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ORIGINAL PAPER

Implications of a "Null" Randomized Controlled Trial of Mindfulness and Compassion Interventions in Healthy Adults

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Abstract

Objectives Extensive research suggests that short-term meditation interventions may hold therapeutic promise for a wide range of psychosocial outcomes. In response to calls to subject these interventions to more methodologically rigorous tests, a randomized controlled trial tested the effectiveness of a mindfulness meditation intervention and a compassion meditation intervention against an active control in a demographically diverse sample of medically and psychiatrically healthy adults.

Methods Two hundred and four participants completed a battery of questionnaires to assess psychological experience, participated in a laboratory stress test to measure their biological stress reactivity, and wore the Electronically Activated Recorder (EAR) to assess daily behaviors before and after an eight-week intervention (mindfulness meditation intervention, compassion meditation intervention, or health education discussion group).

Results Neither meditation intervention reliably impacted participants' subjective psychological experience, biological stress reactivity, or objectively assessed daily behaviors. Furthermore, post hoc moderation analyses found that neither baseline distress nor intervention engagement significantly moderated effects.

Conclusions Results from this trial—which was methodologically rigorous and powered to detect all but small effects—were essentially null. These results are an important data point for the body of research about meditation interventions. Implications of these non-significant effects are discussed in the context of prior studies, and future directions for contemplative intervention research are recommended.

Clinical Trial Registry Registry Number: NCT01643369.

Keywords Meditation · Mindfulness · Compassion · Stress · Inflammation · Behavior

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Scholarly interest in the utility of meditation interventions for healthy adults has grown in recent years. On the one hand, numerous controlled efficacy studies have documented salutary effects of short-term meditation programs on a broad range of psychosocial outcomes (Creswell, 2017; Hofmann et al., 2011; Khoury et al., 2015; Shapiro & Jazaieri, 2015). On the other hand, the scientific literature is nascent, and there are concerning methodological limitations in the current evidence base (Davidson & Kaszniak, 2015; Dimidjian & Segal, 2015; Goyal et al., 2014). As one illustrative example of these limitations, prior research found that engagement with compassion meditation in young adults was associated with reduced inflammatory and behavioral responses to a standard laboratory stressor (Pace et al., 2013), suggesting that compassion meditation interventions may reduce stress-provoked immune and behavioral responses. However, this promising finding was subject to many of the same limitations found widely in the literature: a demographically homogenous sample, a mono-method measurement approach, and a reliance on laboratory tasks that may have limited generalizability to people's actual daily lives. As aptly pointed out by Dimidjian and Segal (2015), the lack of published data on the boundary conditions of meditation interventions—that is, under what circumstances they fail to outperform their comparator intervention—is an impediment to progressing scientific understanding of when, how and for whom meditation interventions "work best."

The current research aimed to respond to calls to subject mindfulness and compassion meditation interventions to increasingly rigorous testing. This work was therefore informed by several methodological critiques of the current literature. First, meta-analyses have noted that many reviews identifying strong effects of meditation interventions have predominantly included studies that did not use control groups (Creswell, 2017; Dimidjian & Segal, 2015; Goyal et al., 2014), rendering them unable to adequately control for placebo effects and underscoring the importance of active control conditions in meditation research. A second criticism concerns the measurement approaches used in the existing body of research. Trials of meditation interventions have frequently been limited to assessing psychosocial outcomes using subjective and self-reported methods only (Davidson & Kaszniak, 2015). Multi-method research approaches may be important given that self-report measures of behavioral constructs are especially vulnerable to error introduced by dissonance reduction (e.g., participants believing that they have become calmer or kinder because they invested time and money into increasing these qualities but, potentially, without experiencing any real change). Studies that only utilize self-report measures are also limited by demand characteristics (e.g., participants biasing responses in the direction they expect researchers hope to find), social desirability effects, recall biases, and shared-method variance (e.g., mindfulness measures sharing semantic and stylistic features with measures of wellbeing and personality) (Mehl & Conner, 2012; Podsakoff et al., 2012). As a result, the rigorous evaluation of whether meditation actually impacts *objectively assessed* real-world behavior in healthy individuals is an unmet research need.

Another critique noted in the extant literature is that the term "meditation" covers a range of disparate techniques. It is unknown how each of these techniques corresponds to the various physiological, behavioral, and psychological changes that people hope to obtain when undergoing meditation training (Van Dam et al., 2018). Dismantling studies attempting to isolate and test aspects of meditation techniques against one another have found notable differences: for example, one recent dismantling study found that acceptance and monitoring mindfulness techniques in combination (as contrasted to monitoring techniques only) most strongly predicted reductions in cortisol and blood pressure reactivity (Lindsay et al., 2018). Nonetheless, to date, few large-scale studies have directly compared one type of meditation to another or have attempted to isolate and test one component of a meditation program against another. Conducting large-scale, mechanism of action–focused studies is essential for understanding the differential effects of meditation practices.

Motivated by these critiques, this randomized controlled trial used a multi-method design to test the effects of an eight-week mindfulness meditation intervention (Mindful Attention Training; MAT), an eight-week compassion meditation intervention (Cognitively Based Compassion Training; CBCT®), and an active control (health education discussion group) in medically and psychiatrically healthy adults. The study aimed to recruit a demographically diverse sample and was sufficiently powered to detect medium or greater effects. The aim of the study was to examine the effects of the meditation interventions (as compared to the active control) at multiple levels of analysis: (1) psychological experience (assessed through self-report questionnaires), (2) biological stress reactivity (assessed through inflammatory changes following a standardized laboratory psychosocial stress task), and (3) real-world daily social behaviors and interactions (assessed via naturalistic observation using the Electronically Activated Recorder or EAR). It was hypothesized that, compared to the active control, both meditation conditions would reduce subjective psychological distress, increase subjective psychological wellbeing, decrease biological stress reactivity, and increase objectively observed daily prosocial and affiliative behavior with established links to wellbeing (e.g., expression of gratitude and affection; engagement in meaningful conversations). Although not formally pre-registered, these hypotheses are documented in the clinicaltrials.gov registration.

Method

Participants

Participants were 204 medically and psychiatrically healthy adults living in Atlanta, GA ($M_{age}=33.69$, $SD=8.40$, 65.7% identifying as female, 34.3% identifying as male, 0% identifying as any other gender category), who completed at least one of the pre-intervention assessments and were randomized. Participant flow through the study protocol is shown in the CONSORT Flow Diagram (Supplemental Fig. 1). Fifty percent of participants reported being White, 34.3% African American, 7.4% Asian, 4.9% Hispanic, 1.0% Native American or Alaska Native, 1.0% Native Hawaiian or Other Pacific Islander, and 1.5% Other. Regarding socioeconomic status, 30.4% of participants reported $0–$25,000 in annual household income, 34.8% of participants reported $25,000–50,000 in annual household income, 23.5% of participants reported $50,000–$100,000 in annual household income, and 9.3% reported an annual household income of $100,000 or more. Over half of the participants (51.6%) reported completing at least one year of graduate education. A total of 28.2% of participants had a college degree only, and 20.4% reported a high school diploma or some college as their highest level of educational attainment.

Participants were recruited from the metropolitan Atlanta, GA, area. Recruitment procedures aimed to reflect how meditation classes are typically advertised (i.e., through flyers in public places such as coffee shops and bus stops). Interested individuals were screened and excluded if they met any of the following criteria: any chronic medical condition; a current major depressive episode; lifetime history of schizophrenia or bipolar I disorder; current substance abuse; current psychological or psychiatric treatment, including regular psychotherapy and psychotropic medications such as antidepressants, mood stabilizers, antipsychotics, and psychostimulants; current medications that might interfere with the physiological measures included in this study, such as beta-blockers, corticosteroids, and anti-inflammatory drugs; and any current or prior meditation practice. All participants for whom valid data were available were included in an intent-to-treat analysis of study outcomes.

