How Often Should I Meditate? A Randomized Trial Examining the Role of Meditation Frequency When Total Amount of Meditation is Held Constant

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Author Note

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Conflict of Interest

No donors, either anonymous or identified, have participated in the design, conduct, or reporting of research results in this manuscript. The content of this article is solely the responsibility of the authors and does not necessarily represent the official views of any funding parties. Richard J. Davidson is the founder, president, and serves on the board of directors for the non-profit organization, Healthy Minds Innovations, Inc. Cortland J. Dahl is the primary content developer of the Healthy Minds Program and a scientist at Healthy Minds Innovations, Inc. Robin I. Goldman has been a paid consultant at Healthy Minds Innovations, Inc. for work unrelated to this research. Otto Simonsson is a co-founder of Eudelics AB. No other authors have conflicts of interest to disclose.

Abstract

Meditation apps are the most commonly used mental health apps. However, the optimal dosing of app-delivered meditation practice has not been established. We examined whether the distribution of meditation practices across a day impacted outcomes in a distressed population. We investigated the effects of meditation practice frequency in a two-week compassion-based meditation intervention delivered via the Healthy Minds Program app. Undergraduates with clinically elevated depression and/or anxiety ($N = 351$) were randomized to a Massed (one 20-minute meditation per day) or Distributed condition (two 10-minute meditations per day). Psychological distress (primary outcome; composite of depression and anxiety), experiential avoidance, fear of missing out, loneliness, and self-compassion were assessed pre- and post-intervention. Psychological distress, loneliness, and informal meditation practice were also assessed daily. Practice time and frequency were assessed using app data. Results support feasibility of the study design, success of the manipulation, and acceptability of the intervention. Pooled across conditions, participants exhibited pre-post improvements on all outcomes (absolute value of ds = 0.12 to 0.63, ps ≤ .010) and trajectories of improvement on daily distress and loneliness (ps ≤ .010). No between-group differences were observed on changes in pre-post or daily measures (ps =.158 to .729). When total amount of meditation practice per day is held constant, the distribution of practice may not influence outcomes for distressed beginners. Although only a first test of dose frequency effects, findings support flexibility in the distribution of meditation throughout the day, which may increase accessibility.

Keywords: meditation; lovingkindness; compassion; mobile health; dosage

Public Health Significance Statement

This study suggests practicing meditation with a smartphone-based meditation app once per day for 20 minutes or twice per day for 10 minutes each time is associated with equivalent improvements on psychological distress and other outcomes. This supports the possibility that meditation practice may be flexibly distributed across a day and will produce similar outcomes, at least in the context of a meditation app intervention.

Introduction

Relative to the general public, undergraduate students in the United States are at increased risk for mental and behavioral health challenges (Auerbach et al., 2018), including depressive and anxiety disorders (Kim et al., 2022). Alarmingly, the prevalence of such psychiatric symptoms among college-aged individuals has been steadily rising (Hunt & Eisenberg, 2010), a trend exacerbated by historical events such as the COVID-19 pandemic (Kim et al., 2022) and the climate crisis (Hickman et al., 2021). Yet, undergraduates demonstrate low utilization of traditional mental health services, citing reasons such as barriers to accessing services, preferring to deal with issues on their own, or worrying what others might think (Veron et al., 2022). More accessible and acceptable interventions are needed to increase the likelihood they will be utilized by undergraduates in distress. One promising approach involves compassion-based meditation delivered via smartphone app (Andersson et al., 2021).

Within the scientific literature, the term meditation is used to describe various forms of mental training intended to strengthen qualities or skills related to psychological well-being. Such practices include those that train attention and foster an attitude of nonreactivity to present-moment experience, practices that cultivate prosocial qualities like gratitude and compassion, and practices that encourage self-inquiry to gain insight into one's thoughts, emotions, beliefs, and values (Dahl et al., 2015). Formal meditation entails explicit practice of a meditation technique during a discrete period (e.g., 5-minutes sustaining attention on breath sensations). Informal meditation involves the integration of meditation techniques into daily life (e.g., attending to breath sensations during ordinary interactions). There has been a surge of interest in meditation among researchers and in popular culture (Van Dam et al., 2018; Simonsson et al., 2020). There is now robust evidence for the benefits of meditation (Goldberg et al., 2022b).

Compassion-Based Meditation

Among the many forms of meditation, there is increasing evidence supporting compassion-based meditation (Kirby et al., 2017). Compassion-based meditation involves generating feelings of warmth and beneficence toward others and/or the self. A common compassion-based meditation practice involves visualizing a series of individuals and silently repeating phrases such as "may you be safe; may you be happy; may you be free from suffering" while attuning to the affect evoked within the meditator. These methods have been shown to produce benefits on a variety of psychosocial variables, including depression, anxiety, compassion, perspective-taking, and prosocial behavior (Kirby et al., 2017). They have also been found to alter related brain function after just two weeks of practice (Weng et al., 2013).

Relative to mindfulness practices, compassion-based meditation practices may produce greater benefits on variables such as nonjudgmental acceptance, compassion, and self-compassion (Hildebrandt et al., 2017). With their focus on relationality, compassion-based meditation may also be especially salient for young adults during a developmental period in which social relationships are often viewed as paramount (Masuda & Tully, 2012). Indeed, perceived closeness of peers is protective against psychiatric symptoms in undergraduates (Mason et al., 2014). Compassion-based meditation has been previously shown to reduce psychological distress in undergraduates (Martínez-Rubio et al., 2022). However, in contrast to mindfulness meditation, there have been relatively few rigorous randomized trials testing compassion-based meditation on large samples of undergraduates (Graser & Stangier, 2018).

Compassion-Based Meditation and Intrapersonal Processes

While compassion-based meditation practices often focus on others or relationships as the "object" of meditation, this is fundamentally a way for the meditator to strengthen their own

ability to generate compassion (Graser & Stangier, 2018). Thus, compassion is approached as a trainable skill that, through practice, can then be more easily called upon throughout one's life (Graser & Stangier, 2018), including toward oneself and one's inner experiences. Intrapersonal factors that may be especially salient for undergraduates and may be amenable to compassion-based meditation include *experiential avoidance*, *fear of missing out*, and *self-compassion*.

Experiential avoidance describes a transdiagnostic process wherein one's actions are excessively determined by conditioned, often avoidant, reactions to thoughts and emotions at the expense of more valued or effective actions (Levin et al., 2014). This non-accepting orientation toward inner experiences is a significant predictor of undergraduate mental health outcomes (Woodruff et al., 2013). Similarly, fear of missing out is characterized by a devaluing of one's own experiences and a pervasive concern that one is missing out on more fulfilling experiences, often leading to fixation on the activities of others (Przybylski et al., 2013). Fear of missing out is especially salient for young people on social media and is associated with depressive symptoms in undergraduates (Baker et al., 2016).

Compassion-based meditation practices that focus on appreciation of and happiness for others may help to reduce this self-focused dissatisfaction and promote self-compassion (Graser & Stangier, 2018). Self-compassion entails treating oneself with kindness; acknowledging challenges as shared aspects of the human experience; and being mindfully aware of painful thoughts and feelings (Neff, 2011). Self-compassion appears to be a source of resilience for undergraduates facing adversities such as the transition to college (Terry et al., 2013), peer victimization (Jiang et al., 2016), and academic failure (Neff et al., 2005).

Mobile Health Delivery

In the present study, we use the term mobile health (mHealth) to refer to the use of digital technology (e.g., smartphone apps) to provide or supplement health care. Meta-analytic data suggest that mHealth interventions broadly (Goldberg, et al., 2022a) and meditation-based mHealth interventions specifically produce beneficial effects for a range of psychological symptoms, including depression and anxiety (Gál et al., 2021). mHealth interventions may be especially appealing to undergraduate students, given that 96% of 19–26-year-olds in the United States report owning and using a smartphone, compared with 85% of the general population (Pew Research Center, 2021). Among undergraduates, app-based mental and behavioral health interventions appear to be effective and acceptable (Oliveira et al., 2021).

Meditation Dosage

There is evidence for the efficacy of mHealth meditation training (Gál et al., 2021); however, it remains largely unclear how these tools may be most effectively implemented. One crucial aspect of implementation that may impact both effectiveness and acceptability is meditation practice dosage. Thus far, the experimental study of meditation dosage has focused on amount and duration of practice, with mixed findings. In some studies, the benefits of meditation training appear to be positively associated with total minutes of meditation (Adams et al., 2018; Crane et al., 2014; Parsons et al., 2017) and longer duration of practice (Berghoff et al. 2017; Lacaille et al., 2018). However, other studies have found the benefits of meditation training to be unrelated to practice duration (Birtwell et al., 2019) or even negatively associated with total minutes of meditation (including for undergraduate beginners; Strohmaier et al., 2020). To our knowledge, no experimental work has investigated the potential role of *meditation dose frequency* – that is, how frequently an individual practices meditation within a given time period.

Meditation Dose Frequency

Within learning science, the frequency and timing of exposure to a stimulus are known to be important factors in the acquisition of new learning (Cepeda et al., 2006). Meta-analytic evidence reveals that *distributed* practice paradigms (i.e., shorter duration with more frequent repetition) lead to significantly better learning and retention than *massed* practice paradigms (i.e., longer duration with less frequent repetition; Cepeda et al., 2006). Frequency effects are well-known in the literature on language acquisition, with more frequent exposure to words leading to better learning and retention (Ellis, 2002). The effect of frequency is less clear within behavioral interventions. For example, within the context of behavioral therapy for anxiety disorders, the schedule of exposures (i.e., massed into a shorter timeframe vs. distributed into a longer timeframe) has shown inconsistent effects on outcomes (Weisman & Rodebaugh, 2018).

In a cross-sectional study of 218 current or former mindfulness practitioners, meditation dose frequency but not duration was positively associated with well-being (Birtwell et al., 2019); however, the cross-sectional study design makes it impossible to infer causality. Given evidence for dose frequency effects in learning generally and the unanswered questions about meditation dosage specifically (Strohmaier, 2020), it would be useful to experimentally investigate how dose frequency may influence effects of meditation training. It would also be useful to investigate ways to encourage informal meditation practice, given the unique benefits and relative lack of experimental research compared with formal meditation practice (Birtwell et al., 2019; Fredrickson et al., 2019). It is possible that more frequent formal meditation practice might encourage greater recall and application of these techniques throughout daily interactions (i.e., greater informal meditation practice). To our knowledge, this has not been tested experimentally.

