: Please read each statement carefully. Then indicate the extent to which you think or behave in the stated manner using the response scale below.

	Never	Very Rarely	Rarely	Sometimes	Often	Very Often	Always
I watch my feelings without getting carried away by them.	○	○	○	○	○	○	○
I tell myself I shouldn't be feeling the way I'm feeling.	○	○	○	○	○	○	○
I pay attention to physical experiences, such as the wind in my hair or sun on my face.	○	○	○	○	○	○	○
I make judgments about whether my thoughts are good or bad.	○	○	○	○	○	○	○
I find it difficult to stay focused on what's happening in the present moment.	○	○	○	○	○	○	○
I experience myself as separate from my changing thoughts and feelings.	○	○	○	○	○	○	○

	Never	Very Rarely	Rarely	Sometimes	Often	Very Often	Always
When I have distressing thoughts or images, I let myself be carried away by them.	○	○	○	○	○	○	○
Generally, I pay attention to sounds, such as clocks ticking, birds chirping, or cars passing.	○	○	○	○	○	○	○
It seems I am "running on automatic" without much awareness of what I'm doing.	○	○	○	○	○	○	○
When I have distressing thoughts or images, I feel calm soon after.	○	○	○	○	○	○	○
I tell myself that I shouldn't be thinking the way I'm thinking.	○	○	○	○	○	○	○

	Never	Very Rarely	Rarely	Sometimes	Often	Very Often	Always
I rush through activities without being really attentive to them.	○	○	○	○	○	○	○
When I have distressing	○	○	○	○	○	○	○

thoughts or images I can just notice them without reacting.							
I think some of my emotions are bad or inappropriate and I shouldn't feel them.	○	○	○	○	○	○	○
I notice visual elements in art or nature, such as colors, shapes, textures, or patterns of light and shadow.	○	○	○	○	○	○	○
When I have distressing thoughts or images, I just notice them and let them go.	○	○	○	○	○	○	○

	Never	Very Rarely	Rarely	Sometimes	Often	Very Often	Always
I do jobs or tasks automatically without being aware of what I'm doing.	○	○	○	○	○	○	○
I find myself doing things without paying attention.	○	○	○	○	○	○	○
I disapprove of myself when I have illogical ideas.	○	○	○	○	○	○	○

Please click "Sometimes" for this item.	○	○	○	○	○	○	○
I notice the smells and aromas of things.	○	○	○	○	○	○	○
I experience my thoughts as events in my mind, rather than as an accurate reflection of the way things 'really' are.	○	○	○	○	○	○	○

Appendix F: Self-Report Emotional Awareness Measure

Instructions: Please read each statement carefully, and use the rating scale below to indicate how truly or falsely each statement describes you. Respond in regard to how **you generally are most of the time**, not as you wish to be.

1 = Completely False

2 = Mostly False

3 = Slightly False

4 = Neither True nor False

5 = Slightly True

6 = Mostly True

7 = Completely True

Self-Oriented Emotion Awareness:

1. I am rarely confused about what emotion I am feeling. [TAS-20]
2. When I am upset, I don't know if I am sad, frightened, or angry. (r) [TAS-20]
3. When I am glad, I know if I am excited, amused, or content. [JLF based on TAS-20]
4. I have feelings that I can't quite identify. (r) [TAS-20]
5. I often know why I am in a bad mood. [TAS-20 adapted by JLF]
6. I often don't know why I am happy. (r) [JLF based on TAS-20]

Other-Oriented Emotion Awareness:

7. By looking at people's facial expressions, I recognize the emotions they are experiencing. [SREIS]
8. I am unaware of the nonverbal messages other people send. (r) [SREIS]
9. My quick impressions of what people are feeling are usually wrong. (r) [SREIS]
10. I am aware of the non-verbal messages I send to others. [AES]
11. I don't know what other people are feeling just by looking at them. (r) [AES]
12. I can tell how people are feeling by listening to the tone of their voice. [AES]

Note: AES = items from the Perception of Emotion subscale of the Assessing Emotions Scale (Schutte et al., 1998, 2009); JLF = James Lyman Floman; SREIS = items from the Perceiving Emotion subscale of the Self-Rated Emotional Intelligence Scale (Brackett et al., 2006); TAS-20 = items from the Difficulty Identifying Feelings subscale of the Twenty-Item Toronto Alexithymia Scale (Bagby, Parker, & Taylor, 1994); r = reverse coded, i.e., higher scores represent lower emotional awareness.

