

Is dosage of a meditation app associated with changes in psychological distress?

It depends on how you ask

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Abstract

Despite growing popularity, associations between dosage and outcomes in meditation app interventions have not been established. We examined this relationship using a range of operationalizations of dosage (e.g., minutes of use, days of use, number and type of activities completed) and strategies for modeling outcomes (e.g., ordinary least squares regression, multilevel modeling, latent class analysis). We used data from a recently completed randomized controlled trial testing a meditation app ($n=662$; 80.4% with elevated depression/anxiety) which included psychological distress as its preregistered primary outcome. Across 41 models, whether or not an association was detected as well as the shape and direction of this association varied. Although several models indicated that higher dosage was associated with larger decreases in psychological distress, many models failed to show this relationship and some even showed the opposite. These results may have implications for optimizing and studying dosage in meditation apps and for open science practices.

Keywords: mobile health; meditation; mindfulness; optimization; dosage

Mobile health (mHealth) technology has the potential to dramatically expand access to evidence-based psychological interventions for addressing a range of psychiatric symptoms (Torous et al., 2019). Mobile phone-based interventions (e.g., smartphone apps) are increasingly popular among consumers, with meditation apps emerging as far and away the most commonly used mental health apps (Wasil et al., 2020). Although the research base lags behind the widespread adoption of meditation apps, there is growing evidence that meditation apps can modestly reduce psychological distress (e.g., Gál et al., 2021). Indeed, in a recent meta-review of meta-analyses of randomized controlled trials (RCTs) testing the effects of mobile phone-based interventions (i.e., smartphone apps, text-message interventions) on mental health outcomes, meditation apps were the only intervention type showing superiority relative to control conditions that were intended to be therapeutic (i.e., specific active controls; Goldberg, Lam, et al., 2022). In a meta-analysis of 45 RCTs, meditation apps reduced symptoms of depression (Hedges' $g = 0.24$) and anxiety ($g = 0.28$) relative to control conditions (Linardon et al., 2023). Moreover, some early findings point toward dose-response associations, where greater use of meditation apps is associated with greater symptom improvements (Adams et al., 2018; Flett et al., 2020; Goldberg, Imhoff-Smith, et al., 2020). To date, however, a basic question central to interpreting these past findings and systematically advancing research on meditation apps remains unanswered: how should “dosage” be operationalized for meditation apps?

Dosage has been operationalized differently across studies investigating meditation apps. Using non-experimental designs, Flett et al. (2020) and Goldberg, Imhoff-Smith, et al. (2020) both found positive associations between higher app use and larger decreases in psychological distress among adults, although Flett et al. defined app use as total number of times the app was opened while Goldberg et al. defined app use as total number of days on which the app was

opened. The one study that to our knowledge has experimentally manipulated meditation app practice dosage found that among prehypertensive adults, assignment to a higher dose condition (15-minute meditation sessions, twice per day) was associated with larger reductions in systolic blood pressure than lower dose conditions (5- or 10-minute meditation sessions, twice per day; Adams et al., 2018). Not only did these three studies operationalize dosage in three different ways – times the app was opened, days the app was opened, and minutes of use – but also their approaches to modeling the associated outcomes varied. Flett et al. used multiple regression whereas Goldberg et al. and Adams et al. employed multilevel models. The extent to which these differing approaches to operationalizing dosage and modeling its effects drove the observed results is unclear.

Clarifying how different operationalizations of meditation app dosage relate to outcomes is necessary not only for interpreting past findings and designing future RCTs that manipulate dosage, but ultimately may be important for strengthening the effects of existing meditation apps – an essential step toward realizing their public health potential. Underscoring the need for work in this area, the effect sizes observed in meta-analyses of meditation app RCTs suggest effects are smaller than those observed from in-person delivery of meditation training. For example, the effect size from comparisons with no treatment controls on anxiety was Cohen's $d = 0.31$ for meditation apps (Gál et al., 2021) which is considerably smaller than the corresponding effect size for in-person meditation training ($d = 0.89$; Goldberg et al., 2018). Although various factors likely contribute to this discrepancy, it stands to reason that the smaller effect sizes for meditation apps may be driven in part by low rates of user adherence (i.e., low continued app use). Low adherence has been widely observed across app-based interventions, including meditation apps, both in naturalistic settings (Baumel et al., 2019) and RCTs (Linardon & Fuller-

Tyszkiewicz, 2020). Meditation apps may include potent evidence-based strategies, but users may not benefit without adequate adherence. Unfortunately, it is not currently possible to make recommendations regarding the amount or type of practice necessary for a given user to obtain benefits (i.e., the minimal clinically effective dose).

Another potential explanation for the relatively modest effects associated with app-based meditation is that some components of the apps may not be effective, thereby attenuating the intervention effects. The Multiphase Optimization Strategy (MOST) is an increasingly popular framework that proposes the development of maximally effective and efficient behavioral interventions can be achieved through a series of phases in which key treatment components are identified and their dosage optimized prior to testing in traditional RCTs (for details about MOST, see Collins, 2018). Although MOST might be productively applied in future work to strengthen the effects of meditation apps, a necessary precondition is first establishing a clear operationalization of dosage.

Some efforts have been made to operationalize dosage in the context of mHealth. For example, McVay and colleagues (2019) define various user actions that may be relevant to consider, including intervention actions (i.e., users receiving intervention content such as listening to audio recordings), participant actions (i.e., users producing content such as sending text messages with goal progress), and behavioral target actions (i.e., engaging in health behaviors external to the intervention such as eating fewer calorie dense foods). From this perspective, completing guided meditation practices would be an intervention action. To date, however, no study to our knowledge has evaluated dose-response associations across a range of operationalizations of meditation app dosage and across a range of statistical methods for evaluating this association. The present study addresses this gap.

Data for the present study came from a recently completed RCT testing a 4-week meditation app intervention – the Healthy Minds Program (HMP) – in a sample of 662 predominantly distressed (80.4% with elevated Patient-Reported Outcomes Measurement Information System [PROMIS] Depression and/or Anxiety symptoms) school employees gathered during the early months of the COVID-19 pandemic (Hirshberg et al., 2022). School employees during the COVID-19 pandemic provide a relevant context for understanding dosage effects within mHealth meditation training. This is a group that experienced high rates of psychological distress during COVID (Hirshberg et al., 2023) and the effects of meditation training have been theorized to be most pronounced in high-stress contexts (Creswell & Lindsay, 2014). Within the full intent-to-treat sample, HMP was associated with larger reductions on psychological distress relative to waitlist at both post-treatment ($d = 0.53$) and 3-month follow-up ($d = 0.33$; Hirshberg et al., 2022).

Here, in the spirit of the multiverse approach (Stegen et al., 2016), we examine the sensitivity of the relationship between app dosage and changes in psychological distress across 11 operationalizations of dosage and six commonly used analytic approaches. We examined associations when dose was operationalized as: 1) minutes of meditation practice, 2) days of app use, 3) number of activities completed, 4) minutes of practice squared, 5) days of use squared, 6) activities completed squared, 7) minutes of practice excluding those assigned to HMP who did not use the app (i.e., non-users), 8) days of use excluding non-users, 9) activities completed excluding non-users, 10) those assigned to HMP who used the app (i.e., users) versus non-users versus waitlist participants, and 11) patterns of app use based on latent class analysis (LCA; Collins & Lanza, 2009). In terms of analytic approaches, we examined: 1) effects at post-test using ordinary least squared regression (OLS), 2) effects 3-month follow-up using OLS

regression, 3) effects of dosage during a given week on outcomes at the end of that week using multilevel modeling (MLM), 4) dosage group as a moderator of longitudinal changes in psychological distress using MLM, 5) associations between dosage and longitudinal changes in psychological distress (i.e., psychological distress slope derived from MLMs), and 6) associations between week-level patterns of app use derived from the LCA with week-level distress scores. We examined associations with the parent trial's preregistered primary outcome (composite of PROMIS Depression, PROMIS Anxiety, and Perceived Stress Scale; Hirshberg et al., 2022).

Although all of these dosage operationalizations and analytic approaches address the dose-response question in theory, they provide unique perspectives. Regarding dosage, minutes of practice examines effects of meditation practice specifically, days of use examines persistence of engagement over time, and number of activities completed examines the amount of content engaged with within HMP. Quadratic versions of these dosage metrics assess the possibility that dose-response associations are non-linear. Analyses omitting those assigned to HMP who did not engage evaluate effects when only those who used the HMP app at least once are included, which may be important given the influence zero-inflation (i.e., many individuals with no HMP usage) can have on results. Dosage groupings compare those who used the HMP app at least once with the waitlist and those assigned to HMP who did not use the app. Lastly, LCA models examine whether specific patterns of usage (e.g., use of a particular component of HMP) relate to outcomes in unique ways. While all of these reflect dosage (i.e., use of HMP), they capture this construct in distinct ways.

Similarly, the analytic approaches all provide unique perspectives and address at least somewhat distinct scientific questions. OLS analyses at post-test and follow-up assess the degree

to which dosage metrics relate to pre-post and pre- to follow-up changes in psychological distress. MLMs and LCA models examining weekly outcomes assess whether dosage patterns in a given week are related to outcomes at the end of that week. LCA models look specifically at engagement with certain HMP content as it may relate to outcomes (i.e., dosage effects specific to HMP modules and activity types). MLMs using dosage groups (i.e., HMP Users, HMP Non-Users, Waitlist) evaluate whether trajectories of change in distress differed across these groups. Lastly, models using slopes derived from MLMs evaluate whether dosage is associated with different trajectories of change in distress when dosage is treated continuously.

The motivation for reporting results in the way that we have come out of exploratory data analysis. We could have crafted a version of this study that focused on only some of the models we report, which no doubt would have resulted in a study that was easier to interpret. However, from our perspective, each model we conducted in one way or another legitimately addresses the dose-response question. Selecting only a subset to report would have been vulnerable to our scientific biases and desire to obtain a given result. As such, again in the spirit of the multiverse approach (Steenen et al., 2016), we aim to transparently report the range of results derived from what we believe to be a defensible set of dosage operationalizations and analytic approaches. We hope that doing so will both help address the dose-response question as well as highlight potentially important methodological considerations for others examining dose-response associations in this context.

Transparency and Openness

Preregistration

The RCT from which these data were drawn was pre-registered (NCT04426318; <https://osf.io/eqgt7>). However, the analyses reported here were exploratory and not preregistered.

Data, Materials, Code, and Online Resources

Data and associated code are available online (<https://osf.io/dhw4y/>). The HMP app is also available (<https://hminnovations.org/meditation-app>).

Reporting

We report how we determined our sample size, all data exclusions, all manipulations, and all measures in the study.

Ethical Approval

Study procedures were approved by the University of Wisconsin-Madison Institutional Review Board (2020-0533).

