Online Cognitive Bias Modification for Interpretation to Reduce Anxious Thinking During COVID-19

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Abstract

Anxiety disorders are highly prevalent and rates increased during the COVID-19 pandemic. However, most individuals with elevated anxiety do not access treatment due to barriers such as stigma, cost, and availability. Internet-based programs, such as cognitive bias modification for interpretation (CBM-I), hold promise as a tool to increase access to care. Before widely disseminating CBM-I, we must rigorously test its effectiveness, and determine whom it is best positioned to benefit. The present study compared CBM-I against a psychoeducation active comparison condition offered through the public website MindTrails, and also tested whether anxiety tied to COVID-19 moderated the effectiveness of these interventions. Adults with moderate-to-severe anxiety (N = 609) were randomly assigned to receive five sessions of CBM-I or psychoeducation. As predicted (https://osf.io/2dyzr), CBM-I was superior to psychoeducation at reducing anxiety symptoms (on our primary but not secondary anxiety measure: d = -0.30), reducing negative interpretation bias (d range= -0.34 to -0.47), and increasing positive interpretation bias (d = 0.83). Gains were maintained at 2- and 6-month follow-up assessments for all outcomes except positive interpretation bias. Results also indicated that individuals higher (vs. lower) in baseline COVID-19 anxiety experienced stronger decreases in anxiety symptoms while receiving CBM-I and stronger increases in anxiety while receiving psychoeducation (increases within the psychoeducation condition occurred only on one anxiety measure). These findings suggest that CBM-I may be a useful anxiety-reduction tool for individuals experiencing higher (vs. lower) anxiety tied to COVID-19. Future work is needed to further evaluate whether CBM-I is ready to be disseminated on a wider scale, and who may benefit most.

Keywords: cognitive bias modification, COVID-19, psychoeducation, interpretation bias, anxiety

Online Cognitive Bias Modification for Interpretation to Reduce Anxious Thinking during COVID-19

Anxiety disorders are highly prevalent, with some estimates suggesting 19% of adults in the United States experience them each year (Alonso et al., 2018). These prevalence rates have only increased since the onset of the COVID-19 pandemic, with one study finding that in 2020, 25.5% of adults experienced symptoms of an anxiety disorder (Czeisler et al., 2020; though see also Ankin et al., 2021). Additionally, there is evidence that very few people (27.6%) with anxiety disorders access any treatment, and even fewer (9.8%) access minimally adequate treatment (Alonso et al., 2018). This gap between treatment need and treatment access is driven by several barriers, including mental health stigma, cost, and availability of therapists (e.g., Goetter et al., 2020). Digital interventions that individuals can complete via the internet with minimal/no input from a therapist have emerged as a useful tool to increase access to care, as they are well-positioned to overcome many of the usual barriers to treatment access.

Cognitive bias modification for interpretations (CBM-I) is one such tool. It is a digital intervention that can be used as a stand-alone program or a precursor/adjunct/follow-up to other care. CBM-I aims to decrease anxious individuals' negative interpretation bias; namely, their tendency to interpret ambiguous information in a rigidly negative or threatening manner (Amir et al., 2012; Eysenck et al., 1991). Negative interpretation bias has been implicated in the development and maintenance of anxiety symptoms (Mathews & MacLeod, 2005; Rapee & Heimberg, 1997). CBM-I targets this bias by providing participants with repeated practice resolving ambiguity in (typically) benign or positive ways (Mathews & Mackintosh, 2000); in "scenario training," for example, participants read ambiguous scenarios and fill in the missing letter of the final word to resolve the ambiguity in a positive/benign manner. In doing so, CBM-I aims to decrease a rigid, negative interpretation bias and increase cognitive flexibility, which, in turn, is theorized to reduce anxiety symptoms.

Research to date testing CBM-I for anxiety has yielded some promising findings, though there are certainly mixed results. Lab-based studies suggest that positive CBM-I (in which participants are presented with scenarios that always resolve positively) effectively reduces anxiety and negative interpretation bias, and increases positive interpretation bias (e.g., Steinman & Teachman, 2010) relative to comparison conditions (e.g., a 50-50 condition in which scenarios resolve positively vs. negatively half of the time, and no-training conditions). Studies of online CBM-I have also reported shifts in interpretation bias (Hoppitt et al., 2014; Salemink et al., 2009, 2014) and anxiety (Salemink et al., 2009.) However, effects of both lab-based and online CBM-I on anxiety reductions have been equivocal. A meta-analysis conducted by Fodor et al. (2020) found that CBM-I significantly reduced anxiety when compared to waitlist and sham conditions, but the prediction intervals were large and included zero, indicating the need for further investigation of the robustness of these effects.

Given the mixed state of the CBM-I literature, it is important to examine the conditions under which CBM-I is effective, and for whom. Along with variations in the intervention itself[1], these "boundary conditions" may include the comparison conditions used to evaluate CBM-I's relative effectiveness, the specific outcomes evaluated, and characteristics of the individuals completing CBM-I. With this in mind, the present paper aims to examine: (1) whether CBM-I is effective at shifting anxiety and interpretation bias when compared to a psychoeducation

[1] We also tested differences between small variations in the CBM-I conditions as part of the intervention's long-term iterative development work. These variations are detailed in the supplement S1. The four CBM-I conditions were quite similar to one another in terms of their effects on shifts in anxiety and interpretation bias. Further, we anticipated that all CBM-I conditions would result in more improvement than psychoeducation. For more details, see the supplement S1.

intervention (a more rigorous active comparison condition than no training or neutral training alternatives); and, given the intervention was offered during the COVID-19 pandemic, (2) whether individuals' level of anxiety tied to COVID-19 moderates the effectiveness of CBM-I vs. psychoeducation.

Does the Comparison Condition Make a Difference?

Some scholars (e.g., Ji et al., 2021) have posited that the choice of a comparison condition may partially explain the equivocal effects of online CBM-I on anxiety symptoms. They note that a 50-50 CBM-I training condition (a common comparison condition in which scenarios resolve positively 50% of the time and negatively 50% of the time) may provide practice at resolving ambiguous scenarios in both positive and negative ways, which could in turn increase cognitive flexibility and reduce anxiety. Indeed, Ji et al. (2021) observed that prior "null" effects of positive CBM-I on anxiety symptoms have often occurred in studies where the positive (active) and 50-50 (comparison) CBM-I conditions have both been associated with reduced anxiety (e.g., Salemink et al., 2014).

One way to expand our knowledge of the conditions under which CBM-I works involves comparing it against other interventions that have been shown to be effective. Psychoeducation is a good candidate because prior work suggests it effectively reduces anxiety (e.g., Newby et al., 2018; Norr et al., 2017; Shah et al., 2014), though there have also been null findings (e.g., Taylor-Rodgers & Batterham, 2014). Moreover, because psychoeducation interventions target some different processes and are dissimilar in format to CBM-I training, they circumvent some of the challenges that have emerged when using CBM-I 50-50 conditions as a comparison. The present study will test whether CBM-I is effective at reducing anxiety and interpretation bias when compared to a psychoeducation condition.

CBM-I during COVID-19

A prominent and novel source of anxiety for many individuals has been the COVID-19 pandemic and its consequences (CDC, 2021). The pandemic presents a complex set of challenges, including the threat of physical illness, loss of loved ones, job loss, social isolation, changes to the workplace and school, among others (Fruehwirth et al., 2021; Levin, 2021; McGinty et al., 2020). Given this, it is not surprising that anxiety rates have increased since the onset of COVID-19 (e.g., Fruehwirth et al., 2021; Salari et al., 2020). This increase in anxiety raises many questions, including whether existing interventions can meet the needs of individuals experiencing high levels of anxiety tied to COVID-19.

On the one hand, there are at least two reasons to suspect that individuals higher (vs. lower) in COVID-19 anxiety may not benefit from CBM-I. First, it is possible that these individuals' anxiety is an appropriate reaction to real threats in their environment that have potentially severe consequences (e.g., serious illness). In other words, it is possible that elevated levels of COVID-19 anxiety are *not* stemming from interpreting ambiguity and uncertainty in an overly catastrophic way but, rather, from accurately appraising a threatening situation. If this is the case, CBM-I may not be beneficial to those with higher (vs. lower) COVID-19 anxiety. Second, it is possible that individuals with higher (vs. lower) COVID-19 anxiety would not benefit from CBM-I because the training scenarios are not specific to COVID-19, and as such, may not address these individuals' particular concerns.

There are also at least two reasons to suspect that individuals with higher (vs. lower) COVID-19 anxiety may benefit from CBM-I. First, COVID-19 and its associated challenges are characterized by marked ambiguity and uncertainty, including how the virus is transmitted, what level of safety precautions are sufficient, the potential dangers of contracting the virus, potential long-term health consequences, and uncertainty about the health and safety of loved ones. Such high levels of ambiguity and uncertainty create opportunities for overly negative interpretations to play a role in increasing anxiety. That is, although COVID-19 presents a real threat, higher levels of COVID-19 anxiety may be driven at least in part by biased negative interpretations of the ambiguity associated with COVID-19 (e.g., interpreting a single sneeze as a definite sign of COVID-19). If this is the case, then a program such as CBM-I, which targets negative interpretation bias, may be helpful to individuals reporting higher (vs. lower) levels of COVID-19 anxiety. Second, if individuals with higher (vs. lower) COVID-19 anxiety are experiencing anxiety for the first time in response to the pandemic, their interpretation bias may be less ingrained and, as such, malleable by a low intensity program such as CBM-I.

Present Study

The present study tests: (1) whether CBM-I (and its variants; see supplement S1) effectively shifts anxiety and interpretation bias relative to psychoeducation; and (2) whether COVID-19 anxiety moderates the effects of CBM-I vs. psychoeducation. To this end, we recruited community adults with moderate-to-severe anxiety symptoms to participate in a free, online, five-week CBM-I program. Participants were randomly assigned to complete a psychoeducation intervention (active comparison condition) or one of four highly similar versions of CBM-I training (see supplement S1). Regardless of the condition, participants were encouraged to complete five sessions of the program and were assessed before beginning the program, immediately following sessions 1-5, and at 2-month and 6-month follow-ups.