Present Study

The current randomized trial investigated the experimental effects of randomly assigning distressed undergraduates ($N = 351$) to two different dose frequency conditions within the context of a two-week compassion-based meditation intervention delivered via mHealth. We secondarily assessed the feasibility of the study design and the acceptability of the intervention.

Preregistered Hypotheses

Feasibility. At least 75% of those randomized will complete post-test measures (i.e., ≤ 25% study attrition). Demonstrating our ability to manipulate dose frequency, participants in the Distributed condition will complete significantly more practices per day than those in the Massed condition. Demonstrating our ability to keep minutes and days of practice constant, there will be no differences by condition in average minutes of practice per day or total days of practice.

Acceptability. Participants in both conditions will report the intervention to be acceptable (System Usability Scale scores > 70; Bangor et al., 2008).

Dose Frequency Effects. Participants in both conditions will report significant pre-post improvements in psychological distress, loneliness, self-compassion, experiential avoidance, and fear of missing out. Participants in both conditions will show significant trajectories of improvement on daily measures of psychological distress and loneliness. Pre-post and daily improvements will be larger in the Distributed versus Massed condition (preregistered as exploratory). Participants in the Distributed condition will report greater informal practice throughout their day versus those in the Massed condition.

Methods

Participants

Undergraduate students were recruited by email at a large public university in the midwestern United States. Seven-hundred and seventy-two potential participants were assessed

for eligibility, of which 351 met inclusion criteria and were randomized to Massed (n = 176) or Distributed (n = 175) conditions. Inclusion criteria included: ≥18 years old, enrolled as an undergraduate student, access to a smartphone or other device capable of running the intervention app (Android or iOS), elevated anxiety and/or depression (t scores ≥ 55 on the PROMIS Depression and/or PROMIS Anxiety short-forms 4a; Pilkonis et al., 2011), and no significant meditation experience (defined as meditation retreat experience, meditation practice weekly for >1 year or daily practice within the previous 6 months, or previous training under the instruction of a meditation teacher other than an introductory course). Participants were excluded if they reported severe depression (t scores > 70 on the PROMIS Depression short-form 4a; Pilkonis et al., 2011). See Supplemental Materials Figure 1 for the CONSORT diagram.

Procedure

All procedures were approved by the Institutional Review Board of the University of Wisconsin - Madison. This study was preregistered at ClinicalTrials.gov NCT04741529 and through the Open Science Framework (https://osf.io/fmvw4; https://osf.io/rvhsb). We made three deviations from the preregistration. First, we chose to increase the sample size of the present study to allow evaluation of between-group dose frequency effects. Second, we conducted a sensitivity analysis with missing post-test data replaced using multiple imputation. Third, we did not consider adherence to be a metric of intervention acceptability, given participants were incentivized to adhere to their assigned experimental condition (as has been previously done to strengthen an experimental manipulation in mHealth meditation training; Lindsay et al., 2019).

This study took place between March and April of 2021. Recruitment materials described the study as a trial testing a smartphone app for student well-being. A link in the email directed students to a ~2-minute video of the principal investigator sharing additional details about the

study, followed by a brief screener survey based on inclusion/exclusion criteria. All screening procedures and pre-post data collection were carried out online using REDCap (Harris et al., 2019). The day after screening, eligible participants received an automated email invitation to complete an online consent form and a scheduling survey for the randomization meetings outlined below. Once scheduled, participants who confirmed their meeting time via email were sent a link to complete baseline measures.

After completing baseline measures, groups of five to fifteen participants attended a 60-minute online randomization meeting with two members of the study team via the Zoom teleconferencing platform. At this meeting, participants were given further information on participation and payment, after which they were randomized 1:1 to the Massed or Distributed condition using Zoom's random breakout room feature. Each study team member joined one of the breakout rooms, where participants installed and were oriented to the app and given further instruction. Participants in the Massed condition were instructed to complete a single 20-minute practice per day. Participants in the Distributed condition were instructed to complete two 10-minute practices per day, with at least four hours between each practice session. The 10- and 20-minute meditation practices differed in duration but included the same content. During the meeting, participants were instructed to set daily reminders (e.g., phone alarms) to help with adherence. At the end of the meeting, participants and study team members completed a 5-minute meditation practice together using the app.

Participants' progress through the app content was self-guided. Once each evening, participants received automated email reminders to complete brief daily measures via Qualtrics (https://www.qualtrics.com). Participants were provided with a study email address for questions or technical support. At the conclusion of the 14-day intervention, participants were contacted by

email to complete post-test measures. At the conclusion of the study, participants were paid up to $55 and had a chance to win a $200 lottery prize. To be eligible for payment, participants had to complete post-test measures (i.e., no payment was provided for completing only pre-test measures). Participants were paid $25 for completing post-test measures, a $15 bonus for completing 80% of assigned daily practices, and a $15 bonus for 80% of the daily measures. Participants who completed post-test measures and met both bonus criteria were entered into a lottery for one of two $200 prizes.

Intervention

We used a modified version of the Healthy Minds Program (HMP) app which is a freely available, self-guided smartphone app (Healthy Minds Innovations, 2019). HMP includes guided meditation practices along with brief psychoeducational content covering the science of well-being. The structure and content of HMP is based on evidence for core constituents of well-being (Dahl et al., 2020) and the benefits of meditation practices (Goldberg et al., 2022b). HMP has been shown to significantly reduce psychological distress and increase social connectedness in two previous randomized controlled trials (RCTs; Goldberg et al., 2020 [$N = 343$]; Hirshberg et al., 2022 [$N = 662$]).

The full HMP includes four modules of skills-based training in foundational components of well-being. Specific practices are taught to promote attentional skills and mindful awareness (Awareness module), social connectedness and prosociality (Connection module), insight regarding self-concept and mental habits (Insight module), and the clarification and enaction of core values and motivations (Purpose module; Healthy Minds Innovations, 2019). The current study employed content only from the Connection module, which primarily includes

compassion-based meditation practices. See Supplemental Materials Table 1 for an outline of intervention components.

Measures

A demographic questionnaire was completed at baseline.

Feasibility

Feasibility of the study design and success of the manipulation were assessed based on completion of post-test measures and adherence to the assigned dose frequency condition. Adherence was assessed objectively through HMP app usage data.

Acceptability

System Usability Scale. Acceptability of the intervention was assessed at post-test using the System Usability Scale; Bangor et al., 2008). The System Usability Scale is a 10-item scale designed to measure a respondent's affinity for a specific system or product (e.g., "I think I would like to use this product frequently"). Respondents rate each item on a 5-point Likert scale (1 = *strongly disagree*, 5 = *strongly agree*), with total scores scaled to range from 0 to 100. Higher scores indicate higher acceptability, with scores >70 indicating acceptability (Bangor et al., 2008). Internal consistency reliability in the present sample was high at post-test ($\alpha = .83$).

Outcomes Measures

Psychological Distress. Our preregistered primary outcome was a composite of the Patient-Reported Outcome Measures Information System (PROMIS) Depression and Anxiety scales (Pilkonis et al., 2011). Each scale is composed of four items (e.g., "I felt worthless" [depression], "I felt fearful" [anxiety]). Items are rated on a 5-point Likert scale (1 = *never*, 5 = *always*), with higher scores indicating greater severity in the past seven days. The measures yield *t* scores, with $t \geq 55$ defined as clinically elevated and $t > 70$ indicating severe impairment

(Kroenke et al., 2020; Schalet et al., 2014). Both measures have shown acceptable psychometric properties, including convergent validity with legacy measures and high internal consistency reliability (Choi et al., 2014; Schalet et al., 2014). The *t* scores were averaged to create a psychological distress composite. Internal consistency reliability in the present sample was high for both the PROMIS Depression (αs = .88 and .90 for pre- and post-test, respectively) and Anxiety (αs = .81, .81) scales and for the composite of the two *t* scores (αs = .75, .78). Psychological distress was also assessed daily throughout the intervention using the single highest loading items from each scale: "Today I felt hopeless" (depression) and "Today I found it hard to focus on anything other than my anxiety" (anxiety). Internal consistency reliability for the daily diary distress composite was acceptable (αs = .70, .79, and .73, for first day, last day, and averaged across all days, respectively).

Experiential Avoidance. Experiential avoidance was assessed using the Acceptance and Action Questionnaire (AAQ-II), a 7-item scale designed to measure negative and avoidant orientation toward thoughts and feelings (Bond et al., 2011). Respondents use a 7-point Likert scale (1 = *never true*, 7 = *always true*) to indicate how often they have the experience described in each item (e.g., "My painful experiences and memories make it difficult for me to live a life that I would value"). Higher scores reflect higher levels of experiential avoidance. The AAQ-II has shown predictive and discriminant validity and high internal consistency and test-retest reliability across diverse samples (Bond et al., 2011). Internal consistency reliability in the present sample was high (αs = .89, .89).

Fear of Missing Out. The Fear of Missing Out scale (FoMOs) was used to assess fear of missing out (Przybylski et al., 2013). Respondents use a 5-point Likert scale (1 = *no, not true of me*, 5 = *yes, extremely true of me*) to respond to ten items (e.g., "I fear others have more

rewarding experiences than me." Higher scores reflect higher levels of fear of missing out. The FoMOs has shown predictive validity (e.g., correlating with social media use) and high internal consistency reliability (Przybylski et al., 2013). Internal consistency reliability in the present sample was high (αs = .86, .88).

Loneliness. Loneliness was assessed using the NIH Toolbox Loneliness scale (Cyranowski et al., 2013). Respondents use a 5-point Likert scale (1 = *never*, 5 = *always*) to indicate how often they experience loneliness (e.g., "I feel alone and apart from others"). Higher scores reflect higher levels of perceived loneliness. The NIH Toolbox Loneliness scale has shown high convergent validity with legacy measures and high internal consistency reliability (Cyranowski et al., 2013). Internal consistency reliability in the present sample was high (αs = .89, .87). Loneliness was also assessed daily throughout the intervention using a single item adapted from the NIH Toolbox Loneliness scale: "How lonely did you feel today?"