Procedures

Study procedures are illustrated in Fig. 1. All participants were randomized with one-to-one allocation to one of three intervention conditions as described below: a compassion meditation condition, in which they received eight weeks of Cognitively Based Compassion Training (CBCT®), a mindfulness meditation condition, in which they received eight weeks of Mindful Attention Training (MAT), or an active control condition consisting of an eight-week health education discussion group. Prior to learning of their randomization status or commencing the interventions, all participants (1) first completed a comprehensive battery of self-report questionnaires tapping into potentially important experiential outcomes of meditation programs, then (2) provided blood via an intravenous catheter for the collection of biological stress reactivity data (cortisol and IL-6) before, during and after participating in the Trier Social Stress Test (TSST) (Von Dawans et al., 2011), and then (3) wore the Electronically Activated Recorder (EAR) (Kaplan et al., 2020; Mehl, 2017) for one weekend to provide naturalistic observation data about their daily social behaviors and interactions. After receiving the eight-week interventions, participants underwent the same self-report, biological stress reactivity, and EAR assessment again as a post-intervention measure. For an efficient and readable presentation of findings, key outcome variables were selected for the present

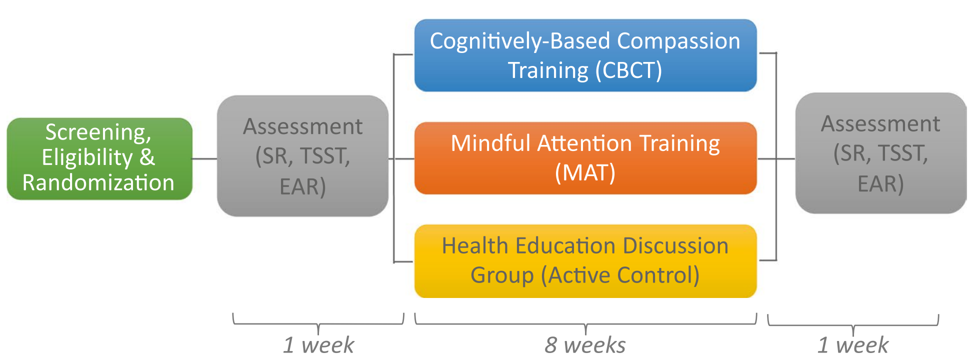

Fig. 1 Overview of the Study Procedures *Note*. SR= completion of a comprehensive battery of self-reported psychological measures; TSST = collection of biological stress reactivity data before and after participating in the Trier Social Stress Test; EAR = wearing the Electronically Activated Recorder for one weekend to provide naturalistic observation data about daily social behaviors and interactions.

paper from the very large number of available self-report and EAR measures (all biological measures are reported). However, data for all available self-report, biological stress reactivity, and EAR variables are available on OSF at: https://osf.io/6jt9q/.

Interventions

Interventions compared in the present trial are described below, and complete protocols for these interventions are published elsewhere (Desbordes et al., 2012). Classes for all three conditions met for eight weeks. The first cohort of participants for all conditions attended class for one hour twice per week (first 45 participants); following the first cohort, this was subsequently changed to a two-hour class once per week to reduce participant burden.

Cognitively-Based Compassion Training

Cognitively-Based Compassion Training (CBCT®) is a compassion meditation program derived from Tibetan Buddhist mind-training (Tibetan *lojong*) practices. The program was developed by Geshe Lobsang Tenzin Negi, Ph.D., senior instructor in the Department of Religion at Emory University and spiritual director of Drepung Loseling Monastery, Inc. in Atlanta. CBCT® differs in important ways from mindfulness-based practices, including Mindful Attention Training (MAT), which typically emphasize the cultivation of nonjudgmental awareness towards one's internal experience. CBCT® incorporates this stance and includes instruction in mindfulness techniques but additionally uses a cognitive, analytic approach to challenge one's unexamined thoughts and emotions and to develop increased compassion towards oneself and others. Practices introduced in CBCT® include mindfulness of breath, mindfulness of mental processes, self-compassion, equanimity, gratitude, and compassion for others. Within healthy populations, CBCT® has been found to increase empathic accuracy (Mascaro et al., 2012) and to reduce self-reported anxiety and depression with corresponding changes in amygdala responses to negative images (Desbordes et al., 2012). In prior studies, engagement with CBCT® has also been found to associate with improved psychosocial functioning (Reddy et al., 2013), reduced C-reactive protein in adolescents in foster care (Pace et al., 2013), and reduced inflammatory responses to stress (Pace et al., 2009). CBCT® has also been found to improve psychological symptoms in a pilot study of breast cancer survivors (Dodds et al., 2015).

For the current study, CBCT® classes met for eight weeks on the same weeknight. Each CBCT® class included guided meditation, pedagogical instruction for connecting the meditation practices to daily life, and discussion. Classes were delivered by teachers certified in CBCT® through a one-year teacher training and certification process overseen by Geshe Lobsang Tenzin Negi, Ph.D.

Mindful Attention Training

Mindful Attention Training (MAT) (Shonin et al., 2014) was developed by B. Alan Wallace, Ph.D., who modified the technique for the current study to eliminate any teaching elements explicitly related to compassion, in order to maximally separate the two conditions and avoid overlap with the compassion training of CBCT®. This allowed for examination of any potential differences between an intervention that includes instruction in mindfulness meditation only (MAT), as contrasted to an intervention that includes instruction in both mindfulness meditation and compassion meditation (CBCT®)—although, notably, the study design did not include an intervention condition that provided instruction in compassion meditation without mindfulness meditation.

In the present study, MAT was taught as an eight-week program in which participants practice sitting meditation techniques for enhancing mindful awareness of one's internal state and external environment. Three categories of meditative techniques were covered over the eight weeks: mindfulness of breath (in which participants are asked to direct nonjudgmental attention to the experience of breathing), mindfulness of mental processes (in which participants are asked to direct nonjudgmental attention to thoughts and other mental activity as they naturally arise), and mindfulness of awareness (in which awareness itself becomes the object of meditation). The meditation practices taught in the MAT program bear many similarities to other widely used mindfulness meditation protocols, including Mindfulness-Based Stress Reduction (MBSR; Santorelli et al., 2017). MAT was selected over MBSR for the present study because MBSR includes practices beyond sitting meditation, including yogic movements and a body scan practice that is completed lying down. Thus, MAT was a more suitable match for CBCT® in the context of a randomized-controlled trial. Prior research has found that within healthy populations, mindfulness meditation training reduces psychological distress, increases psychological wellbeing, increases relationship satisfaction, and increases empathy and other indicators of positive interpersonal relating (Brown et al., 2007, 2015; Shapiro & Jazaieri, 2015).

MAT classes met for eight weeks on the same weeknight. Each class included guided meditation, pedagogical instruction for connecting the practices to daily life, and discussion. MAT classes were delivered by teachers trained in the technique by Dr. Wallace.

Health Education Discussion Group

The active control intervention was adapted from a health education class originally developed for university students by Daniel D. Adame, MSPH, Ph.D., CHES, retired Associate Professor of Health Education at Emory University. The class covered a different topic relevant to health and wellbeing each week, including exercise, nutrition, navigating the healthcare system, maintaining healthy relationships, and effective communication. Matching the format for CBCT® and MAT, the health education discussion group met for eight weeks on the same week night. Classes included pedagogical instruction as well as small group discussion and exercises (e.g., role-playing). Each class was taught by Master's in Public Health (MPH) students who were fully convinced of the utility of this intervention.

Measures

Data were collected about participants' (1) psychological experience (survey measures), (2) biological stress reactivity (endocrine and inflammatory responses to the TSST), and (3) observed daily behavior and language use (EAR assessment).

