

# Testing the Efficacy of Attention Bias Modification for Suicidal Thoughts: Findings From Two Experiments

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*This study explores whether four sessions of attention bias modification (ABM) decreases suicide-specific attentional bias. We conducted two experiments where suicide ideators completed either a Training or Control version of ABM, a computer-based intervention intended to target attentional bias. Suicide-specific attentional bias was measured using adapted Stroop and probe discrimination tasks. The first experiment with community-based suicide ideators did not show that ABM impacts attentional bias or suicidal ideation. The second experiment with clinically severe suicidal inpatients yielded similar results. Post-hoc findings suggest that the type of attentional bias targeted by the current intervention may differ from the type that marks suicide risk. There remains little to no evidence that the ABM intervention changes suicide-specific attentional bias or suicidal ideation.*

**Keywords** attentional bias, cognitive bias modification, suicidal ideation, suicide

Suicide is one of the leading causes of death worldwide, with approximately one person dying by suicide every 40 seconds (Krug, Dahlberg, Mercy, Zwi, & Lozano, 2002). Despite government calls to action to tackle this preventable form of death, suicide mortality rates have *increased* in the past decade (Rockett et al., 2012).

Psychological science has improved our understanding of why people kill themselves. For instance, we know that *suicide-specific attentional bias* is a unique psychological risk factor for suicide attempt (Cha, Najmi, Park, Finn, & Nock, 2010). This risk factor has been measured using the *suicide Stroop task*, a reaction time measure in which delayed response latencies in identifying the color of suicide-related (vs. neutral) words mark stronger suicide-specific attentional bias. Performance on this behavioral measure prospectively predicted suicide attempt controlling for

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well-known risk factors (e.g., mood disorder, previous suicide attempts; Cha et al., 2010). These findings build on earlier cross-sectional research demonstrating that attentional bias among suicide attempters is specific to suicide-related content (Becker, Strohbach, & Rinck, 1999; Williams & Broadbent, 1986). They also confirm earlier theories claiming that selective attention toward suicide—indicative of an underlying suicide schema—contributes to suicidal thoughts and behaviors (Wenzel & Beck, 2008).

Despite these scientific advances, we still do not know whether suicide-specific attentional bias is a viable target of intervention. Initial findings suggest that suicide-specific attentional bias may be state-like and malleable. Cha and colleagues (2010) found that the strength of suicide-specific attentional bias relates to how *recently* a person attempted suicide. This temporal sensitivity suggests that attentional bias for psychopathology can indeed change, and perhaps be purposely changed, over time.

We look to techniques that can help test for malleability. The surge of research around *cognitive bias modification* (CBM) brings forward strategies to change and improve biases around attention, interpretation, and memory (Hertel & Mathews, 2011). One form of CBM tailored to reduce disorder-specific attentional bias is *attention bias modification* (ABM; MacLeod, Rutherford, Campbell, Ebsworthy, & Holker, 2002). ABM is a tailored version of the probe discrimination task, a behavioral paradigm designed to target spatially oriented attentional bias. This brief computer-based intervention had originally been shown to reduce attentional bias toward threat and anxiety symptoms (e.g., Amir, Beard, Taylor, et al., 2009; Najmi & Amir, 2010). Most relevant to the current investigation, ABM has increasingly been shown to reduce engagement toward negative stimuli among depressed and

dysphoric individuals. It essentially breaks the cycle of attending toward negative cues that maintain depressive symptoms, according to Beck's (1967) Cognitive Theory. The effectiveness of ABM on depression has been shown across distinct behavioral and neurobiological measures of attentional bias (Beevers, Clasen, Enock, & Schnyer, 2015), and through symptom-level changes (Wells & Beevers, 2010). These findings suggest that the effectiveness of ABM is not limited to anxiety, and that symptoms that co-occur with depression (e.g., suicidal ideation) may be viable candidates for ABM. Given the parallel roles that attentional bias plays in cognitive theories of depression (Beck, 1967) and suicide (Wenzel & Beck, 2008), ABM may help break the cycle of cognitive biases that maintain suicidal ideation over time. The present experiments adopt a mechanistic approach to determining whether even temporary change in attentional bias and suicidal ideation is possible.

The purpose of this study is to determine whether ABM reduces suicide-specific attentional bias and severity of suicidal ideation. We tested the efficacy of a multiple-session ABM intervention in two experiments: one with community-based suicidal adults in a laboratory setting, and the other with suicidal patients in an adult psychiatric inpatient unit (i.e., more similar to adults presenting to the hospital emergency room in Cha et al., 2010). We hypothesized that those receiving ABM would show reduced suicide-specific attentional bias compared to those in the control condition. We also hypothesized that those receiving ABM would report decreased suicidal ideation compared to those in the control condition immediately after the four training sessions. Given that prior ABM studies have demonstrated longer-term clinical symptom reduction (e.g., four months post-training; Amir, Beard, Taylor, et al., 2009; Schmidt, Richey, Buckner, & Timpano, 2009), we

hypothesized that these observed reductions in suicidal ideation would persist at least up to 2 weeks and 2 months post-training.

## EXPERIMENT 1

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### Method

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*Participants.* Participants in Experiment 1 were 62 adults ( $M = 32.6$  years;  $SD = 13.0$  years) reporting past-month suicidal ideation who were recruited from advertisements posted in outpatient treatment centers, throughout the community, and on the Internet (e.g., Craigslist). Exclusion criteria included any impairment in a person's ability to speak or write English fluently, color blindness, gross cognitive impairment, presence of extremely violent or agitated behavior, or indication of high or imminent risk of suicide during the recruitment process. Participants were randomly assigned to receive either the Training ( $n = 29$ ) or Control version ( $n = 33$ ) of ABM using a random number generator. All experimenters were blind to the assigned condition until all in-person and follow-up telephone assessments were completed. Five participants did not complete the training sessions and withdrew from the study; reasons for withdrawal included scheduling conflicts, physical illness, and anticipated distress from continued participation. Two participants also were excluded due to inconsistent reporting of suicide history. In total, 28 Training and 27 Control group members completed all four lab sessions, including pre- and post-training assessments during their first and last days in the lab, respectively.

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### Measures

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*Demographic Information.* Participants provided information about their age, marital

status, education, and employment status via self-report. This information was used to rule out the possibility that training effects are due to these factors.

*Self-Injurious Thoughts and Behaviors Interview (SITBI).* Participants completed the SITBI (Nock, Holmberg, Photos, & Michel, 2007) to report history of self-injurious thoughts and behaviors. They completed a brief (5-minute) self-report version of this after training on the first day, and a shortened interview version during the follow-up calls.