Hypotheses

We preregistered several directional hypotheses (see https://osf.io/2dyzr for full preregistration). We expected that all CBM-I conditions would be more effective at reducing anxiety and interpretation bias over time relative to the psychoeducation comparison (Hypothesis 1). Additionally, we proposed competing hypotheses regarding whether participants' level of COVID-19 anxiety (measured at baseline) would (Hypothesis 2a) or would not (Hypothesis 2b) moderate the effects of the intervention conditions on reductions in anxiety and interpretation bias over time. Taken together, the present study will advance our understanding of CBM-I in two novel ways. First, it will provide a more rigorous test of the effectiveness of CBM-I as a tool to improve anxiety and interpretation bias by comparing it with another effective anxiety intervention. Second, it will examine whether CBM-I can help address the rising demand for mental health services as individuals cope with the COVID-19 pandemic.

Method

Participants

Participants are 609 visitors to a public web site offering free CBM-I interventions as part of a research study (MindTrails; https://mindtrails.virginia.edu/). Individuals were eligible to participate if they endorsed at least moderate levels of anxiety on either the Depression Anxiety, Stress Scales-Short Form: Anxiety subscale (DASS-AS; adapted from Lovibond & Lovibond, 1995) and/or the Overall Anxiety Severity and Impairment Scale (OASIS; adapted from Norman, Cissell, Means-Christensen, & Stein, 2006). Individuals who scored 10 or above on the DASS-AS or 6 or above on the OASIS, and were 18 years or older (19 or older in Nebraska; 21 or older in Puerto Rico) were eligible to participate. Demographic characteristics for the full sample are presented in Table 1.

Materials

Cognitive Bias Modification for Interpretation (CBM-I). Participants allocated to the CBM-I conditions completed up to five sessions of positive CBM-I (90% positive scenarios and

10% negative scenarios within each session). Most CBM-I scenarios were adapted from Mathews and Mackintosh (2000), Steinman and Teachman (2014) and Ji et al. (2021). They were constructed to depict everyday ambiguous situations that individuals may find anxiety-provoking and that resolve in either a non-threatening (positive or benign) or threatening (negative, anxiety-relevant) manner. Participants were asked to read a series of scenarios and imagine themselves vividly within each situation. Following Ji and colleagues (Ji et al., 2020), each scenario was presented with a picture to promote episodic simulation (i.e., participants vividly imagining the scenario and seeing the ambiguous situation in a new way) and user engagement (making scenarios more relatable and enjoyable to participants). An example scenario was: "You have completed a first aid course and now have to take a practical test in which you demonstrate what you learned in front of your classmates. You spent the evening before practicing for the difficult test. As you begin the test, your classmates observing you think you are…*_repared*" [positive resolution]. After reading each scenario, participants were asked to select one of four letters to accurately complete the final word in the scenario (i.e., "p" to complete "prepared" above). They were also asked to answer a comprehension question that aimed to reinforce the positive/negative interpretation of the scenario (e.g., "Do your classmates think you are prepared for the practical test?") by selecting either (a) "Yes" or "No"; or (b) a positive interpretation or a negative interpretation (the choices presented varied throughout CBM-I training sessions). Moreover, participants were provided with feedback regarding whether their answer was correct or incorrect (participants could answer comprehension questions several times, and had to answer them accurately to progress through the training).

Conditions.

Psychoeducation (no-training comparison). In this condition, participants completed five weekly sessions of psychoeducation. The psychoeducation sessions were interactive (e.g., participants responded to questions throughout) and covered: (1) an introduction to anxiety, (2) anxiety symptoms and disorders, (3) prevalence, causes, and maintenance of anxiety, (4) the impact of anxiety, and (5) anxiety management. Each session took under five minutes to complete (mean completion time per session ranged from 1:03-1:46 minutes).

CBM-I. Participants assigned to a CBM-I condition completed five weekly sessions of positive CBM-I, each consisting of 40 scenarios. Scenarios were presented in blocks of 10. Within each block, five scenarios were tied to social threat (e.g., being evaluated by other people), two to psychophysical and physical symptoms (to address anxiety sensitivity; e.g., concerns about visible signs of anxiety, such as trembling), one-to-two to health anxiety (e.g., going to the doctor) and one-to-two to other threats (i.e., worry or anxious thinking more generally). Moreover, within each session, 36 scenarios ended positively, while the remaining four ended negatively. Across the different sessions, the level of difficulty tied to scenarios varied: in the first two sessions, participants were asked to fill in a single letter to complete the last word in the scenario, in the third and fourth sessions, they were asked to fill in two letters, and in the fifth session, they were asked to fill in two letters for half of the scenarios, and the entire last word for the other half of the scenarios. The comprehension questions also varied to increase engagement, such that in the first, third and fifth sessions, participants were presented with binary Yes/No answer choices, while in the second and fourth, they were asked to select between a positive and a negative interpretation. Each weekly session took approximately 12 minutes to complete.

Of note, in the present study, participants who were randomized to complete CBM-I training (vs. psychoeducation) received one of four, highly similar versions of CBM-I: CBM-I with 40 scenarios (standard CBM-I), CBM-I with 30 scenarios, CBM-I plus a psychoeducation component, and CBM-I plus writing a new scenario (see supplement S1 for additional details about these conditions).

Measures.

COVID-19 anxiety (measure developed by our team). Participants' level of anxiety tied to COVID-19 was assessed via a one-item measure presented as part of a broader seven-item measure that asked participants to rate the extent to which they routinely experience moderate-to-severe anxiety across several domains (social anxiety, panic, agoraphobia, specific phobia, obsessive-compulsive thoughts/behaviors, trauma, and COVID-19.) The COVID-19 item read "Anxiety about COVID-19 and its impact on my and others' health, work, relationships, finances, and/or social life." Participants rated the item on a 5-point Likert-type scale (0 = *Not at all*; 5 = *Extremely*). Higher scores indicate higher anxiety tied to COVID-19.

Anxiety symptoms.

Overall Anxiety Severity and Impairment Scale (OASIS; Norman et al., 2006). The OASIS is a five-item measure that assesses anxiety frequency, anxiety severity, anxiety-linked avoidance, as well as impairment at school, work, and social relationships over the past week. Participants were asked to rate each item on a scale of 0 (lowest impairment/severity) to 4 (highest impairment/severity). The OASIS has demonstrated good psychometric properties, including high internal consistency and convergent validity among adults seeking treatment for anxiety (Bragdon et al., 2016; Campbell-Sills et al., 2009). Additionally, there is evidence of strong test-retest reliability after one month (Norman et al., 2006). Higher scores indicate more frequent, severe, and impairing anxiety. Internal consistency was calculated for the intent-to-treat (ITT) sample (composed of participants who completed at least one CBM-I/psychoeducation session as part of the broader MindTrails web site and subsequently consented to participate in the present study). McDonald's omega[2] for complete item-level OASIS data at screening was good, ω_ts and 95% confidence interval [CI] = .81 [.78, .83]. The OASIS was selected a priori to serve as the primary anxiety outcome measure.

Depression, Anxiety, Stress Scales—Short Form: Anxiety Subscale (DASS-AS; Lovibond & Lovibond, 1995). The DASS-AS asks participants to rate the extent to which they experienced each of seven anxiety symptoms (e.g., "I felt I was close to panic") over the past week on a 4-point Likert-type scale (0 = *Not at all*; 3 = *Most of the time*). The DASS-AS has demonstrated good psychometric properties, including high internal consistency and good convergent and discriminant validity among adults (Sinclair et al., 2012). Higher scores indicate higher anxiety symptoms. Internal consistency for the ITT sample using complete item-level data at screening was good, ω_ts and 95% CI = .82 [.79, .84]. The DASS-AS served as the secondary anxiety outcome measure.

Interpretation bias.

Recognition Ratings (RR) Task. The RR task is a measure of interpretation bias modified from Mathews and Mackintosh (2000). The RR task asks participants to read and imagine nine scenarios about social situations, health, and other domains (e.g., work). These scenarios are similar in format to the CBM-I scenarios, except each scenario included a title and remained ambiguous in terms of valence even after the word fragment is completed. For instance, participants were presented with a scenario titled "The Blood Test" that read: "You are

[2] McDonald's omega was calculated in lieu of Chronbach's alpha given limitations associated with the latter (Dunn et al., 2014). Omega was calculated in R using the package "MBESS" version 4.8.0 (Kelley, 2015).

at a routine doctor's appointment. At the appointment, the doctor decides to run a few blood tests to check your health. The doctor says he will call you in a few weeks, and you will find out your test results at that t_me." After selecting the letter that correctly completed the word fragment for the scenario (e.g., "time" in the example provided), participants were presented with a comprehension question (e.g., "Did you take blood tests?"). After completing all the ambiguous scenarios and comprehension questions, participants were presented with the title of each scenario (e.g., "The Blood Test"), and a stem sentence (e.g., "The doctor says he will call you in a few weeks..."), along with four disambiguated interpretations of the corresponding scenario. Two of the interpretations for each scenario were threat-related (negative interpretation: "And you think that you will not be able to stand your anxiety while you wait"; positive interpretation: "And you know that you can handle your anxiety while you wait") and two were threat-unrelated (negative: "And you are annoyed because your doctor is not very friendly"; positive: "And you think about how nice your doctor is").

To assess participants' interpretation bias, they were asked to rate how similar each disambiguated interpretation was to what they believed was the meaning of the original scenario using a 4-point Likert-type scale (1 = *Very different*; 4 = *Very similar*). The RR task has demonstrated adequate/good internal consistency (Ji et al., 2021). At each timepoint, participants' endorsement of threat-relevant negative interpretations was averaged to index negative interpretation bias, and endorsement of threat-relevant positive interpretations was averaged to index positive interpretation bias. Internal consistency for the ITT sample using complete item-level data at the pretreatment assessment was good for negative interpretation bias, ω_ts and 95% CI = .79 [.77, .82], and adequate for positive interpretation bias, ω_ts and 95%

CI = .74 [.70, .77]. Higher scores indicate higher levels of negative and positive interpretation bias, respectively.

