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ORIGINAL PAPER

Examining How Headspace Impacts Mindfulness Mechanisms Over an 8-Week App-Based Mindfulness Intervention

Larisa Gavrilova[1] · **Matthew J. Zawadzki**[1]

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Abstract

Objectives Theoretical work proposed that mindfulness interventions function by enhancing various mindfulness mechanisms, including acceptance, attention monitoring, decentering, self-compassion, and nonreactivity. However, much of what is currently known about the effects of mindfulness interventions on mindfulness mechanisms comes from studies assessing these mechanisms pre- and post-treatment, which provides limited insights into how these mechanisms develop over the course of a mindfulness intervention. The present randomized, waitlist-controlled trial investigated how the proposed mindfulness mechanisms change over the course of an 8-week app-based mindfulness intervention (Headspace).

Method A sample of university employees ($n = 132$; 76.5% female; age $M \pm SD = 38.5 \pm 11.1$; 54.5% White) was randomly assigned to participate in a mindfulness intervention ($n = 92$) or to a waitlist control group ($n = 40$). Mindfulness mechanisms were assessed using ecological momentary assessment, with participants providing reports on mindfulness mechanisms five times daily for four days during the baseline (pre-treatment), 2nd, 5th, and 8th weeks, resulting in a total of 6,327 assessments.

Results Changes in the mechanisms of acceptance-attention and nonreactivity were observed from the second week of the intervention onwards, with marginal effects for decentering. These effects showed a steady linear increase in the mindfulness group.

Conclusions Results demonstrate the potential for rapid and sustained improvements in mindfulness mechanisms following an app-based mindfulness intervention.

Preregistration The study was preregistered on ClinicalTrials.gov: NCT03652168.

Keywords Mindfulness · Mindfulness mechanisms · Smartphone app · Digital health · Ecological momentary assessment

Mindfulness interventions teach practitioners to cultivate deliberate and non-judgemental awareness of moment-to-moment experience (Kabat-Zinn, 1994). These interventions have been linked to a broad range of positive effects (for review, see Khoury et al., 2013). In explaining these effects, researchers proposed that mindfulness interventions work by promoting an array of adaptive mental states, including acceptance, attention monitoring, decentering, self-compassion, and nonreactivity - herein, called mindfulness mechanisms. Yet, there is no agreed-upon unifying theoretical framework of how mindfulness interventions change these mechanisms (Alsubaie et al., 2017). Moreover, much of what is currently known about the effects of mindfulness interventions on mindfulness mechanisms comes from studies assessing these changes at pre- and post-treatment (e.g., Josefsson et al., 2014; Shapiro et al., 2005) that provide limited insights into how mindfulness mechanisms develop over the course of a mindfulness intervention. Given that mindfulness mechanisms represent distinct skills, it is plausible that they develop at different time points during the intervention (Baer et al., 2012). Establishing the sequence in which mindfulness mechanisms change can allow researchers to have tools to detect if the intervention is working. Thus, the goal of the study was to investigate how mindfulness mechanisms change over the course of an 8-week app-based mindfulness intervention.

Mindfulness interventions can influence the mechanisms of acceptance, attention monitoring, decentering, self-compassion, and nonreactivity. *Acceptance* is commonly defined

✉ Larisa Gavrilova
lgavrilova@ucmerced.edu

✉ Matthew J. Zawadzki
mzawadzki@ucmerced.edu

[1] Psychological Sciences, University of California, 5200 N. Lake Road, Merced, CA 95343, USA

as the ability to observe experiences with an attitude of nonjudgment and openness (Kabat-Zinn, 2009). *Attention monitoring* involves ongoing awareness of one's present moment sensory and perceptual experiences (Hölzel et al., 2011; Lindsay & Creswell, 2017; Shapiro et al., 2006). Theoretical work and empirical evidence provide support that attention monitoring and acceptance should be considered in tandem, as the combination of both attention monitoring and acceptance skills contributes to beneficial health and well-being outcomes (e.g., Chin et al., 2019; Gavrilova & Zawadzki, 2023; Lindsay & Creswell, 2017). *Decentering* is defined as the ability to reflect on negative experiences from a self-distanced, rather than self-immersed perspective (Shapiro et al., 2006). *Self-compassion* involves treating oneself with the same kindness and patience as one would treat a friend in the same situation (Neff, 2003). *Nonreactivity* has been conceptualized as the ability to approach stressful situations without reacting to them and acting in automatic habitual patterns (Bishop, 2002; Kabat-Zinn, 1994).

Despite increasing research suggesting that mindfulness interventions bring about changes in the proposed mechanisms (Alsubaie et al., 2017; Gu et al., 2015; Smith & Whitley, 2022; van der Velden et al., 2015), there is a lack of methodological rigor in this area of research that precludes conclusions (Alsubaie et al., 2017). Moreover, much of this work has predominantly focused on pre-and post-treatment changes (Josefsson et al., 2014; Shapiro et al., 2005) that limit an understanding of the pattern of change over the course of the intervention. As one exception, Lu et al. (2021) investigated the week-to-week development of state mindfulness over the course of an 8-week app-based mindfulness intervention. Researchers found a relatively strong linear increase in state mindfulness, but with a lower rate of increase toward the end of the program. In another study, Baer et al. (2012) explored weekly change in a variety of mindfulness mechanisms and perceived stress in participants who completed an 8-week course in the mindfulness-based stress reduction (MBSR) program (Kabat-Zinn, 1982; Kabat-Zinn & Hanh, 2009). Results showed that early change in mindfulness over the first three weeks predicted change in perceived stress over the rest of the intervention. Related to specific mindfulness mechanisms, the magnitude of change was largest for the nonreactivity mechanism; nonreactivity was notably lower than the other mechanisms (i.e., nonjudge, describe, observe, act with awareness) at Week 1 and showed a large improvement following Week 2. Findings from these studies suggest that mindfulness mechanisms do not change at the same rate over the course of the intervention period. As such, more research is needed to better understand this rate of change in mindfulness mechanisms, as it might predict the extent of improvement in outcomes.

