Mindfulness-based Relapse Prevention for Substance Use Disorders: A Systematic Review and Meta-analysis

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**Objectives:** Substance use disorder (SUD) is a prevalent health issue with serious personal and societal consequences. This review aims to estimate the effects and safety of Mindfulness-based Relapse Prevention (MBRP) for SUDs.

**Methods:** We searched electronic databases for randomized controlled trials evaluating MBRP for adult patients diagnosed with SUDs. Two reviewers independently assessed citations, extracted trial data, and assessed risks of bias. We conducted random-effects meta-analyses and assessed quality of the body of evidence (QoE) using the Grading of Recommendations Assessment, Development, and Evaluation approach.

**Results:** We identified 9 randomized controlled trials comprising 901 participants. We did not detect statistically significant differences between MBRP and comparators on relapse (odds ratio [OR] 0.72, 95% confidence interval [CI] 0.46–1.13, low QoE), frequency of use (standardized mean difference [SMD] 0.02, 95% CI –0.40 to 0.44, low QoE), treatment dropout (OR 0.81, 95% CI 0.40 to 1.62, very low QoE), depressive symptoms (SMD –0.09, 95% CI –0.39 to 0.21, low QoE), anxiety symptoms (SMD –0.32, 95% CI –1.16 to 0.52, very low QoE), and mindfulness (SMD –0.28, 95% CI –0.72 to 0.16, very low QoE). We identified significant differences in favor of MBRP on withdrawal/craving symptoms (SMD –0.13, 95% CI –0.19 to –0.08, I² = 0%, low QoE) and negative consequences of substance use (SMD –0.23, 95% CI –0.39 to –0.07, I² = 0%, low QoE). We found negligible evidence of adverse events.

**Conclusions:** We have limited confidence in estimates suggesting MBRP yields small effects on withdrawal/craving and negative consequences versus comparator interventions. We did not detect differences for any other outcome. Future trials should aim to minimize participant attrition to improve confidence in effect estimates.

**Key Words:** meta-analysis, mindfulness, substance use disorder, systematic review

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Mindfulness-based Relapse Prevention (MBRP) is a recently developed mindfulness intervention specifically for substance use that integrates traditional psychotherapeutic relapse prevention techniques (Marlatt and Gordon, 1985; Carroll, 1996; Irvin et al., 1999; Lancaster et al., 2006; Brandon et al., 2007) with mindfulness-based meditation practices (Bowen et al., 2011). The addition of these mindfulness meditation practices to traditional relapse prevention techniques is intended to further reduce the risk of relapse by helping patients with psychological discomfort that often precipitates relapse. Neurologically, mindfulness is hypothesized to reduce activity in circuitry related to craving (Way et al., 2010), and stimulate activity in circuitry related to cognitive self-regulation of behavior (Seeley et al., 2007; Craig, 2009; Xue et al., 2011; Hasenkamp and Barsalou, 2012; Hasenkamp et al., 2012). The core components of MBRP are typically delivered in weekly 2-hour group sessions for 8 weeks (16 hours total contact time) (Bowen et al., 2011, 2014a). During these sessions, MBRP providers teach patients meditation practices related to a central theme for the session (Table 1), to facilitate patients’ awareness of and healthier responses to challenging emotional, cognitive, and physical states they may experience due to craving or withdrawal from substance use (Bowen et al., 2011, 2014a).

Objective

Rigorous studies that estimate the clinical effects and safety of interventions are critical before recommendations for widespread dissemination, such as the use of mindfulness-based interventions by healthcare professionals to treat SUDs (Institute of Medicine, 2005, 2015). Meta-analytic estimates of specific effects of specific interventions are particularly important for efforts to improve evidence-based practice such as the development of clinical practice guidelines (Institute of Medicine, 2011). Reviews of the overall literature on mindfulness treatments for substance use and addiction suggest such interventions may be an effective tool, yet these have not involved meta-analyses of intervention effects (Zgierska et al., 2009; Brewer et al., 2013; Garland and Froeliger, 2013; Witkiewitz et al., 2013; Black, 2014; Chiesa and Serretti, 2014; Witkiewitz et al., 2014a) or include the SUDs of interest to this review (de Lisle et al., 2011; Oikonomou et al., 2016). MBRP specifically has been evaluated in several randomized controlled trials (RCTs) (Bowen et al., 2009; Witkiewitz and Bowen, 2010; Lee et al., 2011). An up-to-date systematic review is needed to synthesize these findings to provide comprehensive estimates of the effects of MBRP on specific patient-important clinical outcomes to subsequently inform guidelines about whether to recommend its use in everyday practice.

