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Differential effects of mindful breathing and loving kindness meditations: a component analysis study

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ABSTRACT

Objective: Mindful breathing meditation (MBM) and loving-kindness meditation (LKM) are common components of effective mindfulness-based interventions (MBIs). This study examined the differential effects of MBM and LKM on purported therapeutic process variables and mental health outcomes via component analysis.

Method: The research design was a randomized controlled trial with four conditions: MBM, LKM, combined (MBM + LKM), and a relaxation control. All conditions consisted of 10-min. audio-recorded guided meditations that were self-implemented over the course of two weeks. Participants were college undergraduates ($N = 52$).

Results: Findings indicated statistically significant and very large main effects of time, regardless of condition. Statistically significant time by condition interactions were only observed for one process variable (i.e., defusion) and one mental health outcome (i.e., depression). Follow-up descriptive evaluation of between-group effect sizes indicated patterns of favorable effects for MBM and LKM over the combined and relaxation control conditions. Treatment integrity and treatment acceptability data indicated very favorable social validity across conditions.

Discussion: We conclude that our findings make a modest yet value-added contribution to the MBI component analysis literature, suggesting differentiated performance among isolated MBM and LKM exercises compared to combined and control conditions. Yet further research is warranted to improve upon the limitations of this study.

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Mindfulness has commonly been described as "paying attention in a particular way: on purpose, in the present moment, and non-judgmentally" (Kabat-Zinn, 1994, p. 4). Interventions that seek to develop mindfulness to improve therapeutic outcomes have been called a variety of names, including *mindfulness therapies* and *mindfulness training*. For the purposes of the present study, this class of interventions will be referred to as *mindfulness-based interventions* (MBIs). MBIs consist of several different activities, meditations, and other components, hereafter referred to as *exercises*, such as breathing, body scans, yoga, and psychoeducation. The purpose of all mindfulness exercises used within

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MBIs is to help clients develop mindfulness process skills, which, in turn, facilitate changes in valued life outcomes (Renshaw et al., 2022).

MBIs have been studied for utility in treating a variety of mental health problems, such as depression, anxiety, and stress. A recent meta-analysis with adults found that MBIs were moderately effective in facilitating positive change in therapeutic outcomes (Khoury et al., 2015). An earlier meta-analysis found similar effects for reducing depression and anxiety (Hofmann et al., 2010). When compared to other active treatments (i.e., psychoeducation, supportive therapies, relaxation techniques, and imagery or suppression techniques), MBIs have also shown value added for improving therapeutic outcomes. Furthermore, MBIs have proved just as effective as cognitive behavioral therapy, behavior therapy, and pharmacological interventions in treating depression and anxiety with adults (Khoury et al., 2013). MBIs have also become popular for use with youth and in schools, and research with this population shows small to medium effect sizes across a variety of valued outcome domains, including psychological distress, subjective wellbeing, physical health, social behavior, and academic engagement (e.g., Carsley et al., 2018; Klingbeil et al., 2017; Phan et al., 2022).

Given the several systematic reviews and meta-analyses available with both adults and youth, it seems safe to conclude that MBIs are safe and effective treatments for addressing a variety of target problems experienced by both clinical and healthy populations. However, an empirical understanding of *how* MBIs actually work to improve outcomes is still underdeveloped (Renshaw, 2020). We suggest that this is likely because most MBIs were originally developed and tested as treatment packages prior to experimentally validating the theory and principles that guide these interventions. Thus, there is a conspicuous lack of evidence demonstrating causal connections between changes in mindfulness processes and changes in therapeutic outcomes (Hayes & Shenk, 2004; Fletcher & Hayes, 2005).

Additionally, there has yet to be a clear analysis of the individual contribution of component exercises packaged within the most common MBIs used in mental health contexts, such as mindfulness-based stress reduction (MBSR) and mindfulness-based cognitive therapy (MBCT). It has long been argued that researchers should validate and endorse empirically supported principles of behavior change that underlie multicomponent interventions, as practical problems arise from proliferating treatment packages without a clear understanding of the contribution of individual components and their value-added effects (Rosen & Davison, 2003). One of the biggest potential problems arising from the treatment package approach to MBIs, then, is the unintentional inclusion of ineffective or superfluous components or exercises, which may waste precious time and resources that could be better devoted to optimizing treatment reach and impact (Renshaw, 2020).

In addition to the critical issues described above, another limitation with the current scientific understanding of MBIs is that some treatment packages emphasize features or adjuncts to mindfulness that are more-or-less emphasized in other approaches. MBSR and MBCT, for example, emphasize the development of self-compassion and loving-kindness meditation (LKM) much more explicitly than do other MBIs, such as dialectical behavior therapy or acceptance and commitment therapy. This has resulted in practical confusion about the functions and value-added effects of exercises packaged within MBIs, as some seem designed to increase mindfulness per se (e.g., mindful breathing

meditation [MBM]), while others appear intended to supplement or support or extend the application of mindfulness processes (e.g., LKM).

Considering these critical issues, the current study aimed to further the scientific understanding of MBIs by using component analysis methodology to examine the differential effects of two specific exercises commonly packaged within MBIs of the MBSR and MBCT varieties: MBM and LKM. The implied hypothesis with most research investigating MBIs is that an increase in the process variable of mindfulness accounts for the desired changes in the outcome variable (e.g., reductions in depression, anxiety, or stress). Some research has explicitly demonstrated that increases in mindfulness co-occur with changes in the targeted process variable (Levin et al., 2012). Yet much research investigating MBIs fails to explicitly hypothesize process variables and, thus, forgoes attempts to validate MBI components by testing their impact on purported process variables (Hayes & Shenk, 2004). In the present study, we intentionally isolated MBM and LKM exercises to test their differential effects on purported therapeutic processes and common mental health outcomes.

Mindful breathing meditation is one of the most common and straightforward techniques for improving one's state and – when repeated over time – trait mindfulness. In short, MBM instructs clients to (a) bring attentive awareness to their experience of breathing, (b) notice and accept their distraction and mind wandering while engaged in this pursuit, and (c) gently return their attentive awareness to the breath – again and again – until the allotted time for the exercise runs its course. Given the nature of MBM, it is uncontroversial to posit that repeated practice of MBM should produce improvements in mindfulness proper. For the purposes of this study, we operationalized mindfulness processes via Hayes et al.'s (2012) three-pronged conception of (a) present moment awareness, (b) acceptance, and (c) defusion. We anticipated that MBM would positively impact measures associated with these processes.