*Scale for Suicidal Ideation.* The Scale for Suicidal Ideation (SSI; Beck & Steer, 1991) is a widely used, 19-item measure that was used to evaluate various aspects of current suicidal ideation including: presence, frequency, severity, and duration. We hereafter refer to this as Suicidal Ideation (SI) severity. While the SSI originally examines SI severity over the 'past week,' we modified the time frame to "past four days" to fit the 4-day intervention schedule. The SSI was administered as a self-report measure after the first and last sessions in the lab, and over the phone with an interviewer during the follow-up calls. Total Beck SSI scores can range from 0 to 37.

*Mini International Neuropsychiatric Interview.* The Mini International Neuropsychiatric Interview (MINI; Sheehan et al., 1998) is a brief, structured interview assessing various categories of current Axis I disorders. This information on mental disorders was collected to ensure that potential training effects were not due to baseline group differences in psychiatric history. Following the MINI was a brief interview asking participants to indicate history of various categories of Axis I & II disorders, treatments, as well as prior psychiatric hospitalizations.

*Attentional Bias Measures.* We used two measures of suicide-specific attentional bias. First, the suicide Stroop task (Cha et al., 2010) is a computerized behavioral assessment that measures reaction times

to identifying the color of suicide-related words. After a 4 s blank screen, participants saw a centered fixation cross for 1 s, followed by a 1 s blank screen, and finally a word printed in either red or blue font. They were instructed to identify the color of the word by pressing one of two different keys. Following eight practice trials involving neutral words, participants completed critical trials including suicide-related (*dead, funeral, suicide*) and neutral (*paper, engine, museum*) words. Each category was presented 12 times in new random order throughout the task.

Stroop interference scores were calculated based on the response times (RTs) of identifying the color of suicide-related and neutral words. Following scoring procedures reported by Cha et al. (2010), we used critical trial RTs to calculate interference scores. Higher interference scores (ms) indicated relatively slower response to suicide-related word colors, thereby representing fixation on word content.

The second measure of attentional bias is the probe discrimination task, which is similar in design to the ABM intervention described below. Immediately before and after each ABM session, all participants completed a computerized probe discrimination task.<sup>1</sup> They were instructed to identify letters that appeared on the screen as quickly and as accurately as they could. This task first presented a fixation cross in the center of the screen. The cross then disappeared, and a pair of words appeared—one positioned toward the top of the screen, and one positioned toward the bottom of the screen. The word pair

always consisted of one suicide-related word and one neutral word, with positioning on screen (top vs. bottom) counterbalanced. Both words then disappeared, and a probe (the letter E or F) replaced one of the words. The participants were instructed to identify the probe as E or F by pressing the corresponding letter on the keyboard. They first completed a practice version with a separate set of neutral words, and then proceeded to 48 assessment trials featuring suicide-related and neutral words. The trials were counterbalanced according to probe type (E vs. F), probe location (top vs. bottom), and suicide-related word location (top vs. bottom), and presented in a uniquely random order for each participant.

Probe discrimination task scores were calculated based on the RTs from congruent trials (i.e., when the probe replaced a suicide-related word) and incongruent trials (i.e., when the probe replaced a neutral word) during the pre- and post-training trials. Following scoring procedures reported by Najmi and Amir (2010), we calculated scores such that a higher value (in milliseconds) indicated relatively quicker response to probes following suicide-related words, thereby denoting greater engagement with suicide-related words (or difficulty orienting toward neutral stimuli). Since administration of a practice task likely reduced subsequent practice effects, we did not exclude the first 10 critical trials of the pre-training task as reported in Najmi and Amir (2010).

*Attention Bias Modification.* Participants were randomly assigned to either the Training or Control condition of ABM. Both conditions involved four sessions of a 20-minute computer-based task. Each session included 288 trials, with a brief break presented every 100 trials. Similar to the probe discrimination task, both ABM conditions first presented a fixation cross, followed by a suicide-neutral word pair, followed by a letter probe replacing one of the words. The probe discrimination

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<sup>1</sup>One possibility is that inter-session administration of the probe discrimination task hinders learning and consolidation from the ABM intervention. Examples of significant attentional bias change in similarly designed studies (Li, Tan, Qian, & Liu, 2008; see, MacLeod, & Bridle, 2009) counter this possibility. Our study design is thereby an unlikely explanation for our subsequent results.

task and respective ABM condition were embedded within a single computer program, so that participants were unaware of when they were completing the assessment vs. training portion of the seemingly identical tasks.

We used two sets (Set A, Set B) of 12 suicide-neutral word pairs for the assessment and training paradigms. In order to control for potential material or practice effects, half of the participants saw Set A words during the training and Set B for the assessment, and the other half saw Set B during the training and Set A for the assessment. When word sets were used for assessment, the first half of the set (e.g., A1) was used for pre-assessment, and the second half (e.g., A2) was used for post-assessment, and vice-versa. There were a total of eight possible conditions: 2 (Set A vs. Set B for training)  $\times$  2 (first vs. second half of word set for pre-assessment)  $\times$  2 (Training vs. Control).

Neither ABM condition explicitly instructed individuals to direct attention away from suicide-related stimuli; this was instead achieved by manipulating the proportion of times the probe replaces the neutral word compared to the suicide-related word. In the Training condition, the probe replaced the neutral word 100% of the time, and the suicide-related word 0% of the time. We expected the ABM Training condition to implicitly teach participants to expect the probe only following neutral words, thereby directing their attention away from suicide-related words. In the ABM Control condition, the probe replaced the neutral word 50% of the time, and the suicide-related word 50% of the time. We expected participants in this latter condition would be unable to anticipate whether the probe would follow the neutral or suicide-related words, and would therefore not experience any change in suicide-specific attentional bias.

*Placebo Effect & Contingency Awareness Check.* At the end of the fourth laboratory-based session, participants were asked whether they observed anything unusual or any pattern during the computer task, and whether they believed they knew which condition they were assigned to (if so, which one). In order to maintain the double-blind nature of the study, experimenters instructed participants to write down (vs. verbally report) their responses. These recorded observations were later opened by the first author at the end of data collection.