METHODS

We conducted a systematic review to identify RCTs evaluating the effects and safety of MBRP for adults with SUDs. This manuscript updates a previous review that we registered on an international prospective register of systematic reviews, PROSPERO (CRD42015016380), before completing formal screening of search results against eligibility criteria (Grant et al., 2015b); we identified 3 additional completed RCTs in our update search. The specific efficacy outcomes of interest included relapse to substance use (primary outcome), frequency and quantity of substance use, withdrawal/craving symptoms, treatment dropout, depressive and anxiety symptoms, negative consequences from substance use, and health-related quality of life. We evaluated safety via reported adverse events.

Search Strategy

We searched 2 trial registries (ClinicalTrials.gov and the World Health Organization International Clinical Trials Registry Platform) and the following databases from January 2000 through August 2016: Allied and Complementary Medicine Database, Cumulative Index to Nursing and Allied Health Literature, Cochrane Central, PsycINFO, PubMed, and Web of Science. Search strings involved variants of terms related to “mindfulness-based relapse prevention” and “substance use disorder” (the reproducible search strings are available in Online Supplement 1, http://links.lww.com/JAM/A58). We conducted database searches from 2000 onward because MBRP was developed and the first papers by its developers were published after the start of the 21st century (Zgierska et al., 2009; Bowen et al., 2010). In addition, we examined reference lists from included studies and previous reviews of mindfulness meditation for SUDs. We also contacted authors.
of included studies about any RCTs we may have missed, and also data not reported in manuscripts.

Eligibility Criteria
We included parallel group, individually, or cluster-randomized controlled trials with adult patients (male and female) who were 18 years of age or older. Participants must have been diagnosed with alcohol, opioid, stimulant, and/or cannabis use disorder; diagnoses included abuse or dependence using criteria from the Fourth and Fifth Editions of the Diagnostic and Statistical Manual of Mental Disorders (DSM-IV and DSM-V, respectively), or harmful use or dependence syndrome using the International Classification of Diseases (ICD) criteria. We included RCTs that evaluated MBRP as either a monotherapy or adjunctive therapy; we excluded RCTs evaluating other mindfulness-based interventions, such as mindfulness-based cognitive therapy or mindfulness-based stress reduction. We did not limit RCTs by comparator. We did not restrict eligibility by treatment duration, outcome follow-up period, clinical setting, or geographic location. Due to project team capabilities, we included studies published in English language only.

Eligibility Screening
Two independent reviewers screened titles and abstracts of retrieved citations. We obtained full texts for citations judged as potentially eligible by at least 1 reviewer. The reviewers then assessed full texts against the specified eligibility criteria; we resolved any disagreements regarding eligibility through discussion within the review author team.

Data Extraction
Two reviewers independently extracted study-level data using a form designed by the project team (the full data extraction form is available in Online Supplement 2, http://links.lww.com/JAM/A58). They also independently assessed risks of bias of included studies using the Cochrane Risk of Bias tool, Cochrane’s recommended approach for assessing risks of bias in RCTs included in systematic reviews of interventions (Higgins et al., 2011), and also involvement of the developers of the program (Bowen et al., 2010) in the RCT to indicate whether each RCT was an independent replication (Gottfredson et al., 2015). The project lead (S.G.) extracted all outcome data.
During the study, 97 citations were identified through our search strategy. We excluded 21, including 2 terminated trials and 9 ongoing trials (Fig. 1). Of 50 full texts identified as potentially eligible, we excluded 21, including 2 terminated trials and 9 ongoing trials (Higgins et al., 2003). We assessed the quality of the body of evidence (QoE) through the GRADE approach: study limitations via our risk of bias assessments; directness via how well studies addressed our questions of interest; and consistency via the magnitude of heterogeneity; precision via the width of confidence intervals; and publication bias (see below).