Self-Compassion. Self-compassion was assessed using the 12-item Self-Compassion Scale, short-form (SCS-SF; Neff, 2003; Raes et al., 2011). Respondents use a 7-point Likert scale (1 = *almost never*, 5 = *almost always*) to indicate how often they have the experience described in each of the items (e.g., "When I fail at something important to me, I become consumed by feelings of inadequacy"). Higher scores reflect higher levels of self-compassion. The SCS-SF has shown high correlations with the long-form version of the measure as well as factorial validity and high internal consistency reliability (Raes et al., 2011). Internal consistency in the present sample was high (αs = .81, .86).

Informal Practice

Informal practice was measured daily using a single item: "As you reflect on today, to what extent did you apply these practices? (For example, intentionally feeling more warmth or

appreciation toward others).” Participants responded using a visual analogue scale (i.e., horizontal slider; 0 = *not at all*, 100 = *all day long*), with higher scores representing greater amount of informal practice. Daily informal meditation practice has been previously assessed using similar single-item scales (e.g., Fredrickson et al., 2019).

Data Analysis

Independent *t* tests (for continuous variables) and χ^2 tests (for categorical variables) were conducted to assess success of randomization at baseline. A one-sample test of proportions was used to compare study completion with 75% and a one-sample *t* test was used to compare System Usability Scale ratings with 70. Independent *t* tests were used to compare Massed versus Distributed conditions on continuous variables (average number of practices per day, average number of minutes of practice per day, average number of days of practice, and average informal practice) and logistic regression was used to compare Massed versus Distributed conditions on study completion. Paired *t* tests were used to assess pre-post changes in outcomes for the total sample. Linear regression was used to examine between-group differences in pre-post outcomes. Specifically, models regressed post-test scores (e.g., post-test psychological distress composite) onto treatment condition and pre-test scores (e.g., pre-test psychological distress composite). Cohen’s *d*s were calculated as Massed minus Distributed. Two-level multilevel models (observations nested within participants) were used to examine trajectories of daily change in psychological distress and loneliness. Initial models predicted daily ratings of psychological distress or loneliness from time (i.e., day; Equation 1, Supplemental Materials Table 2). Subsequent models evaluated whether trajectories of change differed between treatment condition (Equation 2, Supplemental Materials Table 2). In these models, psychological distress or loneliness was predicted by time, group assignment (i.e., Massed vs. Distributed), and the

interaction between time and group assignment. The interaction term indicated whether trajectories of change in psychological distress varied across groups.

Sensitivity analyses were conducted to examine the effect of removing outliers (i.e., values three standard deviations [*SD*s] from the mean) as outliers can bias study results. We also used multivariate imputation by chained equations (MICE; van Buuren & Groothuis-Oudshoorn, 2011) to examine the sensitivity of our results for the primary regression models to the impact of missing data (which can both bias study results and reduce statistical power; Graham. 2009). We created 100 imputed data sets using all available pre- and post-test outcome measures along with numeric forms of baseline demographic variables (continuous age, dichotomized female gender, White race/ethnicity, lowest income category, and straight sexual orientation). Results were pooled using Rubin's rules implemented in the 'mice' package in R (van Buuren & Groothuis-Oudshoorn, 2011). For multilevel models, missing data were handled using maximum likelihood estimation, which is the default in the 'lme4' package (Bates et al., 2015). Both multivariate imputation and maximum likelihood estimation are robust to data missing at random (Graham, 2009). R code for all analyses is provided in Supplemental Materials Table 3 and de-identified data are available through the Open Science Framework: https://osf.io/aqzmg/

Statistical Power

Using the 'pwr.t.test' function for a two-sample *t* test in the 'pwr' package in R (Champely, 2020), it was determined during data collection that a sample of $n \geq 350$ would be adequate to detect small-to-moderate between-group differences ($d = 0.30$). Accordingly, a sample of $N = 351$ was recruited, 316 of whom completed post-test (i.e., 90.0%). This sample size ($n = 316$) is adequate to detect small pre-post changes in the total sample ($d = 0.16$) and

small-to-moderate between-group differences based on treatment condition ($d \geq 0.32$) with 80% power.

Data Transparency Statement

There are no previously published or currently in press works stemming from this dataset.

Results

Sample demographics are provided in Supplemental Materials Table 4. The sample was predominantly female (77.8%) and non-Hispanic White (83.2%), with a mean age of 20.17 years (SD = 1.58). Groups did not differ at baseline on demographic variables (ps = .277 to .981) or any of the outcome measures (ds ≤ 0.19, ps = .074 to .229), with the exception of age which was higher in the Massed condition (d = 0.34, p = .002) and self-compassion which was lower in the Massed condition (d = -0.29, p = .008). We evaluated the effect of the baseline imbalance on age by conducting sensitivity analyses for the between-group models, controlling for age. Baseline differences on self-compassion were accounted for by modeling baseline levels in the regression analyses. Skewness and kurtosis were below recommended cutoffs for allowable deviations from normality for all five pre-post outcome variables and the daily diary distress ratings (skewness ≤ |0.65|, kurtosis ≤ |0.76|; Curran et al., 1996). Descriptive statistics and intercorrelations of pre-post outcome variables are provided in Table 1 and Supplemental Materials Table 5. Descriptive statistics of daily measures are provided in Figure 1 and Supplemental Materials Table 6. The CONSORT Checklist is provided in Supplemental Materials Table 7.

Feasibility was evaluated based on study completion (i.e., ≥ 75% across the total sample) and success of the manipulation (i.e., more practices per day for Distributed versus Massed participants, absence of group differences in minutes of practice per day and total days of practice). No group differences were observed on study completion (Massed = 92.0%,

Distributed = 88.0%; *OR* = 1.58, *p* = .209), with 90.0% of all randomized participants completing the study which exceeded the pre-specified target of 75% (*p* < .001). Average number of practices per day was greater for participants in the Distributed (*M* = 1.63, *SD* = 0.52) versus Massed (*M* = 0.84, *SD* = 0.22) condition *(d* = 1.99*, p* < .001). Average minutes of practice per day were equivalent for the Massed and Distributed conditions (Massed: *M* = 16.76, *SD* = 4.45; Distributed: *M* = 16.29, *SD* = 5.16; *d* = 0.10, *p* = .360). Total days of practice were also equivalent for the two conditions (Massed: *M* = 11.85, *SD* = 2.95; Distributed: *M* = 12.27, *SD* = 3.47; *d* = -0.13, *p* = .220).

Acceptability was evaluated subjectively (i.e., System Usability Scale scores >70; Bangor et al., 2008). System Usability Scale scores across both groups (*M* = 85.72, *SD* =11.43) were significantly greater than 70 (*t* = 24.44, *df* = 315, *p* < .001).

When pooled across conditions, participants exhibited significant pre-post improvements on the primary outcome of psychological distress (*d* = -0.52, *p* < .001), as well as on experiential avoidance (*d* = -0.34, *p* < .001), fear of missing out (*d* = -0.12, *p* = .001), loneliness (*d* = -0.63, *p* < .001), and self-compassion (*d* = 0.39, *p* < .001). Participants also exhibited significant trajectories of improvement on daily measures of psychological distress (*B* = -0.010, *p* = .002) and loneliness (*B* = -0.024, *p* < .001).

There were no differences between Massed versus Distributed practice on any pre-post measures: psychological distress (*d* = -0.01, *p* = .553), experiential avoidance (*d* = -0.07, *p* = .682), fear of missing out (*d* = -0.09, *p* = .347), loneliness (*d* = -0.16, *p* = .158), or self-compassion (*d* = 0.09, *p* = .729, see Supplemental Materials Table 8). Trajectories of change in daily measures of distress and loneliness did not differ between the Massed and Distributed conditions (time x group *B*s = 0.0038 and 0.014, *p*s = .562 and .197, respectively, with

Distributed coded as the reference group). Daily informal practice also did not differ for participants in Massed (M = 50.30, SD = 16.37) and Distributed (M = 50.44, SD = 18.46) conditions (d = 0.01, p = .940).

We conducted several sensitivity analyses to assess the robustness of the dose frequency effects models. Significance tests were unchanged with and without outliers included, when using multiple imputation to account for missing pre-post data, and when controlling for age.

Discussion

The current randomized trial investigated feasibility, acceptability, and dose frequency effects of a two-week compassion-based meditation intervention delivered via the HMP smartphone app. Undergraduates with elevated depression and/or anxiety (N = 351) were randomized to one of two dose frequency conditions: Massed (i.e., a single 20-minute meditation per day) or Distributed (i.e., two 10-minute meditations per day). Findings supported study feasibility. There were high rates of study completion across the total sample (90.0%), with no between-group differences. This attrition rate of 10.0% is considerably lower than the 24.1% attrition rate common to mHealth mental health interventions generally, and similar to the 11.2% attrition rate found in mHealth studies using in-person interviews for enrollment (Linardon & Fuller-Tyszkiewicz, 2019). Results support success of the manipulation, with participants in the Distributed condition completing significantly more practices per day than those in the Massed condition (1.63 versus 0.84, respectively) and no group differences in average minutes of practice per day or total days of practice. Results also support acceptability of the intervention. System Usability Scale scores (M = 88.58, SD = 9.15) were significantly greater than 70, indicating high acceptability of the HMP app (Bangor et al., 2008).

Contrary to expectations, meditation dose frequency did not influence outcomes. The overall sample showed improvement on all pre-post outcomes (absolute value of *d*s = 0.12 to 0.63) and significant trajectories of improvement on all daily measures (*p*s ≤ .010). However, there were no differences between groups on any pre-post measures (absolute value of *d*s = 0.01 to 0.16, *p*s = .158 to .729), on daily distress or daily loneliness (*p*s = .562 and .197, respectively), or on informal practice (*d* = 0.01, p = .940). Thus, it appears that two 10-minute meditation sessions distributed throughout a day produced effects equivalent to a single, 20-minute practice per day.

