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Evaluating Changes in Judgmental Biases as Mechanisms of Cognitive-Behavioral Therapy for Social Anxiety Disorder

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Abstract

Reductions in judgmental biases concerning the cost and probability of negative social events are presumed to be mechanisms of treatment for SAD. Methodological limitations of extant studies, however, leave open the possibility that, instead of causing symptom relief, reductions in judgmental biases are correlates or consequences of it. The present study evaluated changes in judgmental biases as mechanisms explaining the efficacy of CBT for SAD. Participants were 86 individuals who met DSM-IV-TR criteria for a primary diagnosis of SAD, participated in one of two treatment outcome studies of CBT for SAD, and completed measures of judgmental (i.e., cost and probability) biases and social anxiety at pre-, mid-, and posttreatment. Treated participants had significantly greater reductions in judgmental biases than not-treated participants; pre-to-post changes in cost and probability biases statistically mediated treatment outcome; and probability bias at midtreatment was a significant predictor of treatment outcome, even when modeled with a plausible rival mediator, working alliance. Contrary to hypotheses, cost bias at midtreatment was not a significant predictor of treatment outcome. Results suggest that reduction in probability bias is a mechanism by which CBT for SAD exerts its effects.

Keywords

social anxiety disorder; judgmental biases; threat reappraisal; mechanisms of treatment response

Theoretical models posit that social anxiety disorder (SAD) is maintained in part by judgmental biases concerning the probability and cost of negative social events (e.g., Clark & Wells, 1995; Rapee & Heimberg, 1997). Specifically, individuals with SAD tend to

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believe that negative social events are extremely likely to occur (i.e., probability bias), and that if such events were to happen, the consequences would be awful or unbearable (i.e., cost bias). Foa and Kozak (1986) argued that one mechanism of action of cognitive-behavioral therapy (CBT) for anxiety disorders is a reduction in the exaggerated perception of probabilities and costs associated with feared outcomes. This idea has been termed the threat reappraisal mediation hypothesis. From a CBT perspective, cost and probability biases are modified through challenges to distorted cognitions about the cost and probability of negative social events and through exposure, in which a person learns that feared outcomes are not as likely or as costly as anticipated. It is the shift of the distorted cognitions to more realistic appraisals and the new learning that results from exposure that leads to reduced anxiety.

Some researchers have questioned the causal role of cognitive change in clinical improvement with CBT (e.g., Longmore & Worrell, 2007), leading other scholars to call for tests of existing mediation models using more recently developed methodological guidelines (see Hofmann, 2008). In a recent review of the literature on the threat reappraisal mediation hypothesis in cognitive-behavioral treatment of anxiety disorders, Smits, Julian, Rosenfield, and Powers (2012) described the following criteria as critical to establishing that a variable is a mechanism of treatment: 1) demonstration of statistical mediation; 2) demonstration that CBT causes threat reappraisal; 3) demonstration that threat reappraisal causes anxiety reduction; and 4) demonstration of specificity of the threat reappraisal-anxiety reduction relation. Formal tests of statistical mediation are useful in demonstrating significance of the paths between treatment and the hypothesized mechanism (path *a*) and between the mechanism and the specified outcome of interest (path *b*), and of the indirect mediated $a \times b$ pathway (i.e, Criterion 1). Demonstration that CBT causes threat reappraisal (i.e., Criterion 2) is critical to establishing that threat reappraisal occurred as a result of treatment, versus some other variable or as a function of time. Studies comparing CBT to viable alternative treatments are ideally suited to draw conclusions that CBT—versus some non-specific therapeutic factor—caused threat reappraisal, although studies comparing CBT to a wait-list control can also permit researchers to make causal inferences. Demonstration that threat reappraisal temporally preceded anxiety reduction (i.e., Criterion 3) is essential for demonstrating that changes in the hypothesized mechanism caused changes in the outcome. Finally, demonstration of specificity by ruling out other plausible mechanisms (i.e., Criterion 4) strengthens evidence for the causal relation between the mediator and the outcome.

In their review, Smits et al. (2012) identified eight studies that examined the threat reappraisal hypothesis in relation to SAD. No single study both tested and established all four criteria. The majority ($n = 5$; 62.5%) demonstrated statistical mediation (Foa, Franklin, Perry, & Herbert, 1996; Hoffart, Borge, Sexton, & Clark, 2009; Hofmann, 2004; Rapee, Gaston, & Abbott, 2009; Smits, Rosenfield, McDonald, & Telch, 2006). Fewer than half ($n = 3$; 37.5%), however, established CBT as a cause of threat reappraisal (Hofmann, 2004; Rapee et al., 2009; Taylor & Alden, 2008) or demonstrated specificity of the threat reappraisal-anxiety reduction relation (Hoffart et al., 2009; Rapee et al., 2009; Smits et al., 2006). According to Smits et al., four studies attempted to establish causality of the

mediator-to-outcome effects but did not model the data in ways that permitted strong causal inferences (Hoffart et al., 2009; Hofmann, 2004; Taylor & Alden, 2008; Wilson & Rapee, 2005). In these studies, threat reappraisal in earlier phases of treatment was correlated with symptom improvements later in treatment, but testing of causality by controlling for earlier levels of social anxiety symptoms was absent. Only Smits et al. (2006) demonstrated that threat reappraisal was associated with social anxiety reduction after controlling for earlier levels of social anxiety, providing stronger support for the hypothesis that threat reappraisal caused reduction in social anxiety. In this study, cost and probability biases independently accounted for variance in fear reduction within and between sessions, with change in probability bias accounting for a greater proportion of variance than change in cost bias. Within-session reductions in probability bias predicted within-session reductions in fear, which predicted further reductions in probability bias (i.e., a reciprocal relation), whereas within-session reduction in cost bias did not predict reduction in fear, but was a consequence of it. Although this study is the most methodologically rigorous examination of the threat reappraisal mediation hypothesis to date, it has a major limitation in that fear ratings, but not social anxiety symptoms, were measured during treatment. Furthermore, the cross-lagged panel analyses only examined within-session change in a three-session treatment protocol, so the conclusion regarding mediation is limited to within-session processes using an abbreviated treatment period. The authors encouraged research that applies their analytic strategy to a longer, more typical treatment protocol to provide information about change between treatment sessions – the aim of the present work.

The current project tests the threat reappraisal mediation hypothesis in the context of an eight-week course of CBT for SAD using the criteria outlined by Smits et al. (2012). Specifically this study examined whether or not 1) changes in judgmental biases statistically mediated treatment outcome; 2) CBT caused threat reappraisal, meaning individuals randomly assigned to receive CBT had lower threat appraisal following treatment than individuals assigned to a waitlist control; 3) threat reappraisal caused social anxiety symptom reduction, meaning earlier levels of judgmental bias predicted change in social anxiety; and 4) threat reappraisal remained a significant mediator of treatment outcome when modeled with a plausible rival mediator, working alliance. Working alliance was chosen as the rival mediator for the present study because of its reliable, albeit modest, effect on treatment outcome in psychotherapy in general (approximately 8% of the total variance in therapy outcomes; see Horvath, Del Re, Flückiger, & Symonds, 2011 for a review). Specifically, we hypothesize that changes in both cost and probability estimates will mediate treatment outcome and that judgmental biases will remain a significant predictor of treatment outcome when modeled simultaneously with the rival mediator, working alliance. These hypotheses address each criterion proposed by Smits and colleagues to test the threat reappraisal mediation hypothesis.