Loving-kindness meditation, on the other hand, is a typical practice employed in MBIs to help individuals cultivate compassion and well-wishes toward oneself and others. A common LKM technique involves (a) focusing intention on one's desire for one's personal wellbeing, (b) wishing oneself a variety of desirable wellbeing outcomes (e.g., happiness, safety, good health, peace), (c) turning one's intentions toward the wellbeing of others, and, finally (d) wishing a series of others (e.g., a loved one, a friend, an acquaintance, a stranger, an enemy) the same variety of desirable wellbeing outcomes that were wished for oneself. In contrast to MBM, where the goal is to develop attentive awareness and acceptance of one's experiences (i.e., mindfulness), the goal of LKM is to intentionally generate positive emotions linked with compassionate desires (Hofmann et al., 2011). Although commonly included within MBIs, research suggests that LKM may also be parsed from MBIs and stand alone as an effective intervention for increasing positive emotions, decreasing negative emotions, and enhancing interpersonal relationships (Galante et al., 2014; Shonin et al., 2015). Given the nature of LKM, we anticipated that repeated practice of this exercise would differentially activate other therapeutic processes beyond mindfulness proper – specifically, social connectedness, positive affect, and self-compassion.

To our knowledge, only one study to date has directly compared MBM with LKM to investigate their differential effects on process variables. That study, by Feldman et al. (2010), explored the effect of these two exercises – plus another exercise commonly

used in cognitive behavioral therapy (CBT): progressive muscle relaxation (PMR) – on the mindfulness process of decentering. Results indicated that MBM produced stronger, desirable changes in decentering relative to the LKM and PMR, suggesting a targeted effect of MBM on its purported therapeutic process. That said, Feldman et al. (2010) did not investigate the differential effects of these three exercises on the other purported processes underlying LKM (i.e., compassion) and PMR (i.e., stress reduction). They also did not explore the differential impact of these isolated exercises on valued mental health outcomes (e.g., depression and anxiety). The purpose of the present study, then, was to extend this limited line of research by conducting a component analysis pilot study of MBM and LKM across both purported therapeutic processes and mental health outcomes.

The specific research questions guiding this study were as follows: (1) Are MBM, LKM, and combined (MBM + LKM) exercises more effective than a relaxation control condition for improving therapeutic processes and mental health outcomes? (2) Do MBM, LKM, and combined (MBM + LKM) conditions have differential effects across therapeutic processes and mental health outcomes?

Given these research questions, we predicted that MBM, LKM, and combined (MBM + LKM) conditions would each be more effective than a relaxation control for improving process variables and mental health outcomes. We also predicted that differentiated effects across conditions would be observed for process variables, but not for outcomes, as previous research shows these can be equivalent. Specifically, we expected LKM to have greater effects on some processes (i.e., social connectedness, positive and negative affect, and compassion for self and others) whereas MBM would have greater effects on other processes (i.e., being present, cognitive fusion, and experiential avoidance), and we anticipated the combined condition (MBM + LKM) would have the greatest overall effects across all processes.

Method

Participants and sampling

Participants were undergraduate college students enrolled in psychology courses at a large public university in the southeastern region of the United States. After receiving Institutional Review Board approval, the study was advertised on an online recruitment system hosted by the Psychology Department at the university. Participants were compensated through partial course credit in their psychology courses. Study inclusion criteria included being at least 18 years of age and enrolled in a current psychology course. Participants were also required to own a smart phone or computer with internet access, which needed the capability of setting a daily alarm or reminder and to have headphones or ear buds that could connect with this electronic device. Anticipating a medium effect size across conditions and the use of ANOVA for primary analyses, an a priori power analysis indicated 12 participants should be recruited per each of the four conditions: MBM, LKM, combined (MBM + LKM), and relaxation control.

The original sample was collected using a random sampling procedure in which participants were assigned to an experimental condition upon entering the lab. This was accomplished by assigning each participant a number (1 to 4), generated by a random

number generator, representing an intervention condition. Conditions assignments were as follows: MBM $n = 13$, LKM $n = 14$, combined (MBM + LKM) $n = 13$, relaxation control $n = 14$. Given an unanticipated materials malfunction related to the combined condition, the original randomly-selected combined sample was dropped from the study and a secondary non-random combined sample ($n = 11$) was obtained and given the corrected version of the materials (see the Results section for further description of this procedure). Overall, 52/56 (93%) of recruited participants completed the study. The total sample was predominantly female (75%), identified mostly as white (78.8%), ranged in age from 18–22 years, and the majority were undergraduate freshman (59.6%). Most of the sample reported having either no previous meditation experience (46.2%) or very little meditation experience (38.5%).

Experimental design and procedure

At Time 1, participants completed informed consent and a series of measures, including the demographics questionnaire plus the baseline assessments of therapeutic processes and mental health outcomes (described below). Following completion of the initial surveys, participants received an intervention packet, which included the intervention directions, a treatment integrity calendar log, directions describing how to access the audio-recording associated with their assigned condition, a brief action planning/coping worksheet for promoting adherence to the self-implementation schedule, and information about returning in two weeks to complete post-test measures.

Participants were exposed to the same standardized intervention directions regardless of condition assignment, and a brief action planning primer was provided based on the health action process approach (HAPA; Sanetti et al., 2013). This primer included a discussion of action planning: how to accommodate this new behavior change into one's routine by (a) placing the treatment integrity calendar log on the refrigerator where it will be seen, (b) setting an alarm on one's phone to complete the exercise once per day (10 min total), and (c) ensuring easy access to the audio-recording for the exercise. It also included coping planning: anticipating days that completing the intervention might be challenging and coming up with a back-up plan for when one misses the usual intervention time. Participants completed preparation steps in the initial pre-test session, which included selecting a day/time for self-implementation and then setting an alarm on their electronic device to remind themselves to complete their assigned exercise. Next, participants downloaded the audio-recordings associated with their assigned condition on their personal electronic device and ensured they could access and stream their condition's audio recording using their personal headphones.