Method

Participants and Procedure

Participants were drawn from an RCT initiated during the early months of the COVID-19 pandemic (enrollment occurred between June and August 2020). Public school employees ($n = 662$) were randomly assigned to use the HMP app ($n = 344$) or to a waitlist control condition ($n = 318$) who received the app after the 3-month follow-up assessment. Primary results from the RCT are reported elsewhere (Hirshberg et al., 2022). We preregistered a sample size of 400 which was estimated to provide 80% power to detect small-to-moderate between-group differences ($d \geq 0.38$, assuming 43.4% attrition; Linardon & Fuller-Tyszkiewicz, 2020) at $\alpha = .050$. The sample size was increased due to the availability of funding, as noted in the preregistration.

Eligibility criteria included no or minimal prior meditation experience and depression symptoms below the severe range on PROMS Depression (T-score ≤ 70 ; Pilkonis et al., 2011). Severe depression was an exclusion criterion to increase participant safety given the fully remote

trial design. The preregistered primary outcome was psychological distress, operationalized as the composite of depression, anxiety, and stress measures. These measures were gathered at baseline, weekly during the 4-week intervention period, and at a 3-month follow-up assessment. HMP app usage was gathered objectively through the app itself.

The current analyses examine associations between app usage and changes in psychological distress for participants assigned to the HMP arm ($n = 344$), as well as comparisons between app usage categories (i.e., those assigned to HMP who used the app, those assigned to HMP who did not use the app) and the waitlist control ($n = 318$). The full sample ($n = 662$) was on average 42.58 years old ($SD = 10.66$); 87.3% were female, 11.9% male, 0.2% non-binary, and 0.6% of unknown gender; 86.4% were non-Hispanic White, 2.4% Black, 0.6% Latinx, 1.4% Asian/Pacific Islander, 5.4% multiracial, 0.2% Native American, and 3.6% of unknown race/ethnicity; 89.1% had completed college; 16.8% had an annual income of \leq \$50,000. Most (80.4%) reported PROMIS Depression and/or PROMIS Anxiety scores in the clinically elevated range (T-score ≥ 55 ; Choi et al., 2014; Schalet et al., 2014). HMP and waitlist groups did not differ on any demographic or clinical variables at baseline (for further details, see Hirshberg et al., 2022).

Intervention

The HMP app is designed to train skills associated with four pillars of well-being: Awareness, Connection, Insight, and Purpose (ACIP; Dahl et al., 2020). The Awareness module of HMP includes training in mindfulness, attention regulation, and decentering (e.g., meta-awareness of thoughts, emotions, and sensations; focused attention). The Connection module includes training in capacities designed to support positive relationships with oneself and others (e.g., gratitude, compassion). The Insight module includes practices aimed at generating self-

knowledge about the nature of thoughts and emotions and how they influence one's identity and experience. The Purpose module includes practices aimed at clarifying purpose and values and exploring the expression of these in daily life activities. HMP involves a combination of didactic "podcast-style" lessons (i.e., "Learns") covering the science of well-being along with guided meditation practices focused on cultivating well-being skills. Prior to the four modules, there is introductory material that provides a basic orientation of the app and the kinds of practices included. Following the introductory material, participants are advised to move through the four modules in order, although they are able to access any content within the app at any time. Participants are able to choose among varying lengths of guided practices ranging from 5- to 30-minutes. They can also choose between sitting meditation and 'active' meditation which is designed to be done while engaging in simple daily tasks such as commuting or doing dishes. The available clinical trial evidence suggests that HMP reduces psychological distress (Goldberg, Imhoff-Smith, et al., 2020; Hirshberg et al., 2022).

Measures

Psychological Distress

Psychological distress was calculated by combining across the computer adaptive PROMIS Depression and PROMIS Anxiety (v1.0; Pilkonis et al., 2011) and the 10-item Perceived Stress Scale (PSS; Cohen & Williamson, 1988). The PROMIS Depression and PROMIS Anxiety scales show high convergent validity with legacy measures of depression and anxiety (Choi et al., 2014; Schalet et al., 2014). Participants report symptoms of depression (e.g., "I felt worthless") and anxiety (e.g., "I felt fearful") in the past 7 days on a 5-point Likert-type scale (1 = *never*, 5 = *always*). The computer adaptive versions produce T-scores with a mean of 50 and $SD = 10$, with $T \geq 55$ indicating clinical elevations. Internal consistency cannot be

computed from the computer adaptive version. However, fixed form versions of both measures show adequate internal consistency reliability ($\alpha \geq .90$; Pilkonis et al., 2011).

The PSS is a widely used measure of perceived stress. Participants indicate perceptions of stress (e.g., “How often have you felt that you were unable to control the important things in your life?”) on a 5-point Likert-type scale (1 = *never*, 5 = *very often*). The 10-item PSS has shown strong convergent and discriminant validity (Roberti et al., 2006). A total score was computed by taking the mean of all 10 items, with higher scores indicating higher levels of perceived stress. Internal consistency was acceptable ($\alpha = .85$).

Consistent with the primary RCT’s preregistration (<https://osf.io/eqgt7>), a psychological distress composite was computed by z-transforming PROMIS Depression, PROMIS Anxiety, and PSS scores using the mean and *SD* of the scores at baseline, and then taking their average. Internal consistency for this composite was acceptable ($\alpha = .87$). As noted, the three psychological distress measures were assessed at baseline, weekly during the 4-week intervention period, and at 3-month follow up.

App Usage

Usage of the HMP app was assessed objectively through the app itself. Activity logs documented participants’ interactions with the HMP app. From these logs, we derived counts of the number of minutes of meditation practices completed using the HMP app (Sum Mins), the number of days of HMP app use (Sum Days), and the number of activities completed (Sum Activities). ‘Activities’ in the HMP app included completing guided meditation practices and listening to didactic content.

Data Analysis

We aimed to thoroughly evaluate associations between dosage and outcomes across a range of operationalizations of dosage and methods for modeling outcomes. We first examined this association using data visualization, including visualizations that did not assume a linear relationship between dosage and outcomes (i.e., local regression [loess] curves; Jacoby, 2000). Next, we conducted a series of regression-based models (ordinary least squares [OLS] and multilevel models [MLMs]) and LCA with 11 operationalizations of dosage with six modeling strategies (Supplemental Materials Table 1). In keeping with Steegen et al. (2016), we did not apply a p -value correction in order to allow assessment of how uncorrected significance tests varied across models.

Dosage Operationalizations

We examined associations with minutes of meditation practice (Sum Mins), days of HMP app use (Sum Days), and number of activities completed (Sum Activities). To explore potential non-linear dose-response associations, we also examined quadratic versions of these three variables (Sum Mins², Sum Days², Sum Activities²). A proportion of participants assigned to HMP (21.2%) did not download and complete at least one activity in the app. To investigate the influence of this group on associations between dosage and outcomes, we ran models with this group excluded (i.e., restricted the sample to HMP participants with Sum Days > 0 [i.e., HMP Users]). We also categorized participants into those assigned to HMP who downloaded and completed at least one activity in the app (HMP Users), those assigned to HMP but who did not download and complete at least one activity in the app (HMP Non-Users), and those assigned to the waitlist control (Waitlist). Lastly, for the LCA, dosage was conceptualized as the pattern of app modules accessed (or not accessed).

Regression-Based Modeling Approaches

Dose-response associations were assessed using five regression-based modeling strategies. All models focused on predicting psychological distress. First, we used ordinary least squares (OLS) regression to examine the association between dosage and psychological distress at post-test, controlling for pre-test levels of psychological distress:

$$Y = \beta_0 + \beta_1(Dosage) + \beta_2(Pre - Test Distress) + e, \quad (\text{Equation 1})$$

where Y = post-test psychological distress predicted from the linear combination of the intercept (β_0), dosage (β_1), pre-test psychological distress (β_2), and residual error (e).

A second set of models used Equation 1, but examined the association between dosage and psychological distress at follow-up, controlling for pre-test levels. Thus, Y in Equation 1 represented follow-up rather than post-test psychological distress.

The third, fourth, and fifth set of models examined weekly changes in psychological distress during the 4-week intervention period using MLMs. The third set of models evaluated whether dosage on a given week was associated with psychological distress at the end of that week, controlling for baseline psychological distress. This was examined in a two-level MLM with observations (Level 1) nested within participants (Level 2) using the ‘lme4’ package in R (Bates et al., 2015):

$$Y_{ij} = \beta_{00} + \beta_{10}(Week) + \beta_{20}(Weekly Dosage) + \beta_{01}(Baseline Distress) + [U_{0j} + U_{1j}(Week) + e_{ij}], \quad (\text{Equation 2})$$

where Y_{ij} = psychological distress for participants i at week j predicted from the linear combination of the fixed intercept (β_{00} , overall mean across all participants and all weeks), fixed effect for time (β_{10} , in weeks), fixed effect for dosage (β_{20} , a Level 1 effect), fixed effect for the baseline psychological distress (β_{01}), as well as a participant-level random intercept (U_{0j}), a participant-level random slope for time (U_{1j}), and residual error (e_{ij}). As no HMP usage occurred

prior to baseline, models only included as the dependent variable psychological distress reported at Week 1 or later.

The fourth set of models examined associations between dosage and longitudinal changes in psychological distress (i.e., psychological distress slopes). In order to investigate associations between dosage groups (which was a participant-level [Level 2] variable, i.e., HMP Users, HMP Non-Users, Waitlist) and changes in psychological distress, we modified Equation 2 to include an interaction between dosage group and week and omitted baseline psychological distress (as this was now modeled longitudinally):

$$Y_{ij} = \beta_{00} + \beta_{10}(Week) + \beta_{01}(Dosage\ Group) + \beta_{20}(Week\ X\ Dosage\ Group) + [U_{0j} + U_{1j}(Week) + e_{ij}], \quad (\text{Equation 3})$$

Finally, to examine associations between the various remaining dosage operationalizations (i.e., Sum Mins, Sum Days, etc.) with longitudinal changes in psychological distress, we extracted random slope coefficients and assessed the association between dosage and outcomes within OLS. For this, an initial two-level MLM was fit without dosage included:

$$Y_{ij} = \beta_{00} + \beta_{10}(Week) + [U_{0j} + U_{1j}(Week) + e_{ij}], \quad (\text{Equation 4})$$

where Y_{ij} = psychological distress for participants i at week j predicted from the linear combination of the fixed intercept (β_{00} , overall mean across all participants and all weeks), fixed effect for time (β_{10} , in weeks), as well as a participant-level random intercept (U_{0j}), a participant-level random slope for time (U_{1j}), and a residual error term (e_{ij}). The random slope values were then entered as the outcome in an OLS regression:

$$Y = \beta_0 + \beta_1(Dosage) + e, \quad (\text{Equation 5})$$

where Y = random slope coefficients from Equation 4 reflecting pre-post changes in psychological distress during the 4-week intervention period predicted from the linear combination of the intercept (β_0), dosage (β_1), and residual error (e).