We also explore the extent to which improvement in SAD is better accounted for by changes in cost versus probability bias. Foa and Kozak (1985) originally theorized that inflated cost estimates are the primary variable mediating change in SAD because, whereas other anxiety disorders are characterized by overestimates of the probability of objectively catastrophic outcomes (e.g., heart attack, death of a loved one), the feared outcomes in SAD (e.g., appearing foolish, being embarrassed) are not objectively dangerous. Empirical research

with clinical samples has, however, yielded mixed findings: two studies found cost bias to be more important (Foa et al., 1996; Rapee et al., 2009), two studies found probability bias to be more important (McManus, Clark, & Hackmann, 2000; Smits et al., 2006), and two studies found each to be significant predictors and did not make inferences about their relative importance (Hoffart et al., 2009; Taylor & Alden, 2008). Based on Foa and Kozak's original theory, we hypothesize that reductions in cost will be a stronger predictor of treatment outcome than reductions in probability when modeled simultaneously.

Method

The present study uses data from two treatment studies: a randomized controlled trial comparing Exposure Group Therapy (EGT; Hofmann, 2002) and Virtual Reality Exposure Therapy (VRE; Anderson, Zimand, Hodges, & Rothbaum, 2005) for SAD to wait-list controls (Anderson et al., 2013; Study 1) and an uncontrolled trial examining amygdala activity as a predictor of treatment response to VRE using functional magnetic resonance imaging (fMRI; Study 2). For the purposes of this study, procedures in these two trials were identical, with one exception: participants in Study 2 were not randomly assigned to treatment; they all received VRE.

Participants

Participants were 86 individuals who met DSM-IV-TR (APA, 2000) criteria for a primary diagnosis of generalized (n = 40) or non-generalized SAD (n = 46), completed eight weeks of the waitlist or treatment protocol, and identified public speaking as their most feared social situation. Participants were included only if they identified public speaking as their most feared social situation because both the VRE and EGT protocols exclusively utilized public speaking exposures. Eligible participants on psychoactive medication were required to be stabilized on their current medication(s) and dosage(s) for at least 3 months and to remain on the stabilized regimen throughout the course of the study. Exclusion criteria included (a) history of mania, schizophrenia, or other psychoses; (b) recent prominent suicidal ideation; (c) current alcohol or drug abuse or dependence; (d) inability to wear the virtual reality helmet; (e) history of seizures; and (f) inability to undergo an fMRI (e.g., claustrophobia, metallic implants) (Study 2 only). Additionally, participants were required to be literate in English.

Most participants (n = 68; 79.1%) received a diagnosis of SAD alone. The most common secondary diagnoses were specific phobia (n = 5), panic disorder without agoraphobia (n = 3), generalized anxiety disorder, (n = 3), and major depression (n = 3). The sample consisted of 60.5% females (n = 52) and 39.5% males (n = 34). Participants' ages ranged from 19 to 69 with a mean age of 39.8 (SD = 11.3). Most participants self-identified as "Caucasian" (n = 43; 50%) or "African American" (n = 25; 29.1%). Four participants (4.7%) self-identified as "Hispanic," three (3.5%), as "Asian American," nine (10.5%) as "Other" ("African American/Indian/Caucasian" = 1; "Chinese" = 1; "African" = 1; "Biracial" = 1; "Eritrean American" = 1; "Arabic" = 1; "African American/Caucasian" = 1; Unspecified = 2), and two declined to answer. Sixty-five percent reported that they had completed college, 51%

were married or living with someone as though married, and 48% had an annual income of $50,000 or greater.

Measures

Structured Clinical Interview for the DSM-IV (SCID; First, Gibbon, Spitzer, & Williams, 2002)—The SCID was used to determine eligibility and diagnostic status on Axis I conditions within the mood, substance use, and anxiety disorders modules. In both studies, all pretreatment diagnostic assessments were videotaped, and a randomly selected subset was reviewed by a licensed psychologist to calculate the inter-rater reliability of pretreatment assessments (100% agreement for primary diagnosis, with one disagreement on severity).

Brief Fear of Negative Evaluation (BFNE; Leary, 1983)—Social anxiety symptoms were measured using the BFNE, a 12-item self-report questionnaire that measures the degree to which individuals fear being negatively evaluated by others across a number of social settings (e.g., “I often worry that I will say or do wrong things.”). Only the eight straightforwardly-worded items were included in the scoring algorithm for the present study, given concerns noted in prior studies about the psychometric properties of the reverse-scored items (Rodebaugh et al., 2004; Weeks et al., 2005). Items are rated on a 5-point Likert-type scale, and scores range from 8 to 40, with higher scores representing greater evaluative concerns. The BFNE has demonstrated excellent internal consistency (Cronbach’s α = .97) and one-month test-retest reliability (*r* = .94) (Collins, Westra, Dozois, & Stewart, 2005). The internal consistencies for the current study were excellent for pretreatment (α = .94), midtreatment (α = .93), and posttreatment (α = .95).

Outcome Probability Questionnaire (OPQ; Uren, Szabó, & Lovibond, 2004)—The OPQ is a 12-item self-report questionnaire that assesses an individual’s estimate of the probability that negative socially threatening events will occur (e.g., “You will sound dumb while talking to others.”). As recommended by the original scale development paper, the 10-item version was used in the present study. Items are scored on a 9-point Likert-type scale with summary scores ranging from 0 to 80. Internal consistency for the measure has been found to range from good to excellent (Cronbach’s α = .89 – .90; Uren et al., 2004). The internal consistencies for the current study were as follows: good for pretreatment (α = .85) and excellent for midtreatment (α = .92) and posttreatment (α = .91).

Outcome Cost Questionnaire (OCQ; Uren et al., 2004)—The OCQ is a 12-item self-report questionnaire that assesses an individual’s estimate of the cost of negative social events (e.g., “You sounded dumb to others.”). As recommended by the original scale development paper, the 10-item version was used in the present study. Items are scored on a 9-point Likert-type scale with summary scores ranging from 0 to 80. Internal consistency for the measure has been found to be consistently in the excellent range (Cronbach’s α = .92 – .94; Uren et al., 2004). The internal consistencies for the current study were as follows: good for pretreatment (α = .85) and excellent for midtreatment (α = .92) and posttreatment (α = .93).

Working Alliance Inventory – Short Form (WAI-S; Tracey & Kokotovic, 1989)— The WAI-S is a 12-item instrument used to evaluate the therapeutic alliance. Like the original WAI (Horvath & Greenberg, 1989), the WAI-S assesses working alliance regardless of therapeutic orientation. Participants are asked to rate items (e.g., "My therapist and I are working towards mutually agreed upon goals.") on a 7-point Likert-type scale to best represent their feelings, with answers ranging from 1 (Not at All) to 7 (Very Much). Total scores range from 7 to 84, with higher scores indicating a stronger alliance. The WAI-S demonstrates good psychometric properties, including content validity and internal consistency ($\alpha = 0.93$) (Tracey & Kokotovic, 1989), and shows similar properties as the original WAI (Busseri & Tyler, 2003). The WAI was administered following each treatment session. The internal consistencies for the current study were as follows: excellent for Session 1 ($\alpha = .90$), acceptable for Session 4 (midtreatment; $\alpha = .79$), and good for Session 8 (posttreatment; $\alpha = .88$).

Procedure

Both studies were approved by a university Institutional Review Board. Participants were self-referred or recruited through area professionals, newspaper advertising, posted flyers, and public service announcements. Study eligibility was determined through a two-part process consisting of a brief telephone screening and a subsequent in-person pretreatment assessment. After expressing interest and verbally consenting to complete a telephone screening, study candidates completed a short phone interview with a doctoral student to determine if they met obvious exclusion criteria (e.g., current substance abuse in both studies, metallic implants in Study 2 only). Those who were not excluded during the telephone screening were given the opportunity to participate in an in-person pretreatment assessment. Written informed consent for study procedures was obtained at the pretreatment assessment.