To our knowledge, this is the first time that dose frequency effects have been investigated in a randomized trial of either a meditation-based or mHealth intervention. The null between group findings are notable in light of the importance of dose frequency for other types of learning (Cepeda et al., 2006; Ellis, 2002). It is possible that the learning of meditation skills differs in meaningful ways from other forms of learning, such that having more frequent opportunities for meditation practice may not produce larger benefits. This null finding is consistent with a lack of clear impact of exposure therapy timing (Weisman & Rodebaugh, 2018). At once, it remains possible that meditation dose frequency may impact outcomes in some instances. For example, it may be that a different frequency (i.e., varied interstudy interval; Cepeda et al., 2006) may have produced different results. Future studies could examine wider variations in dose frequency (e.g., twenty 1-minute sessions, four 5-minute sessions, one 40-minute session every other day). It also may be that some individuals may find a particular dose frequency especially helpful. Moreover, it may be that the impact of dose frequency is different in the context of traditional in-person meditation interventions (e.g., mindfulness-based cognitive therapy [MBCT]; Segal et al., 2013) which provide more intensive training (e.g., weekly 2-hour

classes for 8 weeks with a recommendation of 45 minutes of daily home meditation practice) than the mHealth meditation intervention tested here.

Given a lack of difference between conditions, findings support flexibility in how individuals distribute meditation practice throughout their day at least in the mHealth context. In theory, this may help to increase accessibility of meditation practice. Some individuals may find longer, less frequent meditation practices easier to complete each day than shorter, more frequent practices. In keeping with this possibility, prior research has found that medication adherence decreases as dose frequency increases (Coleman et al., 2012). At once, some individuals may instead prefer shorter, more frequent practices. In beginning meditators, for example, there is evidence that increased duration of practice may be negatively associated with adherence to daily practice (Adams et al., 2018).

Future Directions

Continuing to clarify the total amount, duration, and frequency of meditation needed to yield benefits is important for optimizing mHealth and meditation interventions. As noted above, future RCTs could investigate interactions between various aspects of dosage and participant-level characteristics (e.g., individual preference, early response to meditation, expectation of benefit, level of distress at baseline). It is possible that such characteristics may moderate response to different amounts, durations, or frequencies of meditation practice. It would be especially useful for future RCTs to examine these research questions over a longer period to investigate the possibility that such factors are dynamic and may change both between and within participants across time (e.g., effects and preferences may fluctuate with affect, or may evolve as beginners gain more meditation experience; Lutz et al., 2015). It is also possible that two weeks of practice was simply not enough time to detect dose frequency effects.

Additionally, it would be useful to investigate potential dose frequency effects of other meditation practices, as practice frequency may play a more central role in attention-training practices than in the compassion-based meditation practices in the present study. Examining a wider variety of dose frequency conditions would be worthwhile as would investigating dose frequency within traditional in-person meditation training (e.g., meditation retreats versus months-long daily practice with equivalent total minutes of practice). It would also be interesting to investigate other aspects of dosage (e.g., exact time of practice) that were not well controlled in the design of the present study. Future research will also need to include more diverse samples. It would also be valuable to include objective measures (e.g., behavioral tasks, biological measures) that may be more sensitive to dosage manipulations and may perhaps better reflect the proximal skills acquired through early meditation training.

Limitations

There are notable limitations of the current study. First, due to the poor rates of adherence and attrition seen in many fully remote mHealth interventions (Linardon & Fuller-Tyszkiewicz, 2019), we provided monetary incentives to encourage adherence and study completion. While increasing internal validity (i.e., potency of the experimental manipulation), there were likely costs to external validity (i.e., ability to generalize study findings to real-world conditions, ability to draw inferences about other factors that may have contributed to adherence). Second, the lack of control condition makes it impossible to conclude that the intervention produced changes in outcomes, as there are viable alternative explanations for the observed changes (e.g., regression-to-the-mean, history effects, expectancy or placebo effects; Torous & Firth, 2016). Given prior randomized controlled trials establishing the efficacy of the intervention (Goldberg et al., 2020; Hirshberg et al., 2022), this study instead focused on dose frequency manipulation. Third, the

demographics of the present sample (e.g., predominantly White, female) limit generalizability to other populations. Fourth, we relied heavily on self-report measures which have known limitations (e.g., social desirability bias; Heppner et al., 2016).

Conclusions

Despite these limitations, the present study adds to our understanding of dosage within meditation training. Contrary to the effects of dose frequency on other types of learning (Cepeda et al., 2006), it does not appear that meditation dose frequency generally influences outcomes when total amount of meditation per day is held constant. Having greater flexibility to choose the duration and frequency of one's meditation practice without sacrificing potential benefits may help to increase the acceptability and utilization of meditation as a health behavior.

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Table 1

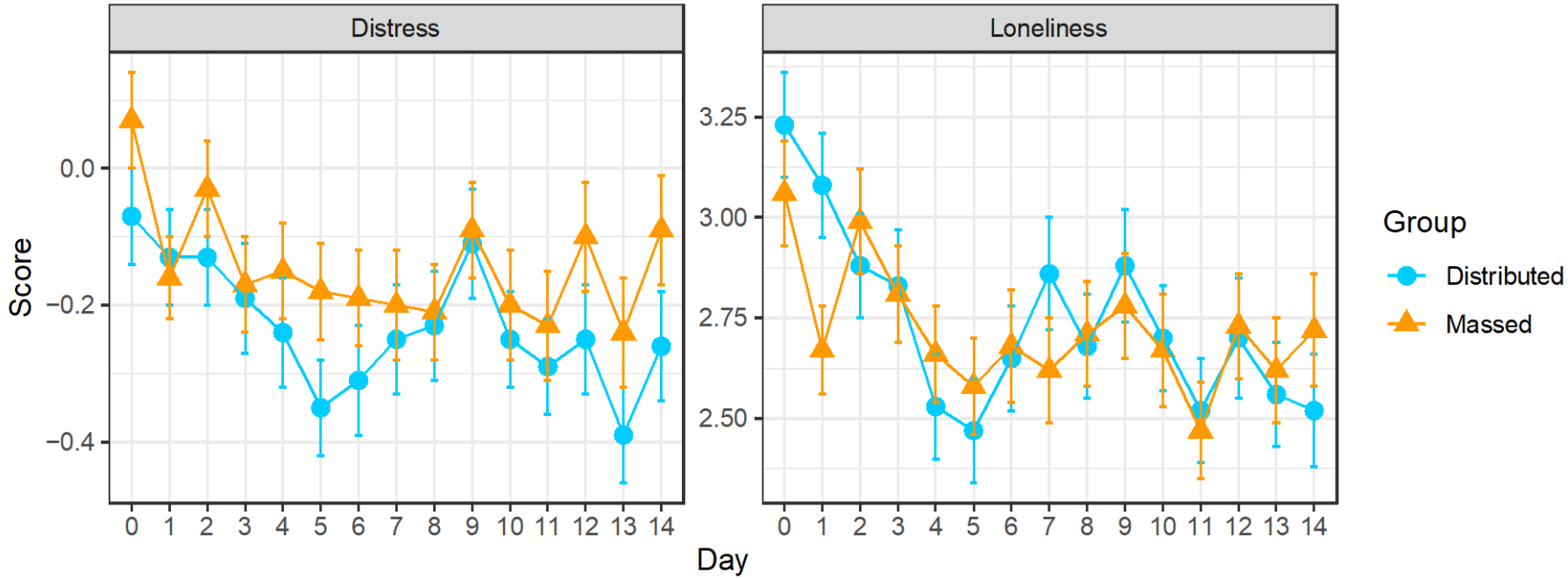
Descriptive Statistics and Effect Sizes for Pre-Post Outcomes

	Pre			Post			Within-Group Effect	
	n	Mean	*SD*	*n*	Mean	*SD*	*d*	*p*
				Massed				
Distress	176	61.58	6.11	162	58.22	6.57	-0.53	< .001
Exp Avoid	176	4.02	1.34	162	3.52	1.33	-0.38	< .001
FoMO	176	2.80	0.78	162	2.67	0.76	-0.16	.001
Loneliness	176	2.94	0.92	162	2.32	0.82	-0.71	< .001
Self-Comp	176	2.56	0.56	162	2.84	0.68	0.44	< .001
				Distributed				
Distress	175	60.59	5.92	154	57.27	6.82	-0.52	< .001
Exp Avoid	175	3.76	1.34	154	3.35	1.31	-0.31	< .001
FoMO	175	2.69	0.81	153	2.63	0.82	-0.07	.207
Loneliness	175	2.82	0.86	154	2.37	0.79	-0.55	< .001
Self-Comp	175	2.74	0.66	153	2.97	0.68	0.34	< .001
				Total Sample				
Distress	351	61.08	6.03	316	57.76	6.70	-0.52	< .001
Exp Avoid	351	3.89	1.34	316	3.44	1.32	-0.34	< .001
FoMO	351	2.74	0.79	315	2.65	0.79	-0.12	.001
Loneliness	351	2.88	0.89	316	2.34	0.80	-0.63	< .001
Self-Comp	351	2.65	0.62	315	2.90	0.68	0.39	< .001

Note. Massed = massed practice condition (one 20-minute session per day); Distributed = distributed practice condition (two 10-minute sessions per day); Distress = psychological distress (composite of the 4-item PROMIS Depression and PROMIS Anxiety scales); Exp Avoid = experiential avoidance (Acceptance and Avoidance Questionnaire – II); FoMO = fear of missing out (Fear of Missing Out scale); Loneliness = NIH Toolbox Loneliness scale; Self-Comp = self-compassion (Self-Compassion Scale – Short Form). Between-group *d* calculated as Massed minus Distributed.

Figure 1

Daily Measures



Note. Massed = massed practice condition (one 20-minute session per day); Distributed = distributed practice condition (two 10-minute sessions per day); vertical bars represent one standard error; Distress = psychological distress (composite of the 4-item PROMIS Depression and PROMIS Anxiety scales); Loneliness = NIH Toolbox Loneliness scale. n = 348 completed one or more daily measures.