Brief Body Sensations Interpretations Questionnaire (BBSIQ; Clark et al., 1997). The BBSIQ is a 14-item measure of interpretation bias. It presents participants with 14 ambiguous situations (e.g., “You have visitors over for dinner and they leave sooner than you expected. Why?”) and three alternatives to disambiguate the scenario. For each situation, one alternative is always negative and threat-relevant (e.g., “They did not enjoy the visit and were bored with your company”), while the other two are benign in terms of the anxiety-linked threat but may still be neutral or negative (e.g., “They did not wish to outstay their welcome”; “They had another pressing engagement to go to”). For each situation, participants were asked to rate the likelihood that each explanation for the situation would be true if they found themselves in that situation on a 5-point Likert-type scale (0 = *Not at all likely;* 4 = *Extremely likely*). Half of the situations depicted external threats (e.g., threatening social situations), while the other half concentrated on bodily sensations. The BBSIQ has demonstrated good psychometric properties, including high internal consistency, convergent validity, and adequate test-retest reliability following a 3-month interval (Casey et al., 2005; D. M. Clark et al., 1997). At each time point, the average of participants’ likelihood ratings for negative alternatives was used as a measure of negative interpretation bias (following Steinman & Teachman, 2010, 2015). Internal consistency for the ITT sample using complete item-level data at the pretreatment assessment was very good, ω_ts and 95% CI = .91 [.90, .92]. Higher scores indicate higher levels of negative interpretation bias.

Measurement schedule

COVID-19 anxiety was measured at the outset of the study, prior to beginning the CBM-I/psychoeducation sessions. All other measures were administered at several timepoints: the

OASIS was administered during screening as a measure of eligibility, following sessions 1-5, and at 2-month and 6-month follow-up assessments; the DASS-AS was administered during screening to determine eligibility, at the end of sessions 3 and 5, and at 2-month and 6-month follow-up assessments; the Recognition Ratings task and BBSIQ were administered pre-training, after sessions 3 and 5, and at 2- and 6-month follow-up assessments.

Procedure

Participants were recruited to the online anxiety reduction program study website (MindTrails; https://mindtrails.virginia.edu/) via invitations from research panels (*Focus Forward* and *L & E Research*), and media coverage in the news and radio. Additionally, participants noted that they found MindTrails through other avenues, including friends/family, internet searches for mental health information, the Project Implicit Health website, Facebook, mental health professional recommendation, etc. Upon arriving at the MindTrails site, individuals had the opportunity to complete a screener questionnaire (consisting of the DASS-AS and the OASIS) to determine their eligibility for the program. Of note, MindTrails is a free website and is open to anyone who is eligible and wishes to enroll. Given that the current study was offering a small financial incentive for completion of the program, enrollment was limited to 609 participants. Participants who did not enroll in the present study were able to participate in a highly similar version of MindTrails that did not include compensation (these participants are not included in analyses).

Upon completing the screener questionnaire, eligible participants were invited to create an account and enroll in MindTrails. Following informed consent, participants completed a series of pre-training questionnaires, including demographic information, mental health and treatment history, anxiety tied to COVID-19, interpretation bias, and other measures of psychopathology and well-being not relevant to the present study (the full list of measures is available upon request). Participants then began the first session of MindTrails (i.e., the first module of psychoeducation or the first CBM-I training session, depending upon their condition). Following the first session, participants were shown a new consent form for the present study and offered the opportunity to participate (during the period from 7/10/2020 to 12/7/2020; see CONSORT diagram in Figure 1). We elected to invite individuals to participate following the first session because we wished to recruit individuals who were truly interested in completing the intervention (vs. just browsing the MindTrails program, which occurs fairly frequently). Participants had to wait at least five days between each training session. Moreover, participants were given 21 days to complete each session (after 21 days of inactivity, their account closed automatically, though they could re-open it and continue from where they left off if they wished). Two and 6 months following the 5^{th} session, participants were able to complete the 2-month and 6-month follow-up assessments, respectively. To maximize response rates, each follow-up assessment remained open for 3 months (following a personal communication with Kristen P. Lindgren).

Analytic Plan

All analyses focused on the ITT sample. We performed analyses using R version 4.0.3 and the package *OpenMx* (version 2.19.5; Neale et al., 2016). All models were univariate, such that anxiety symptoms and positive and negative interpretation bias served as individual outcome variables.

We used latent growth curve (LGC) modeling to test our hypotheses regarding change in anxiety and interpretation bias over time. This approach is well-suited for longitudinal data because it enables modeling of between-person variability in within-person trajectories of change as a function of time (Curran et al., 2010). Within LGC models, repeated observations of a given variable are treated as indicators on one or more latent variables (Curran et al., 2010). In the present study, we estimated intercept and slope latent variables, which captured individuals' level on a given observed variable at a chosen point in time (e.g., participants' OASIS severity at the last assessment point) and their trajectories of change over time, respectively (Raudenbush & Bryk, 2002).

We estimated piecewise linear LGC models to examine changes in anxiety and interpretation bias between the first assessment point and the 6-month follow-up. Specifically, we fit one intercept and two linear slope latent variables (within the same model) for each outcome variable. The first slope variable (Slope 1) captured changes in anxiety/interpretation bias between the first assessment for a given variable and the session 5 assessment (which occurs following the final CBM-I/psychoeducation session). The second slope variable (Slope 2) captured changes between session 5 and the 2- and 6-month follow-up assessments (see Figure 2 for a path diagram of the LGC model for OASIS scores). As such, the "knot" in these models (i.e., the timepoint at which one slope ends and the other begins; Flora, 2008) was set a priori at session 5, given this is when CBM-I/psychoeducation concluded and the follow-up assessment period began. By fitting an individual linear slope for each time period, we allowed for different trajectories of change during (capturing intervention effects) vs. after the intervention (capturing maintenance of gains). Of note, we elected to fit a piecewise linear (vs. linear or a quadratic) model because we expected the rate of change would differ between the intervention and follow-up periods, such that we would observe stronger shifts in anxiety and interpretation bias during the intervention vs. during follow-up.

Across all models, evaluation of model fit relied primarily on estimates of relative (vs. absolute) fit, whenever available. This decision was made because estimates of relative fit provide a more precise test of the importance of a given parameter to model fit (e.g., whether fit worsens or improves if a parameter is estimated freely vs. fixed; Curran et al., 2010), though we also report estimates of absolute fit.

Intervention effects on changes in anxiety and interpretation bias.

To examine the effects of our study conditions on the mean of Slope 1 for each outcome variable, we used multigroup LGC modeling. This approach was chosen because it is well-suited for analyzing differences in one or few parameters (such as the mean slope) across two or more groups (e.g., Clark et al., 2015; McClendon et al., 2019). For each outcome variable, we began by testing mean differences in Slope 1 across the five conditions (the four CBM-I conditions and psychoeducation; this test is analogous to conducting an omnibus analysis of variance before performing pairwise comparisons). We took this approach because, while our primary interest was comparing CBM-I to psychoeducation, we did not want to prematurely combine the CBM-I variants without first checking for variability that would suggest combining them was problematic. To this end, we first estimated a fully saturated model (i.e., a model in which all parameters were estimated freely within each study condition). This fully saturated model (Model 1) was fit by subsetting the data by condition and fitting a fully saturated submodel for each condition independently. The five fully saturated submodels were then input into a "container" model (i.e., a model that is composed of the submodels and allows calculation of fit statistics based on parameters estimated in several groups); we used the *OpenMx* function *mxFitFunctionMultigroup* to estimate absolute fit statistics for a model in which all parameters were estimated freely across the study conditions. Once this fully saturated model had been estimated, we built a nested version of it (Model 2), in which we constrained the mean of Slope 1 to be equal across groups. We then estimated a container model and absolute fit statistics for Model 2 and compared the container models for Models 1 and 2. The difference in log likelihood units between Model 1 and Model 2 indicated whether constraining the mean of Slope 1 to be equal across all conditions significantly decreased model fit, and as such, whether it was unreasonable to assume that the trajectory of change during the intervention was the same across study conditions. Log likelihood units are reported along with *p*-values, with *p*-values < .05 indicating statistically significantly worse fit in Model 2 vs. Model 1. If Model 2 fit significantly worse than Model 1, this indicated that the mean of Slope 1 was different between *at least* two of the five study conditions. As such, this indicated that at least two study conditions had significantly different impact on changes in anxiety/interpretation bias during the intervention.

For each outcome variable, *if* the "omnibus" multigroup LGC model comparison indicated that assuming equal mean slopes across conditions decreased model fit, we performed the follow-up comparisons below:

(1) We first examined differences between CBM-I conditions' effects on changes in anxiety and interpretation bias during the intervention. The purpose of this analysis was to determine whether it was appropriate to collapse across CBM-I conditions when comparing the effects of CBM-I vs. psychoeducation. To this end, we compared a model in which the mean slopes (for Slope 1) were estimated freely across the four CBM-I groups to a model in which the means were constrained to be equal across the four CBM-I groups (across both models, the mean of Slope 1 was estimated freely for the psychoeducation group). For each outcome variable, whenever model comparisons indicated that assuming equal mean slopes produced significantly worse fit, we

conducted pairwise comparisons to identify which CBM-I conditions were linked with significantly stronger or weaker changes in anxiety/interpretation bias during the intervention.

(2) Next, to test our hypothesis that CBM-I (vs. psychoeducation) would lead to greater changes in anxiety and interpretation bias, we compared mean slopes between CBM-I and psychoeducation conditions.

Maintenance of Gains at Follow-Up. We examined the effects of study condition on the mean trajectory of change following the intervention (Slope 2) by following the same analytic plan described above to test intervention effects (i.e., multigroup LGC analyses to compare the mean of Slope 2 across conditions).