*Procedure.* Baseline assessment and all four training sessions took place in a laboratory setting in Cambridge, MA. The first day consisted of: (1) attainment of informed consent; (2) experimenter administration of the MINI and supplemental psychiatric history interview to assess participants' current and past psychiatric symptoms; (3) pre-training suicide Stroop and probe discrimination tasks; (4) ABM (Training vs. Control condition); (5) a self-report packet including the Demographic Information Form, SITBI self-report, and the Beck SSI. Participants then returned to the lab to complete half-hour sessions of the probe discrimination task and assigned ABM condition, once a day for the next three days. A suicide risk assessment was completed at the conclusion of each session to ensure participant safety. The fourth and final day in the lab included additional post-training assessments: (1) post-training suicide Stroop and probe discrimination tasks; (2) Beck SSI; (3) assessment for placebo effect or contingency awareness. Finally, participants completed 2-week and 2-month follow-up telephone calls in which the interviewer administered the Beck SSI and an abbreviated version of the SITBI. Participants were compensated for completing each part of the study: \$15 for the

first session, \$10 for each subsequent session, an additional \$15 for adhering to the consecutive 4-day schedule, and a \$10 gift card for the follow-up telephone interviews. Research team members directly interacting with participants remained blind to the study condition. All procedures were approved by the university institutional review board.

*Data Analyses.* First, we tested group equivalence at baseline using  $t$  tests for continuous measures and chi-square tests for categorical measures. Second, we conducted repeated measures ANOVA to test whether attentional bias changed from immediately before the first session (baseline) to immediately after the last session (session 4) as a function of group assignment. We also calculated zero-order correlations to examine the relationship between short-term (i.e., baseline–session 4) change in attentional bias with short-term change in suicidal ideation severity across both groups. Third, we conducted identical analyses to test whether severity of suicidal ideation changed from baseline to session 4, and to 2 weeks and 2 months later, as a function of group assignment. Relatedly, we conducted chi-square tests to examine whether there were any group differences in follow-up presence of suicidal ideation. Finally, we tested for potential influence of placebo effect and participants' contingency awareness using chi-square analyses. Post-hoc hypotheses were tested using a combination of analyses described later in this results section. Based on prior studies demonstrating efficacy of repeated-session ABM (Amir, Beard, Burns, & Bomyea, 2009; Amir, Beard, Taylor, et al., 2009), we expected a large effect size ( $d=1.0$ ) to mark differences between Training and Control groups. With alpha set at .05 and power (1-beta) set at .80, the current study design required at least 17 participants in each group.

## Results

*Group Equivalence at Baseline.* The groups did not differ significantly at baseline on demographic factors, psychiatric diagnoses and treatment, or history of self-injurious thoughts and behaviors (Table 1). As expected, the Training and Control groups demonstrated comparable performance on the suicide Stroop and probe discrimination tasks at baseline. Performance on the baseline probe discrimination task did not vary across the eight sets of words used,  $F(7,47) = 0.76$ ,  $p = .62$ .

*Change in Attentional Bias.* On the suicide Stroop task, there were no significant main effects of Group,  $F(1,43) = 1.08$ ,  $p = .30$ ,  $\eta_p^2 = .03$ , or Time,  $F(1,43) = 0.13$ ,  $p = .72$ ,  $\eta_p^2 = .003$ . The lack of a Group  $\times$  Time interaction predicting suicide Stroop performance,  $F(1,43) = 0.02$ ,  $p = .88$ ,  $\eta_p^2 = .00$ , suggests that ABM did not change Stroop interference (Figure 1a). Similarly on the probe discrimination task, there were no significant main effects of Group,  $F(1,45) = 2.50$ ,  $p = .12$ ,  $\eta_p^2 = .05$ , or Time,  $F(4,180) = 1.49$ ,  $p = .21$ ,  $\eta_p^2 = .03$ . The lack of significant Group  $\times$  Time interaction,  $F(4,180) = 0.98$ ,  $p = .42$ ,  $\eta_p^2 = .02$ , suggests that ABM also did not change attentional bias as measured by the probe discrimination task (Figure 1b). Finally, performance change on neither the suicide Stroop nor probe discrimination tasks was associated with short-term change in SI severity,  $r_s = -0.18$  to  $-0.08$ ,  $p_s = .21$ – $.62$ .

*Change in Suicidal Ideation.* ABM was not associated with short-term change in SI severity. The Group  $\times$  Time interaction predicting Beck SSI scores was not significant,  $F(1,53) = 0.31$ ,  $p = .58$ ,  $\eta_p^2 = .01$ , nor was the main effect of Group,  $F(1,53) = 0.03$ ,  $p = .87$ ,  $\eta_p^2 = .001$ . Notably, there was a significant decrease in SI severity

TABLE 1. Baseline Comparison Across Training and Control Group Members, Experiment 1

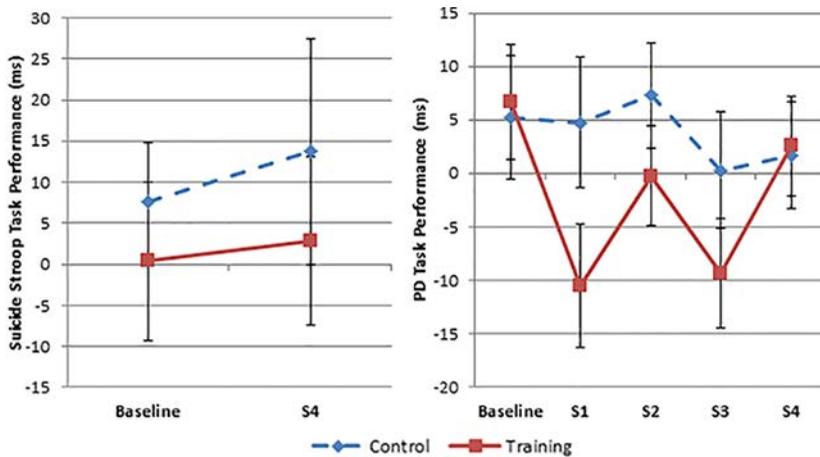
Variable	Training ( $n = 28$ )	Control ( $n = 27$ )	Test	Effect size
Age in years ( $M \pm SD$ )	33.3 $\pm$ 14.6	30.7 $\pm$ 11.5	$t(53) = 0.72$	$d = 0.20$
Sex (% female)	46.4	55.6	$\chi^2(1) = 0.46$	$\Phi = 0.09$
<i>Race (%)</i>				
White	64.3	63.0	$\chi^2(4) = 0.01$	$\Phi = 0.01$
Hispanic	17.9	18.5		
Black	7.1	7.4		
Asian	7.1	7.4		
Other	3.6	3.7		
<i>SITB History</i>				
SI severity, Beck SSI ( $M \pm SD$ )	10.6 $\pm$ 7.5	11.3 $\pm$ 7.7	$t(53) = 0.36$	$d = 0.10$
SI frequency ( $M \pm SD$ )	4.1 $\pm$ 4.4	4.9 $\pm$ 6.0	$t(48) = 0.50$	$d = 0.14$
Nonsuicidal Self-injury (%)	57.1	66.7	$\chi^2(1) = 0.53$	$\Phi = -0.10$
Suicide Plan (%)	46.4	59.3	$\chi^2(1) = 0.91$	$\Phi = -0.13$
Suicide Attempt (%)	39.3	48.1	$\chi^2(1) = 0.44$	$\Phi = -0.09$
<i>Recent Treatment History (%)</i>				
Psychological tx	35.7	59.3	$\chi^2(1) = 3.06$	$\Phi = -0.24$
Pharmacological tx	46.4	55.6	$\chi^2(1) = 0.46$	$\Phi = -0.09$
<i>Total # psychiatric disorders (<math>M \pm SD</math>)</i>				
Current # disorders	2.8 $\pm$ 1.6	2.8 $\pm$ 2.1	$t(52) = -0.03$	$d = -0.01$
Lifetime # disorders	2.8 $\pm$ 2.1	3.1 $\pm$ 2.0	$t(53) = 0.53$	$d = 0.15$
<i>Current Psychiatric disorders (%)</i>				
Mood disorder	82.1	70.4	$\chi^2(1) = 1.10$	$\Phi = 0.14$
Anxiety disorder	82.1	77.8	$\chi^2(1) = 0.16$	$\Phi = 0.06$
Eating disorder	14.3	18.5	$\chi^2(1) = 0.18$	$\Phi = -0.06$
<i>Attentional Bias Tasks (ms) (<math>M \pm SD</math>)</i>				
Suicide Stroop Task	-3.1 $\pm$ 46.2	9.6 $\pm$ 33.8	$t(48) = 1.11$	$d = 0.32$
Probe Discrimination Task	7.1 $\pm$ 24.2	5.0 $\pm$ 29.4	$t(53) = -0.30$	$d = -0.08$