Supplemental Materials Table 1

Outline of Intervention Components

Day	Type	Content	Audio Practice Tip
Baseline	Learn	Train the Mind, Rewire the Brain	
Baseline	Practice	Appreciation	One Good Thing
1	Learn	Negativity Bias	
1	Practice	Feeling Appreciation	Appreciating the Body
2	Practice	Valuing Friends and Loved Ones	Finding Common Ground
3	Learn	What's Right?	
3	Practice	Appreciating Friends and Loved Ones	
4	Practice	Self-Worth	Notice the Little Things
5	Learn	Noticing the Positive	
5	Practice	Seeing the Good in Ourselves	
6	Learn	Our Common Humanity	
6	Practice	Gratitude	Showing Appreciation
7	Practice	Valuing Strangers	People You Don't Notice
8	Learn	New Directions	
8	Practice	Appreciating Those We Don't Know	
9	Learn	A Mirror to the World	
9	Practice	Appreciation for Those We Find Challenging	Transforming Boredom
10	Learn	Feeling Connected	
10	Practice	Be Kind to Yourself	Self-Care
11	Practice	Friends and Family	Motivated by Kindness
12	Learn	Survival of the Kindest	
12	Practice	Compassion is our nature	The Golden Rule
13	Learn	It's Always Here	
13	Practice	Compassion in Difficult Situations	Pause for Compassion
14	Learn	Courage to Heal	
14	Practice	Compassion for All Beings	Everyone Suffers

Note. Learn = brief didactic psychoeducational recordings covering the science of well-being (~3-5 minutes); Practice = guided meditation practices (10 minutes twice per day or 20 minutes once per day, depending on group assignment); Audio Practice Tips = brief guidance on integrating each meditation practice into daily life (~1-2 minutes).

Supplemental Materials Table 2

Multilevel Model Formulas

$$Y_{ti} = \beta_{00} + \beta_{10} * (Time) + [U_{0j} + e_{ij}], \quad \text{(Equation 1)}$$

where distress (*Y*) at time point *t* for participant *i* is predicted by a fixed intercept (β_{00}; i.e., grand mean), a fixed slope for time (β_{10}), a random intercept (U_{0j}), and a residual error term (e_{ij}).

$$Y_{ti} = \beta_{00} + \beta_{10} * (Time) + \beta_{01} * (Group) + \beta_{11} * (Time * Group) + [U_{0j} + e_{ij}], \text{ (Equation 2)}$$

where distress (*Y*) at time point *t* for participant *i* is predicted by a fixed intercept (β_{00}; i.e., grand mean), a fixed slope for time (β_{10}), a fixed slope for group (β_{01}), a fixed slope for the interaction between time and group (β_{11}), a random intercept (U_{0j}), and a residual error term (e_{ij}).