Latent Class Analysis

LCA (Collins & Lanza, 2009) can be used to identify unique patterns of co-occurring behaviors. An alternative operationalization of dosage is to consider the pattern of modules (Intro, Awareness, Connection, Insight, Purpose) and actions (learn, active, sitting) in which participants engaged each week. Because participants were able to have different patterns of app usage each week, week-level person-records were analyzed using variables that indicated whether or not participants engaged in each module and/or action during that week. Although a particular order of module engagement was recommended across weeks, participants were able to access any of the modules at any time. Analysis proceeded in two phases. The first phase identified and described the week-level latent classes of dosage patterns. The second phase examined whether week-level psychological distress was associated with week-level dosage pattern.

First, week-level person-records were analyzed using the marginal-means approach to longitudinal LCA (Diggle et al., 2002; Vermunt, 2010): all participant records across all weeks were used to identify dosage patterns. This approach is similar to a traditional LCA in that latent class membership probabilities and item-response probabilities are the two sets of parameters of interest, but it adjusts standard error estimates for repeated measures within persons. Latent class membership probabilities described the distribution of the classes in the sample. Item-response probabilities described the class-specific probabilities of using modules and actions in a given week; classes were named based on the pattern of item-response probabilities. Model selection

was based on the Akaike information criterion (AIC), Bayesian information criterion (BIC), sample size adjusted BIC (a-BIC), and entropy, as well as model stability and interpretability. Lower values for the AIC, BIC, and a-BIC indicated better model fit; higher values for entropy indicated higher classification utility. Model identification for all models was checked with multiple sets of random starting values; all models were estimated using Mplus version 8.4 (Muthén & Muthén, 1998-2017).

Second, after the optimal latent class model was selected and interpreted, associations between week-level latent class membership and week-level psychological distress were examined. Mean psychological distress was estimated for each latent class and pairwise comparisons were made. The currently recommended approach based on Bolck, Croon, & Hagennars (2004), termed the “BCH approach” (Bakk & Vermunt, 2016; Vermunt, 2010), was used for the outcome analysis. This approach assigned participants to classes each week based on modal posterior probabilities and then regressed psychological distress on assigned class membership, adjusting for classification error via BCH weights. This approach has been shown to be fairly robust to departures from normality on the outcome (Bakk & Vermunt, 2016).

Handling Missing Data

As app usage data were assessed objectively through the HMP app, they were never missing. However, ratings of psychological distress were missing for a minority of participants at each time point (see Supplemental Materials Table 2). Consistent with intention-to-treat (ITT) principles (Montori & Guyatt, 2001), models were run with all available data (i.e., participants were not excluded for not using the HMP app, with the exception of the dosage operationalizations that specifically omitted HMP Non-Users). MLMs and LCAs used maximum likelihood estimation, which is robust to data that are missing at random (MAR; Graham, 2009).

Initial OLS regression models predicting psychological distress at post-test and follow-up used complete case analyses which, although unbiased under MAR data, have reduced statistical power relative to modern missing data handling methods (i.e., maximum likelihood, multiple imputation; Graham, 2009). Thus, as a sensitivity analysis, the OLS regression models predicting psychological distress at post-test and follow-up were rerun with missing outcome values imputed using multiple imputation. For this, we created 100 imputed data sets using multivariate imputation with chained equations in the ‘mice’ package (van Buuren & Groothuis-Oudshoorn, 2011). Results were pooled across imputations using the ‘pool’ function in ‘mice.’ The OLS regression model predicting psychological distress slope had no missing data as slope values were derived from MLMs that employed maximum likelihood estimation.

Results

Descriptive Statistics

HMP usage descriptive statistics are reported in Supplemental Materials Table 3 and displayed in Figure 1 and 2. The average participant used HMP for 10.87 days, completed 127.83 minutes of meditation practice, and completed 20.05 activities within HMP. As can be seen in Figure 1, usage was not normally distributed when HMP Non-Users were included. Removal of these participants increased the normality of the data (Figure 2), although a normal distribution was still not achieved. HMP usage metrics were highly correlated ($r_s = .82$ to $.93$ for all HMP participants, $r_s = .75$ to $.88$ for HMP Users; Supplemental Materials Table 4).

Figures 3 and Supplemental Materials Figure 1 display scatterplots of the association between HMP usage and residualized (i.e., residuals from linear models predicting post-test or follow-up controlling for pre-test) change in psychological distress from pre- to post-treatment (Figure 3) and pre- to 3-month follow-up (Supplemental Materials Figure 1). Equivalent figures

with HMP Non-Users removed are displayed in Figure 4 and Supplemental Materials Figure 2. Loess curves suggest potential non-linear associations between HMP usage and outcomes, particularly for post-test when using all participants assigned to HMP (Figure 3).

Figure 5 displays violin plots showing the distribution of residualized change scores separated by dosage group (i.e., HMP Users, HMP Non-Users, Waitlist). Outcomes appear similar or potentially superior for HMP Non-Users relative to HMP Users, although the HMP Non-User group includes only a small number of data points.

Regression-Based Models

Output from the regression-based models (OLS and MLM) is reported in Table 1. As can be seen, the magnitude and significance of the association between dosage and outcomes was sensitive to both dosage operationalization and modeling approach.

When predicting post-test psychological distress using OLS, no linear association was detected between minutes, days, or number of activities when using the full sample although a small negative association ($\beta = -.11$; i.e., more usage, lower post-test psychological distress) was detected between days of HMP use and outcome when restricted to those assigned to HMP who used the app at least once (i.e., HMP Users). A significant negative quadratic association was detected between days of use and number of activities, indicating that post-test psychological distress was lower at low and high levels of usage and higher at moderate levels of usage. Waitlist participants also showed higher psychological distress at post-test relative to HMP Users ($\beta = .41$) and an equivalent effect was observed across all regression-based modeling approaches. Of note, HMP Users and HMP Non-Users did not differ.

Results were somewhat similar when examining follow-up psychological distress using OLS. Again, no linear association was detected between usage and outcomes when using the full

sample although a small negative association ($\beta = -.11$) was detected between number of activities and outcome when restricted to HMP Users. The significant negative quadratic association between number of activities and outcome remained for follow-up, although the quadratic association for days of use was no longer significant. As for post-test, HMP Users showed superior outcomes relative to Waitlist but unlike post-test, HMP Non-Users showed lower psychological distress relative to HMP Users ($\beta = -.24$).

Dosage operationalizations were not associated with psychological distress in MLMs modeling weekly psychological distress and using weekly dosage as the predictor variable while covarying baseline psychological distress. However, a significant time X dosage group interaction was detected, indicating that Waitlist participants showed a flatter slope for change in psychological distress over time relative to HMP Users. There was no difference between HMP Users and HMP Non-Users in this model.

Interestingly, models that examined associations with MLM random slopes using OLS differed in several instances from previous models. In fact, these were the only models that showed a linear association between usage (minutes, days) and reductions in psychological distress (β s = $-.12$ and $-.11$, for minutes and days of use, respectively). Results were similar when restricted to HMP Users (β s = $-.13$, for both minutes and days of use). HMP Users continued to show superior outcomes relative to waitlist when examining dosage groups ($\beta = .57$).

Sensitivity Analyses

Significance tests from OLS models conducted using imputed data were equivalent to the results reported in Table 1, with two exceptions (Supplemental Materials Table 5). The two previously significant associations between dosage and outcomes observed when restricted to

HMP Users (i.e., sum days for post-test, sum activities for follow-up) were no longer significant. All other significance tests remained unchanged.

Latent Class Analysis

Model fit information and model selection criteria for competing latent class models are shown in Supplemental Materials Table 6. Models with one to six classes were considered; larger models with additional classes were under-identified during estimation. The 6-class model was selected as optimal for interpretation and additional analysis: AIC, BIC, and aBIC were minimized, entropy was acceptable, and all classes were theoretically interpretable and practically useful. Mplus code and output is available at <https://osf.io/dhw4y/>

Parameter estimates for the 6-class model are shown in Table 2. Class 1 (33% of person-weeks) included weeks when those assigned to be HMP Users did not use the app at all; we labeled this class No App Usage. Class 2 (3% of person-weeks) included weeks characterized by use of the Intro module; we labeled this class Intro Only. Class 3 (19% of person-weeks) included weeks characterized by use of the Intro and Awareness modules; we labeled this class Intro and Awareness. Note that all classes including weeks when a content module was used had similar probabilities of ‘learn,’ ‘active,’ and ‘sitting,’ actions, so we did not include these when interpreting and naming the classes. Class 4 (15% of person-weeks) included weeks characterized by use of the Awareness and Connection modules; we labeled this class accordingly. Class 5 (16% of person-weeks) and Class 6 (13% of person-weeks) included weeks characterized by use of the Connection and Insight and the Insight and Purpose modules, respectively; we labeled these classes accordingly. Across classes, it is notable that weeks when HMP Users accessed the app, they tended to use more than one content module per week.

Weekly latent class membership was then associated with weekly psychological distress. Overall, weekly patterns of module engagement was significantly associated with weekly psychological distress ($\chi^2 = 71.17, p < .001$). Mean weekly psychological distress for each latent class and pairwise significance tests are shown in the last row of Table 2. Across classes that include weeks when the app was accessed, Insight and Purpose weeks had the lowest psychological distress (-.72; significantly lower than all other classes except No App Usage). In addition, No App Usage weeks had noticeably low psychological distress (-.59). Intro Only and Intro and Awareness weeks had the highest psychological distress (-.25 and -.27, respectively; significantly higher than all other classes when tests were adequately powered). Finally, weeks when the Awareness, Connection, and Insight modules were used in combination (Awareness and Connection, Connection and Insight), HMP app users had lower psychological distress than weeks when the Intro and Awareness modules were used, but not as low as weeks when the Insight and Purpose modules were used.

Discussion

In the context of increasing popular and scientific interest in meditation apps, it would be valuable to clarify the association between dosage and outcomes for the purpose of optimizing this approach and defining dosage for use in future trials (Collins, 2018). In pursuit of this, we examined the association between several operationalizations of dosage and methods for modeling outcomes within the context of a recently completed RCT ($n = 662$) testing a meditation-based smartphone app (HMP). Across a series of 41 models, to some degree whether or not an association was detected as well as the shape and direction of the association varied depending on how dosage was operationalized and how outcomes were modeled. The most parsimonious linear models (i.e., predicting post-test or follow-up psychological distress

controlling for baseline psychological distress) did not show associations between dosage and outcomes, regardless of whether dosage was defined as minutes, days, or number of activities completed. Moreover, effect sizes reflecting these associations were very small (β s = -.05 to .001), suggesting that the lack of statistical significance was not due to low power. However, when outcomes were modeled using random slopes extracted from MLMs that used weekly reports of psychological distress, a small but statistically significant association was detected between minutes and/or days of use and improved outcomes (β s = -.12 and -.11, for minutes and days of use, respectively). There was also some evidence for a linear association between dosage and outcomes when restricting the sample to participants assigned to HMP who used the app at least once (i.e., HMP Users), although these effects did not persist when using multiple imputation to handle missing data.