Another important issue concerning mindfulness interventions in general is that the literature reports varying lengths of intervention required to observe beneficial effects. The scientific community has often focused on MBSR and other 8-12-week mindfulness-based interventions (Creswell, 2017). These programs are designed to be an intensive training experience requiring significant time demands. The standard 8-week MBSR format requires 32 class hours in addition to 30–45 min per day for home practice (Carmody & Baer, 2009). Although this training has been shown to be effective in improving a broad range of outcomes (for review, see Creswell, 2017), the time commitment is often reported as a primary reason for potential participants to decline to participate in MBSR (Carmody & Baer, 2009). Yet, research on how long a mindfulness intervention has to be for it to produce beneficial effects has produced inconsistent findings. For example, studies showed significant improvements in mindfulness mechanisms after completing six sessions (e.g., Duarte & Pinto-Gouveia, 2016) and as few as three sessions (Beaumont et al., 2016). Strikingly, some work even suggests the lack of a dose-response relationship between the length of the mindfulness intervention and psychological benefits (Carmody & Baer, 2009). If mindfulness programs with lower time demands produce similar improvements in mindfulness mechanisms, the length of the mindfulness intervention might be modified to make mindfulness interventions more accessible and sustainable.

Ecological momentary assessment (EMA) can be used to collect repeated measures of mindfulness mechanisms over time to track how they change during treatment. EMA involves repeated sampling of real-time data of participants' current experiences and behaviors in their naturalistic environment (Shiffman et al., 2008; Smyth & Stone, 2003). Studies have shown that EMA can offer a greater sensitivity to change and increase the precision of detecting intervention effects, particularly to understanding mindfulness mechanisms as dynamic and complex developmental processes over time (Shoham et al., 2017). Furthermore, repeated measurements can be averaged across multiple time points throughout the intervention to characterize the participant's typical state. One major advantage of the EMA approach is that the aggregate-level values contain multiple measurements and are more reliable than assessments at a single time.

Some recent studies have utilized the EMA framework to evaluate the contributions of specific mindfulness mechanisms. One line of research has effectively shown that mindfulness mechanisms are naturally occurring states that vary within person over time (e.g., Blanke & Brose, 2017; Brown & Ryan, 2003; Gavrilova & Zawadzki, 2023; Senker et al., 2022; Shapiro et al., 2006). Another line of research started investigating the potential of mechanisms to change in response to mindfulness training. Chin et al. (2021) found that both the standard 8-week MBSR training program, and a modified 8-week MBSR-adapted training program

that focused on monitoring skills, improved momentary (and trait measures) of attention control. In another study, Uchida et al. (2022) tested the effects of an 8-week standard mindfulness intervention in a sample of 19 patients with depression and/or anxiety. Although no significant changes in mental health were observed, results revealed significant improvements in self-compassionate and mindful behaviors. Despite their important contributions to research on mindfulness mechanisms, the main goal of EMA in both studies was to minimize recall bias and maximize ecological validity and mindfulness mechanisms were measured via EMA at baseline and post-intervention. Therefore, the exact point at which these mechanisms begin to improve and whether these effects compound over time remains unclear.

Most mindfulness interventions are delivered in a traditional in-person format. Yet, approximately 81% of US adults report having a smartphone (Silver et al., 2019), and there is growing interest in mindfulness interventions delivered via digital tools. Some potential advantages of app-based mindfulness interventions include wider availability, anonymity, accessibility at any time or place, standardization of intervention instruction, and personalization of content (Mrazek et al., 2019). Given the growing interest in digital psychotherapeutic interventions (Renn et al., 2019), app-based mindfulness training may represent a promising intervention alternative to traditional in-person training. Although evidence on the effectiveness of app-based mindfulness training is growing and some studies found benefits comparable to traditional delivery methods on outcomes of subjective well-being and mental health outcomes (e.g., Howells et al., 2016), research on the effects of app-based mindfulness training on mindfulness mechanisms is still in a nascent stage. A recent meta-analysis showed some promising results with respect to the mindfulness mechanisms of acceptance and self-compassion. The study demonstrated that these mechanisms can indeed be improved through smartphone apps (Linardon, 2020). Thus, the present study makes an important contribution to the limited work on the effects of app-based mindfulness training on mindfulness mechanisms.

The present study examined the effects of an 8-week app-based mindfulness intervention on the mindfulness mechanisms of acceptance, attention monitoring, decentering, self-compassion, and nonreactivity. Research Question 1 investigated when during the intervention period mindfulness mechanisms improved, examining potential changes from Week 0 (i.e., baseline) to Week 2, Week 5, and Week 8. Consistent with previous work (Baer et al., 2012), we hypothesized that the mechanisms of acceptance and attention monitoring would be the first to change followed by improvements in decentering and nonreactivity. Given that self-compassion is a more advanced skill that is not explicitly taught in mindfulness interventions (although generally the mindfulness training can be applied to approach oneself in a more forgiving and accepting way), we expected improvements in self-compassion to occur at the end of the intervention. Research Question 2 assessed whether these changes compound over the course of the intervention. We hypothesized that mindfulness mechanisms would compound in a linear fashion.

Method

Participants

The study was conducted at a public university in Central California. In order to participate, participants had to be non-student and non-faculty employees at the university. Participants were recruited through university emails and flyers to participate in the study testing the effects of mindfulness training on workplace stress. As compensation, participants received a free one-year subscription to Headspace. For each weekly survey (Week 0, 2, 5, and 8), participants received $15. Additionally, participants could receive up to a $20 bonus for a high completion rate (i.e., over 80% surveys completed) across the study. Interested participants completed an online survey related to eligibility criteria. Inclusion criteria were having access to a smartphone with internet access every day, being fluent in English, being an employee of the university where the study was conducted, and being at least 18 years of age. Participants who were experienced meditators, defined as having participated in a sitting meditation practice more than twice a week (for 10 min or greater) over the last three months, were excluded to ensure that participants did not differ in dispositional mindfulness prior to the intervention.

A total of 291 employees were screened for eligibility, 271 were eligible and 20 were excluded from the study as they were either student employees or faculty. Of the 271 eligible participants, 186 people provided consent. Before the study began, 43 participants dropped out from the study. A total of 143 participants were randomized. Data for 11 participants were excluded from the subsequent analyses: 6 participants dropped out from the study after randomization, and 5 participants had all mechanisms data as missing (after data for participants who had less than 5 observations was set to missing). Numbers of participants at each stage of the trial are illustrated in Fig. 1. At Week 0, our sample consisted of 132 participants, with 92 (69.70%) participants randomized into the Headspace group and 40 (30.30%) into the waitlist control group. At Week 2, 84 (91.30%) participants from the Headspace group and 39 (97.50%) participants from the control group completed EMA survey. At Week 5, 66 (71.74%) participants from the Headspace group and 36 (90.00%) from the control group completed EMA survey.