We conducted random-effects meta-analyses on the longest outcome using the restricted maximum-likelihood estimator method for the amount of heterogeneity and the Hartung-Knapp-Sidik-Jonkman adjustment for standard errors (Hartung, 1999; Hartung and Knapp, 2001; Sidik and Jonkman, 2006), using the “metafor” package in R (Version 3.2.3) (Viechtbauer and Viechtbauer, 2015). Effect estimates are expressed either as odds ratios (ORs) or Hedges g—a small study bias-adjusted estimate of the standardized mean difference (SMD)—along with 95% confidence intervals (CIs). For consistency, we coded outcome data such that an SMDs < 0 or ORs < 1 favor MBRP, and we used common indices for interpreting clinical effect sizes: SMD ≤ −0.2 or OR ≤ 0.60 for a small clinical effect, SMD ≤ −0.5 or OR ≤ 0.29 for a medium clinical effect, and SMD ≤ −0.8 or OR ≤ 0.15 for a large clinical effect (Chen et al., 2010). We used the $I^2$ statistic to assess the degree of heterogeneity in each analysis (Higgins et al., 2003).

We examined publication bias using Begg rank-correlation test for funnel plot asymmetry (Begg and Mazumdar, 1994) and Egger test for funnel plot asymmetry (Egger et al., 1997), and applied Duval trim and fill method (Duval and Tweedie, 2000) in the presence of publication bias. To explore sources of heterogeneity, we conducted meta-regressions when possible to examine whether there were differences in effect sizes by substance targeted, co-intervention status, and comparison group (Viechtbauer et al., 2015). To explore the robustness of our meta-analyses, we conducted sensitivity analyses using earlier time-points than longest follow-up when reported, and we followed recommendations to calculate a prediction interval for considering the whole distribution of effects, and also to examine whether effects exist and are consistent across individual studies (Higgins et al., 2009).

We assessed the quality of the body of evidence (QoE) for each outcome using the Grading of Recommendations Assessment, Development, and Evaluation (GRADE) approach (Balsch et al., 2011), which rates on a 4-item scale (very low, low, moderate, and high) the confidence that an effect estimate is close to the population parameter. We specifically assessed the following aspects of the body of evidence underpinning each effect estimate, as recommend by the GRADE approach: study limitations via our risk of bias assessments; directness via how well studies addressed our questions of interest; and consistency via the magnitude of heterogeneity; precision via the width of confidence intervals; and publication bias (see below).

**RESULTS**

We identified 97 citations through our search strategy (Fig. 1). Of 50 full texts identified as potentially eligible, we excluded 21, including 2 terminated trials and 9 ongoing trials that would likely meet eligibility criteria for this review once completed (see Online Supplement 3, http://links.lww.com/JAM/A58). Overall, we identified nine studies (see Online Supplement 5, http://links.lww.com/JAM/A59) meeting inclusion criteria (Bowen et al., 2009, 2014b; Brewer et al., 2009; Uhlig, 2009; Lee et al., 2011; Zgierska, 2014; Imani et al., 2015; Glasner et al., 2017).

**Description of Included Studies**

**Methods and Setting**

All studies took place in SUD specialty care settings; participants typically were in outpatient care, though 1 study took place in prison (Lee et al., 2011) and another in a residential treatment center (Bowen et al., 2014b). Most RCTs took place in the United States, 1 took place in Iran (Imani et al., 2015), and another in Taiwan (see Table 1) (Lee et al., 2011). All RCTs randomized participants individually (as opposed to cluster randomization); 1 RCT randomized participants to either MBRP or 1 of 2 other comparators (Bowen et al., 2014b), whereas all other RCTs evaluated MBRP against a single comparator. In all, 901 participants were randomized to receive either MBRP (425 participants) or a comparator intervention (476 participants), such as treatment as usual (TAU; 291 participants), health education (32 participants), or cognitive behavioral therapy (CBT) (15 participants).

**Participants**

Average age of participants ranged from 34 to 45 years old (median 39), and percentage of male participants ranged from 0% to 100% (median 72%). The majority of studies did not restrict participants by primary substance of misuse, with participants reporting use of various substances including alcohol, cocaine, marijuana, methamphetamine, and opioids. One study recruited patients meeting DSM criteria specifically for either alcohol or cocaine (Brewer et al., 2009), whereas 3 other studies only recruited participants dependent on stimulants (Glasner et al., 2017), opioids (Imani et al., 2015), or alcohol (Zgierska, 2014). Many RCTs excluded patients with concurrent psychotic disorder, significant suicide risk, or cognitive impairments, though notably 43% (n = 27) of participants in 1 study had an axis I mood or anxiety disorder (Glasner et al., 2017).