Appendix G: Self-Report Emotional Regulation Measure

Instructions: Please read each statement carefully, and use the rating scale below to indicate how truly or falsely each statement describes you. Respond in regard to how **you generally are most of the time**, not as you wish to be.

1 = Completely False

2 = Mostly False

3 = Slightly False

4 = Neither True nor False

5 = Slightly True

6 = Mostly True

7 = Completely True

Self-Oriented Emotion Regulation:

1. I have control over my emotions. [AES]
2. When I experience a positive emotion, I cannot make it last long. (r) [AES adapted by JLF]
3. I often feel overwhelmed by my emotions. (r) [JLF based on AES]
4. I can create or enhance positive moods in myself. [JLF based on SREIS]
5. I can handle difficult or stressful situations without getting too nervous. [SREIS]
6. I have problems dealing with my feelings of anger. (r) [SREIS]

Other-Oriented Emotion Regulation:

7. I can help other people feel better when they are sad or down. [AES adapted by JLF]
8. I share my emotions with others, so they are comfortable sharing their emotions with me. [AES adapted by JLF]
9. When someone I know is in a bad mood, I cannot help the person feel better. (r) [SREIS adapted by JLF]
10. I can create or enhance positive moods in other people. [SREIS adapted by JLF]
11. I am bad at helping others to continue to feel good when they are happy. (r) [SREIS adapted by JLF]
12. I am not the type of person to whom others go when they need help with a stressful situation. (r) [SREIS adapted by JLF]

Note: AES = items from the Perception of Emotion subscale of the Assessing Emotions Scale (Schutte et al., 1998, 2009); SREIS = items from the Perceiving Emotion subscale of the Self-Rated Emotional Intelligence Scale (Brackett et al., 2006); JLF = James Lyman Floman; r = reverse coded, i.e., higher scores represent lower emotion awareness.

Appendix H: Emotional Awareness and Regulation Task

Summary and screenshots of pretest frustration-induction video:

The approximately 1.5-minute video depicts a new teacher (female) interacting with a seasoned principal (female). The teacher is trying to get access to books for her students that she thinks are interesting and challenging. However, the principal strongly dissuades her from doing so because she thinks the students don't want to read and will just damage the materials anyway. This dialogue goes back and forth with the new teacher demonstrating persistence and hope and the principal demonstrating rigidity and cynicism. The video is intended to make participants (who are teachers) feel frustrated.

Summary and screenshots of posttest frustration-induction video:

The approximately 2-minute video depicts a new teacher (female) interacting with two senior teachers (one female and one male) in front of two administrators (a black and a white male). The issue at hand is whether the new teacher can stay with her students into the following school year to continue the great strides they've made and bonds they've developed. The two senior teachers, however, would be adversely affected, that is, have to give up teaching honors classes ("students who want to learn") to help the new teacher (who they clearly do not like). The new teacher and the seasoned teachers argue in front of the administrators and the request of the new teacher is effectively denied. The video is intended to make participants (who are teachers) feel frustrated. Importantly, the same two individuals from the first video are present, to keep the stimuli consistent; however, there are also enough new participants and conflict to trigger real anger.


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Student essay evaluated at pretest:

Fine Arts

"Fine arts are important in curriculum because of what they do for learning," stated Patty Taylor, arts consultant for the California state department of Education. So, the arts, especially music, should be part every of schools curriculum at every grade level. Music is important for a numbers of reasons. Music makes students lots smarter gives children something positive to do building selfconfidence. Most students don't have a chance to learn music outside of school though, and we all should get that opportunty Students would be much more smarter of they had some music experience. They would improve their classroom skills, like paying attention, following directions, and not interrupting