Subsequent models as well as data visualizations provided evidence supporting a non-linear association between dosage and outcomes. Models using a quadratic version of dosage showed negative quadratic associations (i.e., inverted U) with outcomes at post-test (for days and number of activities) and follow-up (for number of activities). The direction of these effects indicated that outcomes were best for those with low or high doses of meditation app use. Partially consistent with this possibility, models comparing dosage groups (i.e., participants assigned to HMP who used the app, those assigned to HMP who did not use the app, and waitlist) indicated that, contrary to what one might expect, participants assigned to HMP who did not use the app (i.e., HMP Non-Users) had better outcomes than the average participant assigned to HMP who did use the app (i.e., HMP Users) at follow-up. This unexpected finding was mirrored in the LCA results where weeks with No App Usage had lower psychological distress than weeks with app usage characterized by the Intro and Awareness modules.

Regarding the LCA findings more broadly, the LCA-derived patterns of app usage that emerged were fairly simple, due in part to the fact that participants largely followed the recommended sequence of modules. For apps that invite more variability in usage of features over time, however, LCA may be a particularly promising method for understanding how complex patterns of usage differentially relate to health outcomes. In that sense, the main contribution of the present LCA may be that it serves as a proof-of-concept for an alternative approach to operationalizing dosage.

Taken together, it may be the uncomfortable reality that at this juncture, it is unclear what, if any, types of dosage relate to changes in psychological distress. Even within a single trial of a single meditation app, effects were sensitive to how dosage was defined (i.e., minutes, days, activities). Similar variation of results across definitions of dosage have been reported for other meditation apps, even in much larger samples (e.g., associations between changes in perceived stress with days vs. minutes of use; Callahan et al., 2024). Notably, meditation apps are a relatively simple mHealth use case where users are primarily being asked to listen to instructional content and complete guided meditation practices. Dosage for other mHealth interventions may prove even more complex to operationalize. Many mHealth tools include a much wider variety of features including actions taken outside of the app context or otherwise not easily logged (e.g., journaling, peer support, mood tracking, physical exercise, etc.; Camacho et al., 2022). It is currently unclear which features may be most important to examine as indicators of dosage that predict outcomes.

Results also highlight sensitivity to modeling approaches. Importantly, the models examined assessed theoretically distinct associations. For example, OLS models predicted outcomes only at post-test or follow-up, while the MLMs evaluated trajectories of change on

weekly measures. Nonetheless, the generally modest effect sizes linking dosage and outcomes highlight the need for large sample sizes that are capable of detecting small effects. In addition, increasingly optimized interventions may be more potent and therefore produce stronger dosage effects that can be more easily detected.

There are several theoretical and methodological reasons that the association between dosage and outcomes may be complex within meditation apps. For one, dosage (or adherence generally) is only one part of the broader construct of engagement which may be the construct more closely tied to outcomes than usage alone (Nahum-Shani et al., 2022). As defined by Nahum-Shani and colleagues, engagement involves the investment of not just physical energy (i.e., completing a particular physical task such as playing a meditation recording) but also the investment of affective and cognitive energies. From this standpoint, focusing solely on one metric of physical energy may miss crucial aspects of engagement that are as or potentially more closely coupled with outcomes. This certainly complicates efforts to determine an optimal dose (e.g., for the purposes of conducting optimization trials; Collins, 2018), given that participants cannot be easily assigned to invest a given amount of affective or cognitive energy. At this stage, an appropriate next step may be simply including measures of affective and cognitive energy investment that can be examined in combination with measures of physical energy investment. Within the context of meditation practice, ratings of the subjective experience of meditation (e.g., state mindfulness, affect experienced during meditation practice, adherence to the meditation practice instructions themselves; Goldberg, Knoopel, et al., 2020; Goldberg et al., 2023; Kiken et al., 2015) may be promising candidate characteristics to consider. More generally, transtheoretical constructs like the therapeutic alliance may similarly index important

aspects of engagement that are associated with outcomes mHealth interventions (Berry et al., 2018; Goldberg, Baldwin, et al., 2022).

A second complicating factor is the possibility that reasons for using or not using a meditation app may be confounded with outcomes in complex and even conflicting ways. It may be that some participants use a meditation app more because they are having difficulties and use a meditation app less when their difficulties cease (Goldberg, 2022). Such a possibility would be consistent with the good-enough level model in psychotherapy where participants discontinue treatment as their symptoms abate (Barkham et al., 2006). Alternatively, participants who are not finding the app helpful (i.e., their symptoms are worsening) may discontinue their use. Across these two scenarios, we might predict that low levels of adherence would be associated with contrasting outcomes.

A third complicating factor is the issue of missing data and the possibility of data missing not at random (MNAR; Graham, 2009). In addition to usage being impacted by outcome trajectories, retention in the study may also be impacted by outcome trajectories. Participants assigned to the intervention arm in particular may choose to drop out after not finding the meditation app helpful. Thus, their outcomes, had they been observed, may be lower than the observed outcomes. There is evidence from the mHealth literature (Goldberg et al., 2021; Linardon & Fuller-Tyszkiewicz, 2020) and a prior HMP trial (Goldberg, Imhoff-Smith, et al., 2020) that imply the presence of data being MNAR. Namely, participants assigned to the intervention arm in these trials are more likely to dropout than those assigned to waitlist. In the context of an RCT, this indicates that attrition is at least partially caused by group assignment. It may therefore be plausible that outcomes for those who remain in the study are different than those who dropped out. This may have occurred for the HMP Non-Users who completed the 3-

month follow-up in the current study, for example. Among the three dosage groups, HMP Non-Users showed lower rates of follow-up assessment completion (61.6%) than HMP Users (87.4%, $p < .001$) and Waitlist (90.9%, $p < .001$). It is possible that the unexpectedly higher outcomes for HMP Non-Users relative to HMP Users at follow-up is an artifact of data being MNAR. Indeed, this same issue could also have produced the quadratic shape observed at post-test as well where HMP Non-Users were again less likely to complete assessments.

Unfortunately, there are no simple solutions for eliminating the impact of missing data. While the current study had relatively high retention (81.4% and 90.9% at post-test for HMP and Waitlist arms, respectively, see Supplemental Materials Table 2) relative to the mHealth literature generally (Linardon & Fuller-Tyszkiewicz, 2020), there still may be influential subgroups (e.g., HMP Non-Users who do not complete assessments). Sensitivity analyses can be helpful for evaluating the impact of varying assumptions about the meaning of missingness (Goldberg et al., 2021; Leurent et al., 2018). More intensive and passive data collection procedures may increase the chances that at least some data are available for more participants which can reduce the impact of MNAR data.

The current study may have implications for open science practices. Given the wide range of conclusions drawn from varying operationalizations of dosage and modeling approaches, it may be prudent for researchers to preregister a set of adherence metrics and modeling approaches and to report all of them. Another possibility could be preregistering how decisions will be made, for example choosing adherence metrics that are most normally distributed and/or visualizing data to guide model selection.

Future studies should also investigate the possibility that the impact of app-delivered meditation practice dosage varies between individuals as well as within individuals across time

and even across types of meditation practice (e.g., mindfulness versus compassion practice; Dahl et al., 2015). We used models that generally focused on associations between dosage and outcomes for all participants (with the exception of the LCA which allowed the formation of latent groups and the dosage group analysis which also separated the sample into groups) and across all meditation practice types. We also focused exclusively on our primary outcome (psychological distress), although dose-response patterns may vary across outcome measure types (e.g., stronger associations on measures directly tied to the skills being trained such as measures of mindfulness or compassion). Studies that include more intensive and momentary data collection (e.g., ecological momentary assessment) and passive data collection may be ideally suited for investigating these more nuanced associations. Ultimately, randomized trials manipulating dosage will be required to create optimized meditation app interventions. Micro-randomized trials (MRTs; Qian et al., 2022) may be another highly relevant method for studying dosage in this context. MRTs allow rapid randomizations of participants to momentary interventions alongside assessment of momentary (proximal) outcomes. Such trials can be useful in identifying participant (i.e., between person), momentary (i.e., within person), and practice type factors (along with their interactions) that impact the association between dosage and outcomes. There is also a need for theoretical work clarifying how best to define and measure dosage and related constructs (e.g., engagement) within the mHealth context. This may require moving beyond simplistic conceptualizations of dosage based on the pharmacology (i.e., milligrams) and psychotherapy (i.e., number of sessions) to understand more deeply (e.g., through qualitative research) how individuals actually engage with mHealth interventions.

Limitations

This study has several important limitations. First, although the sample size was reasonably large, it may have been insufficient for detecting small effects, particularly within the presence of deviations from normality. Second, as noted, we focused solely on practice dosage and did not include assessment of other aspects of engagement (i.e., investment of affective and cognitive energies) which may be important contributors to outcomes (Nahum-Shani et al., 2022). Relatedly, we did not assess the degree to which participants applied what they were learning in the app informally in their daily activities (i.e., informal meditation practice; Fredrickson et al., 2019). Moreover, we examined only a subset of potential dosage operationalizations (i.e., minutes, days, number and types of activities completed). Other dosage operationalizations (e.g., composite variables created by combining across correlated dosage metrics, day of last app use, usage from post-test to follow-up) may have yielded a different and perhaps more consistent pattern of findings. Third, also as noted, it may be possible that data in the current trial were MNAR. While we could have conducted pattern mixture models that made increasingly pessimistic assumptions about the meaning of missing data (Goldberg et al., 2021; Leurent et al., 2018), we knew that participants who used HMP were more likely to complete follow-up assessments. Thus, such models would involve assuming HMP Non-Users who did not complete assessments had poorer outcomes and may have artifactually increased the likelihood of observing associations between dosage and outcomes. Fourth, we relied entirely on self-reported outcomes that are vulnerable to a host of biases (e.g., social desirability). Fifth, our sample was predominantly non-Hispanic White and female. It will be crucial to investigate associations between dosage and outcomes in more racially and gender diverse samples. Sixth, we did not apply a *p*-value correction. Although this practice is in keeping with multiverse approaches (e.g., Steegen et al., 2016), the large number of tests conducted without correction

greatly increases the likelihood for Type I error. This should be considered when interpreting any individual test we report and may be particularly relevant for interpreting unexpected results with relatively large but still statistically significant p -values (e.g., lower psychological distress for HMP Non-Users vs. HMP Users at follow-up, $p = .034$).

Conclusions

These limitations notwithstanding, the current study adds to the small existing literature empirically investigating the association between dosage and outcomes in mHealth interventions generally and meditation apps specifically. Clarifying the optimal dosage of app-based meditation training may be a key element for the development and ultimate dissemination of effective and efficient mHealth interventions. Our results raise more questions than they answer; they imply that the association between dosage and outcomes in this context is not straightforward and rather is sensitive to decisions regarding the operationalization of dosage as well as strategies for modeling outcomes. Given the early stage of research in this area, investigators may be encouraged to explore a variety of operationalizations of dosage and modeling strategies, while maintaining transparency in reporting. Ideally, the use of similar dosage operationalizations and modeling strategies across studies will ultimately allow aggregation of results (e.g., via meta-analysis). Future studies randomly assigning participants to dosages through traditional RCT as well as MRTs may be essential for optimizing mHealth meditation training and maximizing the public health potential of this intervention approach.

