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Untangling attention bias modification from emotion: A double-blind randomized experiment with individuals with social anxiety disorder

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ABSTRACT

Background: Uncertainty abounds regarding the putative mechanisms of attention bias modification (ABM). Although early studies showed that ABM reduced anxiety proneness more than control procedures lacking a contingency between cues and probes, recent work suggests that the latter performed just as well as the former did. In this experiment, we investigated a non-emotional mechanism that may play a role in ABM.
Methods: We randomly assigned 62 individuals with a DSM-IV diagnosis of social anxiety disorder to a single-session of a non-emotional contingency training, non-emotional no-contingency training, or control condition controlling for potential practice effects. Working memory capacity and anxiety reactivity to a speech challenge were assessed before and after training.
Results: Consistent with the hypothesis of a practice effect, the three groups likewise reported indistinguishably significant improvement in self-report and behavioral measures of speech anxiety as well as in working memory. Repeating the speech task twice may have had anxioltyic benefits.
Limitations: The temporal separation between baseline and post-training assessment as well as the scope of the training sessions could be extended.
Conclusions: The current findings are at odds with the hypothesis that the presence of visuospatial contingency between non-emotional cues and probes produces anxiolytic benefits. They also show the importance of including a credible additional condition controlling for practice effects.

1. Introduction

Recently, a growing body of research has accumulated on a new treatment for reducing social anxiety disorder (SAD), called attention bias modification (ABM). ABM builds upon cognitive theories of psychopathology that implicate attentional bias (AB) for social-threat cues, such as faces expressions anger or disgust, in the maintenance, and perhaps the etiology, of SAD (Morrison & Heimberg, 2013). The clinical purpose of ABM is to reduce excessive AB, thereby diminishing anxiety symptoms (MacLeod & Mathews, 2012).

The most common ABM procedure is a modification of the visual dot-probe task (MacLeod, Rutherford, Campbell, Ebsworthy, & Holker, 2002) based on the classic work of MacLeod, Mathews, and Tata (1986). In the original version of the task (MacLeod et al., 1986), participants viewed two stimuli (e.g., a threatening word/photograph and a neutral word/photograph) presented in two distinct locations (left/right or up/down) on a computer screen for a brief duration (usually 500 ms). Immediately thereafter, a probe appeared in the location previously occupied by one of the two stimuli. In different versions, participants had to indicate the location of the probe (right/left or up/down) or to indicate the identity of the probe (e.g., “E” or “F”) as quickly as possible. An AB was demonstrated when participants responded faster to the probe when it replaced a threatening stimulus than when it replaced a nonthreatening stimulus, indicating that their attention was directed to the location occupied by the threatening stimulus.

In ABM, researchers typically modify the original task so that the probe nearly always (e.g., 95% of the trials) replaces the neutral or positive stimulus, thereby redirecting subjects' attention to nonthreatening cues. In the control condition, there is no contingency between cues and probes. Relative to the control condition,

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ABM reduces symptoms in people with SAD, as several studies have shown (e.g., Amir et al., 2009; Heeren, Reese, McNally, & Philippot, 2012; Li, Tan, Qian, & Liu, 2008; Schmidt, Richey, Buckner, & Timpano, 2009). These findings have suggested that ABM could have important clinical potential for treating SAD, as it entails a very simple protocol, little effort and motivation from the patient, little contact with a mental health professional, and can be easily disseminated.

However, despite these promising initial results, recent evidence suggests that the picture may be more complicated than initially thought as several studies with inconsistent findings have been published. More specifically, some studies have shown that ABM and the no-contingency condition did not significantly differ at post-training, neither for AB nor for anxiety symptoms (e.g., Julian, Beard, Schmidt, Powers, & Smits, 2012; McNally, Enock, Tsai, & Tousian, 2013). On measures of anxiety, socially anxious participants in the control group exhibited statistically significant improvement indistinguishable from that of participants in the ABM group. Several explanations have been formulated.

According to Klumpp and Amir (2010), such a training procedure, regardless of the direction of the contingency between emotional probes and cues, may bolster top-down executive control in ways that strengthen one's ability to reduce anxiety proneness. In an experiment providing data congruent with this hypothesis, they randomly allocated moderately socially anxious individuals to one of three different conditions: (1) training to attend to non-threat (i.e., ABM), (2) attend to threat, or (3) a control condition in which there was no contingency between cues and probes. After a single-session, individuals who were trained to attend to threat as well as those receiving ABM reported less state anxiety in response to an impromptu speech compared to individuals in the no-contingency control condition.