Supplemental Materials Table 3

R Code

```
#R code####
library(lme4)
library(lmerTest)

#creating cohen's d function
cohens_d <- function(x, y) {
  lx <- length(x)- 1 #n for vector x
  ly <- length(y)- 1 #n for vector y
  md  <- mean(x, na.rm=TRUE) - mean(y, na.rm=TRUE)       ## mean difference
(numerator)
  csd <- lx * var(x, na.rm=TRUE) + ly * var(y, na.rm=TRUE) #Ignores NAs
  csd <- csd/(lx + ly)
  csd <- sqrt(csd)                     ## common sd computation

  cd  <- md/csd                        ## cohen's d
  print(cd)
}

#Reading in Data
df <- read.csv(file.choose()) #df_jcp.csv
df.day <- read.csv(file.choose()) #df_day_jcp.csv

#Compare Groups at Baseline
#demographics
t.test(df[df$groupMassed==1,"sr_t1_demog_age"],df[df$groupMassed==0,"sr_t1_de
mog_age"]) #p = .002
cohens_d(df[df$groupMassed==1,"sr_t1_demog_age"],df[df$groupMassed==0,"sr_t1_
demog_age"]) #d = 0.34

table(df$groupMassed,df$sr_t1_demo_female)
chisq.test(df$groupMassed,df$sr_t1_demo_female) #p = .875

table(df$groupMassed,df$sr_t1_demog_raceWhite)
chisq.test(df$groupMassed,df$sr_t1_demog_raceWhite) #p = .981

table(df$groupMassed,df$sr_t1_demog_orientStraight)
chisq.test(df$groupMassed,df$sr_t1_demog_orientStraight) #p = .277

table(df$groupMassed,df$sr_t1_demog_ses)
df$sr_t1_demog_ses_low <- ifelse(df$sr_t1_demog_ses==1,yes=1,no=0)
table(df$groupMassed,df$sr_t1_demog_ses_low)
chisq.test(df$groupMassed,df$sr_t1_demog_ses_low) #p = .481

#outcomes
t.test(df[df$groupMassed==1,"sr_t1_distress"],df[df$groupMassed==0,"sr_t1_dis
tress"]) #p = .122
cohens_d(df[df$groupMassed==1,"sr_t1_distress"],df[df$groupMassed==0,"sr_t1_d
istress"]) #d = 0.17

t.test(df[df$groupMassed==1,"sr_t1_aaqII_avoid"],df[df$groupMassed==0,"sr_t1_
aaqII_avoid"]) #p = .075
```

```
cohens_d(df[df$groupMassed==1,"sr_t1_aaqII_avoid"],df[df$groupMassed==0,"sr_t
1_aaqII_avoid"]) #d = 0.19

t.test(df[df$groupMassed==1,"sr_t1_fomo_fomo"],df[df$groupMassed==0,"sr_t1_fo
mo_fomo"]) #p = .183
cohens_d(df[df$groupMassed==1,"sr_t1_fomo_fomo"],df[df$groupMassed==0,"sr_t1_
fomo_fomo"]) #d = 0.14

t.test(df[df$groupMassed==1,"sr_t1_nihToolbox_lonely"],df[df$groupMassed==0,"
sr_t1_nihToolbox_lonely"]) #p = .229
cohens_d(df[df$groupMassed==1,"sr_t1_nihToolbox_lonely"],df[df$groupMassed==0
,"sr_t1_nihToolbox_lonely"]) #d = 0.13

t.test(df[df$groupMassed==1,"sr_t1_scsSf_selfComp"],df[df$groupMassed==0,"sr_
t1_scsSf_selfComp"]) #p = .008
cohens_d(df[df$groupMassed==1,"sr_t1_scsSf_selfComp"],df[df$groupMassed==0,"s
r_t1_scsSf_selfComp"]) #d = -0.29

#Assess Feasibility
#study completion
prop.table(table(df$t2_completion))
prop.test(x = table(df$t2_completion)[2],
          n = dim(df)[1], p = .75, alternative = "two.sided",
          correct = TRUE) #p < .001

table(df$groupMassed,df$t2_completion)
prop.table(table(df$groupMassed,df$t2_completion),margin=1)
summary(glm(t2_completion ~ groupMassed, data = df, family = "binomial")) #p
= .209
exp(summary(glm(t2_completion ~ groupMassed, data = df, family =
"binomial"))$coef[2,1]) #OR = 1.58

#practices per day
t.test(df[df$groupMassed==1,"sum_practice_per_day"],df[df$groupMassed==0,"sum
_practice_per_day"]) #p < .001
cohens_d(df[df$groupMassed==1,"sum_practice_per_day"],df[df$groupMassed==0,"s
um_practice_per_day"]) #d = -1.99

#minutes per day
t.test(df[df$groupMassed==1,"sum_mins_per_day"],df[df$groupMassed==0,"sum_min
s_per_day"]) #p = .360
cohens_d(df[df$groupMassed==1,"sum_mins_per_day"],df[df$groupMassed==0,"sum_m
ins_per_day"]) #d = 0.10

#days of practice
t.test(df[df$groupMassed==1,"sum_days"],df[df$groupMassed==0,"sum_days"]) #p
= .220
cohens_d(df[df$groupMassed==1,"sum_days"],df[df$groupMassed==0,"sum_days"])
#d = -0.13

#Acceptability
#sus vs. 70
t.test(df$sr_t2_sus_usabilityR, mu = 70, alternative = "two.sided") #sig
higher

#Combined Group Pre-post Changes
#distress
```

```
t.test(df$sr_t2_distress,df$sr_t1_distress,paired=TRUE) #p < .001
cohens_d(df$sr_t2_distress,df$sr_t1_distress) #d = -0.52

#aaq
t.test(df$sr_t2_aaqII_avoid,df$sr_t1_aaqII_avoid,paired=TRUE) #p < .001
cohens_d(df$sr_t2_aaqII_avoid,df$sr_t1_aaqII_avoid) #d = -0.34

#fomo
t.test(df$sr_t2_fomo_fomo,df$sr_t1_fomo_fomo,paired=TRUE) #p = .001
cohens_d(df$sr_t2_fomo_fomo,df$sr_t1_fomo_fomo) #d = -0.12

#loneliness
t.test(df$sr_t2_nihToolbox_lonely,df$sr_t1_nihToolbox_lonely,paired=TRUE) #p
< .001
cohens_d(df$sr_t2_nihToolbox_lonely,df$sr_t1_nihToolbox_lonely) #d = -0.63

#self-compassion
t.test(df$sr_t2_scsSf_selfComp,df$sr_t1_scsSf_selfComp,paired=TRUE) #p < .001
cohens_d(df$sr_t2_scsSf_selfComp,df$sr_t1_scsSf_selfComp) #d = 0.39

#Combined Group Daily Diary
#distress
summary(lmer(distress ~ day + (1|idR), data = df.day)) #sig decrease (p <
.001)

#loneliness
summary(lmer(lonely ~ day + (1|idR), data = df.day)) #sig decrease (p < .001)

#Within-Group Pre-Post Changes
#Massed condition
#distress
t.test(df[df$groupMassed==1,"sr_t2_distress"],df[df$groupMassed==1,"sr_t1_dis
tress"],paired=TRUE) #p < .001
cohens_d(df[df$groupMassed==1,"sr_t2_distress"],df[df$groupMassed==1,"sr_t1_d
istress"]) #d = -0.52

#aaq
t.test(df[df$groupMassed==1,"sr_t2_aaqII_avoid"],df[df$groupMassed==1,"sr_t1_
aaqII_avoid"],paired=TRUE) #p < .001
cohens_d(df[df$groupMassed==1,"sr_t2_aaqII_avoid"],df[df$groupMassed==1,"sr_t
1_aaqII_avoid"]) #d = -0.38

#fomo
t.test(df[df$groupMassed==1,"sr_t2_fomo_fomo"],df[df$groupMassed==1,"sr_t1_fo
mo_fomo"],paired=TRUE) #p = .001
cohens_d(df[df$groupMassed==1,"sr_t2_fomo_fomo"],df[df$groupMassed==1,"sr_t1_
fomo_fomo"]) #d = -0.16

#loneliness
t.test(df[df$groupMassed==1,"sr_t2_nihToolbox_lonely"],df[df$groupMassed==1,"
sr_t1_nihToolbox_lonely"],paired=TRUE) #p < .001
cohens_d(df[df$groupMassed==1,"sr_t2_nihToolbox_lonely"],df[df$groupMassed==1
,"sr_t1_nihToolbox_lonely"]) #d = -0.71

#self-compassion
t.test(df[df$groupMassed==1,"sr_t2_scsSf_selfComp"],df[df$groupMassed==1,"sr_
t1_scsSf_selfComp"],paired=TRUE) #p < .001
```

```
cohens_d(df[df$groupMassed==1,"sr_t2_scsSf_selfComp"],df[df$groupMassed==1,"s
r_t1_scsSf_selfComp"]) #d = 0.44

#Distributed condition
#distress
t.test(df[df$groupMassed==0,"sr_t2_distress"],df[df$groupMassed==0,"sr_t1_dis
tress"],paired=TRUE) #p < .001
cohens_d(df[df$groupMassed==0,"sr_t2_distress"],df[df$groupMassed==0,"sr_t1_d
istress"]) #d = -0.52

#aaq
t.test(df[df$groupMassed==0,"sr_t2_aaqII_avoid"],df[df$groupMassed==0,"sr_t1_
aaqII_avoid"],paired=TRUE) #p < .001
cohens_d(df[df$groupMassed==0,"sr_t2_aaqII_avoid"],df[df$groupMassed==0,"sr_t
1_aaqII_avoid"]) #d = -0.31

#fomo
t.test(df[df$groupMassed==0,"sr_t2_fomo_fomo"],df[df$groupMassed==0,"sr_t1_fo
mo_fomo"],paired=TRUE) #p = .207
cohens_d(df[df$groupMassed==0,"sr_t2_fomo_fomo"],df[df$groupMassed==0,"sr_t1_
fomo_fomo"]) #d = -0.07

#loneliness
t.test(df[df$groupMassed==0,"sr_t2_nihToolbox_lonely"],df[df$groupMassed==0,"
sr_t1_nihToolbox_lonely"],paired=TRUE) #p < .001
cohens_d(df[df$groupMassed==0,"sr_t2_nihToolbox_lonely"],df[df$groupMassed==0
,"sr_t1_nihToolbox_lonely"]) #d = -0.55

#self-compassion
t.test(df[df$groupMassed==0,"sr_t2_scsSf_selfComp"],df[df$groupMassed==0,"sr_
t1_scsSf_selfComp"],paired=TRUE) #p < .001
cohens_d(df[df$groupMassed==0,"sr_t2_scsSf_selfComp"],df[df$groupMassed==0,"s
r_t1_scsSf_selfComp"]) #d = 0.34

#Between-Group Pre-Post Changes
summary(lm(sr_t2_distress ~ sr_t1_distress + groupMassed, data = df)) #p =
.553
summary(lm(sr_t2_aaqII_avoid ~ sr_t1_aaqII_avoid + groupMassed, data = df))
#p = .682
summary(lm(sr_t2_fomo_fomo ~ sr_t1_fomo_fomo + groupMassed, data = df)) #p =
.347
summary(lm(sr_t2_nihToolbox_lonely ~ sr_t1_nihToolbox_lonely + groupMassed,
data = df)) #p = .158
summary(lm(sr_t2_scsSf_selfComp ~ sr_t1_scsSf_selfComp + groupMassed, data =
df)) #p = .729

#Between-Group Daily Diary
summary(lmer(distress ~ day*groupMassed + (1|idR), data = df.day))
#interaction p = .413
summary(lmer(lonely ~ day*groupMassed + (1|idR), data = df.day)) #interaction
p = .084

#Between-Group Informal Practice
t.test(df[df$groupMassed==0,"informal_avg"],df[df$groupMassed==1,"informal_av
g"]) #p = .940
cohens_d(df[df$groupMassed==0,"informal_avg"],df[df$groupMassed==1,"informal_
avg"]) #d = 0.01
```

```
#Between-Group Model Controlling for Age
summary(lm(sr_t2_distress ~ sr_t1_distress + sr_t1_demog_age + groupMassed,
data = df)) #ns
summary(lm(sr_t2_aaqII_avoid ~ sr_t1_aaqII_avoid + sr_t1_demog_age +
groupMassed, data = df)) #ns
summary(lm(sr_t2_fomo_fomo ~ sr_t1_fomo_fomo + sr_t1_demog_age + groupMassed,
data = df)) #ns
summary(lm(sr_t2_nihToolbox_lonely ~ sr_t1_nihToolbox_lonely +
sr_t1_demog_age + groupMassed, data = df)) #ns
summary(lm(sr_t2_scsSf_selfComp ~ sr_t1_scsSf_selfComp + sr_t1_demog_age +
groupMassed, data = df)) #ns

summary(lmer(distress ~ sr_t1_demog_age + day*groupMassed + (1|idR), data =
df.day)) #interaction p = .413
summary(lmer(lonely ~ sr_t1_demog_age + day*groupMassed + (1|idR), data =
df.day)) #interaction p = .085

#Between-Group Regression Models with Multiple Imputation
vars <- c("groupMassed","sr_t1_demog_age","sr_t1_demo_female",
          "sr_t1_demog_raceWhite","sr_t1_demog_orientStraight",

"sr_t1_demog_ses_low","sr_t2_distress","sr_t1_distress","sr_t2_aaqII_avoid",

"sr_t1_aaqII_avoid","sr_t2_nihToolbox_lonely","sr_t1_nihToolbox_lonely",

"sr_t2_scsSf_selfComp","sr_t1_scsSf_selfComp","sr_t2_fomo_fomo","sr_t1_fomo_f
omo")

head(df[,vars]) #looks good, all the variables in numeric form
str(df[,vars])

df.mi <- df[,vars] #create copy for multiple imputation

#impute data sets
library(jomo);library(mitools);library(mice)

set.seed(1234)
imp10<-jomo1(df.mi,nimp=100) #produces multiple imputed data sets, saved in
one big df
outjomo<-subset(imp10,Imputation>0)
mi_list <- imputationList(split(outjomo, outjomo$Imputation))

#run models again here
#distress
mi_results <- with(mi_list, lm(sr_t2_distress ~ sr_t1_distress +
groupMassed))
summary(pool(as.mira(mi_results))) #ns
round(summary(pool(as.mira(mi_results)))[,-1],3) #ns
#aaq
mi_results <- with(mi_list, lm(sr_t2_aaqII_avoid ~ sr_t1_aaqII_avoid +
groupMassed))
summary(pool(as.mira(mi_results))) #ns
round(summary(pool(as.mira(mi_results)))[,-1],3) #ns
#fomo
mi_results <- with(mi_list, lm(sr_t2_fomo_fomo ~ sr_t1_fomo_fomo +
groupMassed))
```

```
summary(pool(as.mira(mi_results))) #ns
round(summary(pool(as.mira(mi_results)))[,-1],3) #ns
#loneliness
mi_results <- with(mi_list, lm(sr_t2_nihToolbox_lonely ~
sr_t1_nihToolbox_lonely + groupMassed))
summary(pool(as.mira(mi_results))) #ns
round(summary(pool(as.mira(mi_results)))[,-1],3) #ns
#self-compassion
mi_results <- with(mi_list, lm(sr_t2_scsSf_selfComp ~ sr_t1_scsSf_selfComp +
groupMassed))
summary(pool(as.mira(mi_results))) #ns
round(summary(pool(as.mira(mi_results)))[,-1],3) #ns

#Between-Group Regression Models with Outliers Removed
df.outlier <- df

df.outlier[!is.na(df.outlier$sr_t1_distress) &
df.outlier$sr_t1_distress>mean(df$sr_t1_distress,na.rm=TRUE)+3*sd(df$sr_t1_di
stress,na.rm=TRUE),
           "sr_t1_distress"] <- NA
df.outlier[!is.na(df.outlier$sr_t1_aaqII_avoid) &
df.outlier$sr_t1_aaqII_avoid>mean(df$sr_t1_aaqII_avoid,na.rm=TRUE)+3*sd(df$sr
_t1_aaqII_avoid,na.rm=TRUE),
           "sr_t1_aaqII_avoid"] <- NA
df.outlier[!is.na(df.outlier$sr_t1_fomo_fomo) &
df.outlier$sr_t1_fomo_fomo>mean(df$sr_t1_fomo_fomo,na.rm=TRUE)+3*sd(df$sr_t1_
fomo_fomo,na.rm=TRUE),
           "sr_t1_fomo_fomo"] <- NA
df.outlier[!is.na(df.outlier$sr_t1_nihToolbox_lonely) &
df.outlier$sr_t1_nihToolbox_lonely>mean(df$sr_t1_nihToolbox_lonely,na.rm=TRUE
)+3*sd(df$sr_t1_nihToolbox_lonely,na.rm=TRUE),
           "sr_t1_nihToolbox_lonely"] <- NA
df.outlier[!is.na(df.outlier$sr_t1_scsSf_selfComp) &
df.outlier$sr_t1_scsSf_selfComp>mean(df$sr_t1_scsSf_selfComp,na.rm=TRUE)+3*sd
(df$sr_t1_scsSf_selfComp,na.rm=TRUE),
           "sr_t1_scsSf_selfComp"] <- NA

df.outlier[!is.na(df.outlier$sr_t2_distress) &
df.outlier$sr_t2_distress>mean(df$sr_t2_distress,na.rm=TRUE)+3*sd(df$sr_t2_di
stress,na.rm=TRUE),
           "sr_t2_distress"] <- NA
df.outlier[!is.na(df.outlier$sr_t2_aaqII_avoid) &
df.outlier$sr_t2_aaqII_avoid>mean(df$sr_t2_aaqII_avoid,na.rm=TRUE)+3*sd(df$sr
_t2_aaqII_avoid,na.rm=TRUE),
           "sr_t2_aaqII_avoid"] <- NA
df.outlier[!is.na(df.outlier$sr_t2_fomo_fomo) &
df.outlier$sr_t2_fomo_fomo>mean(df$sr_t2_fomo_fomo,na.rm=TRUE)+3*sd(df$sr_t2_
fomo_fomo,na.rm=TRUE),
           "sr_t2_fomo_fomo"] <- NA
df.outlier[!is.na(df.outlier$sr_t2_nihToolbox_lonely) &
df.outlier$sr_t2_nihToolbox_lonely>mean(df$sr_t2_nihToolbox_lonely,na.rm=TRUE
)+3*sd(df$sr_t2_nihToolbox_lonely,na.rm=TRUE),
           "sr_t2_nihToolbox_lonely"] <- NA
df.outlier[!is.na(df.outlier$sr_t2_scsSf_selfComp) &
df.outlier$sr_t2_scsSf_selfComp>mean(df$sr_t2_scsSf_selfComp,na.rm=TRUE)+3*sd
(df$sr_t2_scsSf_selfComp,na.rm=TRUE),
           "sr_t2_scsSf_selfComp"] <- NA
```

```
df.outlier[!is.na(df.outlier$sr_t1_distress) &
df.outlier$sr_t1_distress<mean(df$sr_t1_distress,na.rm=TRUE)-
3*sd(df$sr_t1_distress,na.rm=TRUE),
           "sr_t1_distress"] <- NA
df.outlier[!is.na(df.outlier$sr_t1_aaqII_avoid) &
df.outlier$sr_t1_aaqII_avoid<mean(df$sr_t1_aaqII_avoid,na.rm=TRUE)-
3*sd(df$sr_t1_aaqII_avoid,na.rm=TRUE),
           "sr_t1_aaqII_avoid"] <- NA
df.outlier[!is.na(df.outlier$sr_t1_fomo_fomo) &
df.outlier$sr_t1_fomo_fomo<mean(df$sr_t1_fomo_fomo,na.rm=TRUE)-
3*sd(df$sr_t1_fomo_fomo,na.rm=TRUE),
           "sr_t1_fomo_fomo"] <- NA
df.outlier[!is.na(df.outlier$sr_t1_nihToolbox_lonely) &
df.outlier$sr_t1_nihToolbox_lonely<mean(df$sr_t1_nihToolbox_lonely,na.rm=TRUE
)-3*sd(df$sr_t1_nihToolbox_lonely,na.rm=TRUE),
           "sr_t1_nihToolbox_lonely"] <- NA
df.outlier[!is.na(df.outlier$sr_t1_scsSf_selfComp) &
df.outlier$sr_t1_scsSf_selfComp<mean(df$sr_t1_scsSf_selfComp,na.rm=TRUE)-
3*sd(df$sr_t1_scsSf_selfComp,na.rm=TRUE),
           "sr_t1_scsSf_selfComp"] <- NA

df.outlier[!is.na(df.outlier$sr_t2_distress) &
df.outlier$sr_t2_distress<mean(df$sr_t2_distress,na.rm=TRUE)-
3*sd(df$sr_t2_distress,na.rm=TRUE),
           "sr_t2_distress"] <- NA
df.outlier[!is.na(df.outlier$sr_t2_aaqII_avoid) &
df.outlier$sr_t2_aaqII_avoid<mean(df$sr_t2_aaqII_avoid,na.rm=TRUE)-
3*sd(df$sr_t2_aaqII_avoid,na.rm=TRUE),
           "sr_t2_aaqII_avoid"] <- NA
df.outlier[!is.na(df.outlier$sr_t2_fomo_fomo) &
df.outlier$sr_t2_fomo_fomo<mean(df$sr_t2_fomo_fomo,na.rm=TRUE)-
3*sd(df$sr_t2_fomo_fomo,na.rm=TRUE),
           "sr_t2_fomo_fomo"] <- NA
df.outlier[!is.na(df.outlier$sr_t2_nihToolbox_lonely) &
df.outlier$sr_t2_nihToolbox_lonely<mean(df$sr_t2_nihToolbox_lonely,na.rm=TRUE
)-3*sd(df$sr_t2_nihToolbox_lonely,na.rm=TRUE),
           "sr_t2_nihToolbox_lonely"] <- NA
df.outlier[!is.na(df.outlier$sr_t2_scsSf_selfComp) &
df.outlier$sr_t2_scsSf_selfComp<mean(df$sr_t2_scsSf_selfComp,na.rm=TRUE)-
3*sd(df$sr_t2_scsSf_selfComp,na.rm=TRUE),
           "sr_t2_scsSf_selfComp"] <- NA

psych::describe(df[,c("sr_t2_distress","sr_t1_distress","sr_t2_aaqII_avoid",

"sr_t1_aaqII_avoid","sr_t2_nihToolbox_lonely","sr_t1_nihToolbox_lonely",

"sr_t2_scsSf_selfComp","sr_t1_scsSf_selfComp","sr_t2_fomo_fomo","sr_t1_fomo_f
omo")])
psych::describe(df.outlier[,c("sr_t2_distress","sr_t1_distress","sr_t2_aaqII_
avoid",

"sr_t1_aaqII_avoid","sr_t2_nihToolbox_lonely","sr_t1_nihToolbox_lonely",

"sr_t2_scsSf_selfComp","sr_t1_scsSf_selfComp","sr_t2_fomo_fomo","sr_t1_fomo_f
omo")])
#no outliers, but for good measure:
```

```
summary(lm(sr_t2_distress ~ sr_t1_distress + groupMassed, data = df.outlier))
#ns
summary(lm(sr_t2_aaqII_avoid ~ sr_t1_aaqII_avoid + groupMassed, data =
df.outlier)) #ns
summary(lm(sr_t2_fomo_fomo ~ sr_t1_fomo_fomo + groupMassed, data =
df.outlier)) #ns
summary(lm(sr_t2_nihToolbox_lonely ~ sr_t1_nihToolbox_lonely + groupMassed,
data = df.outlier)) #ns
summary(lm(sr_t2_scsSf_selfComp ~ sr_t1_scsSf_selfComp + groupMassed, data =
df.outlier)) #ns

#Between-Group Daily Diary Models with Outliers Removed
df.day.outlier <- df.day

df.day.outlier[!is.na(df.day.outlier$distress) &
df.day.outlier$distress>mean(df.day$distress,na.rm=TRUE)+3*sd(df.day$distress
,na.rm=TRUE),
               "distress"] <- NA
df.day.outlier[!is.na(df.day.outlier$lonely) &
df.day.outlier$lonely>mean(df.day$lonely,na.rm=TRUE)+3*sd(df.day$lonely,na.rm
=TRUE),
               "lonely"] <- NA

df.day.outlier[!is.na(df.day.outlier$distress) &
df.day.outlier$distress<mean(df.day$distress,na.rm=TRUE)-
3*sd(df.day$distress,na.rm=TRUE),
               "distress"] <- NA
df.day.outlier[!is.na(df.day.outlier$lonely) &
df.day.outlier$lonely<mean(df.day$lonely,na.rm=TRUE)-
3*sd(df.day$lonely,na.rm=TRUE),
               "lonely"] <- NA

psych::describe(df.day[,c("distress","lonely")])
psych::describe(df.day.outlier[,c("distress","lonely")])

summary(lmer(distress ~ day*groupMassed + (1|idR), data = df.day.outlier))
#ns
summary(lmer(lonely ~ day*groupMassed +(1|idR), data = df.day.outlier)) #ns
```