Note.  $M$  = mean;  $SD$  = standard deviation. SITB = Self-injurious thoughts and behaviors. SI = Suicidal Ideation. Beck SSI = Beck Scale for Suicidal Ideation. SI frequency = Number of days of experiencing suicidal ideation over the past 2 weeks. There were no significant group differences,  $p > .05$ . Additional analyses examining the demographic categories of education ( $\chi^2(3) = 1.54, p = .67, \Phi = 0.17$ ), employment status ( $\chi^2(4) = 4.46, p = .35, \Phi = 0.29$ ), and marital status ( $\chi^2(3) = 2.21, p = .53, \Phi = 0.20$ ) yielded non-significant results. Finally, groups did not differ across suicide Stroop and probe discrimination task error rates,  $t_{(53)} = 0.04-1.00, ds = 0.01-0.27, ns$ .

immediately after four ABM sessions,  $F(1,53) = 7.57, p = .01, \eta_p^2 = .13$ .

Forty-three participants completed both the 2 week and 2 month follow-up assessments. Consistent with short-term findings, there was a significant decrease

in SI severity across the follow-up period,  $F(3,120) = 6.11, p = .001, \eta_p^2 = .13$ . Follow-up results also revealed a nonsignificant main effect of Group,  $F(1,40) = 0.001, p = .98, \eta_p^2 < .001$ , and Group  $\times$  Time interaction,  $F(3,120) = 0.30, p = .83$ ,



**FIGURE 1.** ABM effects on suicide Stroop and probe discrimination task performance, Experiment 1. Note. ABM = Attention bias modification; PD task = probe discrimination task; ms = milliseconds; Baseline = Task performance immediately before session 1; S1–S4 = Task performance immediately after sessions 1–4, respectively. Error bars represent standard error.

$\eta_p^2 = .01$ , for longer-term SI severity. Finally, the Training group was no more likely to report suicidal ideation 2 weeks (54.2%) or 2 months (62.5%) after ABM compared to the Control group (80.0%, 66.7%, respectively),  $\chi^2_{s(1)} = 0.09$ –3.72,  $p_s = .05$ –.77,  $\Phi_s = 0.04$ –0.28.

*No Influence of Placebo Effect or Contingency Awareness.* Supplementary analyses were conducted to examine whether participants were aware of the experimental manipulation. Of the 13 participants ( $n = 7$  from the Training group) who claimed that they knew which version of ABM they received, participants in the Training group were no more likely to correctly guess than Control group members,  $\chi^2(1) = 1.04$ ,  $p = 0.31$ ,  $\Phi = -.28$ . Participants in the Training group also were not more likely than the Control group to: notice anything unusual,  $\chi^2(1) = 0.02$ ,  $p = .88$ ,  $\Phi = .02$ , notice any pattern,  $\chi^2(1) = 1.52$ ,  $p = .22$ ,  $\Phi = .17$ , or be able to identify which version of ABM they received,  $\chi^2(1) = 0.10$ ,  $p = .75$ ,  $\Phi = .04$ .

*Testing ABM Efficacy Among Those with Elevated Baseline Bias.* Next, we tested several post-hoc explanations for why results thus far might have failed to support our hypotheses. First, perhaps ABM only effectively reduces attentional bias that is elevated at baseline. If this is true, we would expect a significant Group  $\times$  Time interaction among those demonstrating relatively high suicide Stroop interference or probe discrimination task scores prior to the intervention. To test this, we first isolated suicide ideators who demonstrated *positive* suicide Stroop interference scores at baseline; that is, those who were on average slower at identifying the color of suicide-related words compared to neutral words. Among the 22 suicide ideators demonstrating positive suicide Stroop interference at baseline, ABM did not affect Stroop performance across the 4 days. There was no significant Group  $\times$  Time interaction,  $F(1,20) = 0.34$ ,  $p = .57$ ,  $\eta_p^2 = .02$ , nor a significantly main effect of Group,  $F(1,20) = 0.83$ ,  $p = .37$ ,  $\eta_p^2 = .04$ .

There was a significant main effect of Time,  $F(1,20) = 10.13$ ,  $p = .01$ ,  $\eta_p^2 = .34$ .

Similar post-hoc analyses were conducted with 26 suicide ideators demonstrating positive probe discrimination task scores at baseline. These ideators were faster at identifying a letter that replaced suicide-related words compared to neutral words on average. Among these ideators, there was no significant Group  $\times$  Time interaction,  $F(4,96) = 0.19$ ,  $p = .94$ ,  $\eta_p^2 = .01$ , nor a main effect of Group,  $F(1,24) = 3.78$ ,  $p = .06$ ,  $\eta_p^2 = .14$ . Similar to Stroop results, there was a significant decrease in probe discrimination task scores from baseline to session 4,  $F(4,96) = 6.10$ ,  $p < .001$ ,  $\eta_p^2 = .20$ .