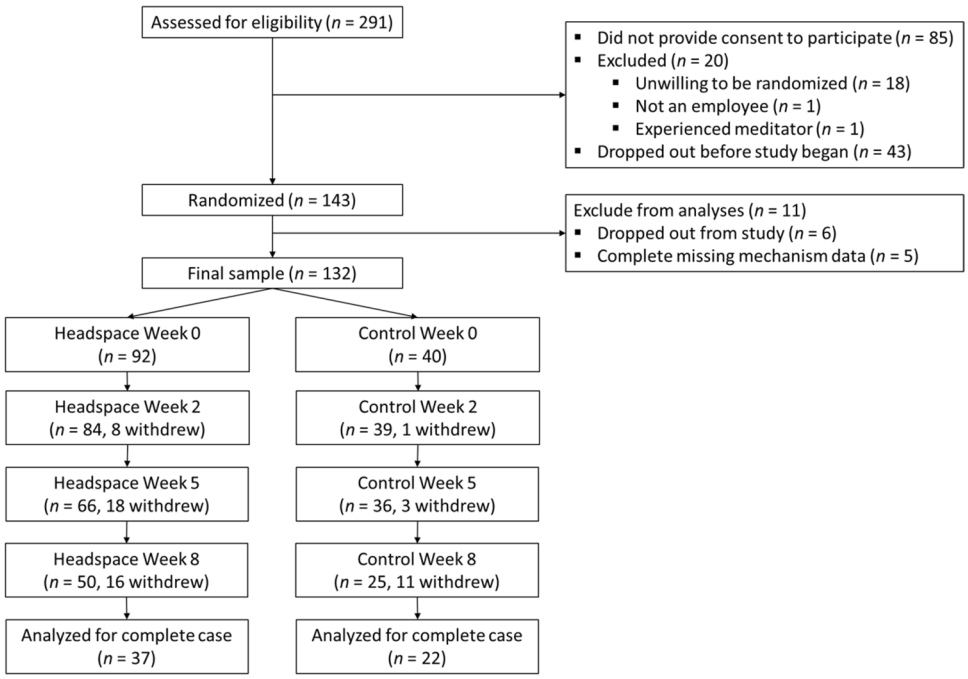

Fig. 1 CONSORT diagram of participant flow

Table 1 Demographics of participants

Characteristic	Intervention group ($n = 92$)	Waitlist control group ($n = 40$)
Age		
Range	21–63	22–65
Mean (Standard Deviation)	37.2 (11.0)	41.6 (10.8)
Sex		
Female (n, %)	71, 77.17%	30, 75.00%
Male (n, %)	21, 22.83%	10, 25.00%
Missing (n, %)		
Ethnicity		
Non-Latinx (n, %)	48, 52.17%	24, 60.00%
Latinx (n, %)	23, 25.00%	6, 15.00%

At Week 8, 50 (54.35%) participants from the Headspace group and 25 (62.50%) participants from the control group completed EMA survey.

Participants ($n = 132$) were between the ages of 21 and 65 ($M = 38.5$, $SD = 11.1$) and self-identified as primarily White and Latinx females (76.52% female; 54.55% White, 21.97% Latinx). Table 1 shows baseline characteristics of the sample for both groups. There were no differences between the intervention and control group on gender and race/ethnicity (chi-square tests, all $p\text{-values} > 0.153$). Participants in the intervention group were significantly younger than those in the control group (t-test, $p = 0.037$). Groups did not differ on mindfulness mechanism scores at baseline (two-sample t-tests, all p-values> 0.159). Table 2 presents descriptive statistics for mindfulness mechanisms at all four time points for the intervention group and the waitlist control group.

Procedure

The intervention was delivered through a commercially available mindfulness meditation app (Headspace; https://www.headspace.com) that has been widely used in previous intervention studies (e.g., Flett et al., 2020). Headspace provides a variety of formal guided and unguided meditation practices, with the content of training built on well-established concepts and practices within the mindfulness literature (Kabat-Zinn et al., 1992). Instructions are delivered through short, animated videos and sound files. Headspace is available as an iOS, Android, and web app.

Participants were instructed to use the app to meditate for 10 min a day for 8 weeks. Intervention group was instructed to start meditating using the Basics pack. This pack was designed as an introduction to mindfulness meditation and can be used as an opportunity for participants to get familiar with the Headspace teaching style. It presented the concept of mindfulness and encouraged one to pay attention to breathing and notice patterns of mind-wandering and thoughts. These sessions first began with controlled breathing, with continued prompts through the session to continue to monitor and slow down respiration. Participants then were encouraged to observe the thoughts they had (attention) with an emphasis on not getting involved with the thoughts (non-reactivity) but rather accepting these thoughts as occurring (acceptance). As lessons continue, decentering concepts were introduced to help participants develop tools to observe

Table 2 Means (standard deviations) for mindfulness mechanisms at Week 0, 2, 5, 8

Week	Acceptance-attention	Decentering	Self-compassion	Nonreactivity
Headspace				
Week 0	5.85 (1.25)	4.71 (1.32)	6.25 (1.53)	5.72 (1.49)
Week 2	6.13 (1.51)	5.26 (1.66)	6.53 (1.62)	6.15 (1.56)
Week 5	6.41 (1.56)	5.76 (1.76)	6.60 (1.49)	6.41 (1.60)
Week 8	6.48 (1.65)	6.11 (1.66)	6.43 (1.77)	6.33 (1.73)
Control				
Week 0	6.07 (1.33)	5.08 (1.42)	6.24 (1.42)	6.08 (1.43)
Week 2	5.93 (1.24)	5.10 (1.19)	6.08 (1.24)	5.96 (1.25)
Week 5	6.04 (1.21)	5.42 (1.20)	6.12 (1.35)	6.12 (1.19)
Week 8	5.97 (1.28)	5.49 (1.59)	6.17 (1.50)	5.98 (1.29)

thoughts from a third-party perspective. Additionally, participants were increasingly encouraged to be kind to themselves in regards to how they may have acted in a situation (self-compassion). As such, all the mechanisms we tested in this paper were targeted with increasing complexity and emphasis as participants complete the training program. More information is available through the Headspace website: https://www.headspace.com/meditation/basic-meditation.

Once participants completed the Basics 1, 2, and 3 (10 days each), they were instructed to move on to the Stress pack. The Stress pack lasted for 30 sessions, about 10 min each. Each Stress pack had the same format as the Basics pack except that the focus of the content was more specialized to reducing stress. It combined visualization and body scanning to help users learn to accept their emotions and pay close attention to the present moment. Also, it encouraged participants to probe thoughts in the past, present, and future that are stressful in nature.