In addition to the HAPA planning, other steps were taken by the experimenter to incentivize treatment integrity. Specifically, participants were informed that to receive full credit for participating in the study they would need to complete the exercise once per day (10 min total) for a specified number of days. However, participants were told that the exact number of days required for full credit would remain a mystery until they returned at Time 2. Thus, they were encouraged to complete the audio recording and associated materials daily, with the proviso that 100% compliance would not ultimately be required for participation credit. When

participants returned at Time 2, they were then informed that they would receive full credit for participation in the study, regardless of how many days they actually completed the exercise. This slight deception procedure was intended to encourage high participation during the interim self-implementation period (between Time 1 and Time 2), coupled with honest reporting about treatment fidelity at Time 2. This motivational procedure was approved by the IRB as appropriate given that it was unlikely to cause undue stress, participants could voluntarily choose if they agreed to the ambiguity of the compensation condition (i.e., they could choose to forgo participation or discontinue the study at any time), and because it ultimately favored compensation (i.e., in the end, participants were compensated regardless of their compliance with the study requirements).

At Time 2, which occurred two weeks after pre-test/baseline (Time 1), participants returned to complete the post-test assessment (i.e., therapeutic process and mental health outcome measures, plus the treatment acceptability rating) and turned in their self-monitored treatment integrity calendar log. Following completion of all post-test measures, participants were provided with links to access the audio-recordings associated with all conditions as well as links to additional MBM and LKM exercises freely available online. When participants handed in their post-test measures, the experimenter checked each measure for completion. After data collection, all measures were scored and then entered into SPSS version 23 for analysis.

Independent variables

There were four experimental conditions: (1) MBM, (2) LKM, (3) combined (MBM + LKM), and (4) relaxation control. Each condition consisted of a unique 10-minute audio recording with a spoken script. The recordings were formatted as similarly as possible. They were read by the same experimenter (i.e., the first author), were of equal length (i.e., 10 min), and were read with the same calm tone and steady pace of voice. The scripts each began with similar introductory statements and ended with similar closing statements. The experimental manipulation across conditions consisted of the differences in the spoken script located between these standardized starting/stopping statements. The 10-minute MBM script was adapted from scripts used in MBSR and MBCT (e.g., Segal et al., 2002; Stahl, & Goldstein, 2010). The 10-minute LKM script was based on strategies suggested in various LKM resources (e.g., Kabat-Zinn, 1994; Kornfield, 2002). The combined (MBM + LKM) script included 5 min of mindful breathing based on an abbreviated version of the MBM script, and 5 min of loving-kindness from an abbreviated version of the LKM script. Participants assigned to the relaxation control condition also received a 10-minute audio recording; however, this script instructed them to enjoy "quiet time" in which they did not engage in any activity other than listening to the recording and relaxing. The control condition was structured and formatted similarly to the treatment conditions but was intended to be devoid of active therapeutic ingredients. Participants were instructed to complete their assigned exercise once per day (10 min total), regardless of condition. We wish to emphasize that the relaxation condition was not intended as PMR, which Feldman et al. (2010) tested as an alternative treatment relative to MBM and LKM, but rather as an active control comparison.

Dependent variables

Given the short duration of the self-implementation intervention period (i.e., 2 weeks), the prompts and instructions for all self-report measures were standardized to reflect moods, attitudes, and behavior experienced within the past week.

Therapeutic process variables

Defusion. The mindfulness process of defusion was measured using the short (8-item) version of the Avoidance and Fusion Questionnaire for Youth (AFQ; Greco et al., 2008). Although this measure is primarily validated and used with youth (e.g., Renshaw, 2017), it has also been validated for use with adults, specifically undergraduate college students (Renshaw, 2018). This 8-item measure is on a five-point scale from 0 = *Not at all true* to 4 = *Very true*. In a sample of undergraduate college students, the 8-item version had stronger internal consistency and better data-model fit than the original 17-item measure (Renshaw, 2018).

Present moment awareness. Another mindfulness process – present moment awareness – was measured using the short 5-item version of the Mindful Attention Awareness Scale (MAAS, Osman et al., 2016). This is an abbreviated version of the original 15-item MAAS for measuring trait mindfulness (Brown & Ryan, 2003). All items are arranged on a six-point scale from 1 = *Almost always* to 6 = *Almost never*. Preliminary analyses testing the short version of the MAAS indicate strong internal consistency and adequate structural validity (Osman et al., 2016).

Social connectedness. Social connectedness was measured as a target process for LKM using the inverse score derived from the UCLA Loneliness Scale – Revised (UCLA; Russell et al., 1980). The 20-item measure is on a four-point Likert-type scale from 0 = *I never feel this way* to 3 = *I often feel this way*. The measure has strong internal consistency and test-retest reliability over a one-year period (Russell, 1996). Concurrent and convergent validity with other measures of loneliness has also been established and the measure has good construct validity (Russell, 1996).

Affect. Other target processes for LKM included positive and negative affect, which were measured by the Positive and Negative Affect Schedule (PANAS; Watson et al., 1988). The PANAS is a 20-item measure on a five-point Likert scale. Respondents were asked to indicate the extent to which they have experienced different desirable and undesirable feelings. Response options scale from 1 = *Very slightly or not at all* to 5 = *Extremely*. The PANAS has a history of good test-retest reliability and construct validity (e.g., Crawford et al., 2004).

Self-compassion. Self-compassion was measured by the Self-Compassion Scale – Short Form (SCSSF; Raes et al., 2011), which is based on the full-version scale (Neff, 2003). The SCSSF is a 12-item measure on a five-point Likert-type scale, where respondents rate the frequency of how they behave towards themselves in difficult times on a scale from 1 = *Almost never* to 5 = *Almost always*. When validated in English, internal consistency for the total score was high and correlation with the full-length score was excellent; data–model fit was also adequate (Raes et al., 2011; Neff, 2003).

Compassion for others. Compassion for others was measured using the Santa Clara Brief Compassion Scale (SCBCS; Hwang et al., 2008), which is based on the full-length Compassionate Love for Humanity Scale (Sprecher & Fehr, 2005). The SCBCS is a 5-

item measure that uses a seven-point Likert scale from 1 = *Not at all true of me* to 7 = *Very true of me*. The correlation between the full and brief versions is strong and the internal consistency is high. The original scale is based on a single factor that is related to but distinct from qualities such as empathy and hope (Sprecher & Fehr, 2005).

Mental health outcomes

Anxiety. General anxiety was assessed through a screening tool for Generalized Anxiety Disorder (GAD; Spitzer et al., 2006). This 7-item measure is on a frequency-based Likert-type scale ranging from 0 = *Not at all* to 3 = *Nearly every day*. The measure has been psychometrically validated. Internal consistency of the screener is excellent and test–retest reliability is good. Criterion, construct, factorial, and procedural validity are also established for the GAD (Spitzer et al., 2006).