Psychological Experience

Participants completed a comprehensive battery of psychosocial questionnaires before (time 1) and after (time 2) receiving the study interventions to provide data about their subjective psychological experience. These questionnaires included the Beck Anxiety Inventory (BAI) (Fydrich et al., 1992), Cronbach's $\alpha_{T1}=0.86$, McDonald's $\omega_{T1}=0.83$, Cronbach's $\alpha_{T2}=0.85$, McDonald's $\omega_{T2}=0.85$; the Beck Depression Inventory (BDI) (Beck & Steer, 1984), Cronbach's $\alpha_{T1}=0.89$, McDonald's $\omega_{T1}=0.89$, Cronbach's $\alpha_{T2}=0.89$, McDonald's $\omega_{T2}=0.90$; the Five Facet Mindfulness Questionnaire (FFMQ) (Baer et al., 2006), Cronbach's $\alpha_{T1}=0.92$, McDonald's $\omega_{T1}=0.91$, Cronbach's $\alpha_{T2}=0.91$, McDonald's $\omega_{T2}=90$; the Perceived Stress Scale (PSS) (Cohen, 1988a, 1988b), Cronbach's $\alpha_{T1}=0.86$, McDonald's $\omega_{T1}=0.83$, Cronbach's $\alpha_{T2}=0.88$, McDonald's $\omega_{T2}=0.88$; the Satisfaction with Life Scale (SWLS) (Diener et al., 1985), Cronbach's $\alpha_{T1}=0.85$, McDonald's $\omega_{T1}=0.85$, Cronbach's $\alpha_{T2}=0.89$, McDonald's $\omega_{T2}=0.89$; and the UCLA Loneliness Scale (Russell et al., 1980), Cronbach's $\alpha_{T1}=0.91$, McDonald's $\omega_{T1}=0.91$, Cronbach's $\alpha_{T2}=0.90$, McDonald's $\omega_{T2}=0.90$.

Biological Stress Reactivity

To assess biological stress reactivity, participants completed a standardized laboratory psychosocial stress task, the Trier Social Stress Test (TSST). The TSST reliably increases circulating cortisol and inflammatory biomarkers via the induction of social evaluative threat (Dickerson et al., 2009; Kirschbaum et al., 1993; Marsland et al., 2017). The TSST consists of an instruction/anticipation phase followed by a 5-min public speaking task and a 5-min mental arithmetic task. To allow for acclimatization and other assessments (e.g., physical exam, self-report questionnaires) participants arrived at the Atlanta Clinical and Translational Science Institute at Emory University Hospital 6 h before the start of the TSST. Participants were provided a low-fat lunch 2.5 h prior to the TSST, and an IV catheter was placed in the antecubital vein 1 h before the start of the TSST. To maintain the social evaluative threat of the task the second time participants completed it (at post-assessment), participants were told that their performance on the first TSST was at or below the 50th percentile compared to others in the study. Participants were also informed of the lowest number they achieved on the mental arithmetic task. They were told that the second TSST was a chance to improve on their prior poor performance. Based on prior findings demonstrating a time-lag in IL-6 responses (Marsland et al., 2017; Pace et al., 2006), IL-6 concentrations were assessed in plasma collected immediately before the TSST and then again at 90 and 210 min after the start of the challenge. Cortisol was assessed in plasma collected immediately before the TSST and then again every 15 min, until 90 min after the start of the TSST. Whole blood was collected from an indwelling catheter into EDTA-coated monovettes and immediately centrifuged at 4 °C, and plasma was aliquoted and stored at -80 °C until batch assay. Concentrations of IL-6 were determined using high-sensitivity enzyme-linked immunosorbent assay (ELISA) kits from R&D Systems (Minneapolis, MN), and cortisol using ELISA kits from IBL International (Hamburg, Germany) according to manufacturer instructions. Intra and inter-assay coefficients of variability for IL-6 were 5.7% and 12.8%, respectively, and for cortisol were 4.7 and 5.4, respectively.

Cortisol and IL-6 responses to the TSST were evaluated according to the area under the curve (AUC) methodology. Prior findings demonstrate that different but comparably important patterns of findings are obtained when calculating cortisol and IL-6 AUC both from the initial value (AUC*i*) or from the zero (or ground) value (AUC*g*) (Pruessner et al., 2003). Thus, in the present study both AUC*i* and AUC*g* metrics for cortisol and IL-6 were computed using the trapezoidal formula.

Observed Daily Behavior and Language Use

Participants wore the EAR before (time 1) and after (time 2) receiving the intervention. The EAR is a digital audio recorder worn by participants that captures snippets of ambient sound from the wearer's environment, yielding an "acoustic diary" of participants' daily activities, behaviors, and social interactions (Kaplan et al., 2020; Mehl, 2017; Mehl et al., 2001). This ecological behavioral observation method has been used in a wide range of both clinical and healthy populations, ranging from childhood to old age (Mehl, 2017). In prior research, it has been used successfully to study behavioral correlates for a wide range of psychological phenomena, including behavioral manifestations of mindfulness, daily moral behaviors, and social behaviors during a depressive episode, among numerous others (Baddeley et al., 2013; Bollich et al., 2016; Kaplan et al., 2018).

Participants wore the EAR for one weekend (Friday afternoon through Monday morning) prior to receiving an intervention, and for a second weekend following the end of the intervention. The EAR recorded 50 seconds every 9 minutes (initial 90 participants) or 30 seconds every 12 minutes. This study protocol adjustment was implemented to render the coding and transcription process more feasible; in simulation analyses, effect estimates were found to be robust to changes in the sampling rate (Mehl et al., 2012). Trained research assistants then transcribed all files for the derivation of speech variables and coded all sound files for aspects of participants' location, activities, social interactions and affect. All sound files were fully and independently double-coded. Verbatim transcripts were processed with Linguistic Inquiry and Word Count (LIWC) 2015 (Pennebaker et al., 2015) to generate measures of language content and style.

Of all coded behavioral and text-analytically derived speech variables, nine variables comprising four theoretically important domains were selected. These domains and associated variables were as follows: *perceptual orientation* (comprised of text-analytically derived perception word use), *emotional orientation* (comprised of text-analytically derived positive emotion word use and negative emotion word use), *interpersonal orientation* (comprised of behaviorally coded interaction quantity, ICC[1,2] = 0.98 and interaction quality, ICC[1,2] = 0.67), and *prosocial orientation* (comprised of behaviorally coded gratitude, ICC[1,2] = 0.66, affection, ICC[1,2] = 0.81, gossip, ICC[1,2] = 0.67, and complaining, ICC[1,2] = 0.56). These variables and domains were selected as primary targets because of their relevance to contemplative interventions and their demonstrated validity in prior research. In addition, these variables have previously been tested as key behavioral domains for the manifestation of dispositional mindfulness (Kaplan et al., 2018). These ICC statistics are consistent with prior studies and reflect acceptable reliability for real-world behavioral coding (Heyman et al., 2014; Mehl et al., 2012). The remaining outcome EAR variables, with all statistical tests, are provided on OSF and do not affect the conclusions drawn by this paper.

Data Analyses

All analyses were conducted in IBM SPSS 24.

Overall Intervention Effects

Primary outcomes were assessed through ANCOVA, controlling for baseline values to test for intervention effects in the presence of potential baseline differences between conditions. Even with random assignment of study participants, baseline differences can occur in outcome measures. Although repeated measures ANOVA models would also have been appropriate, an a priori decision was made to assess outcomes using ANCOVA, because this approach allows for adjustment of any baseline differences that existed before interventions were administered.