In Study 1, the pretreatment assessment included a structured diagnostic clinical interview (SCID) administered by a doctoral student, video-recorded behavioral avoidance task (10-minute speech), and completion of self-report measures. Eligible participants were then randomly assigned to the VRE, EGT, or WL condition. The VRE and EGT treatment groups were designed to be as similar as possible. Both treatments specifically targeted public speaking fears via exposure therapy. Furthermore, both treatments sought to address specific aspects of SAD identified in the psychopathology literature, including self-focused attention, perceptions of self and others, perceptions of emotional control, rumination, and realistic goal setting for social situations. In both treatments, probability and cost biases were specifically targeted and challenged via a combination of psychoeducation, cognitive restructuring, cognitive preparation, exposure, and/or social mishap exercises. The mechanism and setting through which exposure was delivered varied for the two treatment groups. Individual study therapists relied on the virtual environment to facilitate exposure to public speaking fears (VRE), whereas group therapists relied on other group members to help facilitate exposure (EGT). For participants in both treatments, elements of exposure were present as early as the pretreatment assessment, during which participants gave a video-recorded speech that they subsequently viewed during their second treatment session. Structured exposure for the EGT treatment condition began in Session 2, when participants

gave a speech in front of the group, whereas structured exposure for the VRE treatment condition began in Session 4 or 5, when participants gave a speech in front of the virtual audience. Though structured exposures began in different sessions for the two treatment conditions, the study was designed so that all participants, regardless of treatment condition, received the same total amount of exposure by the completion of treatment. The number of therapists and session length also varied across treatment conditions. In the VRE condition there was one individual therapist, and sessions lasted for 60 minutes; in the EGT condition, there were two group co-therapists, and sessions lasted for 120 minutes. See Anderson et al. (2013) for a detailed description of the two treatments. The WL lasted eight weeks, after which participants completed a battery of questionnaires similar to the battery that was administered after both the EGT and VRE treatments. WL participants were then re-randomized to VRE or EGT following the waiting period.

In Study 2, the pretreatment assessment was identical to that of Study 1 except that it included an additional "mock" fMRI to ensure that participants could tolerate an actual fMRI. Following the pretreatment assessment, eligible participants then underwent an fMRI at a nearby hospital. These participants were not randomly assigned to treatment groups; all received VRE.

Participants in both studies completed study measures at pre-, mid-, and posttreatment. The same therapists were used in Study 1 and Study 2, and each therapist delivered both types of treatment.

Figures 1 and 2 were prepared in accordance with guidelines outlined in the CONSORT (Consolidated Standards of Reporting Trials; Altman, et al., 2001) and TREND (Transparent Reporting of Evaluations with Nonrandomized Designs; Des Jarlais, Lyles, & Crepaz, 2004) statements. The figures show the flow of participants through the two treatment studies. In Study 1, following the initial randomization, 26 individuals completed EGT, 25 completed VRE, and 25 completed the WL. Following the re-randomization of the WL participants, an additional eight participants completed EGT, and an additional seven completed VRE, for a total of 34 EGT completers and 32 VRE completers in Study 1. In Study 2, all 10 participants who completed the study received VRE. Thus combining participants from Study 1 and Study 2 who completed an active treatment, a total of 34 participants completed EGT, and a total of 42 completed VRE.

Data Analytic Plan

Preliminary analyses determined whether or not participants could be collapsed across studies and, within Study 1, across treatment conditions. Hypotheses were then evaluated in accordance with the guidelines outlined by Smits et al. (2012). First, a multiple mediators path model assessed the indirect effect of treatment on change in social anxiety symptoms through change in cost and probability biases. This analysis determined whether or not symptom improvement was statistically mediated by reductions in cost bias and probability bias (Criterion 1). This model was also used to examine the effect of treatment on cost bias and probability bias to test whether CBT caused threat reappraisal (Criterion 2). Participants who were initially assigned to a treatment condition ($N_{\text{Treated}} = 61$) or to the waitlist ($N_{\textit{Not Treated}} = 25$) were included in the analyses. Data were modeled using path analysis,

and the significance of the mediated pathway was tested using bootstrapping (MacKinnon et al., 2010).

According to Smits et al. (2012), "…testing the causal effects of threat reappraisal on anxiety reduction requires (at a minimum) relating previous levels of the threat appraisal to later levels of anxiety (and vice versa; i.e., bidirectional effects)" (p. 626). Other scholars have written about the limitations of simple pre-post designs and the importance of establishing temporal precedence, i.e., demonstrating that changes in the proposed mediator occurred prior to changes in the dependent variable (Kraemer, Wilson, Fairburn, & Agras, 2002; Kazdin & Nock, 2003). Therefore, additional analyses were conducted using treated participants only that included the midtreatment data (See Table 1 for descriptive statistics). Fifteen participants who were re-randomized to and completed treatment following the WL period were included in these analyses to increase power (N_{Total} = 76; See Figure 1). Associations between midtreatment levels of cost and probability biases and posttreatment levels of social anxiety, while controlling for previous levels of social anxiety, were examined to test the hypothesis that threat reappraisal caused social anxiety reduction (Criterion 3). A cross-lagged panel design path model was employed to investigate the presumed causal interplay among social anxiety symptoms (BFNE) and cost and probability biases (OCQ, OPQ) at three time points: pretreatment, midtreatment, and posttreatment. Cross-lagged panel designs allow for examination of the direct effects of one variable on another over time and of reciprocal relations among variables (Kessler & Greenberg, 1981; Menard, 1991). They examine the predictive association of two variables over time, each controlling for the effects at earlier time points, such that the effect of X1 on Y2 controlling for Y1 represents the effect of X1 on changes in Y over time (Finkel, 1995).

Lastly, because this study aimed to establish threat reappraisal as a specific cognitive mediator of CBT, a plausible rival mediator (the nonspecific factor working alliance) was modeled simultaneously with threat appraisal to demonstrate specificity of the relation between threat reappraisal and social anxiety reduction (Criterion 4). A second cross-lagged panel design path model was utilized to analyze the relation between social anxiety symptoms and the candidate and rival mediators at three time points throughout treatment. As working alliance was necessarily not measured at pretreatment, the model includes measures of social anxiety symptoms and judgmental biases at pretreatment, midtreatment, and posttreatment, and measures of working alliance after Session 1, Session 4 (i.e., midtreatment), and Session 8 (i.e., posttreatment).

For all analyses, SEM software Mplus 7 (Muthèn & Muthèn, 1998–2012) was used to model the data. Maximum likelihood estimation with robust standard errors (MLR) was used to test the fit of the hypothesized models to the observed variance-covariance matrix. MLR provides robust estimates of standard errors and uses Full Information Maximum Likelihood (FIML) to handle missing data. The MLR estimator was used because the assumption of multivariate normality was not met. Specifically, the BFNE and OPQ at posttreatment were significant positively skewed, and the OCQ at pretreatment was significantly negatively skewed and significantly positively kurtotic. Results of Little's MCAR test revealed data were missing completely at random ($\chi 2(127) = 132.037$, $p = 0.362$), further supporting the use of FIML.

Results

For Study 1, a series of ANOVAs and chi-square tests showed no differences in the variables of interest at pretreatment (BFNE, OPQ, OCQ) across the VRE, EGT, and WL conditions (*p*'s = .213 to .830) or demographic characteristics (SAD subtype, gender, ethnicity, educational achievement, income, relationship status; *p*'s = .402 to .841). Thus, random assignment successfully created three conditions that were comparable at pretreatment with regard to symptom severity, judgmental biases, and demographic factors. Participants receiving EGT reported slightly higher first-exposure SUDS ratings than participants receiving VRE, but this difference was not statistically significant (M_{EGT} = 7.4; M_{VRE} = 6.2; $t(40) = -1.817$, $p = .077$). At posttreatment, there were no differences between the EGT and VRE groups on any measure (*p*'s = .348 to .802). Thus participants in the EGT and VRE groups were combined, forming a total of two experimental groups (Treated [EGT + VRE], Not Treated [WL]). Independent samples *t*-tests and chi-square tests were then conducted to determine whether participants from the uncontrolled trial (Study 2) were significantly different in terms of symptom severity, judgmental biases, or demographics at the pretreatment assessment from participants in the controlled trial (Study 1). There were no significant differences between Study 1 and Study 2 on any of the metrics listed above (*p*'s = .254 to .969); as such, participants from Study 2 were added to the Treated group from Study 1 to increase sample size and statistical power. With regard to SAD subtype, there were no significant between-group differences in cost bias or probability bias at pretreatment or working alliance at Session 1 (*p*'s = .064 to .691); however, participants with generalized SAD reported significantly higher levels of social-evaluative fears at pretreatment than participants with non-generalized SAD ($M_{generalized}$ = 44.69; $M_{non\text{-}generalized}$ = 36.80; $t(74) = -3.362$, $p = .001$).