Moderation Effect of COVID-19 Anxiety. The interactive effect of anxiety tied to COVID-19 and study condition on outcome variables was tested via multigroup LGC modeling. This method was chosen over a moderator model because it allows the estimation of absolute fit statistics as well as relative fit statistics (i.e., log likelihood differences) in *OpenMx*. To examine moderation effects of COVID-19 anxiety on Slope 1 and Slope 2, we compared a fully saturated model (in which the effect of COVID-19 anxiety on Slopes 1 and 2 was estimated freely within each study condition) with two nested models. The first nested model (Nested Model 1) constrained the effect of COVID-19 anxiety on Slope 1 to be equal across the CBM-I and psychoeducation conditions, while the second nested model (Nested Model 2) constrained the effect of COVID-19 anxiety on Slope 2 to be equal across the CBM-I and psychoeducation conditions. The fully saturated model was compared with each nested model separately. Comparisons between the fully saturated model and Nested Models 1 and 2 indicated whether it was reasonable to assume that COVID-19 anxiety influenced the outcome variable's Slope 1 and Slope 2 in a similar way across conditions.

General Modeling Considerations. All LGC models included parameter estimates for the means and variances. Given we expected that missing data would increase as time went on in the study, we set the origin (i.e., the loading that is set to '0' on the slope latent variables) as the final assessment point for a given variable (following Hohensee et al., 2020; Ji et al., 2021). Doing so made the intercept an estimate of values at the last assessment point for a given variable. Moreover, fixing the intercept at the last assessment point maximized the amount of data we could use to estimate the slope and minimized bias due to attrition.

Effect Size. The size of the difference between conditions' effects on Slope 1 and Slope 2 was computed as growth-modeling analysis *d* (GMA *d*; Feingold, 2009). GMA *d* has the same metric as Cohen's *d* and was calculated whenever two groups had significantly different values on the mean of Slope 1 or Slope 2. We computed GMA *d* at session 5 and at the 6-month follow-up assessment to capture differences between conditions at the end of the intervention and at follow-up, respectively. We also computed GMA *d* at session 5 and at the 6-month follow-up to estimate effect sizes for changes in the outcome variable within each condition. See supplement S4 for details. Of note, GMA *d* has not yet been developed for models featuring continuous independent variables so was not computed for the moderation models (Feingold, 2018).

Missing Data. There were two forms of missing data that needed to be accounted for: (1) attrition (i.e., participants completing some but not all assessment timepoints) and (2) missing data within scales/surveys. To manage (1), we used Full Information Maximum Likelihood (FIML) to estimate model parameters, which uses raw data instead of covariance matrices. Importantly, FIML is an appropriate estimation method as long as the pattern of missingness is random (vs. predictable) based on relevant study variables (Enders & Bandalos, 2001). With this in mind, we tested the relationships between the pattern of missingness and demographic variables and study conditions (i.e., CBM-I variations or psychoeducation).

To manage (2), within each scale, whenever participants endorsed "prefer not to answer" on a given item (e.g., on one of the OASIS items), we calculated total scores (e.g., the mean OASIS score) based on available items. Because we anticipated that the proportion of missing data of this type would be small, using available items (vs. using another method such as multiple imputation) should not make much of a difference (Kline, 2015).

Results

Missing Data

The proportion of item-level missing data was low, such that across the five outcome variables, only 0.08-1.58% of participants' total scores were computed from items with at least one item missing. At the scale level, the proportion of missing data ranged from 64.21% to 71.21% across all outcome variables (see table S1 for the number of observations for each outcome per condition at each timepoint). We used non-parametric tests to examine the relationship between the pattern of missing data and demographic variables (because the amount of missing data throughout the study was not normally distributed; we selected non-parametric tests following Eberle et al., 2020). None of the demographic variables tested (i.e., age, gender, race, ethnicity, marital status, education level, employment status, income, and country of residence) significantly predicted the number of missing assessment points (see supplement S2 for more details). Given this, we did not include any demographic variables as covariates in analyses. The pattern of missing data also was not different depending on participants' randomly assigned study condition.

Intervention Effects

Means and standard deviations for the main study variables by group and study timepoint are shown in Table S1. A summary of the intervention effects is provided in Tables 2-3 and detailed below, with a focus on the effects of being assigned to any CBM-I condition (referred to as “CBM-I” below) vs. psychoeducation (see supplement S6.1 for a summary of tests comparing the effects of the four CBM-I condition variants on changes in anxiety and interpretation bias during the intervention). Intervention effects were examined by comparing models in which the mean of Slope 1 (mS1) was estimated freely to models in which it was constrained to be zero (within-condition effects) or constrained to be equal (differences between conditions) across CBM-I and psychoeducation conditions. Whenever model fit decreased significantly after constraining the mean, this was interpreted as evidence that the condition had a significant effect on the outcome (within-condition effect) or that the conditions had different effects on the trajectory of change in the outcome variable (differences between conditions).

Anxiety

OASIS

Parameter estimates for all tests of intervention effects involving the OASIS are included in Tables S2-S3, and absolute fit statistics are listed in Table S4. Participants in the CBM-I and psychoeducation conditions exhibited reductions in OASIS scores during the intervention. Reductions in OASIS scores were statistically significantly different from zero within the CBM-I group: difference in log likelihood units (diffLL) = 253.38, difference in degrees of freedom (diffdf) = 1, $p < 0.001$, difference in Akaike’s Information criteria (diffAIC) = -251.38, GMA d = -1.16. Reductions were also significantly different from zero within the psychoeducation group: diffLL = 53.03, diffdf = 1, $p < 0.001$, diffAIC = -51.04, GMA d = -0.87.

In terms of condition differences, the omnibus multigroup LGC model comparison indicated that at least two conditions had significantly different trajectories of change on the OASIS: diffLL = 10.01, diffdf = 4, $p = 0.04$, diffAIC = -2.01. This difference in fit indicated that mS1 was statistically significantly different between at least two of the study conditions. Follow-up comparisons indicated that mS1 within the CBM-I condition (mS1 = -0.73) was significantly different than mS1 within the psychoeducation condition (mS1 = -0.54), diffLL = 6.22, diffdf = 1, $p = 0.01$, diffAIC = -4.22, GMA $d = -0.30$. As shown in Figure 3, and consistent with hypotheses, participants assigned to the CBM-I conditions exhibited a stronger decline in OASIS scores over time relative to participants in the psychoeducation comparison condition.

DASS-AS

Parameter estimates for all tests of intervention effects involving the DASS-AS are included in Tables S5-S6, and absolute fit statistics are listed in Table S7. Participants in all groups reported decreased DASS-AS scores during the intervention. These reductions were significantly different from zero within the CBM-I (diffLL = 170.84, diffdf = 1, $p < 0.001$, diffAIC = -168.85, GMA $d = -0.69$) and psychoeducation (diffLL = 50.13, diffdf = 1, $p < 0.001$, diffAIC = -48.14, GMA $d = -0.74$) conditions.

In terms of condition differences, contrary to hypotheses, the omnibus multigroup LGC model comparison indicated no differences in mS1 across study conditions, diffLL = 7.39, diffdf = 4, $p = 0.11$, diffAIC = 0.60. That is, the mean trajectory of change in DASS-AS scores during the intervention was comparable across study conditions. As shown in Figure 4, DASS-AS scores decreased over time within CBM-I and psychoeducation conditions.

Interpretation bias

BBSIQ

Parameter estimates for all tests of intervention effects involving the BBSIQ are included in Tables S8-S9, and absolute fit statistics are listed in Table S10. Participants in CBM-I and psychoeducation conditions reported decreased BBSIQ scores over the course of the intervention. These reductions were significantly different from zero in the CBM-I (diffLL = 179.14, diffdf = 1, $p < 0.001$, diffAIC = -177.14, GMA $d = 0.71$) and psychoeducation (diffLL = 19.83, diffdf = 1, $p < 0.001$, diffAIC = -17.84, GMA $d = -0.23$) conditions.

In terms of condition differences, the omnibus multigroup LGC model comparison indicated a significant difference in mS1 between at least two study conditions, diffLL = 40.70, diffdf = 4, $p < 0.001$, diffAIC = -32.70. Follow-up comparisons indicated that mS1 within the CBM-I condition (mS1 = -0.12) was significantly different than mS1 within the psychoeducation condition (mS1 = -0.04), diffLL = 32.34, diffdf = 1, $p < 0.001$, diffAIC = -30.34, GMA $d = -0.47$[3]. As shown in Figure 5, and consistent with hypotheses, participants assigned to receive CBM-I exhibited a stronger decline in BBSIQ scores over time relative to participants in the psychoeducation comparison condition.

RR negative interpretation bias

Parameter estimates for all tests of intervention effects involving the RR negative interpretation bias are included in Tables S11-S12, and absolute fit statistics are listed in Table S13. Participants in all conditions reported decreased RR negative interpretation bias scores during the intervention. These reductions were significantly different from zero within the CBM-

[3] Follow-up comparisons also revealed significant differences between CBM-I conditions, and these findings are reported in the Supplement (Section S6.1). Because at least two CBM-I conditions differed significantly from one another, we compared each CBM-I condition to psychoeducation in terms of the mS1 value for BBSIQ scores. The pattern of findings for these comparisons mirrored the one reported in the main text, such that in every comparison, CBM-I condition exhibited a stronger reduction in BBSIQ scores relative to the psychoeducation condition.

I (diffLL = 75.89, diffdf = 1, $p < 0.001$, diffAIC = -73.90, GMA $d = -0.60$) and psychoeducation (diffLL = 6.70, diffdf = 1, $p = 0.009$, diffAIC = -4.70, GMA $d = -0.25$) conditions.