For all outcome variables, the following procedures were conducted prior to testing ANCOVA models: (1) data distributions and statistical assumptions were assessed for each treatment group separately, (2) assumptions of normality were assessed using a Kolmogorov-Smirnoff (K-S) test, and if significant, (3) bootstrap simulations were conducted to assess normality of the underlying sample distributions in order to determine if parametric tests based on the Central Limit Theorem were appropriate, (4) assumptions of homogeneity of regression were assessed to ensure that the relationship between the dependent variable at time 2 and the covariate at time 1 is the same for each treatment group, and if not met, the interaction was computed for inclusion in subsequent ANCOVA analyses, (5) assumption of Missing at Random was tested by correlating time 1 with time 2 "missingness," and (6) baseline measures for group differences were tested using a three-group, one-way analysis of variance (ANOVA). Assumptions of normality were met and parametric testing was deemed appropriate for all variables reported in this manuscript. Some of the additional variables included in the OSF supplement had non-normal distributions, and the OSF supplement therefore indicates where non-parametric tests were used for these additional variables.

Missing data were handled through an intent-to-treat approach using Multiple Imputation (van Ginkel & Kroonenberg, 2014). Five imputations of all outcome variables were computed and subsequently tested and the statistical information from the five iterations were combined into (a) pooled post-intervention means and (b) pooled estimated marginal means. The SPSS version used cannot provide pooled inferential statistics for multiple imputations. Given the broad and consistent lack of effect of intervention across domains and variables, tables report inferential statistics (F, p, partial eta-square) for the most "optimistic" analytic scenario, which is the imputation iteration that yielded the lowest p-value, to be sensitive to even a minor "hint" of a potential effect. For any effects that are statistically significant in at least one imputation, results for the other imputations are reported in the manuscript text. Three pairwise contrasts were examined, MAT vs. control, CBCT® vs. control, and CBCT® vs. MAT.

Post Hoc Moderation Analyses

Post-hoc moderation analyses were additionally conducted in order to examine any potential moderation effects of two theoretically likely candidate moderators: (1) participant distress at baseline (in recognition of the large evidence base for meditation interventions for alleviating symptoms of depression, anxiety and stress in clinical populations; Brown et al., 2015); and (2) participant engagement in the intervention (in recognition of prior research that has found that practice time moderates intervention effects; Pace et al., 2009, 2013).

To capture two distinct but correlated (Steer et al., 1995, 1999) aspects of distress, baseline distress was computed as a composite of participants' standardized scores on the BAI and BDI. Z scores were computed for participants' BAI and BDI scores and these scores were summed to create a composite index of baseline distress. Two distress groups, low and high, were then created using a median split. To test moderation effects of baseline distress, ANCOVA models with baseline distress as a factor (low vs. high) were tested for all psychological experience, biological stress reactivity, and observed daily behavior and language use dependent variables. Missing data were handled through an intent-to-treat approach using MI and the procedures outlined above, and three pairwise contrasts were examined: MAT vs. control, CBCT® vs. control, and CBCT® vs. MAT.

For the participant engagement moderation analyses, two key components of participant engagement, class attendance and home meditation practice time, were computed and examined separately. Class attendance was computed as the total number of minutes of class attendance, including zero classes attended. Two class attendance groups (low and high) were computed using a median split (Table 1). To test moderation effects of class attendance using a three-group comparison, ANCOVA models with class attendance as a factor (low vs. high) were tested for all psychological experience, biological stress reactivity, and observed daily behavior and language use dependent variables. Practice time was computed as the total number of minutes of participant-reported home meditation practice time, including zero minutes of practice. Two practice groups (low and high) were computed using a median split (Table 1). Practice time data were only available for MAT and CBCT® groups because, by design, the control condition did not include home meditation practice. To test moderation effects of practice time using a two-group comparison, ANCOVA models with practice time as a factor (low vs. high) were tested for all dependent variables. Missing data were handled through an intent-to-treat approach using MI and the procedures outlined above.

Table 1 Class attendance and practice time by condition

Participant engagement	Control *N* (%)	MAT *N* (%)	CBCT *N* (%)
Class attendance			
Low (< 720 min.)	29 (45.3%)	28 (41.2%)	32 (44.4%)
High (720 min. or more)	35 (54.7%)	40 (58.8%)	40 (55.6%)
Practice time			
Low (< 43 min.)		23 (43.4%)	32 (57.1%)
High (43 min. or more)		30 (56.6%)	24 (42.9%)

Note. Low and high groups for class attendance and practice time are based on a median split.

Power Analyses

A post hoc power analysis using G*Power indicated that, based on the final sample size, the present study has power of at least 0.80 to detect medium-sized ($f = 0.25$; J. Cohen, 1988a, 1988b) effects for differences among the three treatment groups on the primary self-report, behavioral and physiological variables in intent-to-treat (ITT) one-way ANCOVA, $\alpha = 0.05$. Therefore, the absence of statistically significant results suggests that any potential intervention effects were likely, at most, small by traditional classification of effect sizes.

Results

Main Effects of Interventions

Psychological experience

There was no evidence of differences between the three conditions on target indicators of psychological experience. Across the five imputation iterations, no statistically significant effects between CBCT®, MAT, and the control group were observed on depression, mindfulness, perceived stress, satisfaction with life, or loneliness (Table 2). For anxiety, counter to the idea that meditation interventions might reduce anxiety, two imputations yielded a statistically significant effect such that post-intervention estimated marginal means were slightly lower in the control group relative to the CBCT® and MAT groups, $p = 0.040, 0.045$, $\eta_p^2 = 0.035, 0.033$. However, this effect was not statistically significant in the other three iterations tested ($p = 0.082, 0.186, 0.055$).

Table 2 Effect of condition on psychological experience, biological stress reactivity, and observed daily behavior and language use

Variable	Time 1 mean			Time 2 mean			Effect of condition		
	CTR	MAT	CBCT	CTR	MAT	CBCT	F	p	η_p^2
Psychological experience									
Anxiety (BAI)	4.3	4.5	5.0	3.0	4.1	4.8	3.26	.040*	.035
Depression (BDI)	3.6	4.5	4.8	3.3	3.8	3.5	0.59	.554	.006
Mindfulness (FFMQ)	3.6	3.6	3.6	3.6	3.6	3.6	1.24	.292	.014
Stress (PSS)	19.2	20.1	20.7	19.2	20.3	20.9	1.57	.211	.017
Life Satisfaction (SWLS)	26.0	24.6	24.8	26.0	25.3	25.8	0.81	.446	.009
Loneliness (UCLA)	18.0	19.6	18.8	18.0	19.2	18.5	0.34	.712	.004
Biological stress reactivity									
Cortisol AUC*g*	57.3	66.8	57.7	54.0	58.9	58.1	1.95	.147	.018
Cortisol AUC*i*	25.4	23.0	23.6	20.6	12.3	19.3	2.35	.101	.012
IL-6 AUC*g*	388.0	389.3	362.1	460.7	422.2	536.3	1.15	.319	.011
IL-6 AUC*i*	219.0	212.9	172.3	320.3	281.7	332.5	0.95	.388	.006
Observed daily behavior and language use									
Affection[a]	1.7%	1.7%	1.8%	1.8%	1.9%	1.9%	0.22	.806	.002
Complaining/whining[a]	2.4%	2.6%	2.3%	2.3%	2.6%	2.0%	2.64	.074	.026
Gossip[a]	2.4%	2.2%	2.3%	2.2%	2.1%	1.6%	1.55	.215	.021
Gratitude[a]	1.1%	0.8%	0.9%	0.8%	0.9%	1.0%	1.20	.304	.012
Interaction quality[a]	13.3%	13.2%	11.9%	13.3%	13.3%	11.4%	1.81	.166	.018
Interaction quantity[a]	42.8%	40.9%	40.8%	43.3%	40.4%	37.7%	1.24	.292	.012
Positive emotion words[b]	3.4	3.1	3.2	3.3	3.0	3.3	3.75	.025*	.037
Negative emotion words[b]	1.1	1.2	1.1	1.2	1.2	1.2	0.68	.509	.007
Perception words[b]	2.2	2.3	2.3	2.2	2.1	2.2	1.8	.168	.018

Note. [a] = behaviorally coded variable expressed as a percentage of all waking and valid sound files, [b] = text-analytically derived variable expressed as a percentage of all sampled words. Five imputation iterations were computed for all variables. Effect of condition is based on the least conservative (i.e., lowest) *p*-value of the five imputations in the omnibus F-test. This strategy aimed at documenting the most optimistic analytic scenario across the five imputations, although for any statistically significant imputations reported in this table, the results of the remaining four imputations are reported in the manuscript text. * $p < .05$. *BAI* = Beck Anxiety Inventory; *BDI* = Beck Depression Inventory; *FFMQ* = Five Factor Mindfulness Questionnaire; *PSS* = Perceived Stress Scale; *SWLS* = Satisfaction with Life Scale; *AUCg* = area under the curve from ground; *AUCi* = area under the curve from initial value.