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Table 1

Associations between Dosage and Outcomes Across Dosage Operationalizations and Modeling Approaches

Modeling Approach	Dosage Operationalization									Dosage Groups
	Sum Mins	Sum Days	Sum Activities	Sum Mins ²	Sum Days ²	Sum Activities ²	Sum Mins HMP Users	Sum Days HMP Users	Sum Activities HMP Users	
Post-test OLS	$\beta = -.05,$ $p = .292$	$\beta = -.04,$ $p = .357$	$\beta = -.02,$ $p = .739$	$\beta^2 = -.15,$ $p = .166$	$\beta^2 = -.40,$ $p = .014$	$\beta^2 = -.43,$ $p = .016$	$\beta = -.10,$ $p = .056$	$\beta = -.11,$ $p = .032$	$\beta = -.08,$ $p = .108$	HMP Non-Users $\beta = -.12,$ $p = .277$ Waitlist $\beta = .41,$ $p < .001$
Follow-up OLS	$\beta = -.01,$ $p = .752$	$\beta = .002,$ $p = .956$	$\beta = .001,$ $p = .983$	$\beta^2 = -.05,$ $p = .617$	$\beta^2 = -.24,$ $p = .122$	$\beta^2 = -.45,$ $p = .008$	$\beta = -.08,$ $p = .089$	$\beta = -.09,$ $p = .065$	$\beta = -.11,$ $p = .026$	HMP Non-Users $\beta = -.24,$ $p = .034$ Waitlist $\beta = .25,$ $p < .001$
Weekly psychological distress MLM	$b = -4.66,$ $p = .330$	$b = -107.86,$ $p = .163$	$b = 27.33,$ $p = .442$	$b^2 = -0.096,$ $p = .164$	$b^2 = -23.82,$ $p = .428$	$b^2 = 4.28,$ $p = .477$	$b = -6.77,$ $p = .195$	$b = 173.43,$ $p = .054$	$b = 17.90,$ $p = .663$	HMP Non-Users $b = -66.34,$ $p = .815$ Waitlist $b = 1130.84,$ $p < .001$
Psychological distress slope OLS	$\beta = -.12, p = .028$	$\beta = -.11, p = .035$	$\beta = -.09,$ $p = .085$	$\beta^2 = -.09,$ $p = .465$	$\beta^2 = -.24,$ $p = .197$	$\beta^2 = -.34,$ $p = .108$	$\beta = -.13,$ $p = .031$	$\beta = -.13,$ $p = .030$	$\beta = -.11,$ $p = .079$	HMP Non-Users $\beta = .07,$ $p = .568$ Waitlist $\beta = .57,$ $p < .001$

Note. Sum Mins = minutes of meditation practice using the Healthy Minds Program (HMP) app; Sum Days = days of HMP use; Sum Activities = number of meditation practices and/or recordings of didactic content completed; HMP Users = participants assigned to HMP who used the app at least once; Dosage Groups = comparisons between HMP Users with HMP Non-Users (i.e., assigned to HMP but did not use the app at least once) and Waitlist, with HMP Users as the reference group; β = standardized regression coefficient; b = unstandardized regression coefficient; ² = coefficient for quadratic dosage term; bold values indicate $p < .050$; OLS = ordinary least squares; MLM = multilevel model; Post-test = predicting post-test controlling for pre-test; Follow-up = predicting 3-month follow-up controlling for pre-test. Psychological distress values multiplied by 10,000 for MLM to simplify reporting of model coefficients. Final column displays coefficients for models testing dosage groups with HMP Users as the reference group.

Table 2

Parameter Estimates for the Six-Class Latent Class Analysis and Effects of Class Membership on Psychological Distress

		1	2	3	4	5	6
		No App Usage	Intro Only	Intro and Awareness	Awareness and Connection	Connection and Insight	Insight and Purpose
<i>Latent Class Prevalences</i>							
		0.33	0.03	0.19	0.15	0.16	0.13
	<i>Overall Proportion</i>	<i>Class-Specific Proportions for 'Yes' App Feature Used</i>					
Intro	0.24	0.00	1.00	1.00	0.00	0.07	0.04
Awareness	0.35	0.00	0.00	0.95	1.00	0.06	0.01
Connection	0.31	0.00	0.00	0.13	0.70	1.00	0.07
Insight	0.23	0.00	0.00	0.00	0.03	0.66	0.92
Purpose	0.10	0.00	0.01	0.00	0.01	0.00	0.77
Learn	0.62	0.00	1.00	1.00	0.86	0.89	0.96
Active	0.37	0.00	0.00	0.72	0.49	0.50	0.58
Sitting	0.61	0.00	0.36	0.98	0.93	0.90	0.93
		<i>Class-Specific Proportions for the Outcome (Standard Errors)</i>					
Psychological Distress		-0.59 ³ (0.09)	-0.25 ⁶ (0.15)	-0.27 ^{1,4,5,6} (0.060)	-0.41 ^{3,6} (0.07)	-0.46 ^{3,6} (0.06)	-0.72 ^{2,3,4,5} (0.07)

¹Significantly different from No App Usage.

²Significantly different from Intro Only.

³Significantly different from Intro and Awareness.

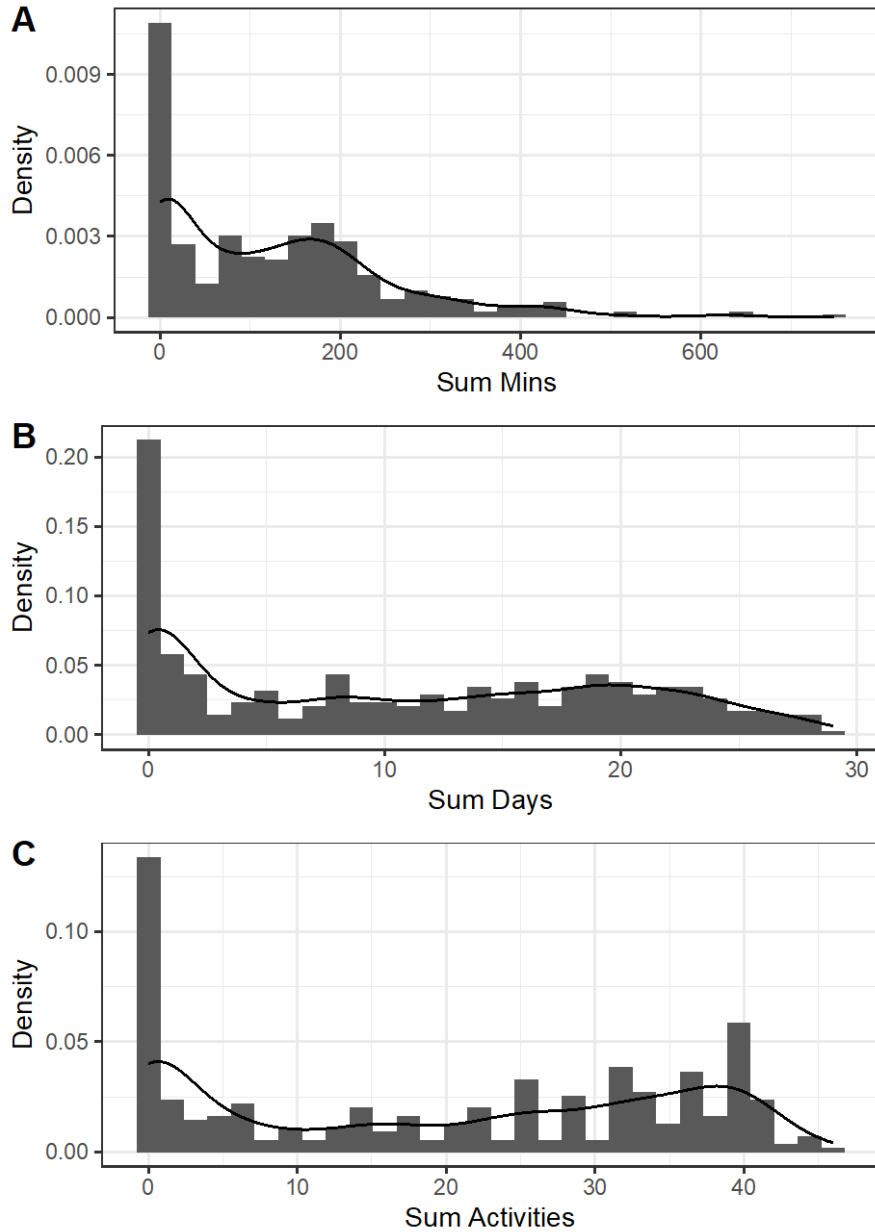
⁴Significantly different from Awareness and Connection.

⁵Significantly different from Connection and Insight.

⁶Significantly different from Insight and Purpose.