An alternative account is that attention training is effective to bolster top-down control in ways that reduce anxiety regardless of the presence of a contingency. Accordingly, McNally et al. (2013) reported an experiment in which they randomly assigned speech-anxious individuals to one of the three training conditions mentioned above while also including self-report and behavioral measures of executive attention control before and after the training. After four sessions of training, participants, irrespective of group assignment, exhibited significant decreases in self-report, behavioral, and physiological measures of anxiety associated with a speaking task. More importantly, all three training conditions improved attentional control. Heeren, Mogoaşe, McNally, Schmitz, and Philippot (2015) corroborated these findings.

Finally, several authors have suggested a third explanation (e.g., Cristea, Kok, & Cuijpers, 2015; Emmelkamp, 2012). Because ABM and the no-contingency condition performed indistinguishably well, one cannot rule out the possibility of mere practice/test-retest effects. Indeed, all three groups in the McNally et al. (2013) experiment improved on multiple measures of anxiety, and this finding is consistent with a practice effect or a placebo effect. Merely undergoing the speech task twice may have reduced anxiety in all three groups. Alternatively, positive expectancy or placebo effects may be engendered by any sort of computerized training that participants believe may help them. A positive expectancy fostered by such training may encourage socially anxious subjects to engage in previously-avoided social activities, emboldened by the belief that training has equipped them to enter social situations with ease and confidence. Consequently, repeated exposure to previously-avoided situations would likely diminish their distress and correct any problematic beliefs that can sustain social anxiety. Consistent with this possibility, Enock, Hofmann, and McNally (2014) found that highly socially anxious subjects who were randomized to either ABM or no-contingency conditions exhibited indistinguishably larger reductions in self-reported anxiety symptoms than did individuals in a wait-list control group. Subjects who merely completed online questionnaires without any sort of training at all did not improve.

As a consequence, these puzzling findings raise questions about the mechanisms of ABM's effectiveness. Moreover, the understandable focus on AB for emotional stimuli has led to neglect of other non-emotional mechanisms that may drive ABM (Heeren, De Raedt, Koster, & Philippot, 2013). However, regardless of their emotional valence, repeated exposure to pairs of faces, such as those in most ABM studies for SAD, may act as a traditional exposure therapy as may the speech challenge tests that some investigators have used.

Hence, the main aim of the present study was to examine the impact of both contingency-based and no-contingency-based ABM paradigms that do not involve any emotional material, but rather involve geometric shapes devoid of emotional significance on top-down executive control of attention and on anxiety. In the present double-blind experiment, we randomly assigned 62 individuals with a DSM-IV diagnosis of SAD to one of three conditions: 1) a non-emotional attention training with a contingency between cues and probes (hereafter called the “Contingency Condition”), 2) a non-emotional attention training without such a contingency (No-contingency condition), and 3) a Control condition (a mere discrimination task to control for test-retest effects). Rather than using a wait-list control group, we used this third condition to maintain optimal blinding of both the assessors and the participants.

We had several predictions. First, if attention training is effective because of increased attentional control arising from any contingency-based procedure regardless of the direction of attention, then participants in the contingency condition should exhibit greater improvement than participants in the two other conditions on measures of top-down control as well as measures of anxiety. By constrast, if attention training is effective regardless of the presence of a contingency, the non-emotional training with a contingency and the non-emotional training without a contingency should exhibit greater improvement than should the control condition. Finally, if improvements in both top-down control and anxiety result from a practice/test-retest effect, all groups should exhibit improvement.

2. Method

2.1. Participants

We recruited 62 individuals with a primary DSM-IV diagnosis of Generalized SAD (American Psychiatric Association, 1994) from the Université Catholique de Louvain community. To guard against placebo (expectancy) effects, we did not inform participants of any potential anxiolytic benefits of the training procedures. A total of 603 volunteers responded to our invitation to participate in a study investigating the mechanisms underlying social interaction among shy people. As depicted in Fig. 1, 77 individuals met the initial eligibility criteria as assessed via a screening questionnaire. These criteria were (a) scoring above 56 on the self-report version of the Liebowitz Social Anxiety Scale (LSAS; Liebowitz, 1987), (b) having no current substance abuse or dependence, (b) having no current heart, respiratory, neurological problems, or use of psychotropic medications, (c) having no current psychological or psychiatric treatment, and (d) having normal or corrected-to-normal vision. Subsequently, these 77 individuals completed a structured interview to assess diagnostic eligibility. To confirm the diagnosis of Social Anxiety Disorder, we administered the social phobia section of the Mini International Neuropsychiatric Interview (MINI;