To determine whether or not symptom improvement was statistically mediated by reductions in cost bias and probability bias (Criterion 1) and whether CBT caused threat reappraisal (Criterion 2), a multiple mediators path model was tested. Residualized gain scores were first computed using data from pretreatment and posttreatment to represent a measure of change in social anxiety symptoms (BFNE) and threat appraisal (OPQ, OCQ) during treatment. Residualized gain scores control for initial symptom severity and measurement error associated with repeated assessment and thus have advantages over other measures of change (Steketee & Chambless, 1992). Residualized gain scores were calculated by subtracting the standardized pretreatment scores, which were multiplied by the correlation between the standardized scores at pretreatment and posttreatment, from the posttreatment scores. Using this formula, lower residualized gain scores reflect greater reductions in symptoms. Next, a model was tested that included paths for 1) the effect of treatment on the mediators (pre-to-post changes in probability bias and cost bias) (i.e., Criterion 2); 2) the effect of the mediators on treatment outcome (pre-to-post changes in social anxiety symptoms), 3) correlations between the two mediators, and 4) the indirect effect of treatment on treatment outcome through pre-to-post changes in probability bias and cost bias (i.e., Criterion 1). The multiple mediators path model is presented in Figure 3. Overall, the model fit the data well: Model $\chi 2(1) = 1.652$, $p = 0.199$; RMSEA = .081 [.000, .294]; CFI = .995; TLI = .967; SRMR = .024, with the exception of the upper limit of the RMSEA confidence

interval. First, as predicted, treatment had a significant effect on threat appraisals; receiving treatment compared to not receiving treatment predicted significantly greater reductions in both probability bias (b_{StdYX} = −0.330, z = −3.645, p < .001) and cost bias (b_{StdYX} = −0.320, z = −3.495, p < .001). Second, treatment outcome was predicted by threat appraisals; higher residualized gain scores for social anxiety were predicted by both higher residualized gain scores for probability bias (b_{StdYX} = 0.432, z = 4.433, p < .001) and cost bias (b_{StdYX} = 0.285, z = 2.815, p = .005). Third, the mediators were significantly positively correlated (b_{StdYX} = 0.625, z = 10.015, p < .001); and fourth, the effect of treatment on treatment outcome was statistically mediated by pre-to-post changes in both cost bias and probability bias. That is, the indirect $a \times b$ pathway was significant for the OPQ (b_{StdYX} = −0.143, z = −2.780, p = .005) and OCQ (b_{StdYX} = −0.091, z = −2.182, p = .029). Next 5,000 bootstrap samples were generated to obtain the most accurate confidence intervals for indirect effects in mediation (MacKinnon, Lockwood, & Williams, 2004). Neither the confidence interval for the OPQ [95% CI −0.490, −0.081] nor that for the OCQ [95% CI −0.381, −0.027] overlapped with zero, further supporting the finding of statistically significant mediation.

To test whether or not threat reappraisal causes anxiety reduction (Criterion 3), a cross-lagged panel design path model was employed to investigate the presumed causal interplay among social anxiety symptoms (BFNE) and cost and probability biases (OCQ, OPQ) at three time points: pretreatment, midtreatment, and posttreatment. The model incorporates autoregressive effects that control for temporal stability within threat appraisal and social anxiety scores across time, synchronous correlations between variables at each time point that account for covariances between variables not already explained by the influences of the variables from earlier time points, and cross-lagged direct effects. Thus any cross-lagged effects can be considered effects that add predictive power over and above that which can simply be obtained from within-construct stability over time and synchronous and other IV effects. The cross lags between social anxiety and judgmental biases at mid- and posttreatment are of primary importance to our study hypothesis, as they allow for evaluation of three potential scenarios: 1) whether earlier levels of judgmental biases predicted later changes in social anxiety, 2) whether earlier levels of social anxiety predicted later changes in judgmental biases, and 3) whether any relations were reciprocal. Social anxiety and judgmental biases at pretreatment are also important for this study, because their inclusion serves as a control for pretreatment symptom severity, thereby providing an indicator of change (Finkel, 1995; Rieckmann et al., 2006). For example, social anxiety at midtreatment represents residualized change in social anxiety from pretreatment to midtreatment. The cross-lagged panel design model is presented in Figure 4. Results indicated the fully cross-lagged model had acceptable fit according to all indices (Model $\chi 2(13) = 22.053$, p = 0.055; CFI = 0.974; TLI = 0.933; SRMR = 0.053), with the exception of the RMSEA (RMSEA = .096 [.000, .163], which is slightly above conventional standards for good fit (MacCallum, Brown, & Sugawara, 1996). It should be noted, however, that the RMSEA tends to reject acceptable models when sample sizes are small. For this reason, some scholars argue against computing the RMSEA for models with low degrees of freedom (Kenny, Kaniskan, & McCoach, 2014). Figure 4 also shows the standardized path coefficients for the model. As predicted, examination of individual paths revealed significant autoregressive effects and intercorrelations between variables at each time point. The cross

lag model revealed a significant effect of midtreatment OPQ on posttreatment BFNE (b_{StdYX} = .350, z = 4.473, p < .001), specifically lower probability bias predicting lower social anxiety symptoms. However, the cross lag from midtreatment OCQ to posttreatment BFNE was not significant (b_{StdYX} = −.059, z = −.719; p = 0.472); nor was the cross lag from midtreatment BFNE to posttreatment OPQ (b_{StdYX} = .038, z = .419, p = 0.675) or from midtreatment BFNE to posttreatment OCQ (b_{StdYX} = .049, z = .364, p = .716). Findings suggest that lower midtreatment levels of probability bias, but not cost bias, predicted greater reduction in social anxiety symptoms. That the inverse is not supported (i.e., that midtreatment BFNE does not predict change in OPQ) suggests the relation is not reciprocal and provides further evidence supporting probability bias as a specific cognitive reappraisal mediator of CBT for SAD[1].

To test the specificity of the relation between threat reappraisal and social anxiety reduction (Criterion 4), a second cross-lagged panel design path model was utilized to analyze the relation between social anxiety symptoms (BFNE), probability bias (OPQ), and working alliance (WAI), a plausible rival mediator, at three time points throughout treatment. Fit indices, again with the exception of the upper limit of the RMSEA confidence interval, indicated good model fit (Model $\chi 2(14)$ = 18.705, p = 0.177; RMSEA = .066 [.000, .138]; CFI = 0.984; TLI = 0.963; SRMR = 0.037). Figure 5 shows the standardized path coefficients for the model. Examination of individual paths revealed significant autoregressive effects between variables at each time point. However, working alliance was not significantly correlated with probability bias or with social anxiety symptoms at any time point. The cross lag model again revealed a significant effect of midtreatment OPQ on posttreatment BFNE (b_{StdYX} = .352, z = 3.480, p = .001), whereas the cross lag from midtreatment WAI to posttreatment BFNE was not significant (b_{StdYX} = −.050, z = −.657; p = 0.511). Findings suggest that earlier levels of probability bias, but not working alliance, predicted later change in social anxiety symptoms. These findings provide further support for threat reappraisal, specifically probability bias, as a mediator of CBT for SAD by demonstrating specificity of the threat reappraisal-social anxiety reduction relation.