**Interventions**

Several RCTs evaluated MBRP according to the original manual (Bowen et al., 2009, 2014b; Zgierska, 2014). As such, sessions in these RCTs likely included 20 to 30 minutes of guided meditations, experiential skills-based practices, and discussion of practical applications, with participants also receiving handouts, audio-recorded mindfulness homework exercises, and daily craving and mood tracking sheets, as per the MBRP manual (Bowen et al., 2011, 2014a). One RCT evaluated the manual translated into Farsi (Imani et al., 2015), and the remaining RCTs shortened the MBRP manual to be delivered in 9 to 15 hours of total contact time (Brewer et al., 2009; Uhlig, 2009; Lee et al., 2011; Glasner et al., 2017). MBRP providers ranged from trained graduate-level therapists with experience in CBT and mindfulness meditation, to...
certified meditation instructors, to “trained instructors.” We confirmed involvement of the MBRP developers on the study team of 5 RCTs (Bowen et al., 2009, 2014b; Lee et al., 2011; Zgierska, 2014; Imani et al., 2015), and consultation with the MBRP developers in the development and implementation phases of another RCT (Glasner et al., 2017).

Five RCTs reported additional interventions or services received by MBRP participants, including TAU services the participants in the comparator group received (Uhlig, 2009; Zgierska, 2014; Imani et al., 2015), contingency management (which participants in the health education comparison group also received as a co-intervention) (Glasner et al., 2017), and “multiple other treatment programs” (which participants in the relapse prevention comparison group also received as a co-intervention) (Bowen et al., 2014b). Comparator interventions included relapse prevention, health education, cognitive behavioral therapy, and TAU (ie, substance use education, the Matrix Model, a predominantly 12-step process-orientated group, or medical management including pharmacotherapy and weekly individual counseling sessions).

**Risks of Bias**

Regarding selection bias, the majority of studies reported adequate random sequence generation methods, though only 4 reported an adequate concealment of the allocation sequence. All studies were de facto high risk of performance bias due to knowledge of the allocated interventions by participants and providers, as blinding participants and providers to assigned interventions is generally not possible for behavioral interventions. We rated 4 RCTs as low risk of detection bias due to use of blinded outcome assessors, 1 RCT as high risk of detection bias due to lack of blinding outcome assessors, and the remaining 4 RCTs as unclear risk of detection bias due to insufficient information. Attrition bias is a significant concern for this body of evidence, as we rated 4 RCTs as high risk of attrition bias at all follow-up points due to substantial attrition rates and 1 RCT as low risk at 1 follow-up point and high at all others due to varying attrition rates. Lastly, we rated 4 RCTs as low risk of reporting bias due to complete reporting of all outcomes contained in a trial registration or providing all outcome data in response to e-mails asking for study data (our justifications for all risk of bias assessments can be found in Online Supplement 5, http://links.lww.com/JAM/A59).

**Effects of MBRP**

The below analyses are summarized in Table 2 (outputs for all analyses and underlying data can be found in Online Supplements 4, http://links.lww.com/JAM/A58 and 5, http://links.lww.com/JAM/A59).