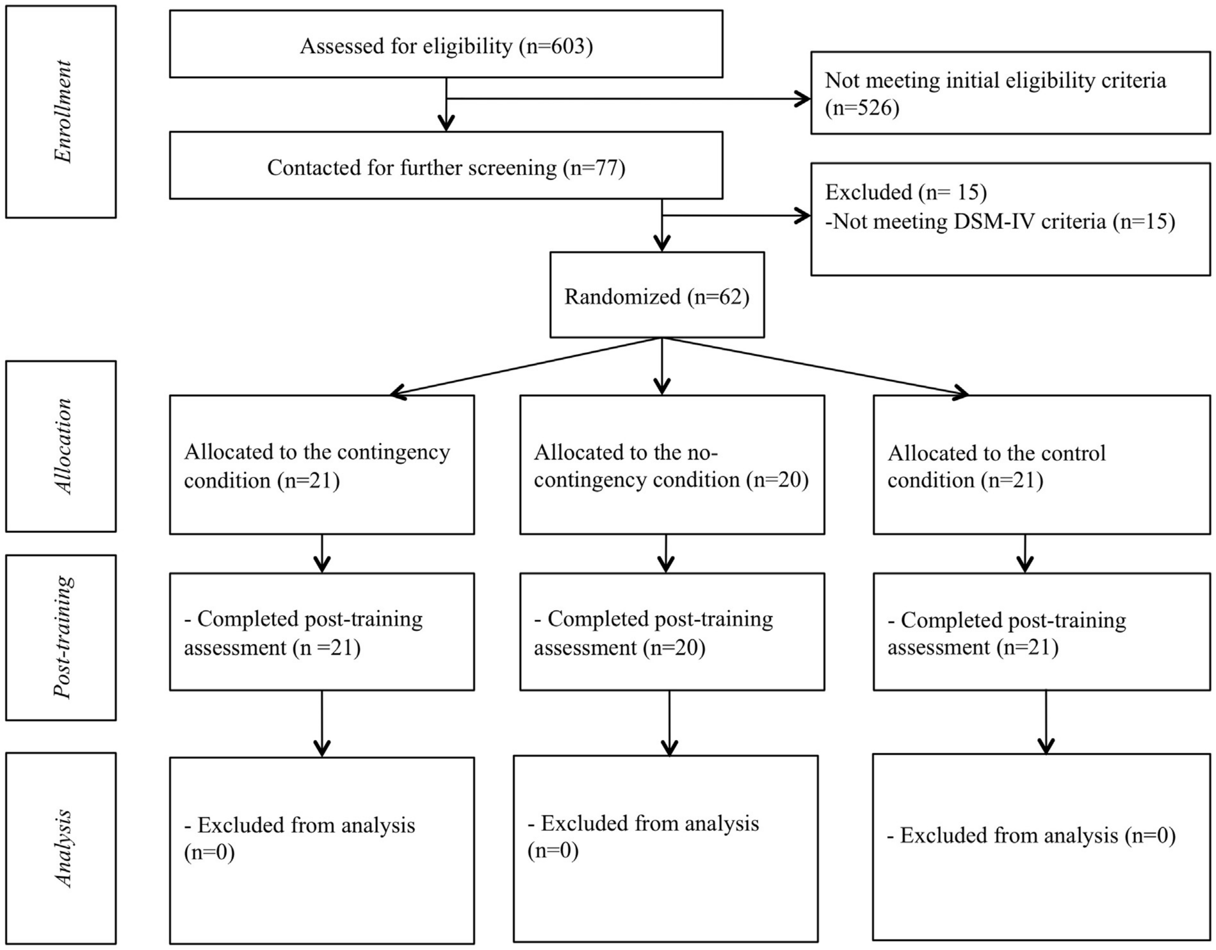

Fig. 1. Flowchart depicting passage of participants through the study. *Note.* Contingency, training with a contingency between non-emotional cues and probes; No contingency, training without contingency between non-emotional cues and probes; Control, a discrimination task to control for potential practice effect.

Lecrubier, Weiller, Bonora, Amorin, & Lépine, 1998). Of these 77 participants, 62 met the criteria for DSM-IV diagnosis of Social Anxiety Disorder and were included in the study. Their characteristics are listed in Table 1.