One possible explanation for these results is that performance on both attentional bias tasks are regressing to the mean over time. As an example, suicide ideators with baseline negative scores demonstrated an *increase* in scores. This is further suggested by the significant inverse relationship between baseline scores and baseline–session 4 change scores on both the suicide Stroop,  $r = -0.73$ ,  $p < .001$ , and the probe discrimination tasks,  $r = -0.70$ ,  $p < .001$ . That is, the lower or higher participants' baseline score yielded a greater subsequent increase or decrease by session 4, respectively.

*Testing ABM Efficacy Among Those With More Severe Suicidal Ideation.* Another possibility is that ABM only effectively reduces attentional bias among those more severely suicidal. All 55 participants reported experiencing suicidal ideation within the past month, but these community-based adults reported relatively low severity of thoughts as indicated by a mean Beck SSI score of 10.96 (SD = 7.53). This is significantly lower than the mean SSI score from previous papers with more severe suicidal patient samples, such as Steer, Rissmiller, Ranieri, and Beck (1993) inpatient sample of suicidal adults ( $M = 15.63$ ),  $t(54) = 4.60$ ,  $p < .001$ .

To test ABM efficacy among more suicidal adults, we conducted a median split of session 1 Beck SSI scores and re-ran repeated measures ANOVA only among those above the sample median (Beck SSI score = 12). Among severe suicide ideators ( $n = 26$ ), there was no significant Group  $\times$  Time interaction accounting for change in SSI scores,  $F(1,24) = 0.27$ ,  $p = .61$ ,  $\eta_p^2 = .01$ , nor a significant main effect of Group,  $F(1,24) = 0.01$ ,  $p = .94$ ,  $\eta_p^2 = .00$ . There remained a significant decrease in SSI scores as indicated by the main effect of Time,  $F(1,24) = 10.01$ ,  $p = .004$ ,  $\eta_p^2 = .29$ . When extending this to the 2-week and 2-month follow-up time frames, the results were similar to original analyses except that there was no significant main effect of Time  $F(3, 48) = 1.07$ ,  $p = .37$ ,  $\eta_p^2 = .06$ . To more rigorously test ABM efficacy among a more suicidal sample, and to test the reliability of the results observed in Experiment 1 more generally, we replicated the Experiment 1 study design with a sample of psychiatric inpatients (described below). Notably, one methodological improvement in the replication below is assessing true baseline SSI scores.

*Lack of Association Between Attentional Bias Measures.* One last post-hoc explanation we explored is that the cognitive process that ABM was designed to target may be distinct from suicide Stroop task performance. That is, perhaps the probe discrimination and suicide Stroop tasks capture discrete processes. This would indicate a potential gap in knowledge, as only the latter has previously been shown to directly relate to suicidal thoughts and behaviors (Becker et al., 1999; Cha et al., 2010; Williams & Broadbent, 1986). If these tasks indeed capture discrete aspects of attentional bias, intervening on one may not necessarily change the other or impact suicidal ideation. We may therefore not expect to see a significant relationship between these two tasks.

Indeed, there was little to no correspondence between performance on the suicide Stroop and probe discrimination tasks at baseline,  $r = .04$ ,  $p = .80$ . To check whether this lack of relationship is due to the varying suicide-related and neutral words across these tasks, we compared reaction times only across similar words used (i.e., extracting RTs from probe discrimination task trials with the Stroop words of *suicide*, *dead*, and *funeral*). When the two tasks were compared across the same words, there was a significant inverse association between the two tasks,  $r = -0.38$ ,  $p = .03$ .

## EXPERIMENT 2

### Method

*Participants.* Participants in Experiment 2 were 37 adults ( $M = 41.70$  years;  $SD = 14.77$  years) recently admitted to a psychiatric inpatient unit for suicidal ideation or attempt. A research assistant attended daily clinical rounds on the unit to identify recently admitted patients meeting study inclusion criteria, identical to those listed for Experiment 1. The one exception is that participants *could* be at high or imminent suicide risk, which they often were given their current inpatient status. Fifty-four potential participants were approached for participation, and 37 (68.5%) agreed to do so and provided written informed consent. Participants were randomly assigned to either the Training ( $n = 19$ ) or Control ( $n = 18$ ) ABM group. Three participants did not complete all four sessions because they were discharged before session 4. Two participants withdrew after the first session for other reasons, including: inability to recall the first session and subsequent loss of interest in the study, and not wanting to continue in light of an unpredictable treatment and

discharge schedule. Two participants were excluded because it was later discovered that they were inadvertently administered inconsistent sessions (e.g., one Control session, three Training sessions). The final sample consisted of 15 Training and 15 Control group members who completed all four ABM sessions while in the inpatient unit.

### Procedure

Potentially eligible patients were approached by a hospital-affiliated research assistant, who first checked in on the daily status of the patient with the unit staff members. The research assistant provided information about the study, and gained consent from those interested patients under the supervision of Dr. Matthews. Upon recruitment and consent, participants were escorted by the research assistant to an interview room located within the unit. The research assistant administered the computer tasks and ABM sessions on an encrypted laptop with pre-labeled keys. Measures and procedures remained identical to that of Experiment 1 from this point on, with a few exceptions. First, the self-report measures were administered *before* the first session of ABM to capture true baseline SI severity. Second, the MINI was not administered since psychiatric history was collected from patients' medical records by research assistants with patients' consent. Finally, the probe discrimination task and respective ABM paradigm used four rather than eight conditions. All procedures were approved by the hospital institutional review board.

*Data Analyses.* Data analyses are identical to those from Experiment 1.

### Results

*Group Equivalence at Baseline.* There were no significant baseline differences on demographic factors, psychiatric diagnoses

and treatment, history of self-injurious thoughts and behaviors, or performance on behavioral measures (Table 2). Baseline performance on the probe discrimination task did not vary across the four sets of words used,  $F(3,26) = 0.13, p = .95$ .