Supplemental Materials Table 4

Baseline Characteristics by Group

	Massed (n = 176)	Distributed (n = 175)	Total (N = 351)	Between-group p-value
Age (years)				.002
Mean	20.43 (1.82)	19.91 (1.24)	20.17 (1.58)	
Gender				.875
Female	138 (78.4%)	135 (77.1%)	273 (77.8%)	
Male	34 (19.3%)	38 (21.7%)	72 (20.5%)	
Non-binary	3 (1.7%)	2 (1.1%)	5 (1.4%)	
Race / Ethnicity				.981
White	147 (83.5%)	145 (82.9%)	292 (83.2%)	
Asian American	20 (11.4%)	21 (12.0%)	41 (11.7%)	
Non-U.S. Asian	9 (5.1%)	10 (5.7%)	19 (5.4%)	
Hispanic or Latino	9 (5.1%)	8 (4.6%)	17 (4.9%)	
African American	1 (0.6%)	6 (3.4%)	7 (2.0%)	
Native American, Pacific Islander, Alaskan Native, or First Nations	1 (0.6%)	2 (1.1%)	3 (0.9%)	
Non-U.S. African	0 (0.0%)	1 (0.6%)	1 (0.3%)	
Other	2 (1.1%)	2 (1.1%)	4 (1.1%)	
Prefer not to respond	2 (1.1%)	3 (1.7%)	5 (1.4%)	
Sexual Orientation				.277
Straight/Heterosexual	128 (72.7%)	137 (78.3)	265 (75.5%)	
Bisexual	32 (18.2%)	21 (12.0%)	53 (15.1%)	
Gay or Lesbian	7 (4.0%)	8 (4.6%)	15 (4.3%)	
Other	7 (4.0%)	6 (3.4%)	13 (3.7%)	
Prefer not to respond	4 (2.3%)	6 (3.4%)	10 (2.9%)	
Past-year Income				.481
< $25,000	145 (82.4%)	150 (85.7%)	295 (84.1%)	
$25,000 - $50,000	5 (2.8%)	2 (1.1%)	7 (2.0%)	
$50,000 - $75,000	2 (1.1%)	1 (0.6%)	3 (0.9%)	
$75,000 - $100,000	0 (0.0%	0 (0.0%)	0 (0.0%)	
$100,000 - $150,000	1 (0.6%)	0 (0.0%)	1 (0.3%)	
≥ $150,000	2 (1.1%)	1 (0.6%)	3 (0.9%)	
Don't Know/Not sure	15 (8.5%)	16 (9.1%)	31 (8.8%)	
Prefer not to respond	6 (3.4%)	5 (2.9%)	11 (3.1%)	
Outcome Measures				
Distress	61.58 (6.11)	60.59 (5.92)	61.08 (6.03)	.122
Exp Avoid	4.02 (1.34)	3.76 (1.34)	3.89 (1.34)	.075
FoMO	2.80 (0.78)	2.69 (0.81)	2.74 (0.79)	.183
Loneliness	2.94 (0.92)	2.82 (0.86)	2.88 (0.89)	.229
Self-Comp	2.56 (0.56)	2.74 (0.66)	2.65 (0.62)	.008

Note. Data are mean (SD) for continuous variables and n (%) for categorical variables; Massed = massed practice condition (one 20-minute session per day); Distributed = distributed practice condition (two 10-minute sessions per day); Between-group *p*-value = *p*-value from independent *t* test (for continuous variables) and χ^2 tests (for categorical variables); Distress = psychological distress (composite of the 4-item PROMIS Depression and PROMIS Anxiety scales); Exp Avoid = experiential avoidance (Acceptance and Avoidance Questionnaire – II); FoMO = fear of missing out (Fear of Missing Out scale); Loneliness = NIH Toolbox Loneliness scale; Self-Comp = self-compassion (Self-Compassion Scale – Short Form). Gender, Race / Ethnicity, and Sexual Orientation do not sum to 100% because participants were able to select multiple categories. Due to low frequency of some response options, categorical variables were dichotomized i.e., female vs. non-female; White vs. non-White, Straight/Heterosexual vs. non-Straight/Heterosexual, Past-year Income < $25,000 or Past-year Income ≥ $25,000) to compare groups at baseline.