In terms of condition differences, the omnibus multigroup LGC model comparison indicated a significant difference in mS1 between at least two study conditions, diffLL = 13.90, diffdf = 4, $p = 0.007$, diffAIC = -5.90. Follow-up comparisons indicated that participants who completed CBM-I (mS1 = -0.07) exhibited stronger mean reductions in RR negative interpretation bias scores relative to participants who completed psychoeducation (mS1 = -0.03), diffLL = 9.11, diffdf = 1, $p = 0.002$, diffAIC = -7.11, GMA $d = -0.34$. This pattern of results is shown in Figure 6.

RR positive interpretation bias

Parameter estimates for all tests of intervention effects involving the RR positive interpretation bias are included in Tables S14-S15, and absolute fit statistics are listed in Table S16. Participants in the CBM-I and psychoeducation conditions reported increases in RR positive interpretation bias scores during the intervention. These increases were significantly different from zero in the CBM-I (diffLL = 238.29, diffdf = 1, $p < 0.001$, diffAIC = -236.29, GMA $d = 1.13$) and psychoeducation (diffLL = 6.09, diffdf = 1, $p = 0.01$, diffAIC = -4.09, GMA $d = 0.26$) conditions.

In terms of condition differences, the omnibus multigroup LGC model comparison indicated a significant difference in mS1 between at least two study conditions, diffLL = 48.83, diffdf = 4, $p < 0.001$, diffAIC = -40.83. Follow-up comparisons indicated that participants who completed CBM-I (mS1 = 0.12) exhibited a stronger mean increase in RR positive interpretation bias scores during the intervention compared with participants who completed psychoeducation (mS1 = 0.03), diffLL = 46.36, diffdf = 1, $p < 0.001$, diffAIC = -44.36, GMA $d = 0.83$. This pattern of results is shown in Figure 7.

Maintenance of Gains at Follow-Up

A summary of the maintenance of gains effects is provided in Tables 2-3 and detailed below, with a focus on the effects of CBM-I (collapsed CBM-I conditions) vs. psychoeducation (see the supplement S6.2 for tests comparing the effects of the four CBM-I conditions on changes in anxiety and interpretation bias at follow-up). Maintenance of gains was examined by comparing models in which mS2 was estimated freely or constrained to be zero (within-condition effects) or constrained to be equal (differences between conditions) across CBM-I and psychoeducation conditions. Whenever model fit decreased significantly after constraining mS2, this was interpreted as evidence that the condition had a significant effect on the outcome (within-condition effect) or that the conditions had different effects on the trajectory of change in the outcome variable (differences between conditions).

Across the majority of our outcome variables, mS2 was not significantly different from zero or significantly different across conditions: changes in OASIS BBSIQ, and RR negative interpretation bias scores were not significantly different from zero within the CBM-I or psychoeducation conditions, and changes in OASIS, DASS-AS, BBSIQ, and RR negative interpretation bias were not significantly different between any conditions (see details in supplement S7).

Meanwhile, there were significant within-group changes in DASS-AS and RR positive interpretation bias scores. DASS-AS scores decreased during the follow-up period, and these reductions (mS2 = -0.59) CBM-I were significantly different from zero within the CBM-I condition (diffLL = 6.39, diffdf = 1, $p = 0.01$, diffAIC = -4.39, GMA $d = -0.82$) but not the psychoeducation condition (diffLL = 1.52, diffdf = 1, $p = 0.21$, diffAIC = 0.47). Additionally, changes in RR positive interpretation bias scores during follow-up were significantly different from zero in the CBM-I condition (diffLL = 41.97, diffdf = 1, $p < 0.001$, diffAIC = -39.97, GMA $d = 0.67$) but not the psychoeducation condition (diffLL = 0.21, diffdf = 1, $p = 0.63$, diffAIC = 1.78).

Models involving RR positive interpretation bias scores additionally indicated condition differences between at least two study conditions, diffLL = 19.58, diffdf = 4, $p < 0.001$, diffAIC = -11.58. Follow-up comparisons indicated that participants who completed CBM-I (mS2 = -0.12) exhibited a decrease in positive interpretation bias during the follow-up period, whereas participants who received psychoeducation (mS1 = 0.01) exhibited little-to-no change during this period, diffLL = 13.91, diffdf = 1, $p < 0.001$, diffAIC = -11.91, GMA $d = 0.35$. This pattern of results is shown in Figure 7.

Moderation Effect of COVID-19 Anxiety

The effect of baseline COVID-19 anxiety on the trajectory of change during and after the intervention was evaluated in two ways. First, we estimated the effect of COVID-19 anxiety levels on Slope 1 and Slope 2. These parameters, along with their 95% CI, were used to evaluate whether COVID-19 anxiety was significantly related to the rate of change during and after the intervention within each condition. Second, we evaluated whether the relationship between COVID-19 anxiety and the trajectory of change was different across the CBM-I and psychoeducation conditions. This was done by comparing models in which the effect of COVID-19 anxiety on Slope 1/Slope 2 was calculated freely or constrained to be equal across the CBM-I and psychoeducation conditions. Worse fit in a model in which the effect of COVID-19 on the slope was constrained to be equal across conditions (vs. estimated freely) was interpreted as evidence that COVID-19 anxiety interacts with study conditions to influence change in the outcome variable. A summary of the moderation effects is provided below, with a focus on the effects of COVID-19 anxiety on change in outcome variables among participants assigned to any CBM-I condition (i.e., the collapsed CBM-I conditions) vs. psychoeducation (see supplement S8 for a summary of tests comparing the effects of COVID-19 anxiety across all conditions)

Anxiety

OASIS

Participants' baseline levels of COVID-19 anxiety predicted their changes in OASIS scores over time. Within the CBM-I condition, higher COVID-19 anxiety predicted stronger decreases in OASIS scores during the intervention (parameter estimate = -0.06, 95% CI = [-0.12, -0.01]). Meanwhile, within the psychoeducation condition, COVID-19 anxiety was not significantly related to change in OASIS scores during the intervention (parameter estimate = 0.06, 95% CI = [-0.03, 0.16]). Model comparisons indicated that the effect of COVID-19 anxiety on changes in OASIS scores during the intervention was significantly different across the CBM-I vs. psychoeducation, diffLL = 5.08, diffdf = 1, p = 0.02, diffAIC = -3.09. Parameter estimates for all tests of moderation effects involving OASIS scores are included in Tables S27 and S29, and absolute fit statistics are listed in Table S28.

DASS-AS

Participants' baseline levels of COVID-19 anxiety also predicted their changes in DASS-AS scores over time. Within the CBM-I condition, higher COVID-19 anxiety predicted stronger decreases in DASS-AS scores during the intervention (parameter estimate = -0.15, 95% CI = [-0.28, -0.02]). Meanwhile, within the psychoeducation group, higher COVID-19 anxiety predicted *increases* in DASS-AS scores during the intervention (parameter estimate = 0.29, 95%

CI = [0.04, 0.54]). Model comparisons indicated that the effect of COVID-19 anxiety on changes in DASS-AS scores during the intervention was significantly different across the psychoeducation vs. CBM-I conditions, diffLL = 9.38, diffdf = 1, p = 0.002, diffAIC = -7.38. Parameter estimates for all tests of moderation effects involving DASS-AS scores are included in Tables S30 and S32, and absolute fit statistics are listed in Table S31.

Participants' baseline levels of COVID-19 anxiety were weakly associated with the patterns of change in BBSIQ, RR negative interpretation bias, and RR positive interpretation bias scores during the intervention within all conditions. Moreover, the effect of COVID-19 anxiety on the trajectories of change in these variables did not differ significantly across study conditions. Additionally, COVID-19 anxiety was a weak predictor of change in all outcome variables following the intervention, and the link between COVID-19 anxiety and change in outcomes following the intervention did not significantly differ across conditions. See supplement S8 for additional details.

Discussion

The present study tested the efficacy of a free, internet-delivered, multi-session cognitive bias modification for interpretation (CBM-I) program among individuals endorsing moderate-to-severe symptoms of anxiety. Given mixed findings regarding the effectiveness of CBM-I when compared to control conditions, we compared CBM-I to an active condition (psychoeducation) to enable a stringent test of effectiveness. Additionally, we tested whether individuals' level of baseline COVID-19 anxiety influenced their ability to benefit from CBM-I or psychoeducation.

CBM-I was (Mostly) Superior to Psychoeducation During the Intervention

We found that individuals who completed CBM-I as well as those who completed psychoeducation exhibited improvements in anxiety over the course of their assigned intervention. This fits well with prior evidence indicating that both CBM-I and psychoeducation effectively reduce anxiety (e.g., Fodor et al., 2020; Newby et al., 2018; Norr et al., 2017; Salemink et al., 2009). Also in line with prior evidence, we found that CBM-I effectively decreased negative interpretation bias and increased positive interpretation bias (Hoppitt et al., 2014; Salemink et al., 2009, 2014; Steinman & Teachman, 2010, 2014). Changes in interpretation biases also occurred within the psychoeducation condition; perhaps reading about the causal and maintaining factors of anxiety (including interpretation biases) allowed individuals to become more aware of their thinking patterns, and subsequently modify them.

Although we found that anxiety and interpretation biases improved across all groups, CBM-I was superior to psychoeducation at shifting almost all outcome variables. In line with our hypotheses, individuals in the CBM-I (vs. psychoeducation) condition exhibited stronger reductions in OASIS scores (our primary measure of anxiety). This finding adds to a body of literature suggesting that CBM-I can effectively reduce anxiety (e.g., Fodor et al., 2020; Salemink et al., 2009) and expands upon this work by indicating that CBM-I may be superior at reducing anxiety compared to another effective intervention. However, the difference between CBM-I and psychoeducation in terms of changes in anxiety was small (and was not evident on a secondary measure of anxiety, the DASS-AS, which focuses on physical anxiety symptoms). In line with our predictions and prior literature, we also found that individuals who completed CBM-I (vs. psychoeducation) exhibited stronger reductions in negative interpretation bias (small effect) and stronger increases in positive interpretation bias (large effect). We suspect the larger effect for change in positive (vs. negative) interpretations may have occurred because CBM-I actively practices assigning positive or benign meanings while psychoeducation is more focused on reducing anxiety.