The study was registered on clinicaltrials.gov. There were no deviations to the approved protocol, and the analyses presented are those for the secondary set of outcomes proposed. Individuals interested in the study logged on to a secure website and read information about the study. Those interested in taking part in the study, were prompted to complete a screening survey relevant to the inclusion and exclusion criteria. Eligible participants who consented to participate in the study received a link to the baseline questionnaire battery via Qualtrics related to demographics and other surveys relevant to the main study.

Following the baseline survey, participants attended one 60-min in-lab orientation (in groups of up to four) on campus between October 2018 to May 2019. During the orientation, participants received training on how to download and use an application Lifedata (RealLife Exp, Life Data Corporation, Marion, IN) on their smartphone that served as a platform for EMA surveys. Research staff guided participants through each question in a sample survey and allowed participants to practice answering questions. At the end of the in-lab session, each participant received a printed user guide that described each EMA question, the frequency of the surveys, and relevant information about the study, and provided FAQs concerning LifeData and researcher contact information. Surveys were programmed to appear on participants' smartphones at random times within specified time windows – 8:00am-10:00am, 10:30am-12:00pm, 1:00pm-3:00pm, 3:30pm-5:30pm, 6:00pm-8:00pm – five times a day for 4 consecutive days. All EMAs were completed from Wednesday through Saturday to standardize the comparison days across participants. Participants then repeated this protocol at two, five, and eight weeks post-randomization. We chose these intervals balancing the desire for in-depth assessments of mechanisms as they changed while trying to limit participant burden. For the mindfulness mechanisms data, at Week 0 we had up to 2066 assessments ($M = 15.65$ per person), 1844 at Week 2 ($M = 14.99$), 1424 at Week 5 ($M = 13.96$), and 993 at Week 8 ($M = 13.24$).

After the baseline and training sessions, participants were randomized into either the Headspace intervention or waitlist control group using a 2:1 allocation ratio. Any research staff that interacted with participants regarding their data collection were masked to the condition the participant was in. Participants in the intervention group were sent a personalized email with download instructions for the app and a code granting them a 12-month access to Headspace. Participants were instructed to use the app to meditate for 10 min a day for 8 weeks. After using the Basics pack for 30 days, participants moved on to the Stress pack that lasted for 30 sessions. Participants in the intervention group were tracked for downloading and using the app, with participants' objective data being provided by Headspace. If participants did not download and/or use the app, they received text messages reminding them to download and use the app. Participants in the waitlist control group received access to the Headspace app after 4 months. They were also asked not to participate in any mindfulness activities (e.g., yoga, meditation) during this time.

Measures

During baseline, participants first completed demographic information about their age, gender, and ethnicity, and other measures not relevant to the present study.

To assess mindfulness mechanisms at the state level, we utilized items from commonly used scales (e.g., Bishop et al., 2004; Brown & Ryan, 2003; Hölzel et al., 2011; Neff, 2003), similar to prior work (e.g., Blanke & Brose, 2017). For example, nonreactivity items were drawn from the Five Facet Mindfulness Questionnaire (FFMQ; Baer et al., 2006), while the self-compassion items were formulated in line with Neff's (2003) conceptualization of self-compassion as being kind and patient towards oneself. The multi-level factor structure of the items used in the present study to measure mindfulness mechanisms was validated in previous work (Gavrilova & Zawadzki, 2023). This study found distinct dimensions for decentering, self-compassion, and nonreactivity at both the within-person and between-person levels, as has been shown in other work (Blanke & Brose, 2017). Two other potential dimensions – acceptance and attention – were found to load on a single factor; although not *a priori* specified, this finding was in line with the Monitor and Acceptance Theory (Lindsay & Creswell, 2017) that posits that both attention monitoring and acceptance should be considered in tandem as the basic mechanisms underlying mindfulness.

Drawing from experience sampling methodology studies (e.g., Moore et al., 2016; Roesch et al., 2010), each mindfulness mechanism was assessed using two items. All items were rated on a sliding scale from 0 (*not at all*) to 10 (*extremely*) and started with a common question stem "Since the last survey, …?" As an example, one of the items assessing acceptance read, "Since the last survey, how accepting did you feel of your thoughts and feelings?" For attention monitoring, one of the items read "Since the last survey, how much were you paying deliberate attention to your surroundings?" For decentering, one of the items read "Since the last survey, how much were you concerned with openly observing your experiences rather than controlling or changing them?" Lastly, one of the self-compassion items read "Since the last survey, how kind were you to yourself?" To test the psychometrics of these items, we restructured this data to a three-level structure with items from a scale nested within measurement occasion that were further nested within participants. We then decomposed the variance at each level, and performed calculations to determine the extent to which the variance was due to between persons, moments, and/or error (Bolger & Laurenceau, 2013); scores closer to one indicate more variance across measurement occasions than due to within-measurement occasion. The scales demonstrated at least moderate reliability to detect within-subject differences in change over time at all four time points, $R_C = 0.56–0.89$. One exception to this was lower reliability for decentering at Week 8, $R_C = 0.24$.

Data Analyses

Chi-square and *t*-tests were used to examine group differences in baseline demographic characteristics and mindfulness mechanisms. In line with best practices, intervention effects on mindfulness mechanisms were examined using both an intention-to-treat (ITT) and complete-case analysis (Altman, 2009). For both approaches, hierarchical mixed effect modeling was used to test whether the intervention improved mindfulness mechanisms across Week 0, Week 2, Week 5, and Week 8. Although multiple imputation is often used to handle missing data, we did not use this method for two reasons. First, randomized clinical trials rarely meet the requirement of missingness at random (Cornelisz et al., 2020), which is necessary for multiple imputation (Rubin, 2004). Second, the study had considerable attrition rates between Week 0 and Week 8 assessment (30.63% mindfulness mechanisms data missing), which is likely to produce unreliable estimates through imputation (Jakobsen et al., 2017). Considering these issues, all analyses in the present study were conducted in SAS 9.4 using the PROC MIXED procedure with restricted maximum likelihood. Rather than imputing missing data, this approach uses available data to calculate maximum likelihood estimates.