Figure 1

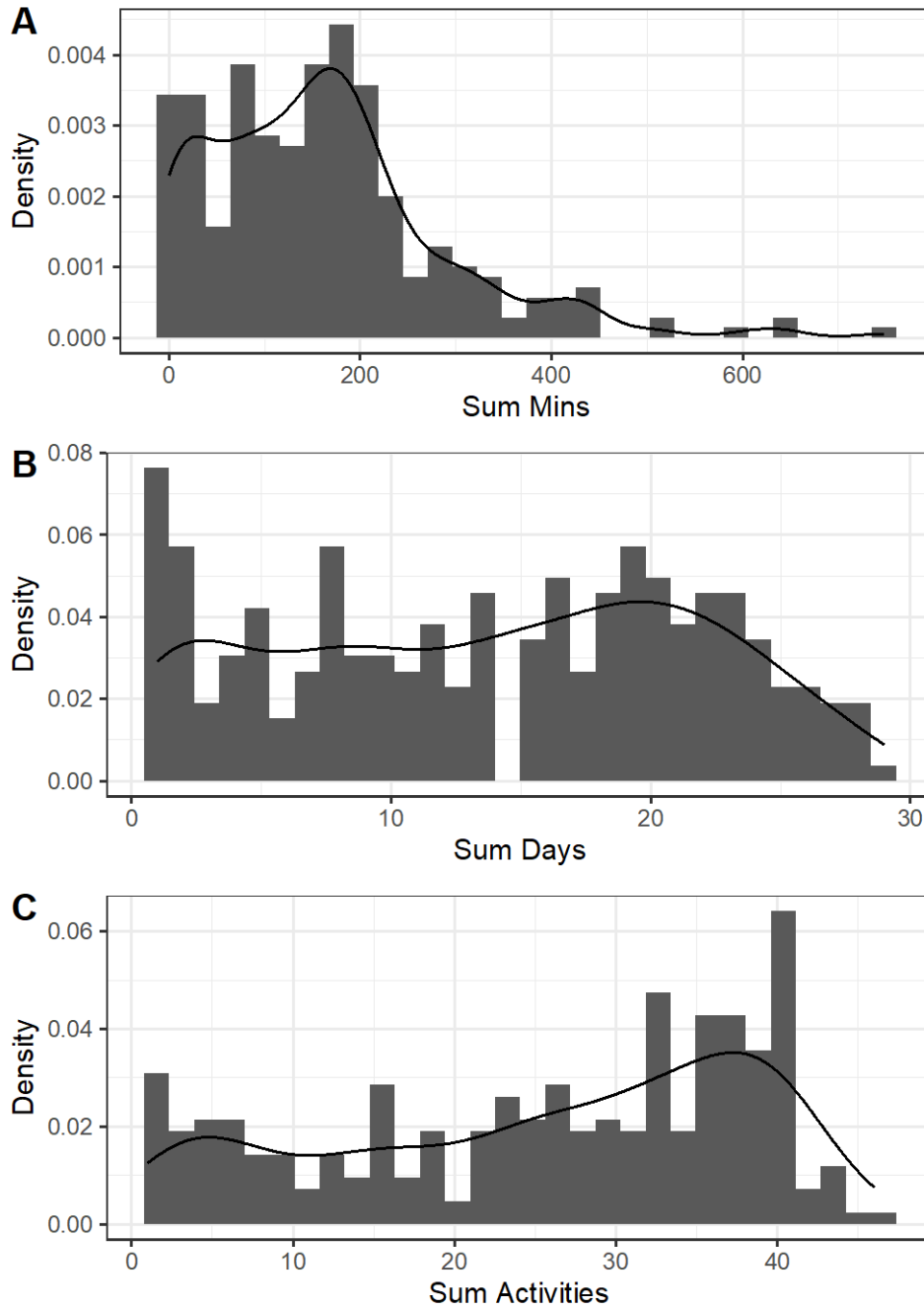
Histograms of HMP App Usage



Note. Sum Mins (Panel A) = minutes of meditation practice using the Healthy Minds Program (HMP) app; Sum Days (Panel B) = days of HMP use; Sum Activities (Panel C) = number of meditation practices and/or recordings of didactic content completed. Curved black lines show density distribution. $n = 344$.

Figure 2

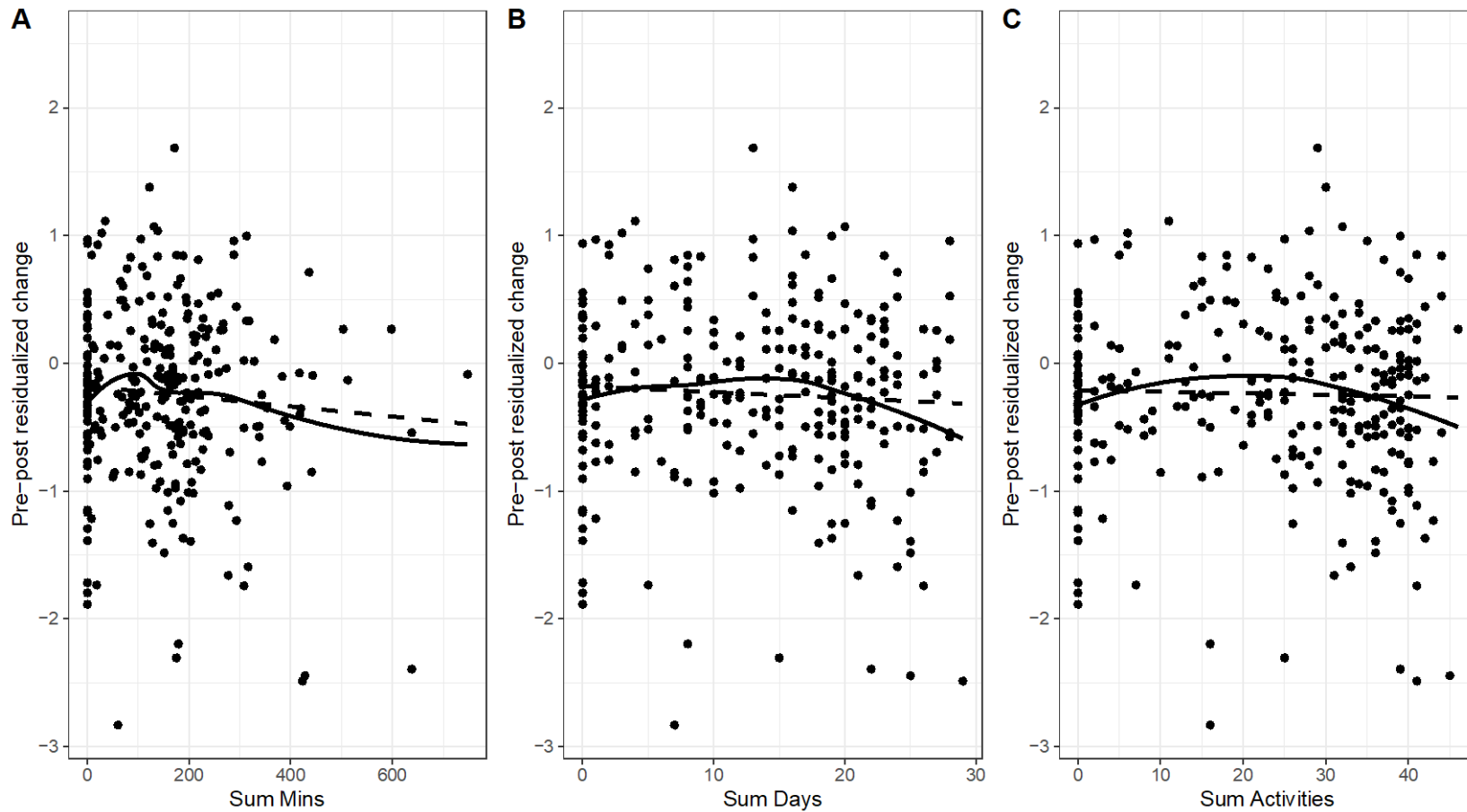
Histograms of HMP App Usage Restricted to HMP Users



Note. Sum Mins (Panel A) = minutes of meditation practice using the Healthy Minds Program (HMP) app; Sum Days (Panel B) = days of HMP use; Sum Activities (Panel C) = number of meditation practices and/or recordings of didactic content completed. Curved black lines show density distribution. Restricted to participants assigned to HMP who used the app at least once (i.e., HMP Users; $n = 271$).

Figure 3

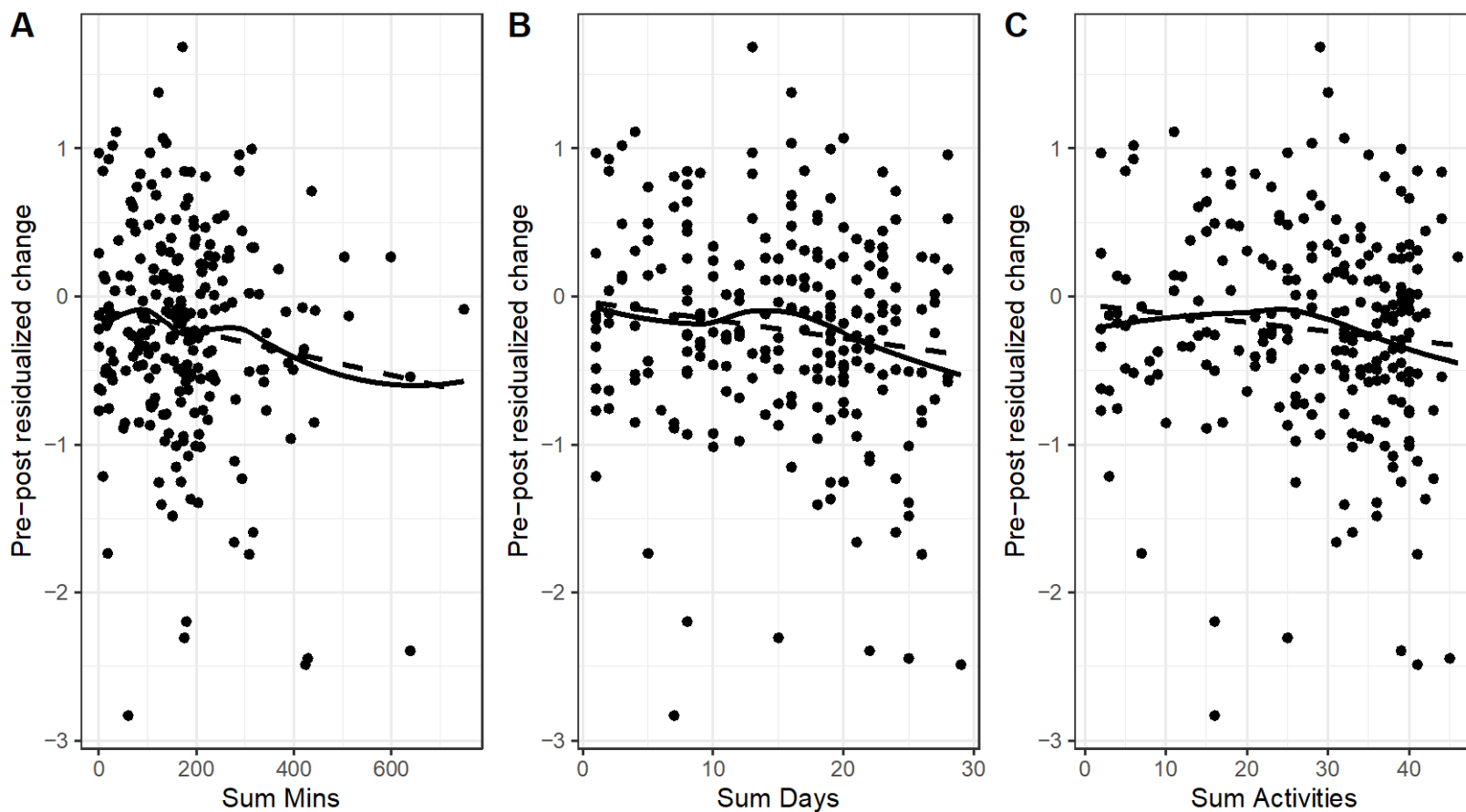
Scatterplots Displaying Associations Between HMP Usage and Pre-Post Change in Psychological Distress



Note. Sum Mins (Panel A) = minutes of meditation practice using the Healthy Minds Program (HMP) app; Sum Days (Panel B) = days of HMP use; Sum Activities (Panel C) = number of meditation practices and/or recordings of didactic content completed; Pre-post residualized change = change in psychological distress; dashed line = linear regression line; solid line = local regression (i.e., loess curve). $n = 344$.