Biological Stress Reactivity

There was no evidence of differences between groups on indicators of biological stress reactivity. No statistically significant effects between CBCT®, MAT, and the control group were observed on Cortisol AUC*g*, Cortisol AUC*i*, IL-6 AUC*g*, or IL-6 AUC*i* (Table 2).

Observed Daily Behavior and Language Use

No statistically significant effects between CBCT®, MAT, and the control group were observed in affection, complaining and whining, gossip, gratitude, conversation quality, conversation quantity, perception word use, or negative emotion word use (Table 2). There was a statistically significant effect of condition on positive emotion words in two of the five imputation iterations, $p = 0.025, 0.048$, $\eta_p^2 = 0.037, 0.031$. However, this effect was not statistically significant in the other three imputations ($p = 0.368, 0.025, 0.940$). Pairwise comparisons for the least conservative iteration indicated a significant difference between CBCT® and MAT, $p = 0.007$. Pooled estimated marginal means indicate that participants in the MAT group ($M = 3.00$) used fewer positive words post-intervention than participants in the CBCT® group ($M = 3.36$). No significant differences were observed between MAT and the control group or CBCT® and the control group across imputations.

Moderation Effects of Baseline Distress

Two distress groups (low and high) were created using a median distress score of − 0.477. Low distress participants had scores less than − 0.477; high distress participants had scores greater than − 0.477.

Table 3 ANCOVA models moderated by baseline distress (low vs. high)

Variable	Estimated marginal means control		Estimated marginal means MAT		Estimated marginal means CBCT		Effect of condition x baseline distress		
	Low	High	Low	High	Low	High	*F*	*P*	η_p^2
Psychological experience									
Mindfulness (FFMQ)	3.6	3.5	3.6	3.6	3.6	3.7	0.905	.406	.010
Stress (PSS)	17.8	21.7	20.8	19.8	20.0	20.9	3.079	.049*	.034
Life Satisfaction (SWLS)	26.0	24.7	26.0	25.1	26.0	26.0	0.823	.441	.009
Loneliness (UCLA)	18.2	18.8	19.1	17.9	18.5	18.3	2.039	.133	.023
Biological stress reactivity									
Cortisol AUC*g*	55.2	57.7	52.1	58.5	61.6	59.0	1.874	.157	.026
Cortisol AUC*i*	15.9	24.8	12.7	12.2	21.9	17.2	1.69	.190	.035
IL-6 AUC*g*	423.4	533.6	423.0	508.0	456.9	600.1	0.444	.642	.006
IL-6 AUC*i*	266.5	373.2	235.3	318.5	277.3	397.9	0.429	.652	.006
Observed daily behavior and language use									
Affection[a]	1.8	2.4	2.0	2.2	2.0	2.1	1.193	.306	.013
Complaining/whining[a]	2.4	2.2	2.1	2.8	1.8	2.3	1.284	.280	.014
Gossip[a]	2.4	2.0	2.0	2.4	1.5	2.1	2.319	.101	.026
Gratitude[a]	0.9	0.8	0.8	0.9	0.9	1.1	0.652	.522	.007
Interaction quality[a]	12.0	14.5	13.3	13.2	11.4	11.4	1.211	.399	.014
Interaction quantity[a]	37.7	45.5	39.3	42.0	36.3	39.7	0.667	.515	.007
Positive emotion words[b]	3.0	3.4	3.1	3.0	3.3	3.4	2.161	.118	.024
Negative emotion words[b]	1.3	1.2	1.3	1.0	1.1	1.2	5.858	.003*	.063
Perception words[b]	2.1	2.3	2.2	2.1	2.2	2.3	1.862	.158	.021

Note.[a] = behaviorally coded variable expressed as a percentage of all waking and valid sound files, [b] = text-analytically derived variable expressed as a percentage of all sampled words. Five imputation iterations were computed for all variables. Estimated marginal means were pooled across all five imputations. Effect of condition is based on the least conservative (i.e., lowest) *p*-value of the five imputations in the omnibus F-test. This strategy aimed at documenting the most optimistic analytic scenario across the five imputations, although for any statistically significant imputations reported in this table, the results of the remaining four imputations are reported in the manuscript text. *$p < .05$. *FFMQ* = Five Factor Mindfulness Questionnaire; *PSS* = Perceived Stress Scale; *SWLS* = Satisfaction with Life Scale; *AUCg* = area under the curve from ground; *AUCi* = area under the curve from initial value.

Psychological Experience

No significant baseline distress by condition interactions was found across any of the imputations for mindfulness, satisfaction with life, or loneliness (Table 3). For stress, a significant baseline distress by condition interaction were found for one of the five imputations, $p = 0.049$. However, significant baseline distress by condition interactions were not found in any of the other four imputations computed ($p = 0.205$, 0.121, 0.079, 0.053).

Biological Stress Reactivity

No significant baseline distress by condition interactions were found across any of the imputations for Cortisol AUC *g*, Cortisol AUC*i*, IL-6 AUC*g*, or IL-6 AUC*i*.

Observed daily Behavior and Language Use

No significant baseline distress by condition interactions were found across any of the imputations for affection, complaining and whining, gossip, gratitude, interaction quality, interaction quantity, positive emotion words, or perception words (Table 3). For negative emotion words, significant or trend-level baseline distress by condition interactions were found across all imputations, $p = 0.083$, 0.012, 0.091, 0.025, 0.003. Pairwise comparisons indicated a significant difference between CBCT® and MAT among the high distress participants in three out of five imputations tested, $p = 0.302$, 0.010, 0.207, 0.046, 0.002. Estimated marginal means indicate that, among high distress participants, negative emotion word use was lower post-intervention for participants who received MAT ($M = 0.957$) than for participants who received CBCT® ($M = 1.241$). However, pairwise comparisons did not indicate significant differences between MAT and control or CBCT® and control.

Moderation Effects of Participant Engagement

In total, 23% of participants attended less than 25% of sessions; 7% attended 25–49% of sessions; 7% attended 50–74% of sessions; and 56% attended 75–100% of sessions. Two class attendance groups (low and high) were computed using a median split from the median value of 720 min. Two practice time groups (low and high) were computed using a median split from the median value of 433 min. Class attendance and at-home practice time were correlated in the 109 participants randomized to either CBCT® or MAT who provided home practice data (for both groups combined: $r(107) = 0.58$, $p < 0.001$; MAT: $r(51) = 0.69$, $p < 0.001$; CBCT®: $r(54) = 0.47$, $p < 0.001$). A summary of results is provided in Tables 4 (Class Attendance) and 5 (Practice Time).