2.2. Measures

2.2.1. Questionnaires

Participants were screened via the self-report version of the LSAS (Liebowitz, 1987), and they also completed the Trait Anxiety Inventory (STAI-T; Spielberger, Gorsuch, Lushene, Vagg, & Jacobs, 1983), the Beck Depression Inventory (BDI-II; Beck, Steer, & Brown, 1996), and the short version of the Personal Report of Confidence as a Speaker scale (PRCS; Hook, Smith, & Valentiner, 2008) prior to beginning the experiment. The LSAS is a 24-item scale that measures anxiety and avoidance of social interactions and performance situations. The STAI-T is a 20-item self-report questionnaire assessing anxiety proneness. The BDI-II is a 21-item self-report measure of symptoms of depression. The PRCS is a 12-item self-report measure of public speaking fear. We used the validated French versions of these scales (LSAS, Heeren, Maurage, et al., 2012; BDI-II, Beck, Steer, & Brown, 1998; STAI-T, Bruchon-

Table 1
Participants' characteristics as a function of training allocation (*SD* in parentheses).

	Contingency (n = 21)	No-contingency (n = 20)	Control (n = 21)	*F* or χ^2	*p*
Age	21.19 (2.45)	22.45 (6.34)	23.52 (7.92)	0.79[a]	0.46
%Female	86%	89%	85%	1.17[b]	0.56
Years of education	9.05 (2.60)	10.45 (3.73)	8.95 (2.69)	0.46[a]	0.24
BDI-II	16.86 (8.82)	14.20 (11.67)	16.71 (6.80)	0.53[a]	0.59
STAI-T	51.14 (9.63)	49.75 (11.55)	52.76 (8.89)	0.46[a]	0.63
PRCS	8.40 (2.13)	8.18 (2.31)	8.95 (2.30)	0.67[a]	0.52
LSAS	67.76 (9.54)	70.80 (13.18)	68.61 (10.51)	0.40[a]	0.67

Note. Contingency, training with a contingency between non-emotional cues and probes; No-contingency, training without contingency between non-emotional cues and probes; Control, a discrimination task to control for potential practice effect; BDI-II, Beck Depression Inventory-II; STAI-T, Spielberger State-Trait Anxiety Inventory-Trait; PRCS, Personal Report of Confidence as a Speaker scale; LSAS, Liebowitz Social Anxiety Scale. Education level was assessed according to the number of years of education completed after finishing primary school.

[a] Value for *F*(2, 59).

[b] Value for $\chi^2(2, N = 62)$.

Schweitzer & Paulhan, 1983; PRCS, Heeren, Ceschi, Valentiner, Dethier, & Philippot, 2013).

2.2.2. Measures of top-down control

We assessed top-down control using the Backward Digit Span Task (BDST) from the WAIS-IV (Wechsler, 2009). A list of numbers was read out loud at the rate of one per second. Participants were instructed to recall the items in the reverse of the presented order. The test begins with two or three numbers, increasing until the participants committed errors (with a maximum of eight numbers). Each level of item includes two trials with a score ranging from zero (*no correct recall*), one (*one correct recall*) to two (*two correct recalls*). The total score thus ranges from 0 to 16. Two lists of items were counterbalanced between times of assessment.

2.2.3. Speech task

We administered a speech task to assess self-report and behavioral measures of anxiety at baseline and post-training. Participants were informed that they would have to make a 2-min speech concerning controversial topics widely discussed in the Belgian media, and that their performances would be video recorded. Two topics (i.e., euthanasia in children and the dissolution of the Belgian country through the separation of the Dutch-speaking from the French-speaking regions) were counterbalanced between times of assessment. Participants were given 2 min to prepare and a sheet of paper to write down their notes; however, they were told that they would not be allowed to use these notes during the speech. After participants had prepared their speech, they were directed to stand in front of a video camera. Just before the speech, the experimenter asked participants to rate, using the *Subjective Units of Discomfort Scale* (SUDS; Wolpe, 1958), their level of situational anxiety from 0 (*not anxious*) to 100 (*extremely anxious*). The participant then performed the speech while being video recorded. Two clinical psychologists, blind to training condition and time of assessment, used the *Behavioral Assessment of Speech Anxiety* (BASA; Mulac & Sherman, 1974) method to later rate the speech of the participants based on the video recordings. The BASA includes 18 molecular categories (e.g., having a clear voice, searching for the words), and the mean score of these categories has excellent concurrent validity with experts' ratings of speech anxiety (Mulac & Sherman, 1974). Interrater reliability of the total score was high ($r = .89$, $p < .001$ at baseline; $r = .86$, $p < .001$ at post-training). Accordingly, we averaged the scores of the two raters. The same two raters assessed both the baseline and post-training speeches of a participant.