Depression. Depression was assessed using the Center for Epidemiological Studies Depression Scale (CES-D; Radloff, 1977). This 20-item measure is arranged on a frequency-based Likert-type scale ranging from 0 = *Rarely or none of the time (less than 1 d)* to 3 = *Most or all of the time (5–7 days)*. Internal consistency for the CES-D is very high; test-retest reliability of the measure is adequate; and construct and convergent-concurrent validity are also established for the measure (Radloff, 1977).

Life satisfaction. Global subjective wellbeing was measured using the Satisfaction with Life Scale (SWL; Diener et al., 1985). The 5-item measure is on a seven-point Likert-type scale from 1 = *Strongly disagree* to 7 = *Strongly agree*. The measure has strong internal consistency and test-retest reliability (Diener et al., 1985). Concurrent-convergent validity of the SWL with other wellbeing measures has also been established (Diener et al., 1985; Pavot & Diener, 1993).

Social validity indicators

Treatment integrity. Treatment integrity was measured using a self-monitoring calendar log completed by each participant. This log was provided at Time 1 and completed each day until it was turned in at Time 2. The treatment integrity log asked participants to mark daily completion or non-completion of their assigned 10-minute exercise. Completion entailed starting and finishing the full 10-minute exercise that day. Non-completion entailed not starting and/or not finishing the assigned exercise that day; partial or attempted completion was therefore classified as non-completion, as it did not fully adhere to implementation instructions. Participants self-monitored their treatment integrity using the calendar log for 14 days.

Treatment acceptability. Treatment acceptability was assessed using an adapted 8-item version of the Abbreviated Acceptance Rating Profile (AARP), which has been shown to have strong internal consistency and a unitary factor structure (Tarnowski & Simonian, 1992). AARP items are on a six-point Likert scale from 1 = *Strongly disagree* to 6 = *Strongly agree*. All items were adapted to reflect the nature of the interventions employed in this study. For example, the original AARP item "I liked this treatment" was replaced with "I enjoyed doing this exercise," whereas the original item "Overall the treatment would help [this person]" was replaced with "Overall, this exercise was helpful for decreasing my distress and improving my wellbeing."

Results

Preliminary analyses

Across all responses from all participants, there were only 11 missing responses for individual rating scale items. These responses were from eight separate participants, with the number of responses missing per participant ranging from one to three. Given there was no more than one missing response per process or outcome measure and that all rating scales had at least 5 items for each scale, missing data was handled via mean replacement (i.e., replacing the few missing scores with the participant's average item score for the other items within the same scale/measure).

When preliminary analyses were conducted on participant responses obtained during the original phase of data collection, we noticed that there was no change in the descriptive statistics representing the combined (MBM + LKM) condition's scores from Time 1 to Time 2, while there was substantial change in descriptive statistics across time for all other conditions. This unexpected finding prompted us to recheck the integrity of the technical materials used in all conditions, which led to the discovery that the audio file for the combined condition had been compromised with background noise that may have detracted from the intended effects of the intervention – and which prevented the basic parameters of the recordings from being considered equivalent on all aspects beyond the content of instruction. Given this methodological confound, the original randomly-selected 13 participants who completed the study using this compromised recording were dropped from the analysis, and a secondary non-random sample of 11 participants was recruited to participate in a new version of the combined (MBM + LKM) condition with a re-recorded audio file. Descriptive statistics for the final sample, across all conditions and measures, are presented in Tables 1 and 2.

Prior to embarking on the primary analyses, normality was assessed for all process and outcome variables using skewness and kurtosis of scores at Time 1, and all scores were deemed to be relatively normally distributed. Homogeneity of variance between groups was tested using Levene's test and results indicated no violations. Furthermore, the assumption of independence of observations was met through the logistics of the experimental design. Internal consistency within measures and correlations between measures were calculated to ensure that expected correlations and adequate reliabilities were observed. Cronbach's alpha coefficients (representing internal consistency reliability) for all measures at both time points were > .80, and Pearson's bivariate correlations between process and outcome variables at both times points showed theoretically consistent directions and magnitudes – increasing confidence that measures were collected and scored correctly.

To determine if there were any meaningful differences between conditions across process and outcome variables at baseline, a series of ANOVA investigating the main effects of condition for all Time 1 measures were conducted. Results indicated that there were not-significant differences between conditions for most process and outcome measures at Time 1, with the exception of self-compassion ($F(3,48) = 5.04$, $p = .004$, $\eta^2 = .386$). Post hoc comparisons with a Bonferroni adjustment revealed that the secondary sampling of the combined (MBM + LKM) condition showed a significantly higher mean self-compassion score at Time 1 compared to other conditions. To ensure that this was not due to a data entry error, we re-confirmed that the raw data had been

Table 1. Descriptive statistics for therapeutic process variables by condition.

Variable	Condition	Time 1 *M*	Time 1 *SD*	Time 2 *M*	Time 2 *SD*	*n*
Defusion	MBM	18.38	7.335	12.77	4.11	13
	LKM	16.07	4.83	11.42	2.53	14
	Combined	14.36	7.99	13.64	7.90	11
	Relaxation	16.07	3.34	14.29	4.27	14
	Total	16.28	6.00	13.00	4.87	52
Present moment awareness	MBM	16.08	5.39	22.15	5.38	13
	LKM	17.93	5.31	23.21	3.98	14
	Combined	17.81	5.51	23.72	6.18	11
	Relaxation	16.14	6.05	19.50	6.52	14
	Total	16.96	5.48	22.06	5.65	52
Social connectedness	MBM	14.85	14.78	8.54	8.24	13
	LKM	11.71	13.00	6.93	7.81	14
	Combined	12.81	14.42	10.91	18.25	11
	Relaxation	14.07	7.38	8.14	7.3	14
	Total	13.37	12.27	8.50	10.60	52
Positive affect	MBM	29.08	7.27	37.77	7.32	13
	LKM	29.14	9.52	36.71	6.70	14
	Combined	29.63	8.24	36.09	12.84	11
	Relaxation	31.07	7.84	31.79	9.51	14
	Total	29.75	8.08	35.52	9.22	52
Negative affect	MBM	28.31	6.81	19.31	6.22	13
	LKM	22.50	5.71	15.93	2.67	14
	Combined	22.00	8.31	16.63	9.62	11
	Relaxation	24.21	6.72	18.86	6.76	14
	Total	24.31	7.10	17.71	6.54	52
Self-compassion	MBM	31.38	7.10	42.93	6.58	13
	LKM	31.43	9.47	42.86	5.35	14
	Combined	41.55	8.27	47.27	11.18	11
	Relaxation	30.64	6.44	39.29	6.43	14
	Total	33.35	8.79	42.85	7.76	52
Compassion for others	MBM	28.92	4.07	28.85	3.34	13
	LKM	27.07	7.03	27.07	7.03	14
	Combined	26.91	8.20	28.82	7.51	11
	Relaxation	25.79	6.59	26.36	6.42	14
	Total	27.27	6.18	27.69	6.16	52

Table 2. Descriptive statistics for mental health outcomes by condition.