Discussion

CBT is theorized to exert its therapeutic effect on SAD by reducing judgmental biases concerning the probability and cost of negative social events (i.e., the threat reappraisal mediation hypothesis; Foa & Kozak, 1986; Clark & Wells, 1995; Rapee & Heimberg, 1996). Many empirical studies have yielded findings consistent with the threat reappraisal mediation hypothesis but failed to evaluate and/or demonstrate criteria critical to establishing mediation, leading to calls for research to test threat reappraisal as a mechanism of CBT using more modern statistical methods (Hofmann, 2008; Smits et al., 2012). The present study tested the threat reappraisal hypothesis by evaluating cost and probability

[1]Models with parameters allowed to vary across treatment group (VRE or EGT) showed that the path from probability bias at midtreatment to social anxiety at posttreatment was significant for both the VRE and EGT groups, the path from cost bias at midtreatment to social anxiety at posttreatment was not significant for either the VRE or the EGT groups, and the model with these paths constrained to be equal across treatment groups fit the data better than models with these paths free to vary across treatment groups. These findings indicate that, irrespective of treatment group, the OPQ at midtreatment was a significant predictor of treatment outcome, but the OCQ at midtreatment was not. Full results of these analyses are available upon request.

biases as mediators of CBT for SAD in accordance with the recent recommendations of Smits et al. (2012). As hypothesized, the effect of CBT on pre-to-post changes in social anxiety symptoms was statistically mediated by pre-to-post changes in judgmental biases, and participants who received CBT had significantly greater reductions in judgmental biases than participants who did not receive treatment. Tests aimed at examining the extent to which threat reappraisal caused social anxiety reduction support probability bias as a significant predictor of treatment outcome when it is modeled simultaneously with cost bias, as well as when it is modeled simultaneously with a plausible rival mediator. Cost bias, however, was not a significant predictor of treatment outcome when modeled simultaneously with probability bias. Taken together, the results of this study broadly support the threat reappraisal mediation hypothesis: reductions in probability bias met all the criteria critical to establishing mediation.

Our methodological approach has a number of strengths. Examining cost and probability biases and their relation to social anxiety symptoms at pre-, mid-, and posttreatment allowed for a finer analysis than was possible in prior studies that assessed relevant variables only before and after treatment. Our approach also addressed the issue of temporal precedence by allowing for tests of whether earlier levels of probability and cost biases predicted later change in social anxiety (or *vice versa*) or whether the relation was reciprocal. The cross-lagged panel design permitted a more conservative examination of the specific contributions of judgmental biases to social anxiety symptom reduction by controlling for autoregressive effects and isolating the effect from cross-variable correlations, which could have inflated effect size estimates. Furthermore, examining both cost and probability as predictors simultaneously allowed us to address a key topic of debate in the literature by comparing the contributions of each to treatment outcome and ascertaining that one (probability bias) was more influential than the other. Finally, this study is the first to examine working alliance as a candidate mediator in the threat reappraisal-social anxiety reduction relation and the only study to test the threat reappraisal mediation hypothesis using all the criteria recommended by Smits et al. (2012).

Our finding that cost bias at midtreatment was not a significant predictor of treatment outcome is consistent with two of the six previous empirical investigations that examined both cost and probability biases as predictors or mediators of treatment outcome (McManus et al., 2000; Smits et al., 2006). The consistency with the Smits et al. (2006) findings is especially noteworthy, given that the authors used similarly robust mediation analyses and different measures of anxiety and judgmental biases. As ours is the first and only study to test the threat reappraisal mediation hypothesis using the most recently developed guidelines, it is premature to conclude that probability bias is a treatment mechanism and cost bias is not. Clearly additional research is needed to replicate these findings. However, the difference between probability and cost bias as a predictor of treatment outcome in the present study was quite pronounced. One might speculate that reductions in probability bias may be especially important for successful treatment of public speaking fears (as compared to other types of social fears), because both the present study and the Smits et al. study specifically targeted public speaking fears. However, McManus and colleagues' (2000)

sample was not restricted to participants with substantial public speaking fears. Thus it seems unlikely that this finding is an artifact of the type of social fear targeted by treatment.

Alternate explanations for this finding raise questions about previously held assumptions about the primacy of cost bias over probability bias in SAD. First, this finding is inconsistent with the idea that inflated estimates of the likelihood of negative social events should only cause anxiety if the anticipated negative social events are also considered to be aversive or to have bad consequences (Foa & Kozak, 1985). Instead, it suggests that simply anticipating negative social events may be sufficient to provoke anxiety, regardless of how severe the events' negative consequences are expected to be. This interpretation suggests that people with social anxiety may have low thresholds for tolerating negative social events that they judge as relatively benign—in other words, even events that carry the mildest negative consequences may be unacceptably anxiety-provoking.

Second, cost judgments may only be relevant for events that are deemed probable. In other words, if an event is perceived as costly but highly unlikely, it may not be appraised as anxiety-provoking. Even individuals with strong convictions that negative social events are costly may not experience anxiety unless they perceive those events as imminent. Research efforts to clarify the boundaries of probability and cost biases may be helpful for testing this possibility; for example, it could be useful to establish whether each type of bias has a different threshold at which it becomes likely to trigger anxiety.

Both of these explanations suggest, at a minimum, that a closer look at distinctions and overlaps between probability and cost biases is warranted. Further, the results of the present study should encourage an increase in the amount of research attention dedicated specifically to understanding probability bias as a treatment mechanism and to developing probability-specific interventions. A randomized controlled trial in which a treatment enhanced with probability-focused interventions is shown to be more efficacious than the original non-enhanced treatment would be the next step toward confirming probability bias as a mechanism of treatment of CBT for SAD. In the meantime, practicing clinicians should continue to routinely assess and target both probability and cost biases in treatment, as the literature suggests that both may function as treatment mechanisms and elements of both are needed to create threat appraisals. That is, without the perception of negative consequences, an extremely likely event would not create fear, and without the possibility (likelihood) of an event occurring, a catastrophic consequence would not create fear.

This recommendation is in line that of researchers (e.g., Wells, 1997; Antony & Swinson, 2008) who advocate for the use of behavioral experiments in which, following exposures that test the probability of negative social events (in which patients ideally learn that feared outcomes are not as probable as they had anticipated), patients should complete exercises in which they purposely commit social errors. For example, if an individual fears her hand will tremble during a presentation, she could first complete an exposure in which she attempts to give her presentation as competently as possible, followed by an additional exposure in which she purposely allows her hand to tremble. The dual nature of this approach allows individuals to learn that, not only are feared outcomes unlikely, even if such outcomes do occur, the consequences are not as catastrophic as imagined.

Though our findings support the centrality of threat reappraisal to treatment outcome, our study design does not allow for conclusions about which specific components of treatment led to reductions in cost and probability biases. As such, our findings do not directly contradict dismantling studies that have challenged the utility of explicit cognitive intervention (e.g., Fedoroff & Taylor, 2001; Feske & Chambless, 1995; Gould, Buckminster, Pollack, Otto & Yap, 1997; Powers, Sigmarsson, & Emmelkamp, 2008). Hofmann and Otto (2008) argued that cost exposures are "the single most effective strategy to target probability and cost estimates" (p. 110), an assertion supported by the only study that experimentally compared cost- and probability-focused treatment interventions (Nelson, Deacon, Lickel, & Sy, 2010). Thus a goal for future research is to determine how cost- and probability-focused interventions exert their effects. Research comparing probability- and cost-focused cognitive interventions (e.g., cognitive restructuring, Socratic questioning, ABC worksheets) and probability- and cost-focused exposure exercises (e.g., traditional exposure, social mishap exercises) may be illuminative.