Data had a hierarchical structure with weeks (i.e., Weeks 0, 2, 5, 8; Level 1) nested within participants (Level 2). A single weekly score for each mechanism was calculated for every participant during Week 0, Week 2, Week 5, and Week 8 as the average of all measurements for that week. Participants could have up to 20 observations at each week. In order to ensure measurement for each week was reliable, participants had to have at least 5 observations for that week. Data for participants who had fewer than 5 observations were set as missing. Age, gender (coded as 0 = *male*, 1 = *female*), and ethnicity (0 = *non-Latinx*, and 1 = *Latinx*) were included as control variables. Weeks (0, 2, 5, 8) and study condition (coded as 0 = *waitlist control group*, 1 = *intervention group*) and the interaction between these terms were entered as predictors in the study. Random intercepts were included to account for individual differences in mindfulness mechanisms at baseline.

Research Question 1 examined at what point in the intervention mindfulness mechanisms begin to improve. To answer this research question, Week 2, 5, 8 were entered as categorical variables, with Week 0 as the comparison group. With this coding we tested whether mindfulness mechanisms at Weeks 2, 5, 8 were significantly different from Week 0. Research Question 2 examined whether there were linear improvements in mindfulness mechanisms during the intervention. In these models, the week variable was used as a continuous variable, indicating the number of weeks that have elapsed from the start of the intervention. Follow-up analyses tested whether the effects of time showed a

quadratic (non-linear) trend. Week was entered both as a linear and quadratic term, with both week terms interacting with condition.

For the complete case analysis, participants who completed the outcome measures at least at Week 0 and Week 8 assessment points were included. As a measure of effect size, we included pseudo R^2 values for each model (Singer & Willett, 2003). These values were calculated using the statistical model to produce an estimated value for each measurement. We then regressed this predicted value on the observed value. The resulting value represents the proportion of the variance in our outcomes that is accounted for by the model.

Results

Intention-to-Treat Analysis

Research Question 1 tested whether mindfulness mechanisms improved from Week 0 to Week 2, Week 5, and Week 8. As Table 3 shows, hierarchical mixed effects analysis revealed that acceptance-attention increased from Week 0 to Week 2 ($p = 0.044$), Week 0 to Week 5 ($p = 0.016$), and Week 0 to Week 8 ($p = 0.023$) in the mindfulness group. Decentering revealed a marginal increase at Week 2 ($p = 0.056$), Week 5 ($p = 0.083$), and Week 8 ($p = 0.063$) compared to Week 0. No significant changes were observed

Table 3 Beta estimates (standard errors) for mindfulness mechanisms with headspace, week (categorical), and the interaction between headspace and week as predictors

	Acceptance-attention		Decentering		Self-compassion		Nonreactivity	
	ITT Model	CC Model	ITT Model	CC Model	ITT Model	CC Model	ITT Model	CC Model
Fixed effects								
Intercept	**5.80 (0.56)**	**5.95 (0.86)**	**5.88 (0.59)**	**4.98 (0.87)**	**6.55 (0.61)**	**6.84 (0.99)**	**5.83 (0.60)**	**5.52 (0.95)**
Latinx	0.30 (0.28)	0.34 0.48	-0.40 (0.29)	0.38 (0.48)	0.001 (0.30)	0.07 (0.55)	0.30 (0.30)	0.60 (0.53)
Age	0.01 (0.01)	-0.01 0.02	-0.02 (0.01)	-0.001 (0.02)	-0.002 (0.01)	-0.02 (0.02)	0.01 (0.01)	0.005 (0.02)
Female	0.04 (0.27)	0.19 0.43	-0.07 (0.28)	-0.06 (0.43)	-0.23 (0.29)	0.06 (0.50)	-0.001 (0.29)	0.08 (0.48)
Headspace	-0.31 (0.28)	-0.13 (0.39)	-0.43 (0.30)	0.08 (0.41)	-0.03 (0.30)	0.02 (0.45)	-0.45 (0.30)	-0.13 (0.43)
Week 2	-0.16 (0.17)	-0.13 (0.21)	0.04 (0.21)	-0.09 (0.25)	-0.08 (0.18)	0.20 (0.21)	-0.08 (0.18)	0.11 (0.21)
Week 5	-0.04 (0.18)	0.10 (0.21)	0.34 (0.22)	0.37 (0.25)	-0.12 (0.20)	0.18 (0.22)	-0.01 (0.19)	0.23 (0.22)
Week 8	-0.001 (0.20)	0.08 (0.21)	**0.52 (0.24)**	**0.52 (0.24)**	-0.06 (0.21)	0.13 (0.21)	-0.04 (0.21)	0.10 (0.21)
Headspace*Week2	**0.41 (0.20)**	0.42 (0.26)	0.47^{+} (0.25)	0.53^{+} (0.31)	0.31 (0.22)	0.04 (0.27)	**0.50 (0.22)**	0.35 (0.27)
Headspace*Week5	**0.54 (0.22)**	**0.58 (0.27)**	0.46^{+} (0.27)	0.48 (0.32)	0.40^{+} (0.24)	0.19 (0.28)	**0.66 (0.23)**	0.52^{+} (0.28)
Headspace*Week8	**0.56 (0.24)**	0.51^{+} (0.26)	0.55^{+} (0.30)	0.31 (0.31)	0.28 (0.26)	0.08 (0.27)	**0.57 (0.26)**	0.46^{+} (0.27)
Random effects								
Intercept	**1.51 (0.21)**	**1.60 (0.33)**	**1.56 (0.23)**	**1.58 (0.34)**	**1.75 (0.25)**	**2.19 (0.49)**	**1.75 (0.25)**	**1.99 (0.41)**
Residual	**0.48 (0.04)**	**0.47 (0.05)**	**0.70 (0.06)**	**0.64 (0.07)**	**0.55 (0.05)**	**0.49 (0.05)**	**0.53 (0.05)**	**0.49 (0.05)**
Model statistics								
AIC	1122.6	642.9	1225.3	697.2	1176.9	667.6	1166.8	661.5
BIC	1128.4	647.1	1231.1	701.4	1182.7	671.7	1172.6	665.7
Pseudo R^2	0.02	0.05	0.08	0.09	0.02	0.03	0.03	0.05

Note. Coefficients in bold are significant at $p < 0.05$. $^{+}p < 0.10$. Week 0 is used as a comparison group, with all comparisons made against this time point. ITT Model is the intention to treat results; CC model is the complete case results

in self-compassion over the course of the intervention (p-values > 0.094). Nonreactivity increased significantly from Week 0 to Week 2 ($p = 0.022$), Week 0 to Week 5 ($p = 0.005$), and Week 0 to Week 8 ($p = 0.027$). For the control group, no significant effects were observed for acceptance-attention (p-values> 0.352). A significant increase was observed for decentering in the control group from Week 0 to Week 8 ($p = 0.029$), but not from Week 0 to Week 2 and Week 5 (p-values > 0.124). No significant effects were found in the control group for self-compassion (p-values > 0.543) and nonreactivity (p-values> 0.668).