Outcome	Condition	Time 1 *M*	Time 1 *SD*	Time 2 *M*	Time 2 *SD*	*N*
Anxiety	MBM	12.38		3.92	2.14	13
	LKM	10.64	6.13	1.43	0.65	14
	Combined	10.27	8.15	4.55	6.33	11
	Relaxation	9.57	5.42	5.64	5.01	14
	Total	10.71	5.88	3.85	4.25	52
Depression	MBM	22.77	11.32	11.31	9.23	13
	LKM	18.79	10.63	6.14	4.18	14
	Combined	16.45	13.96	12.54	17.3	11
	Relaxation	17.79	8.35	12.87	7.92	14
	Total	19.02	10.97	10.58	10.35	52
Life satisfaction	MBM	22.69	7.13	28.08	5.12	13
	LKM	22.14	7.32	26.36	5.99	14
	Combined	25.10	8.44	26.00	10.42	11
	Relaxation	24.71	6.24	26.29	5.48	14
	Total	23.60	7.15	26.69	6.71	52

entered into SPSS correctly and then re-calculated the composite score for this variable, none of which changed the results. Thus, to accommodate this baseline difference for self-compassion, an ANCOVA using Time 1 scores on this variable as the covariate was used to analyze the self-compassion process variable in the primary analyses phase.

Primary analyses

For the primary analyses, to determine if active treatment groups were more effective than a relaxation control at creating therapeutic change in process and outcome variables, and to determine differential effects of each condition on process and outcome variables, a repeated measures ANOVA was run for each process and outcome variable, except for self-compassion. As mentioned earlier, self-compassion was run as an ANCOVA due to significant between-condition differences observed at Time 1. Greenhouse Giesser corrections were used for any analyses in which Mauchly's sphericity test was violated. To answer the research questions, we were primarily interested in the main effects of time as well as the time by condition interaction effects across processes and outcomes.

One-way repeated measures ANOVA indicated statically significant main effects of time for the therapeutic process variables of defusion ($F(1, 48) = 24.35$, $p < .001$, $\eta^2 = .337$), present moment awareness ($F(1, 48) = 47.37$, $p < .001$, $\eta^2 = .497$), social connectedness ($F(1, 48) = 13.89$, $p = .001$, $\eta^2 = .224$), positive affect ($F(1, 48) = 22.12$, $p < .001$, $\eta^2 = .315$), and negative affect ($F(1, 48) = 58.77$, $p < .001$, $\eta^2 = .550$). A one-way repeated measures ANCOVA (accounting for baseline scores as a covariate) also indicated a statistically significant main effect of time on self-compassion ($F(1, 48) = 55.28$, $p < .001$, $\eta^2 = .535$). The only process variable indicating a not-significant main effect of time was compassion for others ($F(1, 48) = .67$, $p = .417$, $\eta^2 = .014$). Effect sizes (see η^2 values reported earlier) for all statistically significant main effects of time were very large, and consideration of descriptive statistics (see Table 1) indicated that all changes from Time 1 to Time 2 were in the therapeutic direction – suggesting desirable improvements across most process variables.

The time by condition interaction effect for defusion was significant and characterized by a large effect size ($F(3, 48) = 3.09$, $p = .036$, $\eta^2 = .162$). However, time by condition interaction effects were not-significant for the other process variables of present moment awareness ($F(3, 48) = .72$, $p = .544$, $\eta^2 = .043$), social connectedness ($F(3, 48) = .56$, $p = .645$, $\eta^2 = .034$), positive affect ($F(3, 48) = 2.17$, $p = .104$, $\eta^2 = .119$), negative affect ($F(3, 48) = 1.00$, $p = .402$, $\eta^2 = .059$), self-compassion ($F(3, 48) = .938$, $p = .430$, $\eta^2 = .056$), and compassion for others ($F(3, 48) = .68$, $p = .572$, $\eta^2 = .040$). Interestingly, these not-significant interactions were all characterized by small to moderate effect sizes (see η^2 values reported earlier), suggesting potentially meaningful effects were not detected as statistically significant due to underpowered analyses.

One-way repeated measures ANOVA indicated statically significant main effects of time for the mental health outcomes of anxiety ($F(1, 48) = 70.00$, $p < .001$, $\eta^2 = .593$), depression ($F(1, 48) = 38.77$, $p < .001$, $\eta^2 = .447$), and life satisfaction ($F(1, 48) = 17.03$, $p < .001$, $\eta^2 = .262$). All effect sizes associated with these main effects were very large (see η^2 values reported earlier). The time by condition interaction effect for depression was at the threshold for statistical significance ($F(3, 48) = 2.79$, $p = .05$, $\eta^2 = .148$),

whereas the time by condition interaction effects were not-significant for anxiety ($F(3, 48) = 2.38$, $p = .081$, $\eta^2 = .130$) and life satisfaction ($F(3, 48) = 2.06$, $p = .117$, $\eta^2 = .114$). Again, however, it is noteworthy that the effect sizes for these not-significant interactions were moderate (see η^2 values reported earlier), suggesting potentially meaningful effects may have been undetected due to underpowered analyses.

Post-Hoc analyses

Given the meaningful effect sizes observed across interaction analyses as well as the presence of a couple statistically significant interactions, we believed post-hoc comparisons might contribute to a fuller understanding of between-condition effects. However, given the research design was only adequately powered to detect statistically significant main and interaction effects (assuming moderate effect sizes), we chose to forgo post-hoc comparisons using statistical significance testing. Instead, we focused on calculating descriptive statistics that could be transformed into standardized mean differences (Hedges' *g*), which could be interpreted and qualified with confidence intervals. Results for these post-hoc between-group comparisons for therapeutic process variables are presented in Table 3, whereas findings for post-hoc comparisons for mental health outcomes are presented in Table 4. We defer further description and interpretation of these follow-up comparisons to the Discussion section (see below).