Psychological Experience

There were significant or trend-level main effects for class attendance on depression (four out of five imputations, $p = 0.160$, 0.060, 0.034, 0.079, 0.042), as well as on satisfaction with life (all five imputations, $p = 0.064$, 0.091, 0.018, 0.025, 0.032). This suggests that across all conditions, including the control condition, class attendance was associated with a reduction in symptoms of depression and an increase in satisfaction with life. There were no significant

Table 4 ANCOVA models moderated by class attendance (low vs. high)

Variable	Estimated marginal means control		Estimated marginal means MAT		Estimated Marginal Means CBCT		Effect of condition x class attendance		
	Low	High	Low	High	Low	High	F	P	η_p^2
Psychological experience									
Anxiety (BAI)	3.7	2.8	4.2	4.2	5.1	4.2	0.808	.447	.009
Depression (BDI)	3.9	3.3	4.7	3.2	4.0	2.7	0.717	.490	.008
Mindfulness (FFMQ)	3.6	3.6	3.5	3.6	3.5	3.7	3.058	.049*	.034
Stress (PSS)	20.1	19.5	20.5	20.2	21.8	19.5	2.171	.117	.024
Life Satisfaction (SWLS)	25.1	25.6	24.5	26.2	25.0	26.7	0.784	.458	.009
Loneliness (UCLA)	18.2	18.7	18.4	18.6	19.6	17.6	2.998	.052	.033
Biological stress reactivity									
Cortisol AUC*g*	56.1	56.2	59.0	53.4	63.6	57.8	1.034	.358	.014
Cortisol AUC*i*	22.0	19.2	18.1	10.8	21.3	18.4	0.585	.559	.012
IL-6 AUC*g*	595.8	384.9	468.3	462.8	655.5	465.9	3.167	.045*	.041
IL-6 AUC*i*	404.9	249.8	307.3	264.8	459.6	268.6	1.709	.185	.023
Observed daily behavior and language use									
Affection[a]	2.2	1.9	2.3	2.0	2.7	1.4	2.07	.129	.021
Complaining/whining[a]	2.7	2.1	2.7	2.3	2.6	1.7	2.476	.087	.025
Gossip[a]	2.2	2.2	1.9	2.4	2.2	1.5	2.99	.053	.03
Gratitude[a]	1.1	0.6	1.0	0.9	1.4	0.7	3.117	.046*	.031
Interaction quality[a]	12.3	13.8	13.2	13.1	12.7	10.9	2.74	.067	.027
Interaction quantity[a]	39.6	42.3	40.1	40.5	40.3	35.8	1.817	.165	.018
Positive emotion words[b]	3.3	3.2	2.9	3.1	3.5	3.2	2.35	.098	.024
Negative emotion words[b]	1.2	1.2	1.3	1.1	1.3	1.0	2.091	.126	.021
Perception words[b]	2.2	2.2	2.1	2.1	2.1	2.3	0.853	.428	.009

Note. [a] = behaviorally coded variable expressed as a percentage of all waking and valid sound files, [b] = text-analytically derived variable expressed as a percentage of all sampled words. Low and high groups were calculated by median split, low = less than 720 min, high = 720 min or more. Five imputation iterations were computed for all variables. Estimated marginal means were pooled across all five imputations. Effect of condition is based on the least conservative (i.e., lowest) *p*-value of the five imputations in the omnibus F-test. This strategy aimed at documenting the most optimistic analytic scenario across the five imputations, although for any statistically significant imputations reported in this table, the results of the remaining four imputations are reported in the manuscript text. *$p < .05$. *BAI* = Beck Anxiety Inventory; *BDI* = Beck Depression Inventory; *FFMQ* = Five Factor Mindfulness Questionnaire; *PSS* = Perceived Stress Scale; *SWLS* = Satisfaction with Life Scale; AUCg = area under the curve from ground; *AUCi* = area under the curve from initial value.

class attendance by condition interactions or practice time by condition interactions on either depression or satisfaction with life.

ANCOVA models indicated evidence of participant engagement moderation on self-reported mindfulness, but no evidence of participant engagement moderation on other psychological experience variables. For mindfulness, there was a significant class attendance by condition interaction for one of the imputations examined, $p = 0.049$, but not for the other four imputations, $p = 0.270$, 0.130, 0.064, 0.165. However, the main effect of class attendance was also present across all imputations, $p = 0.004$, 0.036, 0.004, 0.017, and 0.024, with higher FFMQ scores associated with greater class attendance. Consistent with the idea that meditation classes increase mindfulness, within the high-class attendance group, pooled means across imputations indicate that participants in the CBCT® condition increased in mindfulness over the course of the intervention, as did participants in the MAT group, but scores for participants in the control group remained approximately the same (Table 4). Similarly, although there were no significant practice time by condition interactions on mindfulness, there were significant main effects of practice time across imputations, $p = 0.013$, 0.027, 0.020, 0.047, and 0.013, with higher FFMQ scores associated with higher practice. Within the high practice time group, FFMQ scores increased in the CBCT® condition, but slightly decreased in the MAT condition (Table 5). This suggests that the main effect was carried by the CBCT® group, but not strongly enough to yield a statistically significant interaction effect.

Table 5 ANCOVA models moderated by practice time (low vs. high)

Variable	Estimated marginal mean MAT		Estimated marginal mean CBCT		Effect of condition x practice time		
	Low	High	Low	High	*F*	*P*	η_p^2
Psychological experience							
Anxiety (BAI)	4.9	4.0	4.8	4.6	0.444	.507	.004
Depression (BDI)	4.4	3.9	3.0	4.2	2.083	.152	.020
Mindfulness (FFMQ)	3.5	3.6	3.5	3.7	1.971	.163	.019
Stress (PSS)	20.5	21.1	20.7	21.2	1.228	.27	.012
Life Satisfaction (SWLS)	25.0	25.6	25.4	25.5	0.38	.539	.004
Loneliness (UCLA)	18.6	19.2	19.4	18.8	0.371	.244	.013
Biological stress reactivity							
Cortisol AUC*g*	61.3	50.4	58.3	61.4	8.714	.004*	.088
Cortisol AUC*i*	16.9	12.6	17.8	22.5	2.062	.156	.032
IL-6 AUC*g*	483.1	475.8	591.9	480.7	2.519	.116	.027
IL-6 AUC*i*	312.0	278.6	383.9	300.0	1.615	.207	.018
Observed daily behavior and language use							
Affection[a]	2.2	2.3	2.3	1.3	4.887	.029*	.045
Complaining/whining[a]	2.6	2.3	2.3	1.6	0.348	.557	.003
Gossip[a]	2.0	2.5	1.9	1.6	2.519	.116	.024
Gratitude[a]	0.9	0.9	1.3	0.8	1.881	.173	.018
Interaction quality[a]	14.3	12.1	12.9	10.0	0.185	.668	.002
Interaction quantity[a]	40.5	40.1	40.4	35.0	1.215	.273	.012
Positive emotion words[b]	2.9	2.9	3.4	3.1	1.839	.178	.018
Negative emotion words[b]	1.3	1.1	1.2	1.0	0.495	.483	.005
Perception words[b]	2.1	2.1	2.0	2.5	3.673	.058	.034

Note. [a] = behaviorally coded variable expressed as a percentage of all waking and valid sound files, [b] = text-analytically derived variable expressed as a percentage of all sampled words. Low and high groups were calculated by median split, low = less than 433 min, high = 433 min or more. Five imputation iterations were computed for all variables. Estimated marginal means were pooled across all five imputations. Effect of condition is based on the least conservative (i.e., lowest) *p*-value of the five imputations in the omnibus F-test. This strategy aimed at documenting the most optimistic analytic scenario across the five imputations, although for any statistically significant imputations reported in this table, the results of the remaining four imputations are reported in the manuscript text. *$p < .05$. *BAI* = Beck Anxiety Inventory; *BDI* = Beck Depression Inventory; *FFMQ* = Five Factor Mindfulness Questionnaire; *PSS* = Perceived Stress Scale; *SWLS* = Satisfaction with Life Scale; *AUCg* = area under the curve from ground; *AUCi* = area under the curve from initial value.