2.3. Attention training

We used a modified probe discrimination task to train attention. For all conditions, each trial began with a fixation-cross presented in the center of the screen for 500 ms. With the exception of the control condition, in the two training conditions, the fixation-cross was followed by a 500 ms-presentation of two similar grey-filtered geometric shapes (e.g., two grey squares), one appearing below the center of the screen, and the other appearing above the center of the screen. One of the two shapes had a white dot in the middle of it, whereas the other did not. The geometric stimuli were eight circles, eight triangles, eight squares, eight rectangles, eight ellipses, eight pentagons, eight hexagons, and eight diamonds. Illustrations of these stimuli are provided in Fig. 2 (part A). A probe then appeared (i.e., “E” or “F”), replacing one of the shapes. It remained on the screen until the participant indicated its identity by pressing the corresponding key. The inter-trial interval was 1500 ms.

Participants completed 512 trials in one block. In the contingency condition, the probe replaced the geometric shape with the white dot on 95% of the trials. In the No-contingency training, the probe replaced each shape on 50% of the trials (i.e., no contingency between cues and probes). In these two conditions, each of the 64 pairs of geometric shapes appeared four times, in positions that represented all combinations of the locations and probe types. This procedure was repeated two times (i.e., 512 = 64 stimuli × 2 positions × 2 letters × 2 repetitions).

In the control condition, participants also completed 512 trials in one block. However, they saw a fixation cross for 500 ms followed by a blank white screen for 500 ms, and then a probe (i.e., “E” or “F”) appeared to the top (or the bottom) of the center screen. The inter-trial interval was also 1500 ms. On each trial, their task was simply push a key to indicate whether an E or an F was present. Hence, instead of seeing a pair of geometric shapes, they merely saw a blank screen before indicating the identity of the probe.

We used E-Prime 2 Professional® (Psychology Software Tools, Pittsburgh, PA, USA) to program the task, and we ran it on a Windows XP computer with a 75 Hz, 19-inch color monitor. Participants were instructed to indicate whether the probe was an E or an F by pressing the corresponding button on the keyboard with a finger of their dominant hand.

2.4. Procedure

Participants were randomly assigned to one of the three conditions via a computerized randomization system. The participants and the experimenters were blind to condition. Each participant was tested individually in a quiet room. Participants first performed the BDST and the speech task. For each time point, the order of the administration of these two measurements was counterbalanced across participants. For the speech task, two different topics were counterbalanced between times of assessment. Participants were assigned to one of the two topics for the first speech (and always to the other topic for the second speech). Next, participants completed the training session, lasting about 30 min. After the training, participants completed the second BDST and the second speech task. All participants provided their written informed consent. The study was approved by the Ethical Committee of the Medical School and conducted according to the Declaration of Helsinki. Participants were debriefed at the end of the experiment and received compensation (5 euros and a lottery ticket).

3. Results

3.1. Power analysis

An *a priori* power analysis was conducted to determine the appropriate total sample size for testing hypotheses concerning the primary outcome variables. Based upon recent meta-analysis on the benefits of ABM on anxiety among anxious individuals (e.g., Linetzky, Pergamin-Hight, Pine, & Bar-Haim, 2015), we expected a small-to-medium effect size of $d = 0.42$ (Cohen, 1988). Setting α at .05, power ($1-\beta$) at .80, and expecting a conservative correlation of $\rho = .50$ between repeated measurements, the power analysis (G*Power 3.1.3; Faul, Erdfelder, Lang, & Buchner, 2007) indicated that a least 19 participants per group would yield an adequate power to detect a small-to-medium effect size. These results thus confirmed that the present study has enough statistical power to detect a small-to-medium effect size.

3.2. Group equivalence

As shown in Table 1, the groups did not differ at baseline on the STAI-Trait, BDI-II, PRCS, or LSAS, and were indistinguishable in terms of age, gender, and years of education.