others so much. Musicians are aslo better in math. They get higher S.A.T. scores show studies. For instance, a study by the College Entrance Examination Board reported, "Students with 20 units of arts and music scored 128 points higher on the S.a.T. verbal and 118 points higher in math." Music making is good for children's imaginations also, un-like playing with the video games and electronical stuffed animal. Clearly, music is so good for our minds all of these times. Music only not makes children better students but also gives them something good to do.. In a music program, children can be part of a band or choir instead joining for the gangs, or do both. In band students get to be part of a team, which is cool. While learning and making music, children can also be exploring careers for the future that so matters. Though, true, music is one hard career to make. Music can builds self-confidence. It gives students a sense of accomplishment and success. Making music is something for students to be proud of, and it lets kids practice performing in front of a audience. Imporatntly, there are studies and experts supporting of his idea. Once again, music is important cause it can make children better students, give them something positive to be doing, and build up their selfesteems. Kids who need music lesons the most usually don't have access to it outside of school though. That is why music should be offered in every single grade in every school, What do you want for you children?

Student essay evaluated at posttest:

Summer: 15 Days or 2 1/2 Months?

Schools should continue using the typical calendar and not a year round calender? Their are many downsides to year-round schooling. It doesn't have no positive effects on education, it adds to costs, and it ruins long-awaited summer vacations. This is clear. Year round schooling has no good impacts in education. Most year-round schedules follow the 45-15 method: 45 days of school followed by fifteen days off. Because of this their are some many first and final days of school. Many transitions make problems for the learning process. Also, there isn't a lack of evidence of higher test scores despite what people believe. Due to that, since 1980, 95 of schools that tried the year-round schedule changed back to a regular calendar. It is obvious that changing to year round schooling doesn't help students. So we do not need the change I feel. Keeping a school open costs a great deal. When a school changes to a year-round schedule the costs jump big. Keeping school open in the summer equals airconditioning, and that adds much to the school's expenses. Utility bills will grow too cause of the additional open-school times. With these factors, the cost of keeping schools open becomes bad. Year round school is not where I like to spend money. Summer is excellent. Year-round schedules would make students hardly have any time to be relaxing during the 15 day breaks, they would be thinking about returning to school and corridinating family vacations with parents work will be hard. And, what about summercamp ? Expert Dr. Peter Scales says: "The biggest plus of camp is that camps help young people discover and explore their talents, interests, and values. Most schools don't satisfy all these needs. Kids who have these kinds of experiences end up being healthier and have fewer problems." Why ruin this terrific part of some many children's lives? Surely you can see that year-round schooling is not the best option for the school calendar. That now is something that you

clearly have to see. There's nothing wrong with the traditional school year as it is. Its not broken, so it doesn't need fixing.

Essay Evaluation Questions:

Instructions: Please answer the questions below regarding the Grade 6 student essay you just read.

	Very Poor	Somewhat Poor	Average	Above Average	Very Good	Exemplary
How would you rate the overall quality of the student's essay?						
How would you rate the student's writing abilities?						

Instructions: Please answer the questions below regarding the Grade 6 student essay you just read.

	Not At All Creative	A Little Creative	Moderately Creative	Very Creative	Extremely Creative
How creative was the student's essay?					
	Not At All Accurate	A Little Accurate	Moderately Accurate	Very Accurate	Extremely Accurate
How accurate was the student's spelling and punctuation?					
	Not At All Organized	A Little Organized	Moderately Organized	Very Organized	Extremely Organized
How well organized was the student's essay?					
	Not At All	A Little	Somewhat	Very	Extremely

	Diverse	Diverse	Diverse	Diverse	Diverse
How diverse was the student's range of vocabulary?					
	Not At All Persuasive	A Little Persuasive	Somewhat Persuasive	Very Persuasive	Extremely Persuasive
How persuasive was the student's essay?					

Appendix I: Self-Report Compassion Measure
The Interpersonal Reactivity Index (IRI; Davis, 1983)

Instructions: The following statements inquire about your thoughts and feelings in a variety of situations. For each item, indicate how well it describes you by choosing the appropriate response on the scale below. Read each item carefully before responding, and answer as honestly as you can.