Research Question 2 tested linear effects of the intervention on mindfulness mechanisms. As Table 4 shows, tests of linear effects revealed that the mechanisms of acceptance-attention ($p = 0.013$) and nonreactivity ($p = 0.010$) increased steadily in a linear trend in the mindfulness group. Decentering revealed marginal linear effects ($p = 0.058$). No significant linear effects were observed for self-compassion ($p = 0.198$). For the control group, no significant linear effects were observed for acceptance-attention, self-compassion, or nonreactivity (p-values> 0.718). However, decentering exhibited a significant linear trend ($p = 0.013$).

Follow-up analyses revealed no significant quadratic effects of time (i.e., week) on any of the mindfulness mechanisms in the intervention group (p-values > 0.142). For the control group, no significant quadratic effect was found for acceptance-attention ($p = 0.211$), self-compassion ($p = 0.613$), and nonreactivity ($p = 0.108$). A significant quadratic effect was observed for decentering ($p < 0.001$) for the control group.

Complete Case Analysis

Participants who completed Week 8 of the study were comparable to non-completers on baseline characteristics, except for Latinx participants being more likely to complete Week 8 of the study compared to non-Latinx participants (chi-square test, $p = 0.019$). In testing whether mindfulness mechanisms improved from Week 0 to Week 2, Week 5, and Week 8 (Research Question 1), complete case analysis revealed a somewhat similar pattern of results considering that there were fewer participants than in the ITT analyses. As shown in Table 3, acceptance-attention did not significantly increase from Week 0 to Week 2 ($p = 0.114$) in the

Table 4 Beta estimates (standard errors) for mindfulness mechanisms with headspace, week (linear), and the interaction between headspace and week as predictors

	Acceptance-Attention		Decentering		Self-Compassion		Nonreactivity	
	ITT Model	CC Model	ITT Model	CC Model	ITT Model	CC Model	ITT Model	CC Model
Fixed effects								
Intercept	**5.74 (0.56)**	**5.91 (0.85)**	**5.83 (0.58)**	**4.91 (0.86)**	**6.52 (0.60)**	**6.93 (0.99)**	**5.80 (0.60)**	**5.58 (0.95)**
Latinx	0.30 (0.28)	0.34 (0.48)	-0.39 (0.29)	0.38 (0.48)	0.01 (0.30)	0.07 (0.55)	0.30 (0.30)	0.59 (0.53)
Age	0.01 (0.01)	-0.01 (0.02)	-0.02 (0.01)	-0.001 (0.02)	-0.002 (0.01)	-0.02 (0.02)	0.01 (0.01)	0.005 (0.02)
Female	0.04 (0.27)	0.19 (0.43)	-0.06 (0.28)	-0.06 (0.43)	-0.22 (0.29)	0.06 (0.50)	0.004 (0.29)	0.08 (0.48)
Headspace	-0.20 (0.26)	0.02 (0.38)	-0.30 (0.28)	0.28 (0.39)	0.07 (0.28)	0.05 (0.43)	-0.31 (0.28)	0.002 (0.42)
Week	0.004 (0.02)	0.02 (0.02)	0.07 (0.03)	**0.08 (0.03)**	-0.01 (0.02)	0.01 (0.02)	-0.00 (0.02)	0.01 (0.03)
Headspace*Week	**0.07 (0.03)**	0.06^{+} (0.03)	0.06^{+} (0.03)	0.04 (0.04)	0.04 (0.03)	0.01 (0.03)	**0.08 (0.03)**	0.05^{+} (0.03)
Random effects								
Intercept	**1.51 (0.21)**	**1.60 (0.33)**	**1.56 (0.23)**	**1.58 (0.34)**	**1.76 (0.25)**	**2.18 (0.45)**	**1.74 (0.25)**	**1.98 (0.41)**
Residual	**0.48 (0.04)**	**0.47 (0.05)**	**0.70 (0.06)**	**0.64 (0.07)**	**0.55 (0.05)**	**0.49 (0.05)**	**0.55 (0.05)**	**0.50 (0.06)**
Model statistics								
AIC	1125.3	649.5	1230.5	703.6	1180.3	672.9	1177.0	673.0
BIC	1131.0	653.6	1236.2	707.8	1186.1	677.1	1182.7	677.1
Pseudo R^2	0.02	0.05	0 0.08	0.08	0.01	0.03	0.02	0.04

Note. Coefficients in bold are significant at $p < 0.05$. $^{+}p < 0.10$. Week 0 is used as a comparison group, with all comparisons made against this time point. ITT Model is the intention to treat results; CC model is the complete case results

intervention group. Acceptance-attention showed significant increases from Week 0 to Week 5 ($p = 0.034$) and a marginal effect from Week 0 to Week 8 ($p = 0.051$). Decentering revealed marginal effects only at Week 2 ($p = 0.086$), but not Weeks 5 and 8 (p-values> 0.128), compared to Week 0. No significant increases were observed in self-compassion (p-values > 0.487). No significant improvements were observed in nonreactivity from Week 0 to Week 2 ($p = 0.190$). However, nonreactivity revealed marginal effects at Week 5 ($p = 0.061$) and Week 8 ($p = 0.085$) compared to Week 9. For the control group, no significant effects were found for acceptance-attention (p-values > 0.581). Similar to the results of the intention-to-treat analysis, a significant increase was observed for decentering in the control group from Week 0 to Week 8 ($p = 0.035$), but not from Week 0 to Week 2 and Week 0 to Week 5 (ps-values > 0.141). No significant effects were found in the control group for self-compassion (p-values > 0.358) and nonreactivity (p-values > 0.299).

As shown in Table 4, tests of linear effects (Research Question 2) revealed a marginal linear effect for acceptance-attention ($p = 0.058$). No significant linear effect was observed for decentering ($p = 0.223$), self-compassion ($p = 0.666$), and nonreactivity ($p = 0.095$). For the control group, no significant linear effects were observed for acceptance-attention, self-compassion, or nonreactivity (p-values > 0.466). Follow-up analyses revealed no significant quadratic term for any of the mindfulness mechanisms in the intervention group (p-values > 0.374). For the control group, no significant quadratic effect was found for acceptance-attention ($p = 0.100$) and self-compassion ($p = 0.265$). A significant quadratic effect was observed for decentering ($p < 0.001$), with a marginal quadratic effect for nonreactivity ($p = 0.082$).