Social validity indicators

Results for the treatment integrity and treatment acceptability ratings were quite favorable. For treatment integrity, the average number of days participants reported completing the exercise was 13.80/14 (99%), indicating very high levels of integrity across conditions. For treatment acceptability, the average item rating was 4.87/6, which is between the ratings of 4 = *agree somewhat* and 5 = *agree* on the Likert-type response scale, and which can be reasonably interpreted as good perceptions of acceptability across conditions. It is noteworthy that no meaningful differences were observed for integrity or acceptability scores as a function of condition.

Discussion

Mindfulness-based interventions have been shown to improve a variety of therapeutic processes and mental health outcomes, yet there is not yet a clear scientific understanding of how MBIs produce their effects (Renshaw, 2020). One barrier to improving understanding is the current structure of common MBI, such as MBSR and MBCT, which are delivered as treatment packages that prevent component analysis. In the present study, we intentionally isolated two component exercises that are commonly included within MBSR and MBCT – the MBM and LKM exercises – to test their differential effects on purported therapeutic processes and mental health outcomes. Effects of these component exercises were compared to a combined condition (MBM + LKM) as well as a relaxation control. We anticipated that results from our study would help answer the calls from other researchers (e.g., Hayes & Shenk, 2004; Fletcher & Hayes, 2005) regarding the need to elucidate connections among implementation of

Table 3. Condition comparisons for therapeutic processes.

Variable	Comparison	*M* diff.	Hedges' *g*	Effect size	Lower 95% CI	Upper 95% CI
Defusion	MBM v LKM	−0.98	−0.171	Negligible	−2.269	1.928
	MBM v Combined	−4.89	−.**851**	Large	−3.071	1.369
	MBM v Relaxation	−3.83	−.**781**	Medium	−2.575	1.014
	LKM v **Combined**	−3.91	−.**838**	Large	−2.607	.930
	LKM v Relaxation	−2.85	−.**747**	Medium	−2.119	.624
	Combined v **Relaxation**	1.06	.**305**	Small	−1.013	1.623
Present moment awareness	MBM v LKM	0.79	.124	Negligible	−2.206	2.434
	MBM v Combined	2.72	.031	Negligible	−2.080	2.142
	MBM v Relaxation	0.17	.**472**	Small	−1.635	2.579
	LKM v Combined	−0.62	−.117	Negligible	−2.131	1.898
	LKM v Relaxation	1.93	.**343**	Small	−1.679	2.365
	Combined v. Relaxation	2.55	.**568**	Medium	−1.135	2.270
Social connectedness	MBM v LKM	−1.52	−0.134	Negligible	−4.29	4.03
	MBM v Combined	−4.40	−.**432**	Small	−4.36	3.50
	MBM v Relaxation	−.380	−.038	Negligible	−3.65	3.57
	LKM v Combined	−2.88	−.**325**	Small	−3.68	3.04
	LKM v Relaxation	1.14	.131	Negligible	−3.00	3.26
	Combined v **Relaxation**	4.02	.**608**	Medium	−1.90	3.12
Positive affect	MBM v. LKM	1.12	.119	Negligible	−3.330	3.568
	MBM v. Combined	2.24	.**236**	Small	−3.437	3.909
	MBM v. Relaxation	7.89	.**839**	Large	−2.642	4.319
	LKM v. Combined	1.12	.126	Negligible	−3.249	3.501
	LKM v. Relaxation	6.86	.**763**	Medium	−2.469	3.995
	Combined v Relaxation	5.74	.**638**	Medium	−2.775	4.050
Negative affect	**MBM** v LKM	−2.43	−.**339**	Small	−2.964	2.286
	MBM v Combined	−3.27	−.**462**	Small	−3.196	2.272
	MBM v Relaxation	−3.64	−.**552**	Medium	−2.963	1.859
	LKM v Combined	−0.84	−.138	Negligible	−2.446	2.170
	LKM v Relaxation	−1.21	−.**213**	Small	−2.256	1.830
	Combined v. Relaxation	−0.37	−.070	Negligible	−2.085	1.945
Self-compassion	MBM v LKM	0.11	.013	Negligible	−3.154	3.179
	MBM v Combined	5.82	.**570**	Medium	−3.374	4.514
	MBM v Relaxation	2.90	.**318**	Small	−3.022	3.657
	LKM v Combined	5.71	.**602**	Medium	−2.993	4.197
	LKM v Relaxation	2.79	.**330**	Small	−2.714	3.374
	Combined v **Relaxation**	−2.92	−.**293**	Small	−4.068	3.482
Compassion for others	MBM v LKM	.353	.085	Negligible	−1.431	1.601
	MBM v **Combined**	−1.987	−.**460**	Small	−2.128	1.207
	MBM v Relaxation	−.647	−.168	Negligible	−1.576	1.240
	LKM v **Combined**	−2.34	−.**462**	Small	−2.383	1.459
	LKM v **Relaxation**	−1.00	−.**217**	Small	−1.872	1.437
	Combined v Relaxation	1.34	.**279**	Small	−1.543	2.101

Note. Bolded effect sizes indicate a meaningful effect size. Bolded conditions indicate the group demonstrating a therapeutic advantage.

Table 4. Condition comparisons for mental health outcomes.

Outcome	Comparison	*M* diff.	Hedges' *g*	Effect size	Lower 95% CI	Upper 95% CI
Anxiety	MBM v LKM	.75	.141	Negligible	−1.811	2.093
	MBM v Combined	−2.74	**−.425**	Small	−2.914	2.063
	MBM v Relaxation	−4.53	**−.961**	Large	−.2685	0.762
	LKM v Combined	−3.49	**−.482**	Small	−3.226	2.262
	LKM v Relaxation	−5.28	**−.925**	Large	−2.977	1.126
	Combined v Relaxation	−1.79	**−.265**	Small	−2.823	2.293
Depression	MBM v LKM	1.18	.097	Negligible	−4.336	4.531
	MBM v Combined	−7.55	**−.808**	Large	−4.809	3.192
	MBM v Relaxation	−6.46	**−.621**	Medium	−4.428	3.186
	LKM v Combined	−8.74	**−1.045**	Large	−4.508	2.418
	LKM v Relaxation	−7.64	**−.817**	Large	−4.179	2.545
	Combined v **Relaxation**	8.91	**.297**	Small	−2.138	2.732
Life satisfaction	MBM v LKM	1.17	.181	Negligible	−2.178	2.541
	MBM v Combined	4.47	**.746**	Medium	−1.568	3.061
	MBM v Relaxation	3.81	**.725**	Medium	01.198	2.647
	LKM v Combined	3.30	**.547**	Medium	−1.740	2.834
	LKM v Relaxation	2.64	**.495**	Small	−1.422	2.412
	Combined v Relaxation	−0.66	−.144	Negligible	−1.886	1.598

Note. Bolded effect sizes indicate a meaningful effect size. Bolded conditions indicate the group demonstrating a therapeutic advantage.

mindfulness-related techniques, changes in mindfulness-related processes, and improvements in mental health outcomes.