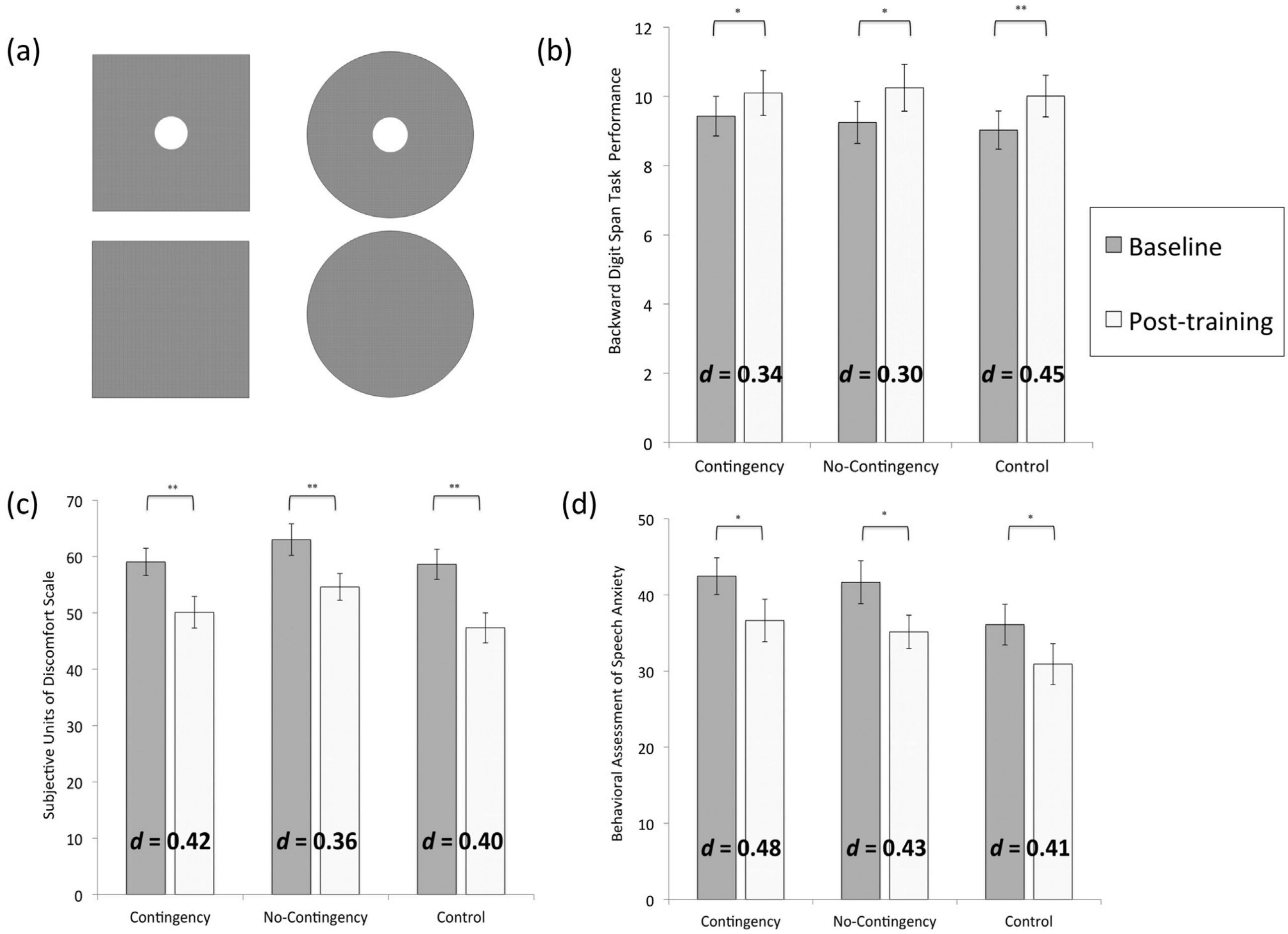

Fig. 2. Illustration of the Stimuli and Change in Outcome Measurements as a Function of Condition and Time. *Note.* (a) Illustration of the geometric shapes used in the experimental manipulation. (b) Scores for the Backward Digit Span Task. (c) Scores for the Subjective Units of Discomfort Scale. (d) Scores for the Behavioral Assessment of Speech Anxiety (mean of the two raters). Error bars represents standard errors of the mean. *d*, effect size (using pooled variance) indexing change from baseline to post-training using the Cohen's formula (1988) for paired *t*-tests; *, $p < .05$; **, $p < .01$ (within-condition paired *t*-tests).

3.3. Change in top-down control

We subjected participants' BSDT performances to a 3 (Condition) x 2 (Time: Baseline, post-training) ANOVA with repeated measurement on the second factor. The ANOVA revealed a main effect of *Time*, $F(1, 59) = 25.08$, $p < .001$, $\eta_p^2 = .30$, but no significant *Time* × *Condition* interaction, $F(2, 59) = 1.53$, $p = .23$, $\eta_p^2 = .05$. As depicted in Fig. 2 (part B), all groups exhibited a small, but significant, improvement in their speech performance from baseline to post-training.