Discussion

The goal of the study was to investigate how mindfulness mechanisms develop over the course of an 8-week app-based mindfulness intervention. Considering high attrition rates common to mindfulness intervention studies (Nam & Toneatto, 2016), the discussion will focus first on the ITT results, which had the most statistical power to detect effects. We will return to complete case analysis results as well as the issue of attrition later in the discussion.

First, the study examined at what point in the intervention mindfulness mechanisms begin to improve. Results indicated that improvements in the mechanisms of acceptance-attention, decentering, and nonreactivity were observed after two weeks of the intervention. Results for decentering were less reliable with only marginal effects observed. Our findings are in line with Baer et al. (2012) who also observed significant increases in general tendency to be mindful as well as the mindfulness mechanisms of observing (similar to attention monitoring) and nonreactivity by the second week of the intervention. Taken together, these findings suggest that acceptance-attention and nonreactivity may develop at a similar, and perhaps quick, rate, with some potential for decentering. Therefore, lack of improvements in these mindfulness mechanisms in the first few weeks of intervention might indicate that intervention is not working as expected. Tracking these early changes in mindfulness mechanisms might be especially important considering previous work suggesting that early changes in mindfulness mechanisms predict changes in well-being outcomes over the course of intervention (Baer et al., 2012). Mindfulness practitioners could be alert to observing whether initial expected changes are occurring, and could adjust training depending on early observed effects. This approach might even suggest the potential for some participants to require less intensive meditation protocols over time, or a shorter intervention period as mechanisms begin to show change. Given that time demands of the standard 8-week MBSR program is the primary reason to decline participation (Carmody & Baer, 2009), having an early detection system for when effects are observed and sustained could lead to more individualized and improved mindfulness training periods.

Next, we examined whether the effects of mindfulness training on mindfulness mechanisms compound over the course of the intervention. We found that the mindfulness mechanisms of acceptance-attention, decentering, and nonreactivity improved steadily over the course of the intervention, with only marginal improvements for decentering. This pattern of change is consistent with the finding of Lu et al. (2021) who found a strong overall linear effect on state mindfulness, with a lower rate of increase in mindfulness toward the end of the program. Similarly, Shoham et al. (2017) found that levels of mindfulness and decentering increased continuously in a linear trend over the course of a one month mindfulness training program. These findings provide evidence that there may be additional benefit to longer programs beyond early effects that might be observed within the first few weeks of training. It is possible that longer intervention allows participants to better grasp the principles of mindfulness and provide more opportunities to apply the principles learned to their everyday life, thus leading to further improvements in these mindfulness mechanisms.

Related to changes in specific mindfulness mechanisms, significant improvements in acceptance-attention and nonreactivity are in line with our predictions. Inherent in mindfulness training is an emphasis on continually bringing attention to the present moment and relating to this experience with a curious, open, accepting stance (Bishop et al., 2004; Kabat-Zinn, 1994). When unpleasant or difficult experiences

arise, students are encouraged to approach these experiences with a gentle curiosity and acceptance, rather than judging, suppressing, or pushing them away. The findings are also in line with previous studies that found that participating in mindfulness intervention leads to increases in both acceptance and attention monitoring (e.g., for review, see Chiesa et al., 2011; Yang et al., 2019). Similarly, we expected to observe significant improvements in nonreactivity observed in the intervention group. During mindfulness practice, practitioners are taught to allow thoughts and feelings come and go without reacting to them. This way, mindfulness practice teaches practitioners to cultivate healthier and adaptive ways of responding to stress and exploring present experiences nonreactively (Kabat-Zinn et al., 1992). Therefore, a systematic retraining of nonreactivity is considered a common process across mindfulness-based interventions (Chambers et al., 2009).

For decentering, only marginal effects were observed suggesting generally smaller effect sizes for changing decentering. Decentering has been described as a fundamental shift in perspective (Shapiro et al., 2006) and "an undoing of the automatic processes that control perception and cognition" (Deikman, 1982, p. 137). As such, it is possible that decentering is a complex skill that takes longer to cultivate, and a longer intervention (or assessment period than was done in this study) is needed to further advance the ability to decenter. We also agree with other authors who argue that the lack of significant decentering effects is a question of what type of meditation exercises are practiced during mindfulness training (Josefsson et al., 2014). Mikulas (2011) makes a strong argument for mindfulness being often conceptualized in terms of relaxation and or/stress reduction in the Western world. As such, mindfulness training emphasizes concentration-based meditation to help focus and calm the mind. In contrast, Buddhist meditation is more of an insight-oriented practice that emphasizes the cultivation of intuitive wisdom. Although many meditation practices involve some combination of concentration and insight meditation, given that concentration meditation is intended for stress reduction it is plausible that mindfulness training for stress reduction focuses more on concentration than insight. One important implication behind this distinction is that it has been proposed that insight-oriented practices activate the decentering mechanism to a greater extent (Josefsson et al., 2014). Therefore, it is possible that marginal decentering effects were due to a more concentration based focus of the intervention.

Contrary to our predictions, no significant changes in self-compassion were observed. We propose several possible reasons for the absence of self-compassion effects. First, although some evidence suggests that mindfulness-based interventions can increase self-compassion (for review, see Golden et al., 2020), these interventions devote relatively little time explicitly teaching self-compassion (Neff & Germer, 2013). According to Neff and Dahm (2015), self-compassion is taught implicitly as an attitudinal foundation of mindfulness practice; it is mainly conveyed in the way the instructor relates to the participants and in the way participants are encouraged to relate to their experiences. Therefore, conveying self-compassion implicitly might not be sufficient to elicit changes, and more targeted interventions are needed. Indeed, results from two systematic reviews suggest that although mindfulness-based interventions and compassion-based programs may both increase self-compassion, there is a trend towards compassion-added programs showing greater increases in self-compassion (Golden et al., 2020; Møller et al., 2019). Thus, the fact that mindfulness intervention in the present study did not include a specific session on self-compassion may partially explain no significant improvements in self-compassion. Another possible explanation to our finding is that self-compassion may take longer to develop. This suggestion is backed up by a study conducted by Pidgeon et al. (2014), who tested the effectiveness of a brief retreat-based mindfulness program targeting mindfulness and self-compassion for increasing resilience in human services professionals. Researchers found no significant differences between the intervention and control groups following the intervention. However, significant improvements in self-compassion were observed over time at one and four months. Authors explain their findings by suggesting that participants require time to practice and apply the skills learned before benefits can be observed. Given that mindfulness is a prerequisite to self-compassion (Neff & Germer, 2013), it is possible that self-compassion develops later, and a longer study is necessary to monitor these changes.