Figure 4

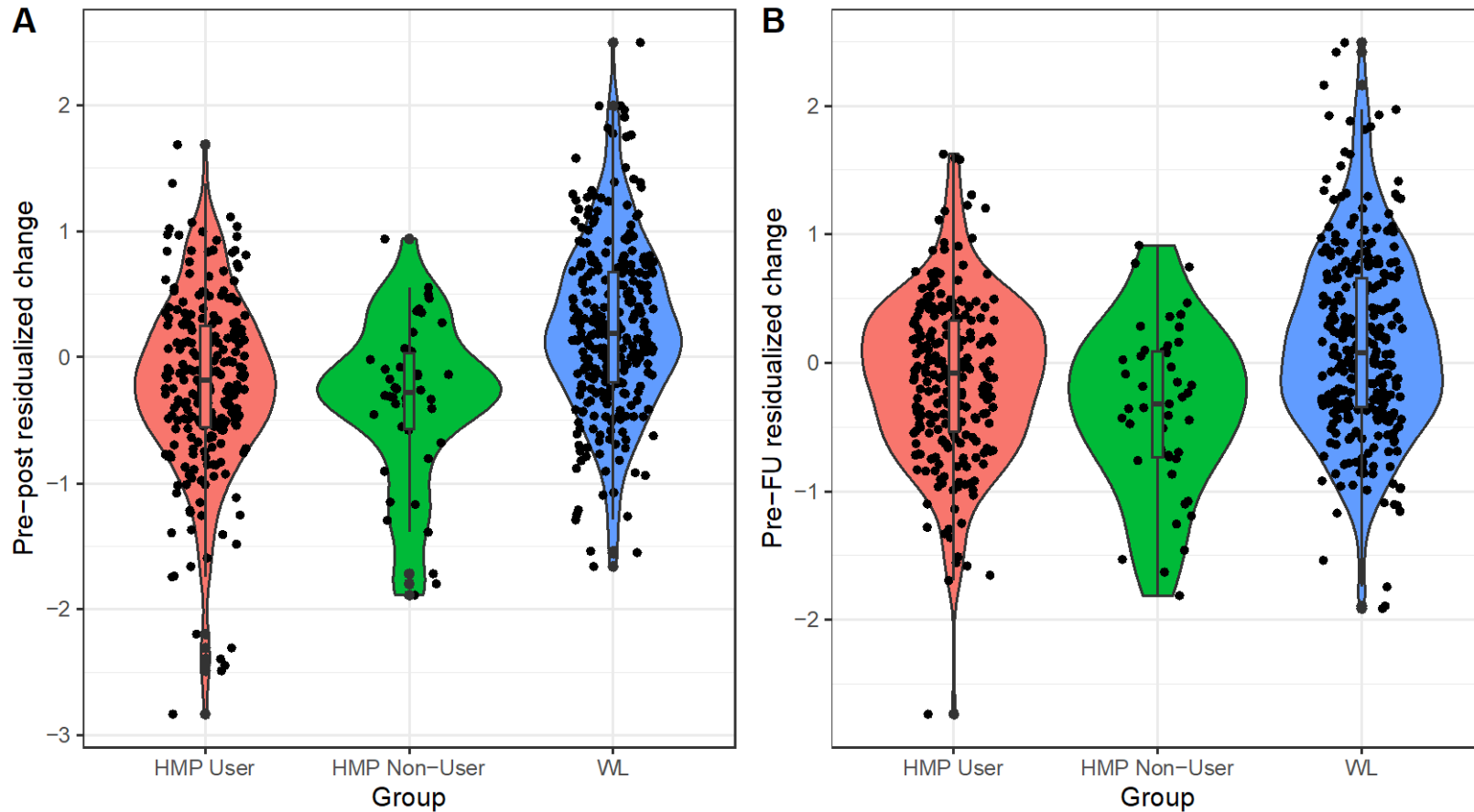
Scatterplots Displaying Associations Between HMP Usage and Pre-Post Change in Psychological Distress Among HMP Users



Note. Sum Mins (Panel A) = minutes of meditation practice using the Healthy Minds Program (HMP) app; Sum Days (Panel B) = days of HMP use; Sum Activities (Panel C) = number of meditation practices and/or recordings of didactic content completed; Pre-post residualized change = change in psychological distress; dashed line = linear regression line; solid line = local regression (i.e., loess curve). Restricted to participants assigned to HMP who used the app at least once (i.e., HMP Users; $n = 271$).

Figure 5

Violin Plots Displaying Associations Between Dosage Groups and Residualized Change in Psychological Distress



Note. HMP User = participants assigned to the Healthy Minds Program (HMP) group who used the app at least once; HMP Non-User = participants assigned to HMP group who did not use the app at least once; WL = waitlist control; Pre-post residualized change = change in psychological distress from pre- to post-treatment (Panel A); Pre-FU = pre- to 3-month follow-up (Panel B). Boxplots display median, 25th and 75th percentile with whiskers out to the interquartile range.

Supplemental Materials Table 1

Dosage Operationalizations and Modeling Approaches

Dosage Operationalizations	
Dosage Variable	Description
Sum Mins	Minutes of meditation practice
Sum Days	Days of HMP use
Sum Activities	Number of activities (meditation practices, didactic content)
Sum Mins ²	Minutes of meditation practice squared
Sum Days ²	Days of HMP use squared
Sum Activities ²	Number of activities squared
HMP Users Sum Mins	Minutes of meditation practice for HMP participants with Sum Days > 0
HMP Users Sum Days	Days of HMP app use for HMP participants with Sum Days > 0
HMP Users Sum Activities	Number of activities for HMP participants with Sum Days > 0
Dosage Groups	HMP participants with Sum Days > 0 (HMP Users), HMP participants with Sum Days = 0 (HMP Non-Users), and Waitlist Participants
Modeling Approaches	
Model	Description
OLS predicting post-test	Post-test psych predicted by dosage controlling for pre-test psychological distress
OLS predicting follow-up	Follow-up psychological distress predicted by dosage controlling for pre-test psychological distress
MLM predicting weekly psychological distress from weekly dosage	Weekly psychological distress scores predicted by dosage during a given week controlling for baseline psychological distress
MLM predicting longitudinal changes in psychological distress from dosage group	Weekly psychological distress scores predicted by the interaction between dosage group and time
OLS predicting weekly psychological distress slope	Weekly changes in psychological distress (random slopes extracted from MLMs) predicted by dosage

Note. HMP = Healthy Minds Program; OLS = ordinary least squares, MLM = multilevel model.

Supplemental Materials Table 2

Outcome Descriptive Statistics

Group	Time	<i>n</i>	% Missing	Mean	<i>SD</i>	Min	Max	Skew	Kurtosis
Waitlist	Pre-test	315	0.9	0.00	0.91	-3.01	2.51	-0.22	0.06
Waitlist	Week 1	276	13.2	-0.10	0.95	-3.43	2.52	-0.14	-0.19
Waitlist	Week 2	270	15.1	-0.10	1.01	-3.18	2.71	-0.12	-0.49
Waitlist	Week 3	271	14.8	-0.14	1.02	-2.87	2.81	-0.11	-0.33
Waitlist	Post-test	289	9.1	-0.20	1.03	-3.29	3.22	-0.07	0.09
Waitlist	Follow-up	289	9.1	-0.24	1.00	-3.03	2.27	-0.02	-0.27
Waitlist	Pre-post slope	318	0.0	0.03	0.11	-0.27	0.52	0.39	1.91
HMP	Pre-test	342	0.6	0.00	0.88	-2.94	2.03	-0.30	-0.17
HMP	Week 1	277	19.5	-0.29	0.89	-3.14	2.30	-0.21	0.49
HMP	Week 2	257	25.3	-0.40	0.85	-3.11	1.78	-0.12	0.13
HMP	Week 3	257	25.3	-0.55	0.88	-3.06	1.70	0.00	-0.04
HMP	Post-test	280	18.6	-0.66	0.89	-3.31	1.91	-0.12	0.26
HMP	Follow-up	282	18.0	-0.55	0.87	-2.79	1.85	-0.01	-0.22
HMP	Pre-post slope	344	0.0	-0.03	0.11	-0.48	0.26	-0.91	2.14

Note. HMP = Healthy Minds Program; Week 1 to 3 = weekly assessments during the 4-week intervention period; Follow-up = 3-month post-treatment follow-up assessment; Pre-post slope = random slope coefficients derived from multilevel models; Outcome = psychological distress (composite of PROMIS Depression, PROMIS Anxiety, and Perceived Stress Scale). Psychological distress values are zero at baseline and negative at all other time points based on psychological distress being z-transformed using baseline means and *SDs* and psychological distress decreasing over time in both groups. Sample sizes out of *n* = 318 assigned to waitlist and *n* = 344 assigned to the HMP condition.

Supplemental Materials Table 3

HMP Usage Descriptive Statistics

Sample	Variable	<i>n</i>	Mean	<i>SD</i>	Min	Max	Skew	Kurtosis
Full Sample	Sum Mins	344	127.83	130.49	0	748	1.32	2.35
Full Sample	Sum Days	344	10.87	9.08	0	29	0.21	-1.37
Full Sample	Sum Activities	344	20.05	15.47	0	46	-0.07	-1.56
HMP Users	Sum Mins	271	162.27	126.59	0	748	1.28	2.55
HMP Users	Sum Days	271	13.80	8.01	1	29	-0.09	-1.19
HMP Users	Sum Activities	271	25.45	12.89	1	46	-0.45	-1.1

Note. Sum Mins = minutes of meditation practice using the Healthy Minds Program (HMP) app; Sum Days = days of HMP use; Sum Activities = number of meditation practices and/or recordings of didactic content completed. HMP Users = participants assigned to HMP who used the app at least once.

Supplemental Materials Table 4

HMP Usage Metrics Intercorrelations

Variable	Sum Mins	Sum Days	Sum Activities
Sum Mins	-	.75	.77
Sum Days	.82	-	.88
Sum Activities	.83	.93	-

Note. Values below the diagonal based on the full sample ($n = 344$); value above the diagonal based on Healthy Minds Program app users (i.e., Sum Days > 0). All $p < .001$.