Supplemental Materials Table 5

Intercorrelations of Outcome Variables at Pre- and Post-Intervention

	Pre					Post				
	Distress	Exp Avoid	FoMO	Loneliness	Self-Comp	Distress	Exp Avoid	FoMO	Loneliness	Self-Comp
Distress	-	-	-	-	-	-	-	-	-	-
Exp Avoid	.68	-	-	-	-	.75	-	-	-	-
FoMO	.24	.26	-	-	-	.25	.32	-	-	-
Loneliness	.62	.52	.33	-	-	.66	.57	.35	-	-
Self-Comp	-.51	-.54	-.27	-.45	-	-.54	-.57	-.35	-.46	-

Note. Distress = psychological distress (composite of the 4-item PROMIS Depression and PROMIS Anxiety scales); Exp Avoid = experiential avoidance (Acceptance and Avoidance Questionnaire – II); FoMO = fear of missing out (Fear of Missing Out scale); Loneliness = NIH Toolbox Loneliness scale; Self-Comp = self-compassion (Self-Compassion Scale – Short Form). $N = 351$ for pre-test; ns = 315 to 316 for post-test. All ps < .001.

Supplemental Materials Table 6

Daily Measures Separated by Group

Outcome	Day	Massed			Distributed		
		n	Mean	*SD*	*n*	Mean	*SD*
Distress	0	160	0.07	0.90	155	-0.07	0.87
Loneliness	0	160	3.06	1.62	155	3.23	1.59
Distress	1	165	-0.16	0.83	157	-0.13	0.90
Loneliness	1	165	2.67	1.47	157	3.08	1.64
Distress	2	156	-0.03	0.86	153	-0.13	0.86
Loneliness	2	156	2.99	1.68	153	2.88	1.55
Distress	3	154	-0.17	0.92	150	-0.19	0.93
Loneliness	3	154	2.81	1.53	150	2.83	1.74
Distress	4	154	-0.15	0.86	144	-0.24	0.92
Loneliness	4	154	2.66	1.50	144	2.53	1.57
Distress	5	151	-0.18	0.86	148	-0.35	0.88
Loneliness	5	151	2.58	1.50	148	2.47	1.63
Distress	6	145	-0.19	0.90	142	-0.31	0.93
Loneliness	6	145	2.68	1.65	142	2.65	1.56
Distress	7	152	-0.2	0.95	143	-0.25	0.95
Loneliness	7	152	2.62	1.56	143	2.86	1.69
Distress	8	153	-0.21	0.92	144	-0.23	0.92
Loneliness	8	153	2.71	1.60	144	2.68	1.58
Distress	9	151	-0.09	0.86	137	-0.11	0.93
Loneliness	9	151	2.78	1.56	137	2.88	1.69
Distress	10	138	-0.2	0.89	145	-0.25	0.86
Loneliness	10	138	2.67	1.65	145	2.7	1.53
Distress	11	148	-0.23	0.95	137	-0.29	0.81
Loneliness	11	148	2.47	1.50	137	2.52	1.55
Distress	12	143	-0.10	0.94	122	-0.25	0.92
Loneliness	12	143	2.73	1.54	122	2.7	1.63
Distress	13	141	-0.24	0.92	133	-0.39	0.81
Loneliness	13	141	2.62	1.56	133	2.56	1.46
Distress	14	139	-0.09	0.92	127	-0.26	0.93
Loneliness	14	139	2.72	1.65	127	2.52	1.63

Note. Participants were asked to complete daily diaries each evening; Massed = massed practice condition (one 20-minute session per day); Distributed = distributed practice condition (two 10-minute sessions per day); Distress = composite of the single highest loading items from the PROMIS Depression and PROMIS Anxiety scales; Loneliness = single highest loading item from NIH Toolbox Loneliness scale.

Supplemental Materials Table 7

CONSORT 2010 Checklist

CONSORT 2010 checklist of information to include when reporting a randomised trial*

Section/Topic	Item No	Checklist item	Reported on page No
Title and abstract			
	1a	Identification as a randomised trial in the title	1
	1b	Structured summary of trial design, methods, results, and conclusions (for specific guidance see CONSORT for abstracts)	2
Introduction			
Background and objectives	2a	Scientific background and explanation of rationale	4-8
	2b	Specific objectives or hypotheses	8-9
Methods			
Trial design	3a	Description of trial design (such as parallel, factorial) including allocation ratio	10-12
	3b	Important changes to methods after trial commencement (such as eligibility criteria), with reasons	10
Participants	4a	Eligibility criteria for participants	9-10
	4b	Settings and locations where the data were collected	9-12
Interventions	5	The interventions for each group with sufficient details to allow replication, including how and when they were actually administered	12-13, Sup Tab 1
Outcomes	6a	Completely defined pre-specified primary and secondary outcome measures, including how and when they were assessed	13-16
	6b	Any changes to trial outcomes after the trial commenced, with reasons	n/a
Sample size	7a	How sample size was determined	17
	7b	When applicable, explanation of any interim analyses and stopping guidelines	n/a
Randomisation:			
Sequence generation	8a	Method used to generate the random allocation sequence	11
	8b	Type of randomisation; details of any restriction (such as blocking and block size)	11
Allocation concealment mechanism	9	Mechanism used to implement the random allocation sequence (such as sequentially numbered containers), describing any steps taken to conceal the sequence until interventions were assigned	11
Implementation	10	Who generated the random allocation sequence, who enrolled participants, and who assigned participants to interventions	11
Blinding	11a	If done, who was blinded after assignment to interventions (for example, participants, care providers, those	n/a

		assessing outcomes) and how	
	11b	If relevant, description of the similarity of interventions	n/a
Statistical methods	12a	Statistical methods used to compare groups for primary and secondary outcomes	16-17
	12b	Methods for additional analyses, such as subgroup analyses and adjusted analyses	16-17
Results			
Participant flow (a diagram is strongly recommended)	13a	For each group, the numbers of participants who were randomly assigned, received intended treatment, and were analysed for the primary outcome	Sup Fig 1
	13b	For each group, losses and exclusions after randomisation, together with reasons	Sup Fig 1
Recruitment	14a	Dates defining the periods of recruitment and follow-up	11-12
	14b	Why the trial ended or was stopped	n/a
Baseline data	15	A table showing baseline demographic and clinical characteristics for each group	Sup Tab 1, Tab 1
Numbers analysed	16	For each group, number of participants (denominator) included in each analysis and whether the analysis was by original assigned groups	Sup Fig 1
Outcomes and estimation	17a	For each primary and secondary outcome, results for each group, and the estimated effect size and its precision (such as 95% confidence interval)	17-19, Tab 1
	17b	For binary outcomes, presentation of both absolute and relative effect sizes is recommended	17-19
Ancillary analyses	18	Results of any other analyses performed, including subgroup analyses and adjusted analyses, distinguishing pre-specified from exploratory	19
Harms	19	All important harms or unintended effects in each group (for specific guidance see CONSORT for harms)	n/a
Discussion			
Limitations	20	Trial limitations, addressing sources of potential bias, imprecision, and, if relevant, multiplicity of analyses	23
Generalisability	21	Generalisability (external validity, applicability) of the trial findings	21, 23-24
Interpretation	22	Interpretation consistent with results, balancing benefits and harms, and considering other relevant evidence	20-22
Other information			
Registration	23	Registration number and name of trial registry	masked
Protocol	24	Where the full trial protocol can be accessed, if available	10
Funding	25	Sources of funding and other support (such as supply of drugs), role of funders	title page

*We strongly recommend reading this statement in conjunction with the CONSORT 2010 Explanation and Elaboration for important clarifications on all the items. If relevant, we also recommend reading CONSORT extensions for cluster randomised trials, non-inferiority and equivalence trials, non-pharmacological treatments, herbal interventions, and pragmatic trials. Additional extensions are forthcoming: for those and for up to date references relevant to this checklist, see www.consort-statement.org.

Supplemental Materials Table 8

Between-group Effects on Pre-post Outcomes

Outcome	*d*	*p*
Distress	-0.01	0.553
Exp Avoid	-0.07	0.682
FoMO	-0.09	0.347
Loneliness	-0.16	0.158
Self-Comp	0.09	0.729

Note. Distress = psychological distress (composite of the 4-item PROMIS Depression and PROMIS Anxiety scales); Exp Avoid = experiential avoidance (Acceptance and Avoidance Questionnaire – II); FoMO = fear of missing out (Fear of Missing Out scale); Loneliness = NIH Toolbox Loneliness scale; Self-Comp = self-compassion (Self-Compassion Scale – Short Form); Between-group *d* calculated as Massed minus Distributed; Between-group *p*-value from linear regression models regressing post-test outcomes (e.g., psychological distress) onto group (i.e., massed vs. distributed) and pre-test outcomes (e.g., psychological distress).

Supplemental Materials Figure 1

CONSORT Diagram