Limitations and Future Directions

The present study focused on acceptance, attention monitoring, decentering, self-compassion, and nonreactivity that are commonly proposed mechanisms of mindfulness training. However, other mechanisms that have not been extensively studied in the context of interventions have been proposed. For example, some of the proposed mechanisms include, but are not limited to, body awareness (Hölzel et al., 2011), acting with awareness (Baer et al., 2006), nonjudging (Baer et al., 2006), and values clarification (Shapiro et al., 2006). An important direction for future research is to better understand these less-studied mechanisms, their relation to well-being, and their responsiveness to mindfulness training.

As is common with most app-based mindfulness studies, the present study utilized an inactive control group (i.e., waitlist). Lack of active control groups has been previously discussed as a methodological limitation in meditation research (Davidson & Kaszniak, 2015), as it does not

allow to account for non-specific effects of intervention (e.g., confidence that intervention will be beneficial). However, it is often challenging to identify appropriate control groups for mind-body interventions delivered via smartphone apps, given the need for controls that are equivalent in structure and delivery (Flett et al., 2020). Given this limitation, future work will need to develop and test appropriate control groups as they attempt to replicate and extend these results. Because we did not have an active control, it is possible that the observed effects were influenced by a placebo effect. The use of the EMA design was intended to offset these potential effects (i.e., the expectancy of the benefit of mindfulness and its resulting influence on reporting would have to be present at each of the assessments across the days and weeks of assessments), as has been done in other areas (Mancia et al., 1995; Wilson et al., 2015). It is also unclear why there would not be placebo effects on all mechanisms. Yet, even if placebo effects were observed, these effects may be critical to maintaining engagement in meditation as one learns to process their environment in a different way. Future work may wish to examine if and when placebo effects emerge, and if and when sustained effects due to meditation replace the placebo effects.

The issue of attrition needs to be considered when interpreting the results. The participant attrition rate from pre- to post-intervention was 56.8.% (75/132), which is comparable with the dropout rates of other studies using digital mental health interventions (Doherty et al., 2012). High attrition rates have been a significant concern for digital interventions, as attrition undermines the potential of interventions to be effective. It is plausible that intensive longitudinal design of the study contributed to increased attrition. Although this powerful design allowed for granularity of measurement, it added significant burden perhaps leading to dropout. This might also explain a slightly higher attrition in the experimental group, given that participants in this group had a larger burden in terms of time commitment or study demands. Despite somewhat high attrition, we were still able to detect effects, with complete case analyses revealing a similar pattern of results compared to ITT. This demonstrates that the results were robust across both complete case and ITT analyses.

We did not have reliable assessments of engagement with the Headspace app. Although we included nightly assessment of engagement level and perceived benefits of meditation, most participants did not complete these assessments. These usage measurements were incumbent on the participant remembering and being available to complete the assessment each evening (i.e., a user-contingent design), and perhaps participants felt overburdened by assessment during the day. Future studies may wish to include less intensive measurements to assess compliance. Likewise, we were given weekly data from Headspace as to assess overall usage, but it is unclear how best to merge this data with EMA responses. For example, some participants engaged in multiple, short sessions each day versus doing meditation all in one session. Participants also engaged with the app at different times of the day. Each of these factors would make it difficult to merge with momentary assessment of mechanisms. Future research may wish to tie the timing of assessments to when meditation is completed each day.

Mindfulness mechanisms in the study were not tracked beyond 8 weeks. Therefore, the long-term effects of the intervention on mindfulness mechanisms are unknown. It is plausible that the beneficial effects of the intervention on the mechanisms continued compounding, or the effects began to taper off post-intervention. Future work should consider a longer study to test at what point in the intervention these benefits start to taper off to better understand how long the intervention should be until its beneficial effects diminish.

Although the present study investigated the effects of mindfulness intervention on mindfulness mechanisms, we did not assess how changes in mindfulness mechanisms relate to health outcomes over the course of the intervention. It has been previously argued that studies of mechanisms are more convincing when they establish that change in mindfulness mechanisms predicts changes in the outcomes of interest over the course of intervention (Baer et al., 2012; Kraemer et al., 2002). Baer et al. (2012) conducted the first study showing that extent of change in mindfulness mechanisms during the first three weeks predicted change in perceived stress over the course of the intervention. Such findings substantiate that early improvement in mindfulness mechanisms predict the extent of overall improvements in the outcome variables. More work assessing change in mindfulness mechanisms and the outcome variables over the course of treatment is needed, as this evidence facilitates more conclusive investigation of the mechanistic role of these mindfulness mechanisms in mindfulness interventions.

Finally, a prevalent concern in mindfulness research is the ability of individuals to accurately self-rate their levels of mindfulness (Grossman, 2011). Therefore, it is important for researchers to establish alternative objective measures that could be combined with self-reports assessment, such as behavioral measures (Levinson et al., 2014; Wong et al., 2018), qualitative assessments (e.g., interview data), or neuropsychological approaches (e.g., Grossman, 2008, 2011). Additionally, it is possible that prompting participants to think and report about mindfulness multiple times throughout the day could bias participants' responses. If this were the case, however, it should be noted that these effects would be present in both the control and experimental group and likely have no impact on the findings.

The study contributes to the growing literature on the effects of app-based interventions on mindfulness mechanisms. Although evidence demonstrates the benefits of

mindfulness mechanisms on well-being (e.g., Neff & Germer, 2013), most interventions teaching these skills are conducted in-person which can limit their dissemination (Kazdin, 2017). Findings of our study demonstrate that app-based mindfulness interventions can effectively improve the mindfulness mechanisms of acceptance-attention and nonreactivity. Therefore, smartphone apps represent an inexpensive, easily accessible, and effective alternative to teaching mindfulness mechanisms for people who cannot access traditional in-person training.

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Data Availability Data is not available on an open science repository as participants did not consent to data sharing, but is available on request pending appropriate data sharing agreements.

Declarations

Headspace® provided access to the app for free for all participants and provided app usage data. They had no other role in the study, including not being involved in study design, data collection, data analysis, nor dissemination.
All study procedures were approved by the University of California, Merced Institutional Review Board. All participants completed informed consent before completing any study materials.
AI was not used in the preparation of this manuscript.

Conflict of Interest The authors have no other conflicts of interest to declare.

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