**MBRP Versus Any Comparator**

Relapse was operationalized across included studies as either any substance use or proportion of substance-free urine
| Study             | Country      | Participants                  | Substance Use Issue                                      | MBRP Program              | MBRP Provider                        | Co-intervention        | Comparator | Longest Follow-up | Level of Care |
|------------------|--------------|-------------------------------|--------------------------------------------------------|---------------------------|--------------------------------------|------------------------|------------|-------------------|---------------|
| Bowen et al., 2009 | United States | 168 randomized; 41 yrs; 64% male | Alcohol and drug use disorders SubSTANCE USE DISORDERS | Standard manual (16 h)    | Experienced masters-level therapists | NR                     | TAU        | 4 mos             | Outpatient    |
| Bowen et al., 2014b | United States | 286 randomized; 39 yrs; 72% male | Substance use disorders                               | Standard manual (16 h)    | Experienced masters/doctoral-level therapists | NR                     | TAU, RP    | 12 mos            | Outpatient    |
| Brewer et al., 2009 | United States | 36 randomized; 38 yrs; 72% male | DSM-IV criteria for alcohol/cocaine abuse/dependence | Shortened version (9 h)   | Experienced doctoral-level therapists | NR                     | CBT        | Postintervention  | Outpatient    |
| Glasner et al., 2017 | United States | 63 randomized; 45 yrs; 71% male | DSM-IV diagnosis of stimulant dependence Opioid dependence according to DSM-IV-TR criteria | Shortened version (10 h)  | Experienced masters-level therapist | CM (both MBRP and comparator) | Health education | 1 mos            | Outpatient    |
| Imani et al., 2015 | Iran         | 30 randomized; 37 yrs; 100% male | Opioid dependence according to DSM-IV-TR criteria    | Translated manual (16 h)  | NR                                   | TAU (ie, comparator)   | TAU        | Postintervention  | Outpatient    |
| Lee et al., 2011  | Taiwan       | 24 randomized; 41 yrs; 100% male | Illicit drug use                                      | Shortened version (15 h)  | Certified clinical psychologists     | NR                     | TAU        | Residential (Prison) | Outpatient    |
| Uhlig, 2009       | United States | 66 randomized; 39 yrs; 73% male | Substance dependence Requiring residential addiction treatment | Shortened version (13 h)  | Certified meditation instructor      | TAU (ie, comparator)   | TAU        | Postintervention  | Outpatient    |
| Witkiewitz et al., 2014b | United States | 105 randomized; 34 yrs; 0% male | Requiring residential addiction treatment            | Shortened Version (13 h)  | Experienced masters-level clinicians | Other programs (both MBRP and comparator) | RP         | 3.5 mos           | Residential    |
| Zgierska, 2014    | United States | 123 randomized; 41 yrs; 57% male | Alcohol dependence diagnosis                          | Standard Manual (16 h)    | Trained instructors                  | TAU (ie, comparator)   | TAU        | 4 mos             | Outpatient    |

Abbreviations: CM, contingency management; NR, not reported; RP, relapse prevention.
Publication Bias

Random-effects meta-analysis of the pooled RCTs yielded no significant difference on average between MBRP and any comparator (relapse prevention, health education, CBT, and TAU) for relapse to substance use (OR 0.72, 95% CI 0.46 to 1.13, 7 RCTs, I² = 0%, low QoE; see Fig. 2). We downgraded the QoE for this outcome due to a high risk of attrition bias and a wide CI.

Random-effects meta-analysis of the pooled RCTs yielded no significant difference on average for the secondary outcomes frequency of use (SMD 0.02, 95% CI –0.40 to 0.44, I² = 42%, 5 RCTs, low QoE), quantity of use (SMD 0.26, 95% CI –0.13 to 0.64, 1 RCT, very low QoE), treatment dropout (OR 0.81, 95% CI 0.40 to 1.62, 5 RCTs, I² = 44%, very low QoE), depressive symptoms (SMD –0.09, 95% CI –0.39 to 0.21, 4 RCTs, I² = 0%, low QoE), anxiety symptoms (SMD –0.32, 95% CI –1.16 to 0.52, 4 RCTs, I² = 78%, very low QoE), and mindfulness (SMD –0.28, 95% CI –0.72 to 0.16, 6 RCTs, I² = 58%, very low QoE). We identified a small clinical effect in favor of MBRP on withdrawal/craving symptoms (SMD –0.13, 95% CI –0.19 to –0.08, 5 RCTs, I² = 0%, low QoE), with QoE downgraded due to high risks of attrition and publication bias; and on negative consequences from substance use (SMD –0.23, 95% CI –0.39 to –0.07, 4 RCTs, I² = 0%, low QoE), with QoE downgraded due to high risk of attrition bias and a wide confidence interval. Lastly, we identified a medium clinical effect on health-related quality of life in favor of MBRP versus an active comparison group (relapse prevention) that shared the same co-intervention as MBRP recipients (SMD –0.64, 95% CI –1.19 to –0.09, 1 RCT, very low QoE). However, we significantly downgraded the QoE for this outcome due to high risks of selection, detection, and attrition bias; only 1 study providing data for this outcome; evaluation of an adapted version of MBRP at a different stage of the clinical pathway than intended; and a wide CI.