	1 = Does Not Describe Me Well	5 = Describes Me Very Well
I often have tender, concerned feelings for people less fortunate than me.		
I sometimes find it difficult to see things from the "other person's" point of view.		
Sometimes I don't feel very sorry for other people when they are having problems.		
In emergency situations, I feel apprehensive and ill-at-ease.		
I try to look at everybody's side of a disagreement before I make a decision.		
When I see someone being taken advantage of, I feel kind of protective towards them.		
I sometimes feel helpless when I am in the middle of a very emotional situation.		
I sometimes try to understand my friends better by imagining how things look from their perspective.		
When I see someone get hurt, I tend to remain calm.		
Other people's misfortunes do not usually disturb me a great deal.		
If I'm sure I'm right about something, I don't waste		

much time listening to other people's arguments.

Being in a tense emotional situation scares me.

When I see someone being treated unfairly, I sometimes don't feel very much pity for them.

I am usually pretty effective in dealing with emergencies.

I am often quite touched by things that I see happen.

I believe that there are two sides to every question and try to look at them both.

I would describe myself as a pretty soft-hearted person.

I tend to lose control during emergencies.

When I'm upset at someone, I usually try to "put myself in their shoes" for a while.

When I see someone who badly needs help in an emergency, I go to pieces.

Before criticizing somebody, I try to imagine how I would feel if I were in their place.

Appendix J: Compassion Task:
Multifaceted Empathy Test–CORE (MET-CORE) (Edele, Dziobek, & Keller, 2013)

Example image, text, and response series (there are 40 in total):

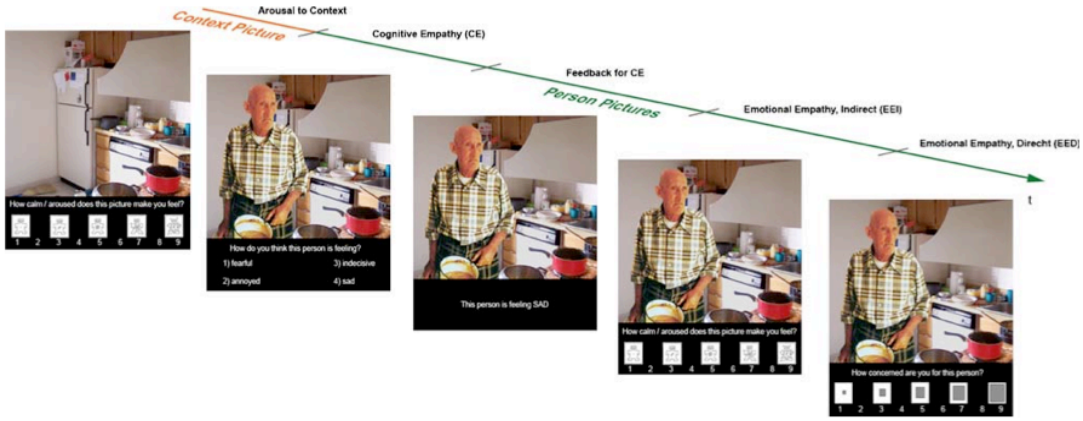
First Image Text: "How calm / aroused does this picture make you feel?" Answer options 1-9 (very calm to highly aroused).

Second Image Text: "How do you think this person is feeling?" Answer options: "fearful", "annoyed", "sad", or "indifferent".

Third Image Text: "The person is feeling SAD".

Fourth Image Text: "How calm / aroused does this picture make you feel?" Answer options 1-9 (very calm to highly aroused).

Fifth Image Text: "How concerned are you for this person?" Answer options: 1-9 (not concerned to very concerned). [***The present study only used the 5th set of images.***]



Appendix K: Prosocial Economic Games

The game instructions, as well as example prosocial games for each of the four game types, are presented on the following pages exactly as participants saw them online.

Instructions for the Prosocial Economic Game Battery

Decision-Making Game Instructions:

You will be playing a series of online decision-making games for **real money**. The rules of these games are listed below.

1. Your decisions in the games will determine the **actual money** you and the other players take home at the end of the study.

2. You will take home money from **only one game** that's randomly selected out of the 10 games you play.

3. Your decision in one game **will not affect** your decisions in other games.

4. Other players' responses were logged earlier in the week when others were available to play.

5. Your decisions **will not affect** your $125 honorarium for study participation.

If you have carefully read and understand these instructions, please write "Yes" in the box below, and click "Next" to begin.