3.4. Change in emotional reactivity to speech task

For the SUDS and BASA data, we computed separate 3 (Condition) x 2 (Time: Baseline, post-training) ANOVAs with repeated measurement on the second factor. For the SUDS ratings, the ANOVA revealed a main effect of Time, $F(1, 59) = 13.46$, $p < .001$, $\eta_p^2 = .19$, but no significant *Time* × *Condition* interaction, $F(2, 59) = 0.46$, $p = .63$, $\eta_p^2 = .02$. For the BASA scores, again, the ANOVA revealed a main effect of *Time*, $F(1, 59) = 21.21$, $p < .001$, $\eta_p^2 = .26$, but no significant *Time* × *Condition* interaction, $F(2, 59) = 0.13$, $p = .88$, $\eta_p^2 < .01$. As depicted in Fig. 2 (parts C and, D, respectively), all groups exhibited a significant medium-sized decrease in both self-reported and behavioral measures of anxiety between the baseline and post-training speeches.

3.5. Complementary analyses

As several authors suggest (e.g., Heeren, Ceschi, et al., 2013; Klumpp & Amir, 2010), training-induced improvement in attention control may attenuate negative emotional reactivity to the post-training speech task. Accordingly, we computed Pearson correlation coefficients between the former (i.e., post-training minus baseline score) and latter variables (i.e., post-training minus baseline score) for both the SUDS and BASA measures and performance on the BDST. However, the correlations were neither significant for the SUDS [$r(62) = -.22$, $p = .10$] nor for the BASA [$r(62) = .19$, $p = .14$].

4. Discussion

The main aim of the present study was to examine the anxiolytic effects of a visuospatial contingency between non-emotional cues and probes in socially anxious individuals. Irrespective of their group assignment, participants reported statistically significant reductions on self-report and behavioral measures of anxiety while delivering their speech at the post-training assessment relative to the baseline assessment. Furthermore, the three groups likewise reported statistically indistinguishable and significant improvement in top-down executive control over attention, as indexed by a working memory task.

Consequently, our findings are at odds with the hypothesis that the anxiolytic benefits resulting from ABM require a visuospatial contingency between non-emotional cues and probes. Yet ours are consistent with previous emotion-based ABM studies reporting that the no-contingency group just performed as well as the contingency group did in the improvement in self-report and behavioral measures of speech anxiety (e.g., Heeren, Mogoaşe, et al., 2015; McNally et al., 2013). Beneficial practice effects arising from the *in vivo* exposure of repeated videotaped speech task may explain pre-post improvements in anxiety and performance. However, Heeren, Mogoaşe, et al. (2015), who used the identical speech challenge procedure, reported larger pre-post effect sizes after training subjects with the usual emotion-based modified probe task procedure (all $ds > 1.20$ for the SUDS; all $ds > .60$ for the BASA) than ours (see Fig. 2, parts C and D). Yet, while Heeren, Mogoaşe, et al. (2015) administered two training sessions, ours only included one.

Although the current findings suggest that the contingency procedure outperformed neither the no-contingency nor the control group, these procedures involve geometric shapes devoid of emotional significance. However, as we did not cross the presence/absence of emotional material during the training, it remains difficult to generalize from the present findings to the usual emotion-based modified version of the dot probe task. Consequently, the critical next step would be to examine whether a contingency and no-contingency emotional versions of the ABM procedures outperform a credible additional condition controlling for practice effect. Although Enock et al. (2014) reported the superiority of both a contingency and no-contingency emotional versions of ABM procedures over a control group, their control condition was a wait-list group. Moreover, Enock et al. did not administer a speech task to subjects in their study. In contrast, the control group in the present study actually performed a discrimination task, albeit not a task one would expect would produce anxiolytic benefits. Moreover, both of Enock et al.'s training groups involved potentially anxiolytic exposure to disgust faces. Consequently, future studies should thus also attempt to identify whether repeated exposure to face-pairs without the inclusion of the other usual elements of dot-probe task (i.e., probe discrimination) would outperform both the contingency and no-contingency ABM procedures with and without emotional material. In the same vein, because Enock et al. (2014) did not include a speech performance in their experiment, future studies should explore whether the presence of speech task alongside ABM training would surpass the efficacy of ABM procedures without a speech.