Publication Bias

We did not detect evidence of publication bias (see Table 2) for any outcomes using the Begg rank correlation test for funnel plot asymmetry (Begg and Mazumdar, 1994), and Egger test for funnel plot asymmetry (Egger et al., 1997). Model results including estimated missing studies did not substantially change results for relapse (OR 0.74, 95% CI 0.53 to 1.05, I² = 0%), depressive symptoms (SMD –0.00, 95% CI –0.16 to 0.16, I² = 0%), anxiety symptoms (SMD –0.20, 95% CI –0.70 to 0.31, I² = 79%), negative consequences (SMD –0.21, 95% CI –0.37 to –0.05, I² = 0%), and mindfulness (SMD –0.18, 95% CI –0.51 to 0.15, I² = 51%), but results for withdrawal/craving symptoms (SMD –0.13, 95% CI –0.30 to 0.04, I² = 0%) were no longer statistically significant when including an estimated missing study.

Meta-regressions

Indirect evidence suggests that MBRP may lead to significantly greater reductions in depressive symptoms when targeting patients specifically with a stimulant use disorder rather than any SUD (SMD –0.46, 95% CI –0.81 to –0.11), and also greater reductions in withdrawal/craving when targeting patients specifically with an alcohol use disorder rather than any SUD (SMD –0.09, 95% CI –0.18 to –0.01). We did not detect differences in results by type of substance targeted for other outcomes. We did not detect differences in results by co-intervention status. Meta-regressions did not indicate that the type of comparator systematically affected the results for any outcome (see Table 2).

Additional Analyses

Results were not sensitive to using earlier time-points from individual studies than longest follow-up when reported for relapse to substance use, frequency of use, negative consequences, withdrawal/craving, anxiety symptoms, and mindfulness. However, results were not statistically significant in 2 of 7 sensitivity analyses for withdrawal/craving, whereas results were statistically significant (and in favor of MBRP) in 1 of 5 sensitivity analyses for depressive symptoms and 2 in 4 sensitivity analyses for mindfulness. The full range of the prediction interval for the true effect in a new study.
favors MBRP for withdrawal/craving symptoms (SMD = −0.19 to −0.07) and negative consequences (SMD = −0.45 to −0.01), whereas the prediction intervals range from clinical benefit to clinical harm for all other outcomes (see Table 2).

**Adverse Events**

Three RCTs indicated that no adverse events were reported (Bowen et al., 2009, 2014b; Brewer et al., 2009). Another RCT listed death as 1 reason for exclusion from analyses in follow-up assessments for standard relapse prevention, and one participant receiving MBRP was admitted to inpatient care at six-month follow-up for reasons unknown (Bowen et al., 2014b). Authors from another RCT indicated in correspondence that no serious adverse events were reported; 1 participant receiving MBRP reported nightmares, increased anxiety, and trauma memories at a follow-up visit (symptoms resolved after medications were changed via psychiatrist consultation) (Zgierska, 2014). The other 4 RCTs did not provide data on adverse events (Uhlig, 2009; Lee et al., 2011; Imani et al., 2015; Glesner et al., 2017).

**DISCUSSION**

Across studies, our analyses did not indicate that MBRP has beneficial clinical effects beyond comparator interventions (such as relapse prevention, health education, CBT, and TAU) on substance use relapse. We also did not identify significant differences between MBRP and comparator interventions at longest follow-up for other substance use outcomes, including frequency and quantity of substance use. We also did not detect systematic differences in several other patient-important outcomes, including treatment dropout, depressive symptoms, and anxiety symptoms, and a purported mediator of MBRP (ie, mindfulness). Although we have limited confidence in results indicating that MBRP yields decreases in withdrawal/craving and negative consequences, the clinical effects were small. Although we also found clinical effects in favor of MBRP on health-related quality of life, we have very limited to no confidence in this effect estimate due to inadequacies of the body of evidence underlying this analysis. The majority of meta-regression analyses did not detect moderators of effect estimates. Whereas the available evidence on adverse events is also very limited, very few adverse events were reported, indicating that MBRP appears relatively safe from direct harm (Table 3).