*Change in Attentional Bias.* Results from Experiment 2 replicated those observed in Experiment 1. For suicide Stroop performance, there were no significant main effects of Group,  $F(1,21) = 4.06, p = .06, \eta_p^2 = .16$ , or Time,  $F(1,21) = 1.65, p = .21$ ,

**TABLE 2. Baseline Comparison Across Training and Control Group Members, Experiment 2**

Variable	Training ( $n = 15$ )	Control ( $n = 15$ )	Test	Effect Size
Age in years (M $\pm$ SD)	41.5 $\pm$ 15.1	45.9 $\pm$ 14.6	$t(28) = 0.81$	$d = 0.31$
Sex (% female)	60.0	40.0	$\chi^2(1) = 1.20$	$\Phi = 0.20$
<i>Race (%)</i>				
White	73.3	93.3	$\chi^2(2) = 2.70$	$\Phi = 0.30$
Hispanic	13.3	6.7		
Black	13.3	0.0		
Asian	0.0	0.0		
Other	0.0	0.0		
<i>SITB History</i>				
SI severity, Beck SSI (M $\pm$ SD)	13.3 $\pm$ 7.7	12.9 $\pm$ 11.5	$t(25) = -0.10$	$d = -0.04$
SI frequency (M $\pm$ SD)	48.3 $\pm$ 85.8	77.0 $\pm$ 121.8	$t(25) = 0.71$	$d = 0.28$
Nonsuicidal Self-injury (%)	26.7	33.3	$\chi^2(1) = 0.16$	$\Phi = -0.07$
Suicide Plan (%)	66.7	80.0	$\chi^2(1) = 0.68$	$\Phi = -0.15$
Suicide Attempt (%)	60.0	80.0	$\chi^2(1) = 1.43$	$\Phi = -0.22$
<i>Inpatient Services Received (%)</i>				
Pharmacotherapy	100.0	100.0	–	–
Individual Psychotherapy	53.3	40.0	$\chi^2(1) = 0.54$	$\Phi = 0.13$
Group Therapy	80.0	46.7	$\chi^2(1) = 3.59$	$\Phi = 0.35$
Electroconvulsive Therapy	26.7	40.0	$\chi^2(1) = 0.60$	$\Phi = -0.14$
<i>Psychiatric disorders</i>				
Mood disorder (%)	100.0	100.0	–	–
Anxiety disorder (%)	46.7	53.3	$\chi^2(1) = 0.13$	$\Phi = -0.07$
Alcohol/Substance use (%)	53.3	46.7	$\chi^2(1) = 0.13$	$\Phi = 0.07$
# disorders (M $\pm$ SD)	2.5 $\pm$ 1.3	2.3 $\pm$ 1.2	$t(28) = -0.60$	$d = -0.22$
GAF at admission (M $\pm$ SD)	31.1 $\pm$ 5.9	30.6 $\pm$ 3.5	$t(20) = -0.25$	$d = -0.11$
<i>Attentional Bias Tasks (ms) (M <math>\pm</math>SD)</i>				
Suicide Stroop Task	13.5 $\pm$ 48.5	-9.5 $\pm$ 53.6	$t(24) = -1.15$	$d = 0.47$
Probe Discrimination Task	2.3 $\pm$ 50.7	3.8 $\pm$ 14.1	$t(16) = 0.11$	$d = 0.06$

*Note.* M = mean; SD = standard deviation. SITB = Self-injurious thoughts and behaviors. SI = Suicidal Ideation. Beck SSI = Beck Scale for Suicidal Ideation. SI frequency = Number of days of experiencing suicidal ideation over the past year. There were no significant group differences,  $p > .05$ . Additional analyses examining the demographic categories of education ( $\chi^2(3) = 7.20, p = .07, \Phi = 0.49$ ), employment status ( $\chi^2(4) = 0.80, p = .94, \Phi = 0.17$ ), and marital status ( $\chi^2(5) = 5.60, p = .35, \Phi = 0.43$ ) yielded non-significant results. Finally, groups did not differ across suicide Stroop and probe discrimination task error rates,  $t(28) = 0.85-1.36, ds = 0.32-0.51, ns$ .

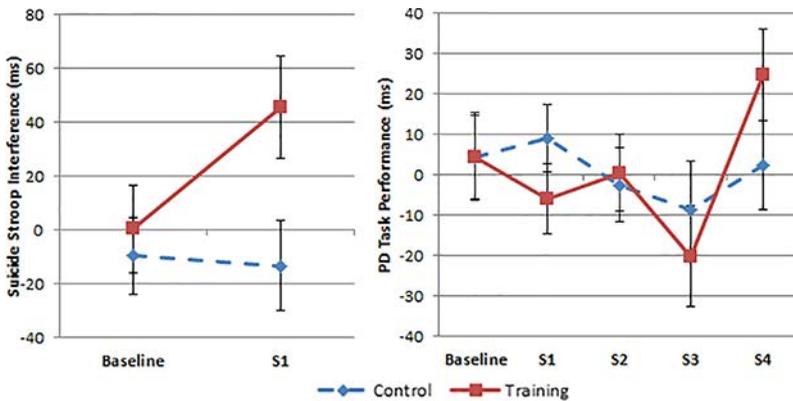
$\eta_p^2 = .07$ , and no significant Group  $\times$  Time interaction,  $F(1,21) = 2.30$ ,  $p = .14$ ,  $\eta_p^2 = .10$  (Figure 2a). For probe discrimination task, there were no significant main effects of Group,  $F(1,25) = 0.002$ ,  $p = .97$ ,  $\eta_p^2 = .00$ , or Time,  $F(3,72) = 1.78$ ,  $p = .16$ ,  $\eta_p^2 = .07$ , and no significant Group  $\times$  Time interaction,  $F(3,72) = 0.94$ ,  $p = .42$ ,  $\eta_p^2 = .04$  (Figure 2b). Also replicating Experiment 1, sessions 1–4 change in suicide Stroop and probe discrimination task performance was not associated with short-term SI severity change,  $r_s = -0.12$ – $0.04$ ,  $p_s = .61$ – $.83$ .

*Change in Suicidal Ideation.* Results from Experiment 2 again replicated those from Experiment 1 regarding change in SI severity, revealing no significant Group  $\times$  Time interaction,  $F(1,24) = 0.28$ ,  $p = .61$ ,  $\eta_p^2 = .01$ , or significant main effect of Group,  $F(1,24) = 0.04$ ,  $p = .85$ ,  $\eta_p^2 = .002$ . Similar to Experiment 1, there was a short-term decrease in SI severity over time,  $F(1,24) = 15.60$ ,  $p = .001$ ,  $\eta_p^2 = .39$ . SI severity significantly decreased from baseline thru session 4 in both the Training group,  $t(11) = 2.48$ ,  $p = .03$ ,  $d = 0.75$ , and

Control group,  $t(13) = 3.35$ ,  $p = .01$ ,  $d = 0.46$ . There were no significant differences between groups at either time point,  $t_s = 0.10$ – $0.24$ ,  $p_s = .82$ – $.92$ ,  $d_s = 0.04$ – $0.09$ .