We predicted that MBM, LKM, and combined (MBM + LKM) conditions would each be more effective than the relaxation control for changing purported therapeutic process variables and for improving mental health outcomes. Moreover, we expected that LKM would have greater effects on its targeted process variables, whereas the MBM condition would have greater effects on its purported therapeutic processes, while the combined (MBM + LKM) condition would demonstrate the greatest overall effects across therapeutic processes and mental health outcomes. Our expectations regarding targeted effects for conditions were grounded in previous literature on this topic (see Levin et al., 2012). Interestingly, our results failed to support most of our predictions, at least when considered at the conventional level of interpretation by statistical significance. That said, consideration of descriptive statistics and effect sizes indicated noteworthy patterns in the results that may inform future research in this area.

Interpretations of results

Firstly, it was striking that statistically significant and large main effects of time were observed for most therapeutic process variables (*sans* compassion for others) and across each of the three mental health outcomes, yet statistically significant time by condition interaction effects were only observed for one process variable (i.e., defusion) and one mental health outcome (i.e., depression). Taken together, we suggest this pattern of statistical significance among main and interaction effects suggests one of two possibilities. One, it is plausible that all conditions – MBM, LKM, combined (MBM + LKM), and relaxation control – had generalizable and desirable treatment effects across a variety of

process variables and mental health outcomes. This possibility entails that the control condition had a placebo effect or otherwise inadvertently functioned as a treatment condition. Previous research has demonstrated that simple, planned relaxation breaks alone can have such effects (e.g., Kim et al., 2017). Furthermore, this interpretation suggests that contrary to our expectations, which were based on previous meta-analytic findings (see Levin et al., 2012), MBM and LKM have indiscriminately positive effects across a variety of therapeutic processes – and, thus, both components may improve mental health through multiple and similar pathways. If this potential interpretation is accepted, it would suggest that although MBM and LKM *techniques* are easily differentiated, their *mechanisms of change* might be overlapping. Alternatively, a more complex mediating relationship, which was not examined in our study, may better explain the relationship among mindfulness and self-compassion process variables (e.g., Keng et al., 2012). These explanations may account for why MBM and LKM both positively affect the same mental health outcomes (see Hofmann et al., 2010; Hofmann et al., 2011).

Another plausible interpretation of our results is that the experimental conditions failed to produce the intended effects and, instead, the results are attributable to an unknown confounding variable or maturation effect. Given the research design employed, we cannot identify a potential confounding variable that could be reasonably suspected to have produced such effects. However, the pattern of lacking interaction effects suggest we admit this possibility. To evaluate the weight of evidence supporting these two interpretations, we recommend future research include a passive (or measurement-only) control condition in the experimental design. If the same pattern of statistically significant results was produced with a measurement-only condition built in, this may provide evidence favoring a confounding-variable or maturation-effect explanation. However, alternatively, it may also demonstrate a *mere measurement effect*, wherein assessment serves a priming function to change one's behavior *sans* active intervention (see Morwitz & Fitzsimmons, 2004). Furthermore, if the pattern of results with a passive (measurement-only) control condition differed, with more interactions favoring the active conditions, this would suggest actual therapeutic and/or placebo effects (assuming the relaxation control performed similarly as in our study). If this proved to be the case, further research would still be needed to clarify the nature of these differentiated effects, even if they were understood to be placebo effects (see Geers & Miller, 2014). Because we do not have such evidence at hand in this study, we conclude conservatively that both interpretations of our results are plausible.

When we consider the descriptive statistics and effect sizes, however, a more nuanced interpretation of the results emerges. Given the meaningful effect sizes observed across interaction analyses (see Results section) as well as the presence of a couple statistically significant interactions, we decided to conduct post-hoc comparisons exploring between-condition effects across processes and outcomes at the level of descriptive statistics and effect sizes. Findings from these follow-up comparisons revealed interesting patterns of between-condition effects that suggested more differentiated performance among conditions compared to the interpretations at the level of statistical significance. Prior to describing these patterns, however, we should emphasize that the 95% confidence intervals associated with most effect sizes were wide-ranging and imprecise, with almost all intervals containing "0" as a plausible value (see Tables 3 and 4). Thus, the patterns of evidence described below should be considered tentative and suggestive, not decisive.

For the effects of condition on therapeutic process variables (see Table 3), the resulting effect sizes suggest a few noteworthy patterns. First, the standardized mean difference between MBM and LKM conditions were negligible across most process variables, except for negative affect, which slightly favored MBM. This pattern failed to support our predictions, as we anticipated, based on previous review of the literature (see Levin et al., 2012), greater effects on those processes that were more theoretically aligned with each condition (e.g., MBM with present moment awareness; LKM with self-compassion). These results also failed to replicate the findings of Feldman et al. (2010), who observed that MBM produced stronger effects on its targeted process variable compared to LKM. Second, MBM and LKM both outperformed the relaxation control across the majority (5/7) of therapeutic processes, with fluctuating effect sizes ranging from small to large. This pattern supported our prediction, suggesting that the relaxation control was incomparable to MBM and LKM for producing positive process change – or at least far less effective in doing so. Third, the combined (MBM + LKM) condition did not appear to have value added compared to the independent MBM and LKM conditions. In fact, only 1/14 comparisons showed an advantage for the combined condition, with all others showing negligible differences or advantages for MBM or LKM conditions. Again, this pattern was contrary to our prediction, which posited that the combined condition would outperform the independent MBM and LKM conditions. Overall, then, the patterns of evidence observed at the descriptive level do not lend more credence to our hypotheses about the effects of conditions on process variables. However, they do suggest more nuanced and differentiated responses between the isolated component conditions (MBM and LKM), the relaxation control, and the combined (MBM + LKM) condition than is evidenced by statistical significance alone.