Several explanations may account for the different condition findings across the OASIS and DASS-AS measures. One possibility is that CBM-I (vs. psychoeducation) facilitated reductions in the impairment and avoidance associated with anxiety (captured by the OASIS) but not (physical) anxiety symptoms per se (captured by the DASS-AS). This seems unlikely given that DASS-AS scores significantly decreased during the CBM-I intervention. Another possible explanation is that the psychoeducation condition was particularly helpful at reducing anxiety symptoms (captured by the DASS-AS) but not anxiety-related avoidance and impairment (captured by the OASIS), resulting in the inconsistent superiority of CBM-I over psychoeducation across anxiety measures. However, psychoeducation significantly reduced OASIS scores, making this possibility unlikely. A third possibility is that tests involving OASIS scores had higher statistical power (by virtue of a higher number of measurement occasions), and thus allowed us to detect a significant difference in tests involving OASIS but not DASS-AS scores. Further work is needed to discern whether and how CBM-I (vs. psychoeducation) alleviates anxiety symptoms, avoidance, and impairment.

Effects of CBM-I and Psychoeducation (Mostly) Persisted at Follow-Up

Tests of whether the benefits of CBM-I and psychoeducation persisted two and six months after individuals completed the intervention revealed three patterns of change. The most common pattern was maintenance of the gains achieved: anxiety (measured by the OASIS) and negative interpretation biases did not significantly change between session 5 and the 2- and 6-month follow up assessments within the CBM-I or psychoeducation conditions. That is, participants' improvements in anxiety and negative interpretation biases were maintained from the time they finished the intervention until we assessed them 6 months later. This maintenance pattern was mostly true for both CBM-I and psychoeducation.

The second pattern of change was continued improvement following the end of scenario training. Within the CBM-I condition, anxiety as measured by the DASS-AS continued decreasing between session 5 and the 6-month follow up assessment. This is in line with work by Eberle and colleagues (2020), who found that anxiety continued decreasing between the end of CBM-I and a follow-up assessment. The discrepancy in findings for anxiety as measured by the OASIS and DASS-AS may indicate that changes in anxiety-linked impairment and avoidance (assessed by the OASIS) occur more rapidly and then plateau while changes in mostly physical anxiety-linked symptoms (assessed by the DASS-AS) happen more slowly over time as people apply their new interpretive style in daily life. It is not obvious though why this different time course would occur for CBM-I, so replication of this pattern will be important.

The third pattern that emerged was a partial loss of gains during follow-up. Within the CBM-I condition, participants' levels of positive interpretation bias significantly *decreased* during the follow-up period. That is, participants' gains made during the intervention were partially lost during the follow-up period. This finding suggests that individuals' gains in their ability to interpret ambiguity in a positive manner faded away to some extent once individuals stopped actively practicing flexible thinking by completing CBM-I scenario training. Indeed, across all CBM-I conditions, mean positive interpretation bias scores decreased between session 5 and the 2-month follow up (see Table S1). Future work will need to examine ways to increase individuals' use of flexible thinking skills even after they have finished CBM-I.

COVID-19 Anxiety Moderated the Effects of CBM-I on Anxiety

Our results indicated that individuals' levels of COVID-19 anxiety at the beginning of the study moderated the effect of the intervention on changes in anxiety. Within the CBM-I group, higher (vs. lower) levels of COVID-19 anxiety predicted stronger decreases in anxiety (as captured by the DASS-AS and OASIS) during the intervention. Meanwhile, within the psychoeducation group, higher (vs. lower) COVID-19 anxiety predicted stronger *increases* in anxiety (as captured by the DASS-AS but not the OASIS) during the intervention. (COVID-19 anxiety did not predict changes in interpretation bias during the intervention, or changes in any variable at follow-up).

The finding that higher (vs. lower) levels of COVID-19 anxiety predicted stronger decreases in anxiety among individuals who completed CBM-I is consistent with our hypothesis (2a) and suggests that CBM-I may be a useful tool for individuals experiencing intense anxiety tied to COVID-19. Interestingly, though, we did not find that individuals' level of COVID-19 anxiety predicted their trajectory of change in interpretation bias during or after CBM-I training. Given this, it is necessary to consider the mechanism through which CBM-I may have reduced anxiety among individuals higher (vs. lower) in COVID-19 anxiety. One possibility is that changes in interpretation bias occurred but were not captured by our assessment instruments (which did not assess interpretation biases specifically tied to COVID-19). It is also possible that higher (vs. lower) COVID-19 anxiety predicted stronger decreases in anxiety among individuals completing CBM-I because these individuals were more anxious to begin with (and so had more room to improve). This seems unlikely, however, given that COVID-19 anxiety was only modestly, although positively, correlated to the DASS-AS and OASIS at the beginning of the study (r range = 0.19 to 0.22). Further work is needed to elucidate in what ways CBM-I can be a useful tool among individuals experiencing elevated anxiety tied to COVID-19. We are also hesitant to overinterpret this moderation finding given that 95% CIs for this effect were wide and approached zero.

The finding that individuals higher (vs. lower) in COVID-19 anxiety reported increases in anxiety over the course of the psychoeducation intervention was unexpected. While this finding only occurred for one measure (DASS-AS) and certainly should be interpreted with caution, it needs to be evaluated carefully given it points to a possible negative effect of an intervention. One potential explanation for this finding is that psychoeducation had iatrogenic effects among individuals higher (vs. lower) in COVID-19 anxiety. For instance, it is possible that the psychoeducation intervention led these individuals to develop increased awareness of their anxiety but did not provide sufficient tools to alleviate this anxiety, which in turn made the anxiety worse. Another potential explanation is that individuals higher (vs. lower) in COVID-19 anxiety were in need of a more potent intervention than psychoeducation, and because psychoeducation did not meet their needs, their anxiety worsened over time (due to factors unrelated to the psychoeducation program). Given the brevity of the psychoeducation intervention, this latter explanation seems plausible. Relatedly, the anxiety that individuals high in COVID-19 anxiety were experiencing may have been maintained and increased by factors tied to COVID-19 that the psychoeducation intervention did not directly target (e.g., the threat of illness, job loss, social isolation; Fruehwirth et al., 2021; Levin, 2021; McGinty et al., 2020).

Limitations

The present study had several limitations that influence the interpretation of our findings. First, although the CBM-I and psychoeducation conditions were structured in a similar format (e.g., the same number of sessions, the use of comprehension questions), the session durations differed, with psychoeducation being completed much more quickly than CBM-I. Second, our assessment tools for measuring interpretation bias give us helpful data on changes in the *valence* of interpretations assigned, but limited insight into whether and how participants are using *flexible* thinking skills to respond to situations in their daily life, which we theorize is a critical element of how CBM-I exerts beneficial effects. Third, our sample was composed of primarily female, White, highly educated individuals, so our findings may not generalize to individuals who hold other identities. Fourth, the rate of dropout was high, and this may have influenced the pattern of findings. Although we tested for predictors of missingness in order to account for them in our models, it is possible that missingness was produced by variables we did not measure and thus could not account for. Moreover, it is possible that the cause(s) of missingness are related to the intervention itself (e.g., people who improved quickly did not complete the intervention; people who did not improve quickly decided to stop).

Conclusion and Future Directions

The present study provided preliminary support for the superiority of CBM-I over psychoeducation as a tool to alleviate individuals' anxiety symptoms and improve their interpretation biases. It is recommended that future research continue testing the extent to which CBM-I is beneficial relative to other credible comparison conditions, particularly conditions that are equivalent in terms of dosage (e.g., duration). The present study additionally suggested that individuals' levels of anxiety tied to COVID-19 may influence how they respond to CBM-I and psychoeducation. Future work is needed to examine this association, especially monitoring for possible iatrogenic effects and considering a range of variables relevant to COVID-19 (e.g., individual and structural factors, such as COVID-19 health risk status, that may contribute to COVID-19 anxiety). Moreover, it will be important to evaluate whether CBM-I can help individuals experience relief from their anxiety specific to COVID-19 (vs. overall anxiety).

CBM-I holds promise as an easily accessible tool that can help meet the very high levels of need for mental health treatment among adults with anxiety. While some findings were mixed, the pattern of results suggested CBM-I can shift anxiety and interpretation bias when compared to another effective intervention (psychoeducation). Moreover, this study provided preliminary evidence that individuals experiencing high levels of anxiety tied to COVID-19 may particularly benefit from CBM-I as an anxiety-reduction tool. Future work is needed to strengthen effects and further evaluate CBM-I's readiness to be disseminated on a wider scale, and determine whom is most likely to benefit from it.

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TABLES AND FIGURES

Table 1.

Demographic Characteristics for the Full ITT Sample

	Mean (*SD*)/Count (%)
Age (years)	42.93 (*14.19*); range = 19 – 82
Gender	
Female	469 (77.14%)
Male	122 (20.06%)
Transgender Male	4 (0.66%)
Other	13 (2.14%)
Race	
White/European Origin	452 (74.34%)
Black/African Origin	54 (8.88%)
Asian	22 (3.62%)
Native Hawaiian/Pacific Islander	2 (0.33%)
American Indian/Alaska Native	3 (0.49%)
More than one race	34 (5.59%)
Other/Unknown	41 (6.74%)
Ethnicity	
Hispanic or Latino	65 (10.69%)
Not Hispanic or Latino	507 (83.39%)
Unknown or Prefer Not to Answer	36 (5.92%)
Marital Status	
Married or in Marriage-like Relationship	289 (47.53%)
Single	163 (26.81%)
Divorced or Separated	132 (21.71%)
Other or Unknown	24 (3.95%)
Employment	
Working Full-time	261 (42.92%)
Working Part-time	80 (13.15%)

Looking for Work	40 (6.57%)
Unemployed	62 (10.19%)
Retired	40 (6.57%)
Student	64 (10.52%)
Homemaker	40 (6.57%)
Other/Prefer Not to Answer	31 (5.09%)
Education	
High School or Below	46 (7.57%)
Some College	105 (17.26%)
College Graduate	160 (26.31%)
Some Graduate School	34 (5.59%)
Advanced Degree	212 (34.87%)
Unknown	51 (8.39%)
Income	
Up to $49,000	226 (37.17%)
$50,000 - $99,000	156 (25.66%)
$100,000-$149,000	92 (15.13%)
$150,000-$199,000	36 (5.92%)
$200,000-$250,000	11 (1.81%)
Over $250,000	22 (3.62%)
Unknown	65 (10.69%)
Region	
North America	559 (91.94%)
Northern Europe	13 (2.13%)
Eastern Europe	9 (1.48%)
Australia and New Zealand	9 (1.48%)
Asia	6 (0.98%)
Latin America and the Caribbean	5 (0.82%)
Unknown	7 (1.15%)

Table 2.