Biological Stress Reactivity ANCOVA models found little evidence of participant engagement moderation on biological stress reactivity specific to the intervention conditions. There were main effects of class attendance for IL-6 AUC *g*, $p=0.037$, 0.011, 0.000, 0.001, 0.034 and main effects of class attendance for IL-6 AUC*i*, $p=0.017$, 0.003, 0.003, 0.006, 0.014. These indicate that across all conditions (including the control condition), IL-6 was lower post-intervention than it was pre-intervention, but is not suggestive of an effect specific to the interventions. No other stable effects on IL-6 emerged. There was a significant class attendance by condition interaction for IL-6 AUC*g* in one of the imputations examined, $p=0.045$, but not for any of the other four ($p=0.126$, 0.125, 0.326, 0.173), and no interaction effect was observed for IL-6 AUC*i*.

For Cortisol AUC*g* and Cortisol AUC*i*, there were no significant class attendance by condition interactions and no stable main effects (for Cortisol AUC*i*, one imputation yielded a significant class attendance main effect, $p=0.049$; however, main effects were not observed in any of the other imputations, $p=0.238$, 0.448, 0.405, 0.571). Practice time by condition interactions yielded different patterns of results between Cortisol AUC*g* and Cortisol AUC*i*. For Cortisol AUC*g*, significant or trend-level practice time by condition interactions emerged for all imputations tested, $p=0.033$, 0.043, 0.088, 0.050, 0.019. Within the high practice group, pairwise comparisons indicate that Cortisol AUC*g* was significantly lower in the MAT group than the CBCT® group at the post-intervention assessment, $p=0.028$, 0.016, 0.010, 0.039, 0.021. Pooled means across all imputations indicate that, among the high practice group, MAT participants experienced a decrease in Cortisol AUC*g*, $M_{MAT\cdot T1}=65.866$, $M_{MAT\cdot T2}=56.0986$, whereas participants in the CBCT® group experienced a slight increase, $M_{CBCT\cdot T1}=57.85$, $M_{CBCT\cdot T2}=59.83$. However, there were no significant practice time by condition interactions for Cortisol AUC*i*.

Observed Daily Behavior and Language Use Overall, there was little evidence of moderation effects of participant engagement. There was a significant main effect of class attendance on complaining and whining across imputations ($p=0.048$, 0.014, 0.095, 0.019, 0.004) such that greater class attendance was associated with less complaining and whining across all conditions, including the control condition; however, there was no evidence of a class attendance by condition interaction, practice time by condition interaction, or main effect of practice time. For affection, there were significant and trend-level main effects of class attendance across all imputations ($p=0.092$, 0.016, 0.015, 0.049, 0.040) such that participants in the high-class attendance groups expressed *less* affection post-intervention than did participants in the low-class attendance groups, an effect that runs contrary to the hypothesis that participation in a meditation class increases expressed affection. No reliable practice time by condition interactions emerged for affection. Although there was a significant practice time by condition interaction for affection in one of the imputations examined, $p=0.029$, the interaction was not significant in any of the other imputations ($p=0.224$, 0.264, 0.141, 0.191). Contrary to the idea that practicing compassion meditation consistently increases expressed affection, pairwise comparisons of the high practice groups suggested that MAT participants expressed more affection than CBCT® participants post intervention, $p=0.041$, 0.076, 0.060, 0.059, 0.043.

No other observed effects were stable across imputations. For gratitude, a significant class attendance by condition interaction was observed in one imputation tested, $p=0.046$, but this interaction was not observed in any of the other imputations ($p=0.119$, 0.377, 0.084, 0.135). There was a significant main effect for practice time on interaction quality in one imputation tested, $p=0.027$, but this effect was at trend-level or non-significant in the other imputations tested ($p=0.070$, 0.091, 0.079, 0.304). Similarly, a significant main effect of practice time by condition on negative emotion word use emerged for one imputation, $p=0.018$, but was not present in any of the other imputations tested ($p=0.076$, 0.158, 0.185, 0.112), and a significant main effect of practice time on perception word use emerged in only two of the five imputations tested ($p=0.261$, 0.038, 0.231, 0.041, 0.634).

Discussion

This randomized controlled trial was motivated by calls to put mindfulness and compassion meditation interventions to more rigorous tests by employing active control interventions, larger and more diverse samples of participants, multimethod measurement approaches, and assessments of potential differences between specific practice elements (Creswell, 2017; Davidson & Kaszniak, 2015; Dimidjian & Segal, 2015; Goyal et al., 2014). This study was powered to detect all but small effects, utilized a health education discussion control condition, and included assessments of subjective psychological experience, biological stress reactivity, and naturalistically observed daily behaviors and social interactions. The sample was demographically heterogeneous, with 50% of participants identifying as non-White and more than half (66.5%) earning below the local median income.

The results from this trial can be succinctly summarized as "impressively null." This study found little evidence of any positive impact of either MAT or CBCT® on participants' psychological experience, biological stress reactivity, or naturalistically assessed daily behaviors and language compared to an active control. Even pre-to-post intervention change was largely absent in any of the conditions. There was also little evidence of moderation effects for baseline distress, arguing against the possibility that there had been a "ceiling effect" and the interventions were only effective among otherwise healthy participants who were experiencing the most psychological distress at baseline. Furthermore, although practice time was found to be associated with increased scores on a self-reported mindfulness measure, there was otherwise little evidence of moderation effects for participant engagement, arguing against the possibility that the meditation interventions were only effective among those who committed to attending class and engaging in at-home practice. Results did suggest that intervention engagement was associated with decreases in depressive symptoms and increases in satisfaction with life across all interventions, including the control condition. This is consistent with the idea that group classes can yield positive outcomes simply by facilitating social engagement and connectedness (Cuijpers et al., 2007; MacCoon et al., 2012). However, this also suggests that in this study population, neither MAT nor CBCT® provided any specific benefits over and above those offered by the active control.

Dimidjian and Segal (2015) aptly point out that, given the current status of the evidence for meditation programs, a rigorously conducted "failed" individual trial "may be a 'success' when viewing the advancement of the field broadly…Such findings help to inform the 'boundary conditions' necessary for scientific progress" (Dimidjian & Segal, 2015, p. 605). Publication bias, though not a novel phenomenon, impedes the ability of science to advance and self-correct (The Importance of No Evidence, 2019). Indeed, funnel plots of mindfulness meditation studies indicate a publication bias, such that negative and null effects are suppressed or remain unpublished (Eberth & Sedlmeier, 2012). As of 2016, sixty-two percent of registered mindfulness-based mental health intervention trials were unpublished 30 months post-trial completion, and moreover, published trials reported positive results at a rate 1.6 times greater than what would be expected given effect size estimates based on those found for similar psychosocial interventions (Coronado-Montoya et al., 2016).

In the spirit of advancing the self-correction of science by exposing null findings to the light of day, this trial—which yielded non-significant results despite being methodologically robust—contributes an important new "data point" for the body of research about meditation interventions. Viewed as a whole, existing literature on meditation is emerging as mixed. On one hand, a large number of basic research and efficacy trials have yielded impressive and promising results on a wide range of psychosocial outcomes (Creswell, 2017; Hofmann et al., 2011; Khoury et al., 2015). On the other hand, meta-analyses have suggested that the available literature shows a bias towards the publication of positive findings (Coronado-Montoya et al., 2016; Eberth & Sedlmeier, 2012) and that "successful" interventions often have small effect sizes (Creswell, 2017; Goyal et al., 2014).

The fact that results from meditation studies are mixed and that the reported results are null may be best understood as situating meditation interventions as comparably impactful to other empirically supported psychosocial interventions. In general, lifestyle and health behavior change interventions demonstrate variance in intervention efficacy as a function of participant and intervention characteristics, and also tend to find small effect sizes when successful (Johnson et al., 2010). This is particularly true of larger studies, as larger sample sizes tend to "shrink" obtained effect sizes (Anderson & Maxwell, 2017; Schönbrodt & Perugini, 2013). Contextualized within the overall body of evidence for meditation interventions and lifestyle interventions (including those that "worked"), these non-findings leave open the possibility that meditation interventions are neither more nor less quick, effective, and reliable at engendering psychological change than any other intervention in the behavioral scientist's armamentarium. These non-findings suggest that caution should be taken in not overselling the potential benefits of these interventions, perhaps especially for the population tested here.