Moreover, given that all groups exhibited indistinguishably significant improvement on the working memory task, the present findings are consistent with the hypothesis that the improvement from baseline to post-training may merely result from a practice effect. That should not come as a surprise; several neuropsychological studies have indeed shown that changes in attention and executive processes, such as those typically targeted by ABM, are likely to be affected by practice effects, specifically for an intervention involving a short test-retest period (for a recent meta-analysis, see Calamia, Markon, & Tranel, 2012). However, researchers from the field of neuropsychological rehabilitation have suggested that practice effects may have prognostic and treatment implications. For instance, in three clinical conditions (i.e., mild cognitive impairment; human immunodeficiency virus, Huntington's disease), practice effects predicted longer-term general cognitive functioning (Duff et al., 2007). Practice effects also predicted treatment response to memory training in older adults (Calero & Navarro, 2007; Duff, Beglinger, Moser, Schultz, & Paulsen, 2010). Unfortunately, to our best knowledge, such issues have never been explored in SAD.

Alternatively, one can argue there are at least two other explanations for our findings. First, because all groups exhibited improvements in the dependent variables, one might argue that a placebo or positive expectancy effect may explain the widespread improvement exhibited by all three groups. However, the absence of awareness from the participants regarding the potential anxiolytic nature of the current study tends to run counter to this interpretation. Second, others may wonder whether an overall promotion of top-down control, irrespective of training condition, may mitigate anxiety reactivity to a stressor (e.g., Heeren, De Raedt, et al., 2013). Indeed, this hypothesis makes sense in view of previous work demonstrating that higher-order cortical structures, such as the prefrontal cortex and its functionally related structures (e.g., anterior cingulate cortex), down-regulate emotion-relevant limbic structures (Miller & Cohen, 2001; Myers & Davis, 2007). Moreover, recent translational studies show that increasing the activity of this brain region by using neuromodulation may facilitate ABM efficiency (Clarke, Browning, Hammond, Notebaert, & MacLeod, 2014; Heeren, Baeken, Vanderhasselt, Philippot, & De Raedt, 2015). However, the absence of significant correlations between change in anxiety and working memory render this possibility unlikely. Furthermore, it is unlikely that merely discriminating between « E » and « F », as performed by subjects randomized to the control condition, did strongly promote top-down control in such a way that it boosts the recruitment of prefrontal regions.

In follow-up research several issues require further examination. First, it remains unclear whether training to increase attentional control in the service of helping anxious people is best achieved in an emotional context. For example, the subjects of Klumpp and Amir (2010) exhibited improvement on anxiety measures regardless of whether they were trained to attend toward or away from threat cues. Hence, improvements in attentional control in their study were achieved in the presence of threatening facial stimuli. This was not the case in the present study where training occurred in the presence of geometric shapes, not faces displaying emotional expressions. Relevant to this issue, neuroimaging studies have demonstrated that higher level of attention control often relate to lower activation of emotion-relevant limbic structures in the presence of an emotional context (e.g., for a review, see Pessoa, Oliveira, & Pereira, 2013). Yet, merely training executive attention (without any emotional context) decreased anxiety in highly anxious individuals (Bomyea & Amir, 2011). Likewise, highly anxious individuals exhibit impoverished attention control in the absence of emotional material (e.g., Bishop, 2009; Peschard & Phillippot, in press). Future studies should thus further delineate when training to optimize attentional control requires doing so in the presence of emotional stimuli.

Second, we did not assess AB in the current study as we wanted to avoid exposing subjects to facial threat stimuli, thereby masking the purpose of the study as well as preventing any exposure to threat cues (other than the speech itself). Accordingly, it would be useful to replicate our procedure by assessing AB for threat before and after training. Third, although our sample size had adequate power to detect small-to-medium effect sizes, one cannot exclude the possibility that some analyses would require a larger sample size. However, neither the *p*-values nor the effect sizes associated with our non-significant effects even approached statistical significance. Moreover, it should be noted that a complementary power analysis indicated that a total sample size of at least 969 participants would be required to yield enough power to detect a small effect size (i.e., $d = .10$) in the present study. However, such small effect sizes have no clinical relevance.

Finally, it would be desirable not only to assess AB before and after training, but include multiple training sessions, and hence a longer temporal gap between baseline and posttest, would be desirable. Indeed, practice effects can occur on many different timescales, often requiring varied designs to study such effects optimally (e.g., Duff, 2012). Our findings might have been different had the gap between baseline and posttest been days, weeks or months.

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