Summary of Within-Group Intervention and Maintenance Effects

Outcome	Effect	GMA *d* (Effect Size Label)
Intervention Effects		
Reduction in OASIS score	**CBM-I mS1 ≠ 0**	Large
	Psychoeducation mS1 ≠0	Large
Reduction in DASS-AS score	**CBM-I mS1 ≠0**	Medium-to-large
	Psychoeducation mS1 ≠0	Medium-to-large
Reduction in BBSIQ score	**CBM-I mS1 ≠ 0**	Medium-to-large
	Psychoeducation mS1 ≠0	Small
Reduction in RR Negative score	**CBM-I mS1 ≠ 0**	Medium
	Psychoeducation mS1 ≠0	Small
Increase in RR Positive Score	**CBM-I mS1 ≠ 0**	Large
	Psychoeducation mS1 ≠0	Small
Maintenance of Gains		
Reduction in OASIS score	CBM-I mS2 = 0	N/A
	Psychoeducation mS2 = 0	N/A
Reduction in DASS-AS score	**CBM-I mS2 ≠ 0**	Large
	Psychoeducation mS2 = 0	N/A
Reduction in BBSIQ score	CBM-I mS2 = 0	N/A
	Psychoeducation mS2 = 0	N/A
Reduction in RR Negative score	CBM-I mS2 = 0	N/A
	Psychoeducation mS2 = 0	N/A
Increase in RR Positive Score	**CBM-I mS2 ≠ 0**	Medium
	Psychoeducation mS2 = 0	N/A

Note. mS1 = mean of Slope 1; *mS2* = mean of Slope 2; **Bolded results** = statistically significant at $p < 0.05$; GMA *d* = growth-modeling analysis *d*; *Effect Size Label* = size of the effect based on Cohen's d cutoffs (Cohen, 1992): 0.20 = small effect, 0.50 = medium effect, 0.80 = large effect; *N/A* indicates that GMA *d* was not calculated.

Table 3.

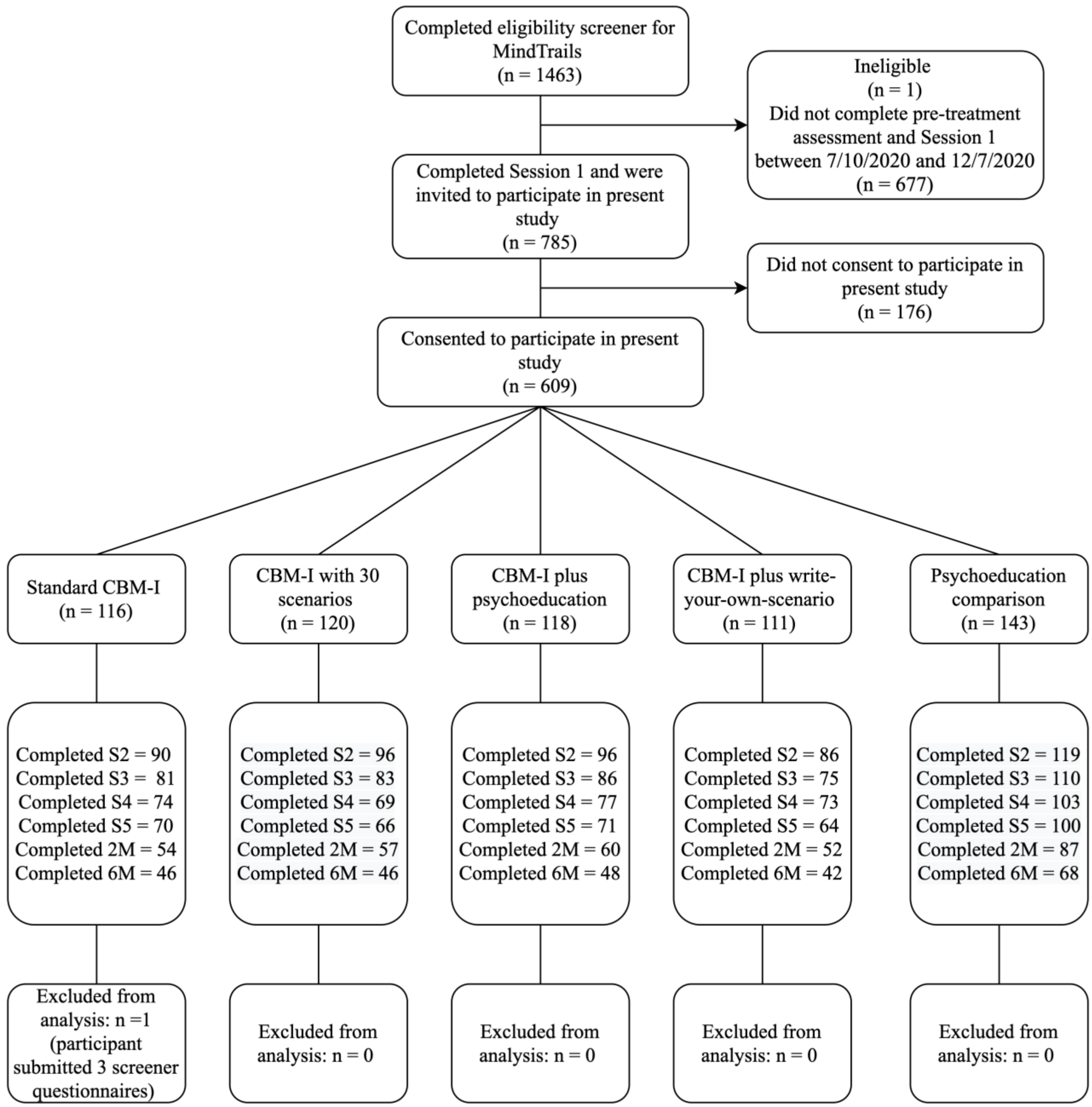
Summary of Between-Group Differences in Intervention and Maintenance Effects

Outcome	Condition x Time Effect	GMA *d* (Effect Size Label)
Intervention Effects		
Reduction in OASIS score	**CBM-I > Psychoeducation**	-0.30 (Small)
Reduction in DASS-AS score	CBM-I = Psychoeducation	N/A
Reduction in BBSIQ score	**CBM-I > Psychoeducation**	-0.47 (Small)
Reduction in RR Negative score	**CBM-I > Psychoeducation**	-0.34 (Small)
Increase in RR Positive Score	**CBM-I > Psychoeducation**	0.83 (Large)
Maintenance of Gains		
Reduction in OASIS score	CBM-I = Psychoeducation	N/A
Reduction in DASS-AS score	CBM-I = Psychoeducation	N/A
Reduction in BBSIQ score	CBM-I = Psychoeducation	N/A
Reduction in RR Negative score	CBM-I = Psychoeducation	N/A
Increase in RR Positive Score	**CBM-I < Psychoeducation**	0.35 (Small)

Note. > indicates greater improvement over time; < indicates less improvement over time. All **bolded results** indicate statistically significant results at $p < 0.05$; GMA *d* = growth-modeling analysis *d*; *Effect Size Label* = size of the effect based on Cohen's d cutoffs (Cohen, 1992): 0.20 = small effect, 0.50 = medium effect, 0.80 = large effect; *N/A* indicates that GMA *d* was not calculated.

Figure 1.

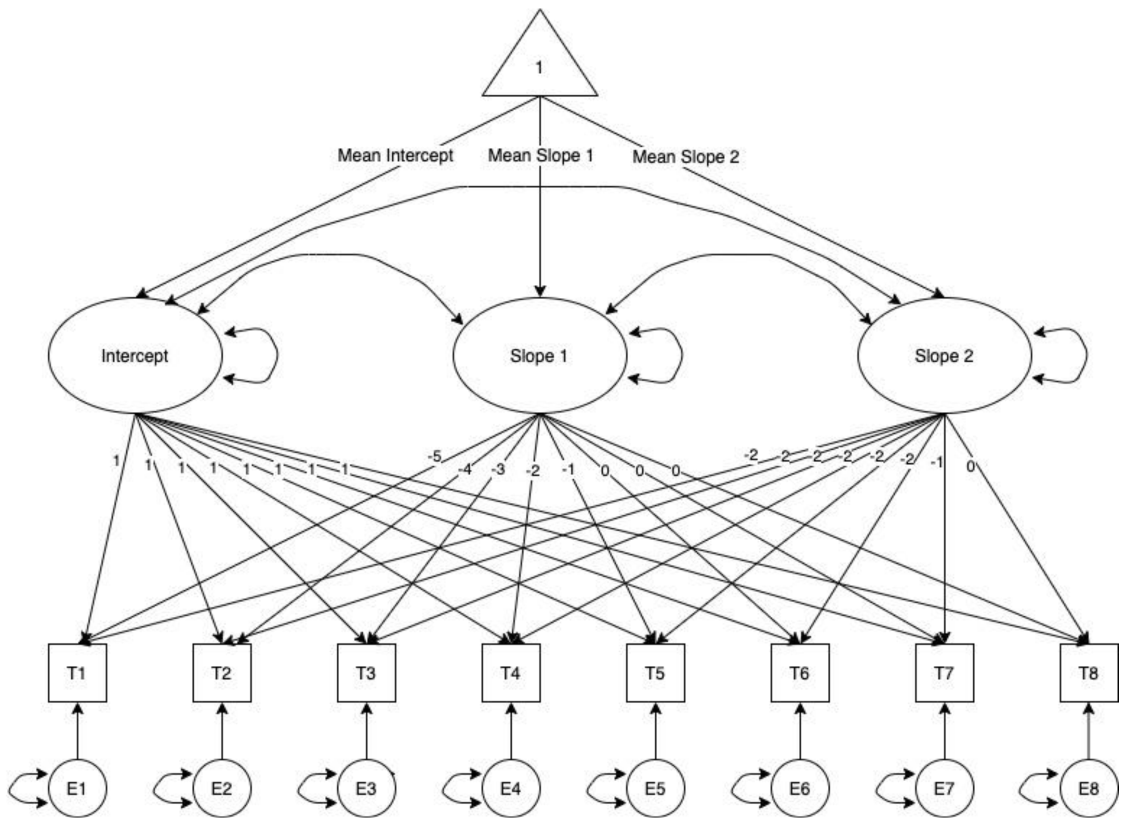
CONSORT diagram



Note. Participant numbers for S2-6M represent number of people who completed each session. *S2* = session 2; *S3* = session 3; *S4* = session 4; *S5* = session 5; *2M* = 2-month follow-up; *6M* = 6-month follow-up.