We decided to update a previous systematic review on MBRP for SUDs (Grant et al., 2015b), commissioned by the Defense Centers of Excellence for Psychological Health and Traumatic Brain Injury to assist with clinical decision-making, to share the results with a wider audience. In addition, since that review of the evidence, more studies had been published in this small research area, so we updated our review in accordance with guidance on when to update reviews (Garner et al., 2016), which indicates the emergence of new studies that were likely to influence the direction, magnitude, and credibility of our previous reviews findings as important factors for embarking on a systematic review update. As a result of identifying 3 new trials, and also obtaining additional information for 5 trials through author correspondence, this manuscript updates our previous review in several important ways. First, we had sufficient power to detect a statistically significant result for 1 outcome included in our previous review (withdrawal/craving symptoms). Second, we included 5 outcomes not included in our previous review (quantity of substance use, negative consequences of substance use, depressive symptoms, anxiety symptoms, and mindfulness), 1 of which also demonstrated a statistically significant result (negative consequences of substance use). Lastly, we increased our GRADE ratings (ie, we had higher confidence in effect estimates) for our primary outcome (relapse to substance use) and 2 key secondary outcomes (frequency of use and withdrawal/craving symptoms). This updated review therefore provides more current and accurate effect estimates of MBRP to guide policy and practice decision-making and recommendations in addiction medicine (Institute of Medicine, 2011).

The conclusions from this review may surprise some, as individual trials on MBRP for SUDs have reported positive conclusions for substance use outcomes. However, some positive conclusions within trial reports were based on analyses comparing combined data from MBRP and relapse prevention with TAU (Bowen et al., 2014b), or focused on select positive results (Bowen et al., 2009), rather than the totality of findings within a trial (Boutron et al., 2010). Those considering MBRP should weigh our reported effect estimates and our confidence in them with other factors, such as resource requirements, impact on health equity, acceptability to patients, feasibility to implement, and opportunity costs, before deciding whether to recommend it as a treatment in lieu of or in combination with other available interventions for patients with SUDs (Alonso-Coello et al., 2016). Furthermore, it is worth noting that we only examined 1 specific mindfulness intervention amongst others that would benefit from focused systematic reviews to inform recommendations for practice (Zgierska et al., 2008).

Limitations in the current body of evidence indicate how future trials can provide data for firmer conclusions about the effects of MBRP and more reliably inform clinical decision-making. First, most RCTs resembled pilot efficacy trials rather than pragmatic effectiveness trials, with more than half of RCTs randomizing less than 40 participants to each trial group; larger samples are needed to reach the optimal information size for detecting robust results (Guyatt et al., 2011). For most subgroup comparisons in our review, there was insufficient power to statistically detect whether MBRP is efficacious for specific substances, more efficacious when offered either adjunctively or as a monotherapy, or more efficacious when compared with certain interventions than others. Second, attrition bias is a critically high-risk for this evidence base. Future researchers should invest more study resources into ensuring adequate follow-up rates. Given that much outcome data were not reported, we implore future researchers to pre-register trial protocols and subsequently report all outcomes measured in trial manuscripts to have greater statistical power to detect effects amongst all outcomes of interest (Chan et al., 2013). Lastly, researchers should write RCT reports that are in compliance with reporting guidelines for RCTs to allow full critical appraisal of all potential risks of bias, understand the settings and populations.
TABLE 3. Summary of Findings Table