Seven participants completed both follow-up assessments. As in Experiment 1, the Group  $\times$  Time interaction,  $F(3,15) = 1.20$ ,  $p = .34$ ,  $\eta_p^2 = .19$ , and main effect of Group,  $F(1,5) = 0.95$ ,  $p = .37$ ,  $\eta_p^2 = .16$ , were nonsignificant in predicting longer-term SI severity. There was a nonsignificant trend of SI severity reduction,  $F(3,15) = 2.80$ ,  $p = .08$ ,  $\eta_p^2 = .36$ . Finally, the Training group was no more likely to report suicidal ideation 2 weeks later (44.4%) compared to the Control group (50.0%),  $\chi^2(1) = 0.05$ ,  $p = .82$ ,  $\Phi_s = -.06$ . Two months later, the Training and Control group were equally likely (77.8%) to have experienced suicidal ideation with that follow-up period.

*No Influence of Placebo Effect or Contingency Awareness.* Out of the ten participants who claimed that they knew which version of ABM they received ( $n = 7$  from the Training group), neither group was more likely to guess their assignment correctly,



**FIGURE 2.** ABM effects on suicide Stroop and probe discrimination task performance, Experiment 2. Note. ABM = Attention bias modification; PD task = probe discrimination task; ms = milliseconds; Baseline = Task performance immediately before session 1; S1–S4 = Task performance immediately after sessions 1–4, respectively. Error bars represent standard error.

$\chi^2(1) = 0.47, p = 1.00, \Phi = -.22$ . Training group participants were not more likely than the Control participants to: notice anything unusual,  $\chi^2(1) = 2.89, p = .14, \Phi = -.32$ , notice any pattern,  $\chi^2(1) = 0.29, p = .59, \Phi = -.10$ , or be able to identify which version of ABM they received,  $\chi^2(1) = 2.04, p = .25, \Phi = .27$ .

*Testing ABM Efficacy Among Those With Elevated Baseline Bias.* We tested the same post-hoc explanations for our null results from Experiment 1, beginning with the possibility that ABM efficacy can only be demonstrated among those with elevated bias. We re-ran analyses among eight patients reporting positive suicide Stroop scores, and found no significant Group  $\times$  Time interaction,  $F(1,6) = 1.43, p = .28, \eta_p^2 = .19$ , or main effect of Time,  $F(1,6) = 2.46, p = .17, \eta_p^2 = .29$ . Diverging from Experiment 1 findings, there was a significant main effect of Group,  $F(1,6) = 19.46, p = .01, \eta_p^2 = .76$ . Simple effects analyses revealed that there was no significant difference in suicide Stroop performance between groups at baseline,  $t(9) = 0.96, p = .36, d = 0.64$ , but that the Training group demonstrated significantly *higher* suicide Stroop interference at session 4 compared to the Control group,  $t(6) = 2.45, p = .049, d = 2.00$ —counter to what we had expected. There was no change in baseline–session 4 suicide Stroop performance in either the Training or Control groups,  $ts = 0.21\text{--}2.41, ps = .07\text{--}.85, d = 0.23\text{--}1.94$ .

Results for the probe discrimination task were similar. Among 15 ideators demonstrating a positive baseline score on the probe discrimination task, there was no significant Group  $\times$  Time interaction,  $F(4,52) = 2.38, p = .06, \eta_p^2 = .15$ , or main effect of Group,  $F(1,13) = 0.96, p = .34, \eta_p^2 = .07$ . There was, however a significant main effect of Time,  $F(4,52) = 3.48, p = .01, \eta_p^2 = .21$ .

*Lack of Association Between Attentional Bias Measures.* The last post-hoc explanation explored in Experiment 2 was the possibility that the suicide Stroop and probe discrimination tasks capture distinct features of attentional bias. The results indeed suggest this, adding to Experiment 1 findings. The correlation between baseline suicide Stroop and probe discrimination task performance was nonsignificant,  $r = .08, p = .70$ . Results did not change when comparing scores calculated from suicide Stroop and probe discrimination trials involving identical words (*suicide, dead, funeral*),  $r = .13, p = .69$ .

## Discussion

Thus far, there is no evidence that the ABM intervention can change suicide-specific attentional bias or severity of suicidal ideation. This held true in the present study among community-based ideators and suicidal inpatients, and across both a suicide Stroop and suicide probe discrimination tasks. There are several potential explanations for these null results: *intervention implementation* and *intervention selection*. We discuss each of these in turn.

*Intervention Implementation.* One possibility is that the current intervention failed to provide the optimal conditions under which ABM changes attentional bias or clinical outcomes. Perhaps this study involved too few sessions of ABM to have an effect, for instance. Prior studies have revealed a dose-response relation between number of ABM sessions and attentional bias change (Beard, Sawyer, & Hofmann, 2012; Hakamata et al., 2010; Hallion & Ruscio, 2011). Some of the strongest evidence for ABM efficacy has been obtained using eight (Amir, Beard, Burns et al., 2009; Amir, Beard, Taylor et al., 2009), to

15 sessions (See, MacLeod, & Bridle, 2009); whereas we were only able to provide four sessions due to the relatively short length of inpatient hospitalizations, and limits on the feasibility of getting suicidal people to return to more than four lab sessions. It is possible that more sessions of ABM are required to in fact change suicide-specific attentional bias. Relatedly, we limited the length of each session to 20 minutes, and scheduled the sessions across four consecutive days. This contrasts with some successful ABM studies where each session lasted as long as 50 minutes, and was administered across the span of weeks or months (see Bar-Haim, 2010).

There are several reasons why less-than-optimal treatment conditions do not fully explain our null results. First, even minimal amounts of ABM—less than what was administered in the current study—have been shown to change attentional bias. Amir, Weber, Beard, Bomyea, and Taylor (2008), for instance, reported decreased attentional bias toward socially threatening stimuli immediately following a single eight-minute ABM session for socially anxious individuals. While it may be more optimal to administer multiple sessions, even one session of this intervention should result in some bias change. Moreover, there was no suggested trend in a dose-response relationship across ABM sessions in the current studies. If the number of sessions was the primary reason for null results, there should be some suggestion that attentional bias decreases in the ABM group across time. Among the ABM group, there was no observed bias decrease across four sessions. Instead, trends suggested that *fewer* sessions (i.e., one or three sessions, not four) better delineated bias change across ABM and control conditions. And when examining all four sessions of ABM in Experiment 2, there was in fact a trending *increase* in suicide-specific attentional bias as

measured by both the suicide Stroop and probe discrimination tasks.<sup>2</sup>

While implementation failure is one possible explanation, we caution dismissal of null findings solely on these grounds. Poor implementation may be a more viable criticism of ABM studies that fail to change anxiety-specific attentional bias and symptoms (Clarke, Notebaert, & MacLeod, 2014)—an area in which the effectiveness of ABM has been more firmly established. As described below, not all psychopathology, nor their underlying information processing biases, may be created alike.