Regarding the effects of condition on mental health outcomes (i.e., anxiety, depression, and life satisfaction), the resulting effect sizes suggested similar empirical patterns as described for the process variables. Specifically, (a) negligible differences were observed for MBM and LKM comparisons across each mental health outcome; (b) MBM and LKM both outperformed the relaxation control across outcomes; and (c) the combined (MBM + LKM) condition did not have value added when compared to the isolated MBM and LKM conditions. These descriptive findings partially conformed with our predictions, as we expected that both LKM and MBM would improve mental health outcomes and that each would do so more effectively than an active (relaxation) control (Hofmann et al., 2010; Hofmann et al., 2011). However, these descriptive findings failed to support our hypothesis that the combined (MBM + LKM) condition would outperform the isolated (MBM and LKM) conditions; rather, descriptive results suggest the combined condition was no more effective than the relaxation control (see Table 4).

Limitations

The interpretative patterns we described above, at both the level of statistical significance and the level of descriptive effect sizes, should be considered in light of a few key methodological limitations. As already mentioned, our interpretation is muddied and indecisive given the lack of a passive (or measurement-only) control condition. We suggest this limitation could be easily remedied in future component analyses by including this

additional experimental condition. Next, in retrospect, our sample size was likely underpowered to detect the most interesting effects relevant to our research questions: the statistical significance of the between-condition comparisons. We suspect this is because the effect sizes observed for the interactions were mostly small, when we assumed (via *a priori* power analysis) that these effects would be at least moderate. By conducting *a priori* power analyses with the anticipation of small effect sizes, future studies can ensure better powered analyses that have a greater likelihood of detecting weaker signals in the data, if they exist. We compensated for this limitation by forgoing statistical significance testing at the post-hoc level and focusing on richer descriptive statistics, yet the small sample size likely contributed to the imprecision in confidence intervals observed at the descriptive level. Thus, beyond bolstering sensitivity to statistical significance, larger sample sizes may also increase confidence in descriptive estimates and patterns of effect sizes, which will contribute to a more stable understanding of the magnitude and differentiation of effects produced by MBM and LKM exercises in future component analyses.

Our method for assessing treatment integrity may be another limitation, as it precluded the evaluation of dosage as a potential moderator in our analyses. As described in the Method section (see above), treatment integrity was assessed using a self-monitoring calendar log, whereby participants marked completion or non-completion of their assigned exercise for 14 days. The purpose of this method was to serve as a validity check for our procedure, so we could determine if the exercises were implemented according to the intended schedule. Descriptive analyses of these logs indicated very high treatment integrity across participants (M completion = 13.80/14 days), suggesting excellent implementation fidelity. However, this metric may underrepresent possible variability in dosage, as the self-monitoring method did not allow participants to report if they had more or less exposure to the exercise relative to the 10-minute per day requirement. For example, it is possible that participants completing near the full exercise (e.g., 8 min per day), the full exercise (10 min per day), and double the full exercise (e.g., 20 min per day) would all report similarly complete ratings for that day and, thus, be quantified the same. Extensions of this line of research may therefore wish to operationalize treatment integrity as a continuous daily variable (e.g., self-reported implementation minutes), as opposed to a categorical variable (i.e., self-monitored completion vs. non-completion), which would allow for evaluation of potential dosage effects within a component analysis framework. Given emerging evidence for dosage or threshold effects for MBI is mixed (e.g., Bambacus & Conoley, 2021; Chin et al., 2019), such analyses should be considered exploratory in nature.

This study is also limited by the lack of random assignment to the combined (MBM + LKM) condition. Although a true randomized controlled treatment design was intended at the outset, an unforeseeable procedural error resulted in random assignment to all conditions except the combined condition (see the Methods section). Although we accounted for the potential bias inherent in this non-random assignment in our preliminary analyses by testing baseline score equivalency across conditions for both process and outcome variables – and then in our primary analyses by using ANCOVA with a baseline-score covariate for the one process variable showing non-equivalency (i.e., self-compassion) – it is still possible that this methodological feature biased the performance of

the combined condition relative to other conditions. Finally, our findings have limited generalizability given the lack of demographic diversity of our sample, as participants were mostly white, female psychology majors. Future research is therefore warranted not only with much larger samples, but also with more diverse participants, which are representative of the populations likely to benefit from MBIs.

Future research

We suggest the present study makes a modest yet value-added contribution to the MBI literature as an initial component analysis pilot study investigating the differential effects of MBM and LKM exercises on therapeutic processes and mental health outcomes. Given the importance of this line of inquiry and the relative lack of research in this area so far, we hope the results of our study – although tentative and suggestive – might motivate and guide future researchers seeking to establish a scientific understanding of how MBI achieve their effects. We acknowledge that component analysis of stand-alone intervention techniques is just one worthwhile line of inquiry within this larger project. Yet we believe it is of utmost importance, as results from such research may help optimize the content, reach, efficiency, and effectiveness of future MBI treatment packages (Renshaw, 2020).

In closing, we believe much work remains to be done to advance this area of research. We therefore encourage replication studies of our protocol using larger and more diverse samples, which will produce more precise and confident conclusions (cf. Dechartres et al., 2013). We also encourage generalization studies that adjust the parameters of our component analysis to explore other potentially impactful variables. Specifically, we suggest studies investigating (a) the effects of other exercises commonly packaged within MBI (e.g., mindful body scan meditation; Dreeben et al., 2013), (b) the immediacy of component effects on processes and outcomes (e.g., Upton & Renshaw, 2019), (c) the relative importance of psychoeducational elements (i.e., talking about mindfulness) versus experiential elements (i.e., practicing mindfulness; Renshaw et al., 2022), (d) dosage considerations (e.g., the frequency and duration of exposure to active treatment elements; Strohmaier, 2020), as well as (e) potential priming or expectancy effects related to implementation procedures (e.g., Upton et al., 2022) are likely to be especially worthwhile for advancing research in this area.

Author contributions

SB: designed and executed the study, conducted the data analyses, and collaborated on writing the paper. TR: supervised the design and execution of the study, supervised the data analysis, and collaborated on writing the paper. MP: collaborated on writing and editing the final manuscript. All authors approved the final version of the manuscript for submission.

Disclosure statement

No potential conflict of interest was reported by the author(s).

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