Supplemental Materials Table 5

Associations between Dosage and Outcomes Across Dosage Operationalizations and Ordinary Least Squares Modeling Approaches with Multiple Imputation

Modeling Approach	Dosage Operationalization									Dosage Groups
	Sum Mins	Sum Days	Sum Activities	Sum Mins ²	Sum Days ²	Sum Activities ²	Sum Mins HMP Users	Sum Days HMP Users	Sum Activities HMP Users	
Post-test OLS	$\beta = -.0002,$ $p = .553$	$\beta = -.002,$ $p = .649$	$\beta = .0001,$ $p = .960$	$\beta^2 = -.16,$ $p = .111$	$\beta^2 = -.44,$ $p = .006$	$\beta^2 = -.45,$ $p = .012$	$\beta = -.06,$ $p = .398$	$\beta = -.07,$ $p = .357$	$\beta = -.05,$ $p = .580$	HMP Non-Users $b = -0.11,$ $p = .359$ Waitlist $b = 0.45,$ $p < .001$
Follow-up OLS	$\beta = .0002,$ $p = .960$	$\beta = .012,$ $p = .771$	$\beta = .0009,$ $p = .732$	$\beta^2 = -.06,$ $p = .515$	$\beta^2 = -.28,$ $p = .061$	$\beta^2 = -.45,$ $p = .008$	$\beta = -.07,$ $p = .176$	$\beta = -.08,$ $p = .118$	$\beta = -.10,$ $p = .087$	HMP Non-Users $b = -0.26,$ $p = .032$ Waitlist $b = 0.26,$ $p < .001$

Note. Sum Mins = minutes of meditation practice using the Healthy Minds Program (HMP) app; Sum Days = days of HMP use; Sum Activities = number of meditation practices and/or recordings of didactic content completed; HMP Users = participants assigned to HMP who used the app at least once; Dosage Groups = comparisons between HMP Users with HMP Non-Users (i.e., assigned to HMP but did not use the app at least once) and Waitlist, with HMP Users as the reference group; β = standardized coefficient; b = regression coefficient with standardized outcome but unstandardized predictor (dosage groups); ² = coefficient for quadratic dosage term; bold values indicate $p < .050$; OLS = ordinary least squares; Post-test = predicting post-test controlling for pre-test; Follow-up = predicting 3-month follow-up controlling for pre-test. Final column displays coefficients for models testing dosage groups with HMP Users as the reference group.

Supplemental Materials Table 6

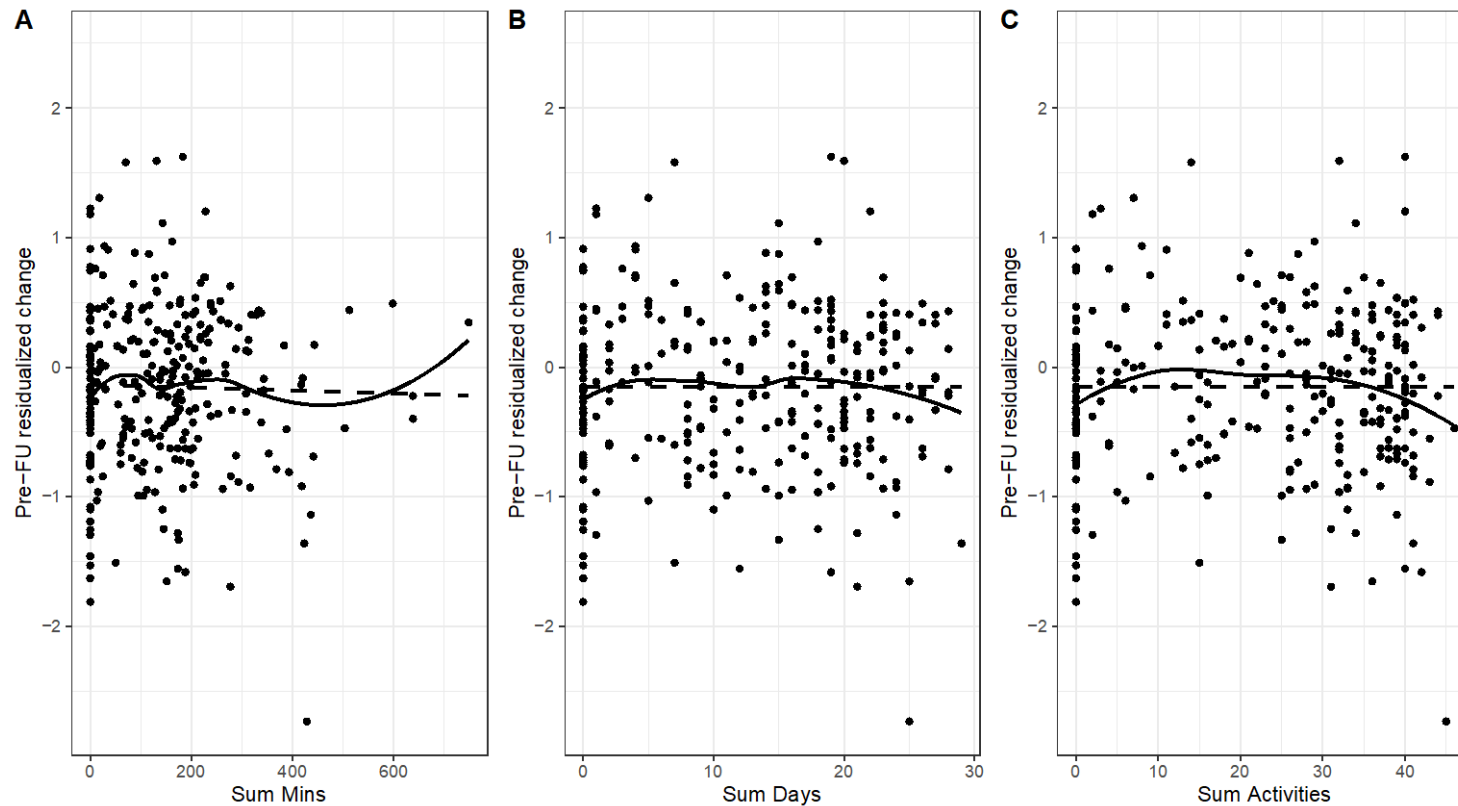
Model Fit Information and Model Selection Criteria for Latent Class Models

Number of Classes	Log- Likelihood	Number of Free Parameters	AIC	BIC	aBIC	Entropy
1	-6430.03	8	12876.05	12917.87	12892.46	---
2	-4874.89	17	9783.78	9872.64	9818.64	1.00
3	-4373.83	26	8799.66	8935.56	8852.97	0.96
4	-4205.13	35	8480.26	8663.20	8552.02	0.96
5	-4097.54	44	8283.08	8513.07	8373.30	0.98
6	-4019.72	53	8145.44	8422.47	8254.11	0.97
7	-3976.99	62	8077.98	8402.05	8205.10	0.97
8	-3950.84	71	8043.68	8414.79	8189.25	0.97

Note. Dashes indicate criterion was not applicable; bold indicates the model that was selected.
AIC = Akaike information criterion; BIC = Bayesian information criterion; aBIC = sample size adjusted BIC.

Supplemental Materials Figure 1

Scatterplots Displaying Associations Between HMP Usage and Pre- to Follow-up Change in Psychological Distress

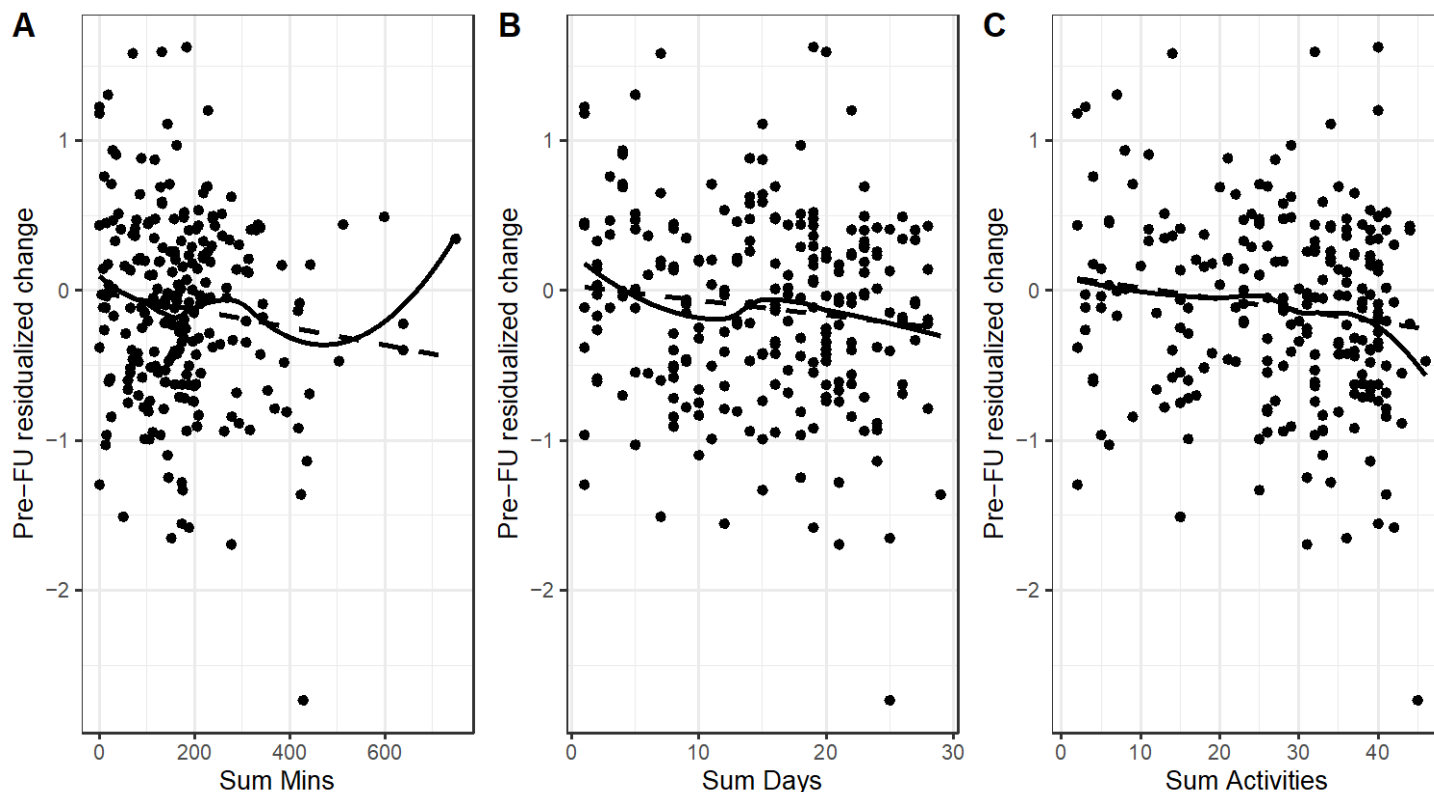


Note. Sum Mins (Panel A) = minutes of meditation practice using the Healthy Minds Program (HMP) app; Sum Days (Panel B) = days of HMP use; Sum Activities (Panel C) = number of meditation practices and/or recordings of didactic content completed; Pre-FU residualized change = change in psychological distress from pre-test to 3-month follow-up; dashed line = linear regression line; solid line = local regression (i.e., loess curve). $n = 344$.

Supplemental Materials Figure 2

Scatterplots Displaying Associations Between HMP Usage and Pre- to Follow-up Change in Psychological Distress Among HMP

Users



Note. Sum Mins (Panel A) = minutes of meditation practice using the Healthy Minds Program (HMP) app; Sum Days (Panel B) = days of HMP use; Sum Activities (Panel C) = number of meditation practices and/or recordings of didactic content completed; Pre-FU residualized change = change in psychological distress from pre-test to 3-month follow-up; dashed line = linear regression line; solid line = local regression (i.e., loess curve). Restricted to participants assigned to HMP who used the app at least once (i.e., HMP Users; $n = 271$).