Limitations and Future Research

Importantly, the present study cannot be taken as evidence that the interventions tested do not have effects—rather, it is simply the case that evidence was not found for differential effects in the outcomes and in the healthy population that were assessed. As is often remarked, the "absence of evidence is not evidence of absence," and null findings are more accurately characterized as "uncertain" than as conclusively negative (Alderson, 2004). It is also possible that the interventions yielded small but statistically significant effects on the outcomes assessed, and that these results would have been different had this study been powered to detect even smaller effects. Recent re-evaluations of effect size guidelines suggest that effect sizes of 0.10 should be regarded as "small" and effect sizes of 0.20 should be regarded as "medium" (Funder & Ozer, 2019). The present results therefore leave open the possibility of undetected medium effects by these new benchmarks. Further, and importantly, this study tested two specific types of meditation interventions: eight-week mindfulness and compassion classes administered to healthy adults. These results cannot be generalized to other types of intervention models.

Other key limitations of this research concern the sample population of healthy adults. It is possible that this design may have been more likely to detect effects in the outcomes tested if the study population had included individuals experiencing greater levels of distress (e.g., clinical depression or anxiety, chronic pain). Relatedly, the study design had limited ability to capture the diversity and heterogeneity within the sample. It is possible that individual differences such as religious beliefs and cultural worldviews and orientations moderated intervention effects (Palitsky & Kaplan, 2021; Watson-Singleton et al., 2019), but because this study did not measure these variables, it is not possible to test cultural moderation hypotheses. An additional limitation of this study is that the active control condition did not incorporate a home practice, making it a less rigorous match for the other interventions tested. Future trials should incorporate a control home practice tailored to the control condition (e.g., daily journaling about health behaviors).

Furthermore, even though a strength of the study design was that shared method variance was minimized by utilizing survey-based psychological measures, naturalistic-observation based behavioral measures, and lab-based physiological measures to assess trial outcomes, shared method variance still affected measures within a single method (Podsakoff et al., 2003, 2012). For example, all survey-based psychological measures share susceptibility to socially desirable responses. Shared method variance tends to artificially create or inflate effects but can, theoretically, also blur or shrink existing effects.

Null findings from the current study point to several fruitful directions for further research on meditation interventions. First, they underscore the importance of mechanistic research that focuses on identifying how, for whom, and under what circumstances meditation interventions work best. To date, little research has focused on identifying candidate mechanisms and moderators of these interventions (Dimidjian & Segal, 2015). One recent set of studies found that participants' religious and existential beliefs are differentially predictive of responses to mindfulness-based interventions (Palitsky et al., under review). There is also evidence to suggest that adverse effects of meditation practice may occur for some individuals as a function of practitioner-level factors such as trauma history (Britton et al., 2021; Lindahl et al., 2017). Scholars have also called for the investigation of intervention characteristics such as teacher effects (Davidson & Kaszniak, 2015). Research designs that test candidate moderators such as these can clarify for whom and under what circumstances meditation interventions do—and do not—engender measurable and beneficial change.

The "dosage" of meditation training (e.g., quantity of practice and quality of practice sessions) necessary to produce psychological change may also be a valuable target for future empirical research. Most popular meditation training programs last a matter of weeks, yet it has been argued that the positive effects of meditation practice may take years to manifest (Davidson & Kaszniak, 2015; Goleman & Davidson, 2017), and other studies have identified practice time effects (Pace et al., 2009, 2013). In the present study, which intentionally did not incentivize home practice in order to best reflect real-world practice conditions, half of all participants in the MAT and CBCT® conditions practiced for fewer than 43 min at home over the entire course of the eight-week interventions (an average of slightly over 5 min per week). Other trials of meditation interventions that tracked but did not enforce practice time, including those of clinical populations, have also identified inconsistent adherence patterns, and many trials do not report practice time statistics. In one trial of mindfulness-based stress reduction for chronic pain that did attempt to collect adherence data, fewer than half of the 99 participants turned in a home practice log at all. Those who turned in a practice log reported very high levels of adherence, rendering the overall adherence pattern for the study ambiguous (Rosenzweig et al., 2010). These statistics are indicative of the challenges of establishing a regular meditation practice and also suggest the potential importance of intervention components that address habit formation. Future research on meditation interventions can focus on identifying factors that contribute to the development of a practice habit that is "automatic" (Lally et al., 2010), but not, contradicting the focus of these interventions, "autopilot."

Finally, null findings from the current study may invite reconsideration of the outcomes assessed in studies of meditation interventions. The present trial assessed subjective distress and wellbeing, physiological indicators of stress reactivity, and behavior in daily life—a multi-method assessment approach that is presently regarded as a gold standard in psychosocial intervention research. Yet, it is worth considering whether these dependent variables (which reflect a modern and Western conceptualization of wellbeing) are the best indicators of the actual effects of meditation interventions (which employ techniques derived from markedly different cultural and temporal antecedents). Although the practices taught in CBCT® aim to be accessible to those of any or no religious background, they derive from Tibetan Buddhist *lojong* meditation, which, as summarized by one scholarly account, aims to "fully awaken the mind and liberate awareness by striking at the heart of suffering itself while simultaneously pointing to the enlightened or liberated mind" (Kyabgon, 2007, p. ix). This aim far outstrips the more quotidian wellbeing goals that draw some students to meditation. It has similarly been argued that "Mindfulness was never supposed to be about weight loss, better sex, helping children perform better in school, helping employees be more productive in the workplace, or even improving the functioning of anxious, depressed people" (Harrington & Dunne, 2015, p. 2). It is thus plausible that while measures such as the BDI and PSS sometimes register change over the course of meditation interventions, the distinct psychological effects of these practices may not be optimally captured by these measures. Put another way, changes such as decreases in stress may constitute "possible side effects" of meditation, rather than primary therapeutic effects.

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Author Contribution DMK: Supported the data analyses and drafted this manuscript with assistance from MRM and CLR. MRM: Collaborated on the design and execution of the study, supported the data analysis, and contributed to writing the manuscript. TWWP: Collaborated on the design and execution of the study and contributed to writing the manuscript. LTN: Collaborated on the design of the study and editing of the manuscript. BOS: Collaborated on the design of the study and editing of the manuscript. BDL: Collaborated on the execution of the study and editing of the manuscript. TS: Collaborated on the execution of the study and editing of the manuscript. AW: Collaborated on the execution of the study and editing of the manuscript. TC: Collaborated on the execution of the study and editing of the manuscript. BP: Collaborated on the execution of the study and editing of the manuscript. VM: Provided data management oversight for the study and contributed to writing the manuscript. MLR: Provided data management oversight for the study and contributed to writing the manuscript. SPC: Conducted statistical analyses for the manuscript, with assistance from DMK and MRM. WEC: Collaborated on the design of the study and editing of the manuscript. CLR: Secured funding, designed and executed the study, and assisted with writing and editing of the manuscript.

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Data Availability Open access to the data and materials associated with this study are provided on the Open Science Framework at: https://osf.io/6jt9q/.

Declarations

Ethics Statement All procedures for this study were approved by Institutional Research Boards at Emory University and the University of Arizona.

Informed Consent Statement All participants in this research provided informed consent for participating in this trial.

Conflict of Interest DMK is a certified instructor of Cognitively Based Compassion Training (CBCT®) and has taught CBCT® on behalf of the University of Arizona's Center for Compassion Studies. LTN is the Executive Director and BOS is an Associate Director for the Center for Contemplative Science and Compassion-Based Ethics at Emory University. The Center holds a registered trademark for CBCT® and offers trainings in the procedure. CLR serves as a consultant for Usona Institute, Sage Pharmaceuticals, Novartis, and Emory Healthcare. The other authors declared that they had no conflicts of interest with respect to their authorship or the publication of this article.

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