| Outcome                               | Studies | Summary Estimate (95% CI) | QoE     | Publication Bias | Meta-regressions | Prediction Interval | Sensitivity to Additional Analyses |
|---------------------------------------|---------|---------------------------|---------|------------------|------------------|---------------------|-----------------------------------|
| Relapse to substance use              | k = 7; n = 841 | OR 0.72 (0.46 to 1.13) | Low<sup>1,2</sup> | $\tau = -0.14, P = 0.77;\ t$ <br> (5) = 0.11, P = 0.92; OR 0.74 (0.53 to 1.05) | Substance: P = 0.44; Co-int: P = 0.99; Comparator: P = 0.42 | OR 0.44 to 1.15 | No significant differences across sensitivity analyses |
| Frequency of use                      | k = 5; n = 718 | SMD 0.02 (−0.40 to 0.44) | Low<sup>1,2</sup> | $\tau = 0.20, P = 0.82; t$ <br> (3) = 1.30, P = 0.28; SMD 0.02 (−0.40 to 0.44) | Substance: P = 0.07; Co-int: P = 0.46; Comparator: P = 0.20 | SMD −0.74 to 0.77 | No significant differences across sensitivity analyses |
| Quantity of use                       | k = 1; n = 123 | SMD 0.26 (−0.13 to 0.64) | Very low<sup>1,2,7</sup> | Insufficient evidence | Insufficient evidence | Insufficient evidence | No significant differences across sensitivity analyses |
| Withdrawal                             | k = 5; n = 718 | SMD −0.13 (−0.19 to 0.08) | Low<sup>1,11</sup> | $\tau = -0.40, P = 0.48; t$ <br> (3) = −1.09, P = 0.35; SMD −0.13 (−0.30 to 0.04) | Substance: P = 0.04; Co-int: P = 0.39; Comparator: P = 0.21 | SMD −0.19 to −0.07 | Results not statistically significant in 2 of 7 analyses |
| Craving symptoms                      | k = 5; n = 556 | OR 0.81 (0.40 to 1.62) | Very low<sup>1,2,4</sup> | $\tau = 0.40, P = 0.48; t$ <br> (3) = −0.65, P = 0.56; OR 0.81 (0.40 to 1.62) | Substance: P = 0.97; Co-int: P = 0.23; Comparator: P = 0.28 | OR 0.19 to 3.42 | N/A |
| Treatment dropout                     | k = 1; n = 105 | SMD −0.64 (−1.19 to −0.09) | Very low<sup>1,4–9</sup> | Insufficient evidence | Insufficient evidence | Insufficient evidence | N/A |
| Health-related quality of life        | k = 4; n = 682 | SMD −0.23 (−0.39 to 0.07) | Low<sup>1,9</sup> | $\tau = -0.67, P = 0.33; t$ <br> (2) = −1.78, P = 0.22; SMD −0.21 (−0.37 to −0.05) | Substance: P = 0.53; Co-int: P = 0.21; Comparator: P = 0.79 | SMD −0.45 to −0.01 | No significant differences across sensitivity analyses |
| Negative consequences                 | k = 4; n = 622 | SMD −0.09 (−0.39 to 0.21) | Low<sup>1,2</sup> | $\tau = -0.67, P = 0.33; t$ <br> (2) = −1.98, P = 0.19; SMD −0.20 (−0.45 to −0.05) | Substance: P = 0.03; Co-int: P = 0.51; Comparator: P = 0.21 | SMD −0.49 to 0.32 | Results statistically significantly favor MBRP in 1 of 5 analyses |
| Depressive symptoms                   | k = 4; n = 553 | SMD −0.32 (−1.16 to 0.52) | Very low<sup>1,2,10</sup> | $\tau = -0.67, P = 0.33; t$ <br> (2) = −2.18, P = 0.16; SMD −0.20 (−0.70 to 0.31) | Substance: P = 0.32; Co-int: P = 0.60; Comparator: N/A | SMD −2.37 to 1.74 | No significant differences across sensitivity analyses |
| Anxiety symptoms                      | k = 6; n = 525 | SMD −0.28 (−0.72 to 0.16) | Very low<sup>1,2,10</sup> | $\tau = -0.20, P = 0.72; t$ <br> (4) = −1.43, P = 0.23; SMD −0.18 (−0.51 to −0.15) | Substance: P = 0.65; Co-int: P = 0.12; Comparator: P = 0.93 | SMD −1.35 to 0.78 | Results statistically significantly favor MBRP in 2 of 4 analyses |

Reasons for downgrading QoE: 1, high risk of attrition bias; 2, CI consistent without benefit/harm; 3, substantial statistical heterogeneity; 4, adapted version of MBRP; 5, high risk of selection bias; 6, high risk of detection bias; 7, only 1 study into replication to assess consistency); 8, not outpatient aftercare; 9, wide CI; 10, considerable statistical heterogeneity; 11, evidence of publication bias.

OR < 1 favors MBRP; SMD < 0 favors MBRP.

k, Number of studies; $\tau$, Kendall tau for Begg rank-correlation test for funnel plot asymmetry; t, Egger regression test for funnel plot asymmetry.
to which results are most applicable, and facilitate replication of the intervention (Moher et al., 2010; Grant et al., 2013).

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