Type here

Dictator Game (DG; Altruistic Giving Game)

Game Decision Information: Click Here to see Game Rules

You are playing a new game with new players (players not from prior games).

In this game you are Player C.

There is one other person in this game: Player A.

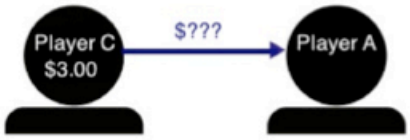
Your Decision:

As Player C, you now have $3.00.

You have the option to **transfer** any amount of your funds - from $0.00 to 3.00 - to **Player A**.

Any remaining funds you will **keep**.

Remember this is for **real money**.



Please enter the dollar amount you wish to transfer to Player A - ranging from $0.00 to $3.00 - in the box below:

$

Third-Party Game Unfair Round (TPG-Unfair; Compassionate Giving Game):

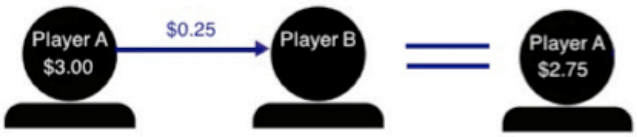
Game Decision Information: Click Here to see Game Rules

Earlier in the week Player A was given $3.00.

Player A had the option to **transfer** any amount of their funds - from $0.00 to 3.00 - to Player B.

Any remaining funds Player A could **keep**.

Player A chose to **transfer $0.25** to Player B, and to **keep the rest: $2.75**.



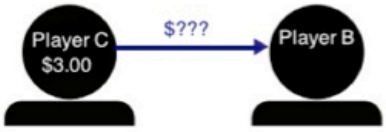
Your Decision:

As Player C, you now have $3.00.

You have the option to **transfer** any amount of your funds - from $0.00 to 3.00 - to **Player B**.

Any remaining funds you will **keep**.

Remember this is for **real money**.



Please enter the dollar amount you wish to transfer to Player B - ranging from $0.00 to $3.00 - in the box below:

$

Third-Party Game Fair Round (TPG-Fair; Altruistic Giving Game)

Game Decision Information: Click Here to see Game Rules

You are playing a new game with new players (players not from prior games).

Earlier in the week Player A was given $3.00.

Player A had the option to **transfer** any amount of their funds - from $0.00 to 3.00 - to Player B.

Any remaining funds Player A could **keep**.

Player A chose to **transfer $2.25** to Player B, and to **keep the rest: $0.75**.

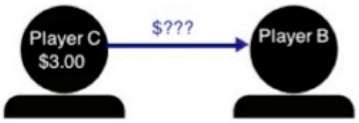
Your Decision:

As Player C, you now have $3.00.

You have the option to **transfer** any amount of your funds - from $0.00 to 3.00 - to **Player B**.

Any remaining funds you will **keep**.

Remember this is for **real money**.



Please enter the dollar amount you wish to transfer to Player B - ranging from $0.00 to $3.00 - in the box below:

Punishment Game (PG; Altruistic Punishment Game)

Game Decision Information: Click Here to see Game Rules

You are playing a new game with new players (players not from prior games).

Earlier in the week Player A was given $3.00.

Player A had the option to **transfer** any amount of their funds - from $0.00 to 3.00 - to Player B.

Any remaining funds Player A could **keep**.

Player A chose to **transfer $0.25** to Player B, and to **keep the rest: $2.75**.

Your Decision:

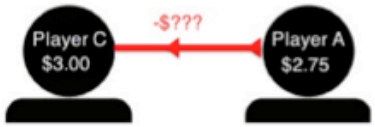
As Player C, you now have $3.00.

You have the option to **take away** any amount of **Player A's** funds from $0.00 to $2.75.

The same amount that you **take away** from Player A you will also **lose.** For example:

If you **take away** $1.00 from Player A, you will **lose** $1.00, and you will **keep** the rest: $2.00.

Remember this is for **real money**.



Please enter the dollar amount you wish to take away from Player A - ranging from $0.00 to $2.75 - in the box below:

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