There are several limitations to the present study, first and foremost of which is the use of pre-to-post residualized change scores in the statistical mediation analyses. Much has been written about the limitations of cross-sectional mediational analyses in treatment outcome studies (Kraemer et al., 2002; Kazdin & Nock 2007). In the present study, the formal test of statistical mediation utilized a simple pre/post design because midtreatment data were only available from participants who completed either of the two treatments in the study (i.e., not from the waitlist control group). As such, the analyses of the present study deviate from Kraemer et al.'s (2002) recommendations for testing mechanisms of action in randomized controlled trials. Our findings were, however, consistent with statistical mediation, and the midtreatment data used in a later analytic step allowed us to assess the temporality of the relations between judgmental biases and social anxiety.

Second, because CBT was compared to a waitlist control and not an active treatment control, it is not possible to conclude that the reductions in threat appraisal and social anxiety found in the Treated group were caused by components of CBT. Rather, it is possible that something that is part of CBT but not its putative active ingredient (e.g., a non-specific factor, such as therapist contact) caused the changes seen in the present study. However, our choice for the plausible rival mediator was strategic in that it allowed us to rule out the nonspecific factor of working alliance.

Additionally, our primary outcome measure, the BFNE, tests a relatively specific, albeit core, feature of SAD (fear of negative evaluation), and thus may not capture the full range of social anxiety symptoms (e.g., avoidance). Use of a social anxiety composite score would rectify this situation, but it was not possible to calculate such a composite in the present study due to the schedule on which measures were administered.

Lastly, all participants identified public speaking as their most feared social situation. Thus, future research is needed to ascertain whether the relations between threat reappraisal and social anxiety observed in the present study generalize across participants with a wider range of social performance and interaction fears. For example, perceived likelihood and/or costs of negative social events associated with public speaking may be higher across the board

than are those associated with social events that involve fewer participants or that are more private. Such differences in variability could, in turn, drive differences in patterns of association.

In conclusion, this study responds to the call for increased methodological rigor in investigation of cognitive mediators of CBT by adding to our knowledge about how changes in judgmental biases and social anxiety symptoms unfold over time. Our design and analytic approach used a more conservative test of changes in judgmental biases as mechanisms by which CBT results in reduced social anxiety symptoms. Our findings contribute to our understanding of *why* treatment works and encourage research into enhanced probability-focused interventions for improving the efficacy of CBT for SAD.

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Highlights

- Changes in cost and probability biases mediated treatment outcome.
- Probability bias at midtreatment was a significant predictor of treatment outcome.
- Cost bias at midtreatment was not a significant predictor of treatment outcome.
- Working alliance at midtreatment was not a significant predictor of outcome.
- Findings support the threat reappraisal mediation hypothesis.

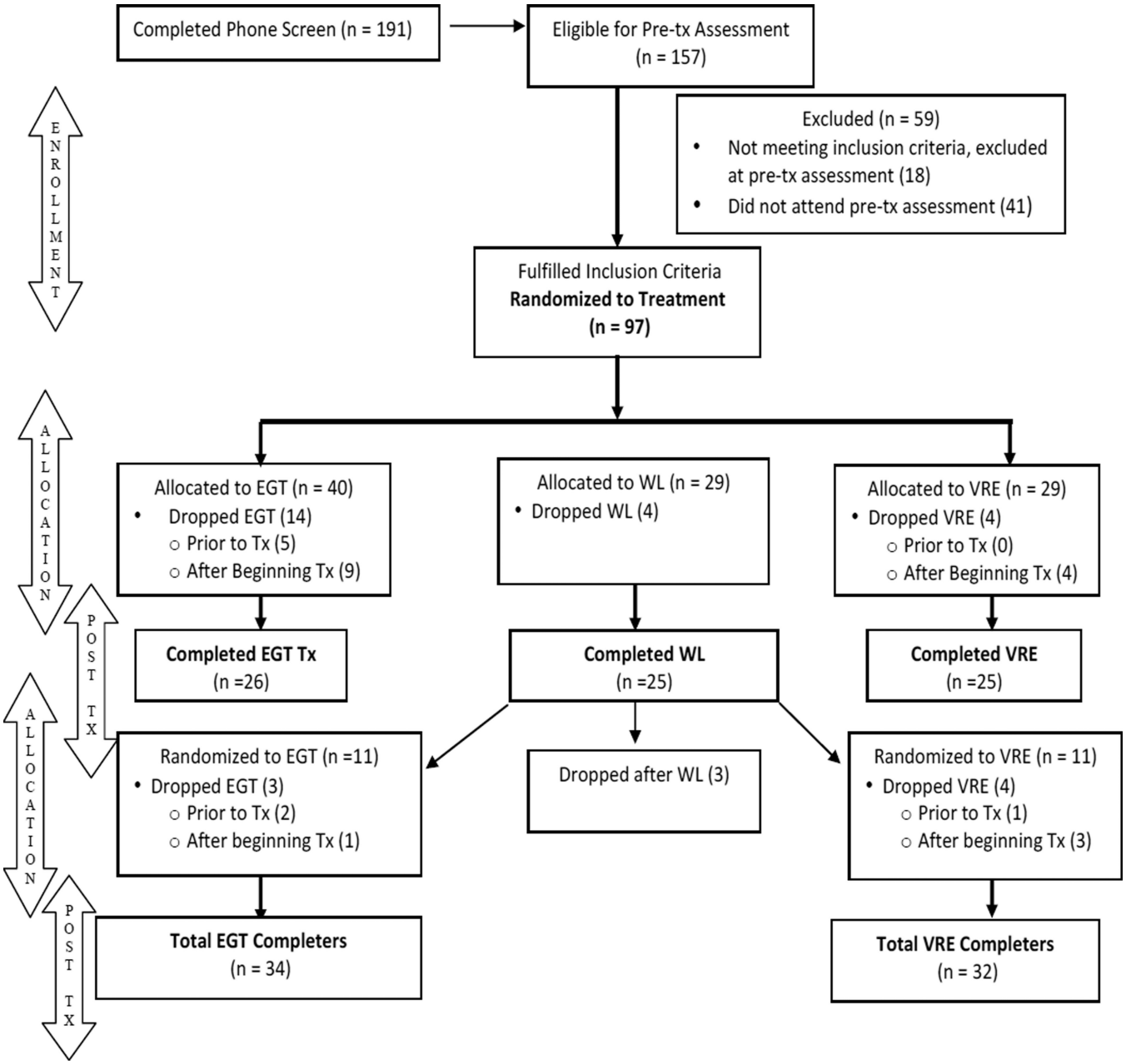

Figure 1.

CONSORT participant flow chart for Study 1. EGT = Exposure Group Therapy; WL = Wait List; VRE = Virtual Reality Exposure Therapy.