*Intervention Selection.* The most likely explanation for our null results is that ABM was not the ideal intervention to change suicide-specific attentional bias, as measured by the probe discrimination task. Suicidal individuals may be characterized by a bias that is distinct from the one that ABM has been designed to target. This is highlighted by the fact that suicide Stroop performance did not correspond with the probe discrimination task—the original behavioral task from which ABM is derived. This observed incongruence across attentional bias measures also has been found in recent studies with other clinical samples. Dalgleish et al. (2003) examined emotional Stroop and dot probe task performance among children with mood and anxiety disorders, and reported no significant association. Similarly, Asmundson, Wright, and Hadjistavropoulos (2005) demonstrated a lack of association between these two tasks measuring attentional bias toward pain words.

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<sup>2</sup>It remains unknown why the Training condition of Experiment 2 demonstrated a trending increase in suicide-specific attentional bias. This counters the typical pattern of *both* Training and Control conditions decreasing attentional bias. Although these increases within the Training condition were not significant, these medium effect sizes warrant attention and further investigation in the future.

In line with this observed incongruence, cognitive psychology theories and evidence suggest that the Stroop and probe discrimination tasks capture distinct sub-components of attentional bias. This is plausible given how phenomenologically different one is from the other (Cislar, Bacon, & Williams, 2009). On the one hand, the Stroop task draws attention toward two dimensions of a single word (font color vs. semantics). Cognitive psychologists argue that this form of attentional bias captures *dimensional separability* of a single object (Garner, 1962, 1974). On the other hand, the probe discrimination task and related dot probe task require spatial scanning across two words (presented on top vs. bottom). This form of attentional bias captures *spatial separability* (Posner, 1978, 1980). Prior work on the Garnerian and Posnerian models of selective attention increasingly suggests these theories and behavioral tasks capture separate forms of attentional bias (Chajut, Schupak, & Algom, 2009; Shalev & Algom, 2000). Since ABM was originally designed to change spatially-oriented Posnerian attentional bias, it conceivably may not be able to change the type of Stroop-based Garnerian form of attentional bias that has been shown to predict suicidal behaviors.

As a result of the unsuccessful experimental manipulation, it remains unclear whether change in attentional bias corresponds to change in suicide-related outcomes. Without modifying the targeted risk factor at hand, experimental demonstration of causality is not possible (Kraemer et al., 1997). Importantly, the fact that suicide-specific attentional bias was not modified here does not definitively mean that it cannot be changed. It remains possible that suicide-specific attentional bias, as captured by the suicide Stroop task, is a causal risk factor. We recommend the examination of alternative methods of testing the malleability of suicide-specific

attentional bias. Future studies utilizing paradigms that more precisely target change in emotional Stroop-specific processes may inform interpretation of the current findings.

There are several other findings from the current investigation worth noting. Among all participants, severity of suicidal ideation significantly decreased across 2 months in the first study, and across the four in-person meetings in the second study. Changes in suicidal ideation are typically sensitive and meaningful, such that they correspond with changes in depression and hopelessness (Beck, Kovacs, & Weissman, 1979). It is possible that participants experienced a reduction in suicidal ideation due to developing rapport with experimenters across repeated meetings, or conversing with experimenters at the end of each meeting to assess suicide risk. Anecdotal evidence also suggests that participants enjoyed knowing they were contributing to this area of research. On balance, this reduction may represent regression to the mean, an alternative explanation that we cannot rule out.

Finally, and fortunately, neither study produced evidence that participants could identify the experimental manipulation (training vs. control). That is, participants could not consciously identify ABM as a form of treatment. In adherence with the double-blind design, experimenters did not know participants' assigned condition. This marks a significant advantage that CBM studies have overall compared to psychotherapy RCTs, where therapists are typically aware of whether they are delivering a treatment or placebo intervention. And counter to suggestions that expectancy and demand effects help facilitate the effectiveness of ABM (Emmelkamp, 2012), our findings highlight the absence of mechanisms that would facilitate such demand characteristics.

These results should be interpreted in light of several limitations. First, each study

included a small sample. Increasing sample sizes would allow us to compare whether the association between the pre-training Stroop and probe discrimination task performance varies as a function of which sets of words were used in the probe discrimination task (e.g., A1, A2, B1, B2). On balance, the very small effect sizes obtained suggest that the null results were not due to insufficient power.

Second, the current findings do not necessarily generalize to all suicide-related outcomes. These two studies focused on suicidal ideation, a relatively milder outcome compared to what has been examined in previous studies on suicide-specific attentional bias. Outcomes from earlier studies have ranged from overdosing (Williams & Broadbent, 1986) to suicide attempt more broadly (Becker et al., 1999; Cha et al., 2010). It is possible that ABM may effectively change suicide-specific attentional bias among suicide attempters, and that measures of suicide-specific attentional bias may be more sensitive to change among attempters.

We recommend several important directions for future research. First, the field must tease apart whether these nonsignificant ABM effects are truly due to implementation or selection of the ABM intervention. There are several ways to do this. To address intervention implementation concerns, future work could test the efficacy of longer, more frequent, and more dispersed ABM sessions. Demonstration of ABM efficacy for suicide ideators under these more optimal conditions would not only clarify whether it works, but how it works.

To address the concern about intervention selection, future work may explore alternative methods of attention training. Perhaps an intervention targeting the sub-component of attentional bias captured by the suicide Stroop task will reduce bias and suicide-related outcomes. The suicide Stroop task could be used as an intervention just as the probe discrimination task has

been manipulated into an intervention. MacLeod (1998), for instance, demonstrated that 10 consecutive sessions of 288 traditional Stroop trials can in fact significantly reduce interference scores across time among nonclinical samples. It may be worth applying this to suicidal individuals.

Finally, we as field must continue using experimental designs to clarify the causal role of risk factors. Enforcing this methodological rigor creates opportunity for theory falsification and clarification—a key component to upholding the credibility of psychological science (Ferguson & Heene, 2012). The rigor of experimental designs likely increases the chance of achieving null results, but the practice of demonstrating what does not work is just as important as demonstrating what does. We encourage future work to use these null findings to inform modification and advancement in this important area of research.

#### AUTHOR NOTE

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