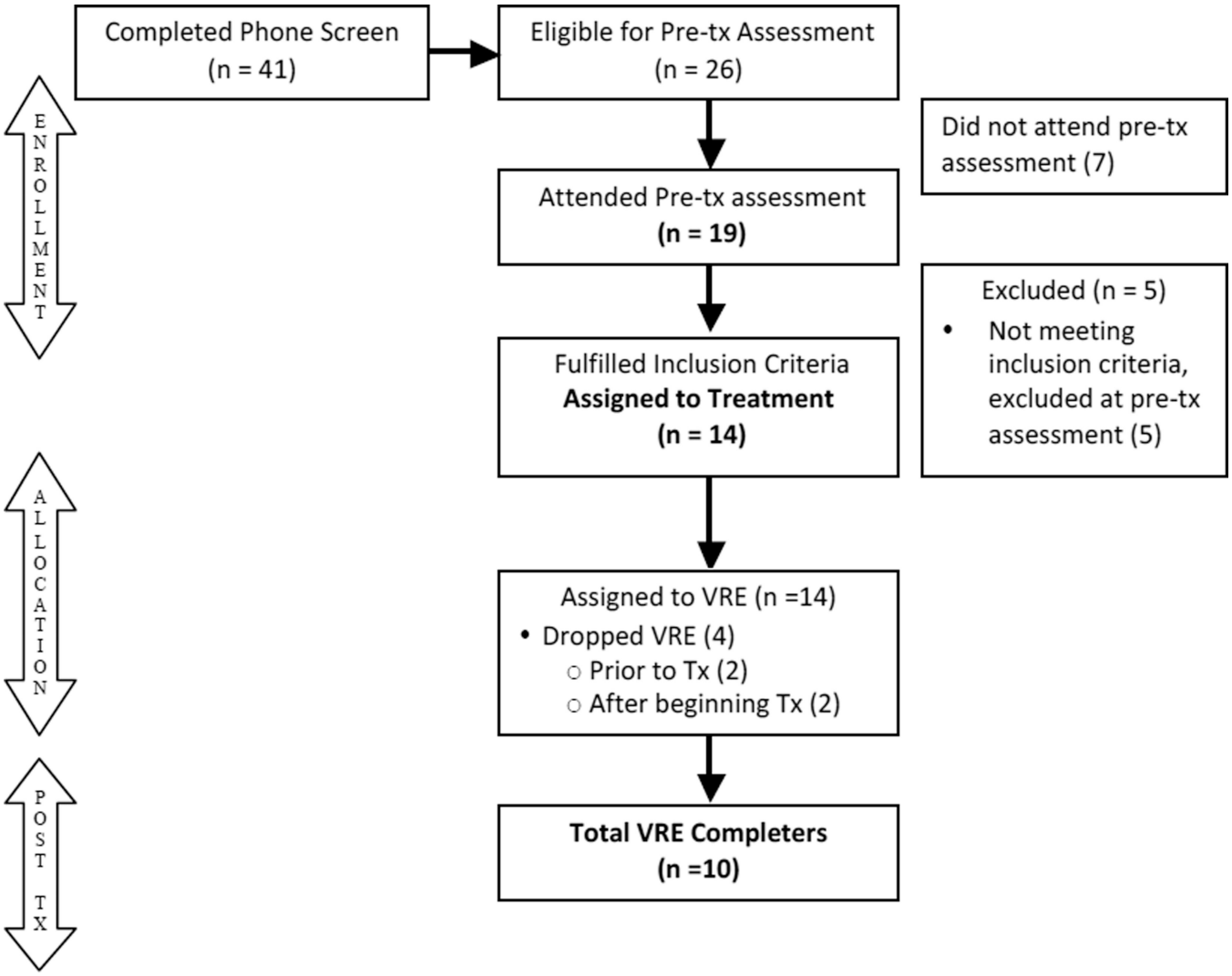

Figure 2.
TREND participant flow chart for Study 2. VRE = Virtual Reality Exposure Therapy.

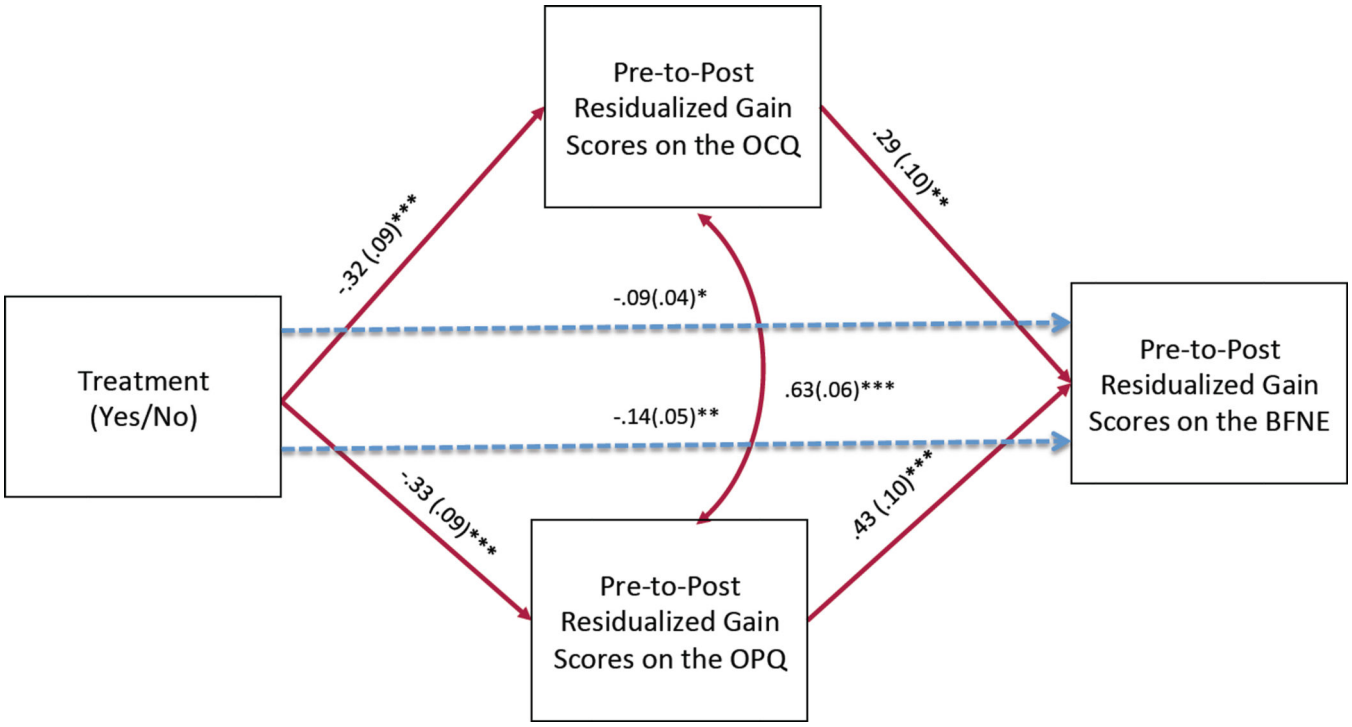

Figure 3.

Multiple mediators path model. BFNE = Brief Fear of Negative; OCQ = Outcome Cost Questionnaire; OPQ = Outcome Probability Questionnaire. Parameter estimates are reported with standard errors in parentheses. * = Parameter estimate is significant at the .05 level; ** = Parameter estimate is significant at the .01 level; *** = Parameter estimate is significant at the .001 level.

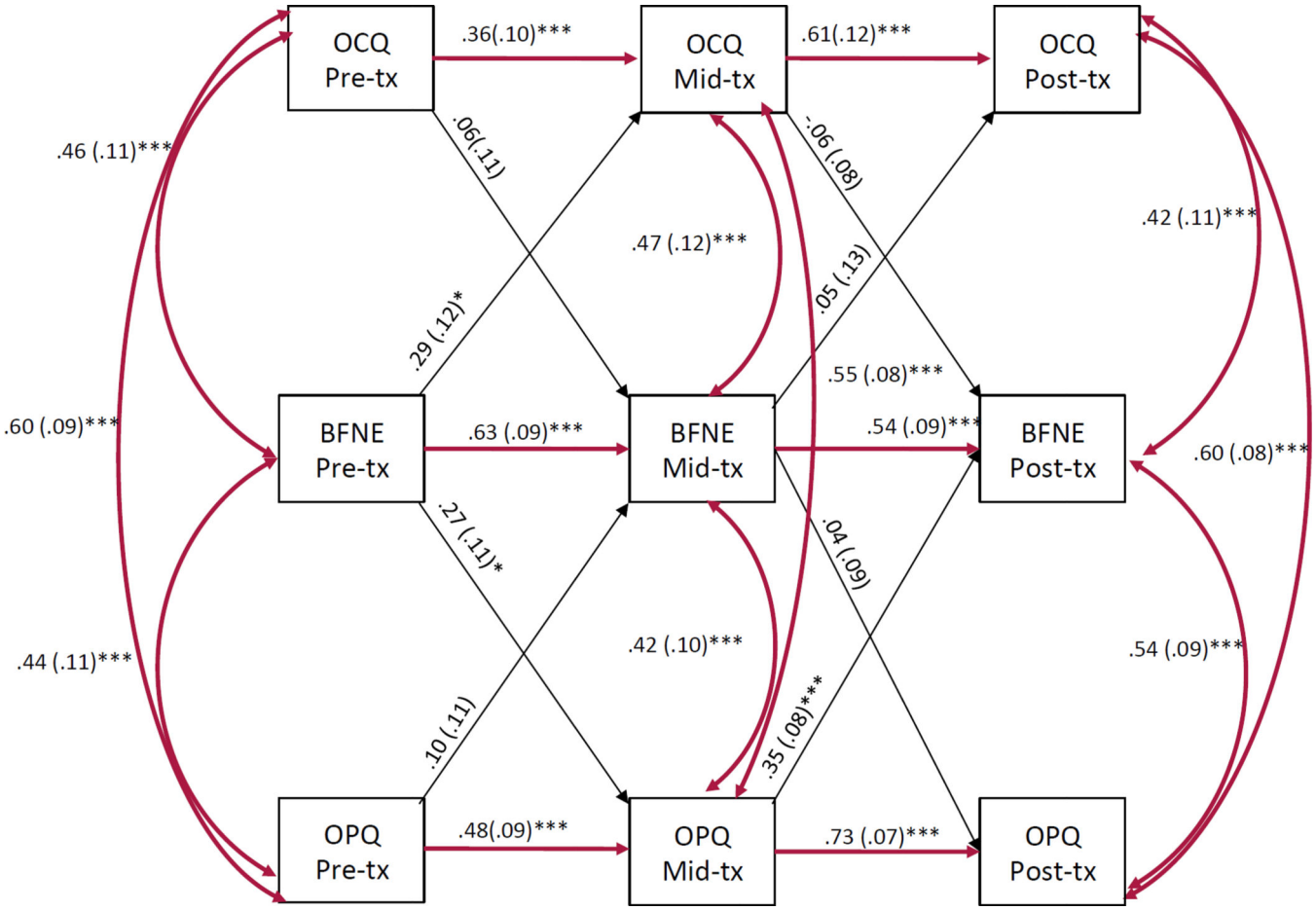

Figure 4.

Cross-lagged panel design path diagram relating the BFNE, OCQ, and OPQ. BFNE = Brief Fear of Negative; OCQ = Outcome Cost Questionnaire; OPQ = Outcome Probability Questionnaire. Standardized parameter estimates are reported with standard errors in parentheses. * = Parameter estimate is significant at the .05 level; ** = Parameter estimate is significant at the .01 level; *** = Parameter estimate is significant at the .001 level.

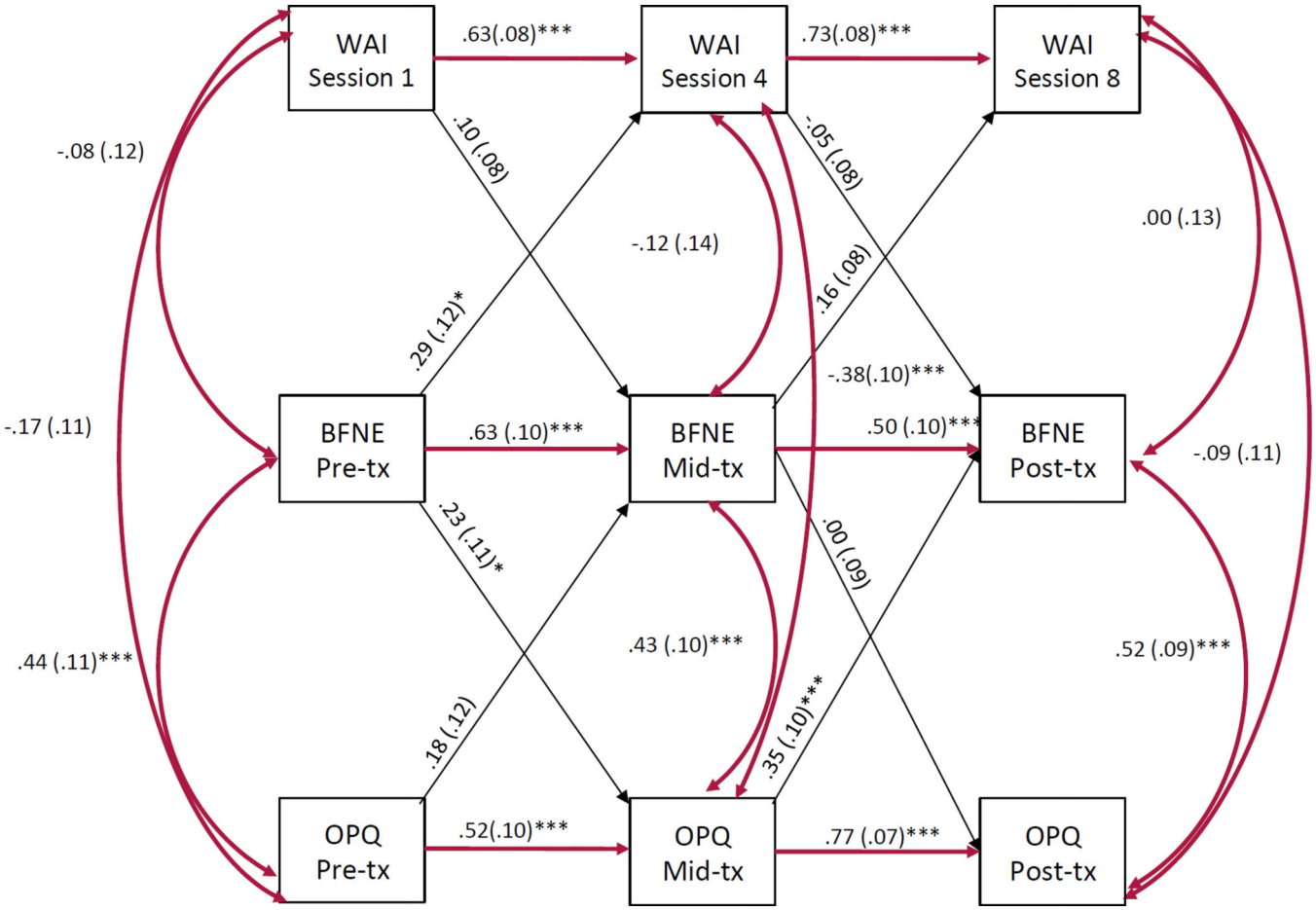

Figure 5.

Cross-lagged panel design path diagram relating the BFNE, OPQ and WAI. BFNE = Brief Fear of Negative; OPQ = Outcome Probability Questionnaire; WAI = Working Alliance Inventory. Standardized parameter estimates are reported with standard errors in parentheses. * = Parameter estimate is significant at the .05 level; ** = Parameter estimate is significant at the .01 level; *** = Parameter estimate is significant at the .001 level.

Table 1

Descriptive Statistics for the Outcome and Candidate and Rival Mediator Variables at All Time Points

	Time 1 ***M (SD)*** ***n***	**Time 2** ***M (SD)*** ***n***	**Time 3** ***M (SD)*** ***n***
BFNE	27.42 (7.89) *n* = 76	25.34 (7.68) *n* = 74	21.74 (7.06) *n* = 73
OPQ	47.48 (16.56) *n* = 76	34.30 (16.78) *n* = 74	25.57 (15.51) *n* = 75
OCQ	55.88 (10.84) *n* = 73	43.58 (17.21) *n* = 73	33.24 (19.23) *n* = 74
WAI	74.03 (8.63) *n* = 65	78.01 (6.49) *n* = 62	79.01 (6.75) *n* = 64

Note: BFNE = Brief Fear of Negative Evaluation; OPQ = Outcome Probability Questionnaire; OCQ = Outcome Cost Questionnaire; WAI = Working Alliance Inventory. BFNE, OPQ, and OCQ were collected at pretreatment, midtreatment, and posttreatment. WAI was collected after Session 1, Session 4, and Session 8.