Figure 2.

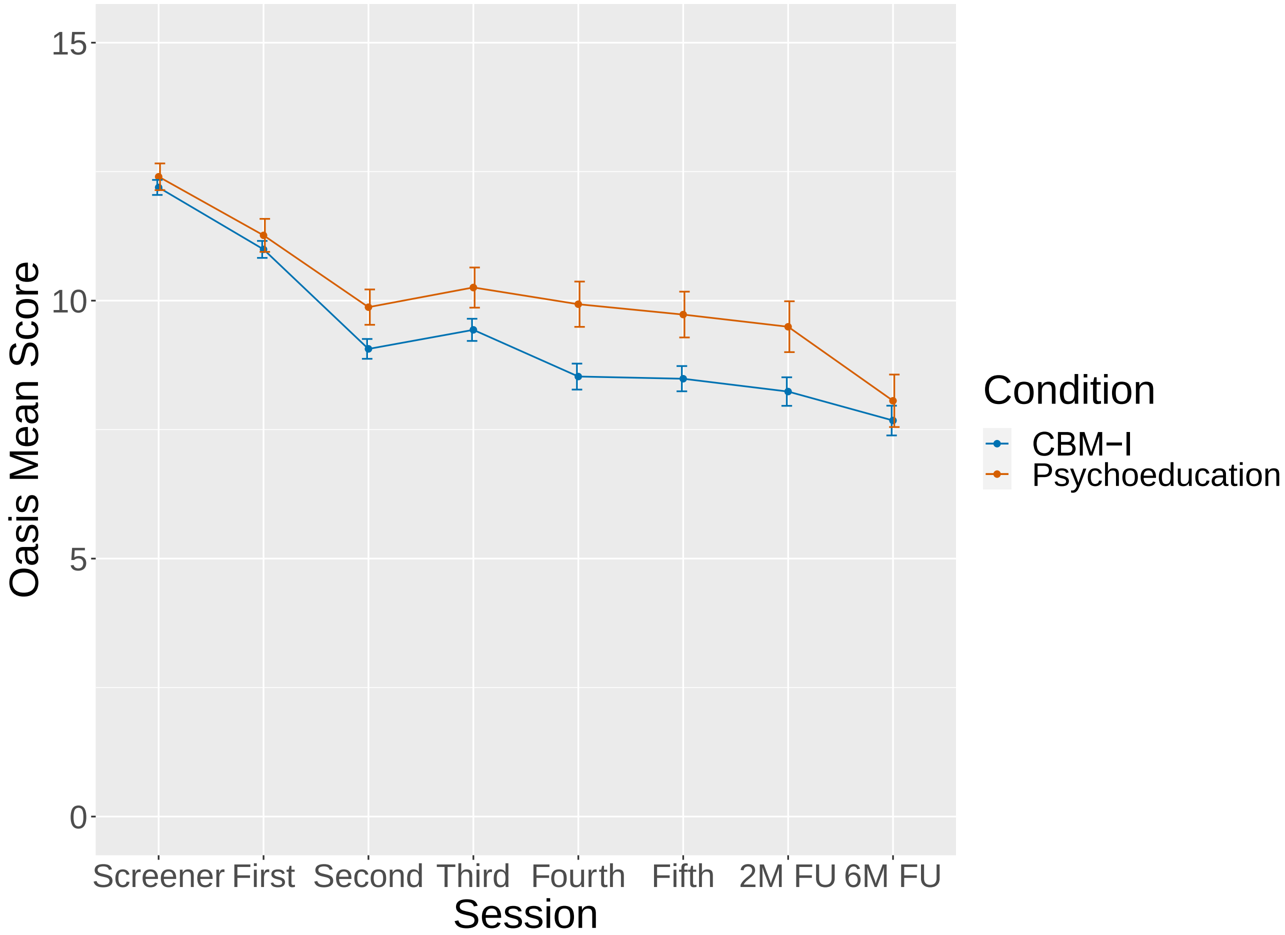
Path Diagram for Piecewise Linear Growth Curve Model.



Note. The path diagram above includes all assessment points for OASIS scores. *T1-T8* indicates the assessment point, starting from the screening questionnaire (T1) through the 6-month follow-up survey (T8). E1-E8 represents error terms for each observed score. Unlabeled double-headed arrows that begin and end on the same variable represent variances. Unlabeled double-headed arrows that connect two variables represent covariances.

Figure 3.

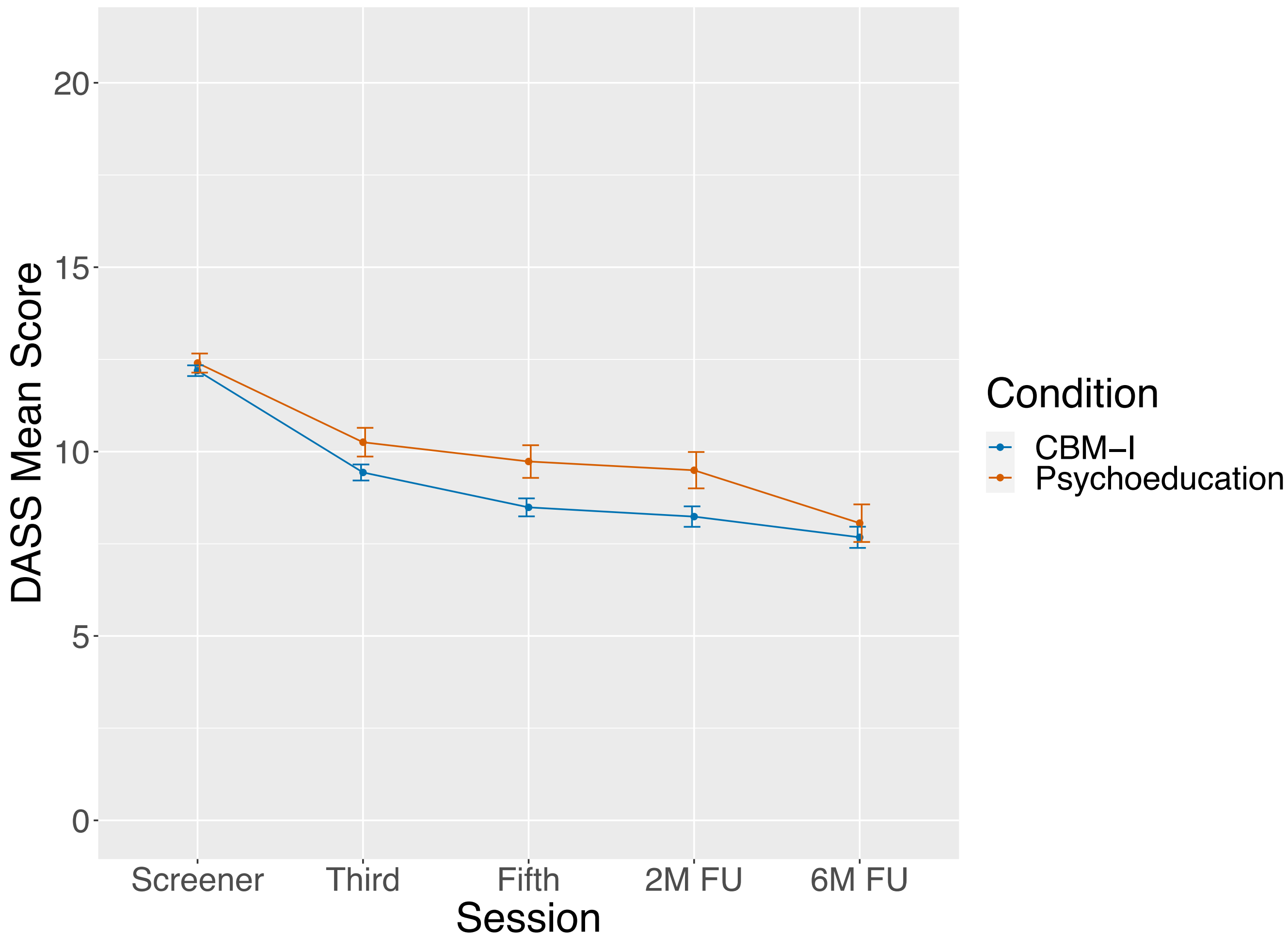
Means with standard errors for OASIS Scores across sessions by condition (CBM-I vs. Psychoeducation).



Note. The four CBM-I conditions were collapsed to calculate means and standard errors for the CBM-I group shown above; *2M FU* = 2-month follow-up; *6M FU* = 6-month follow-up.

Figure 4.

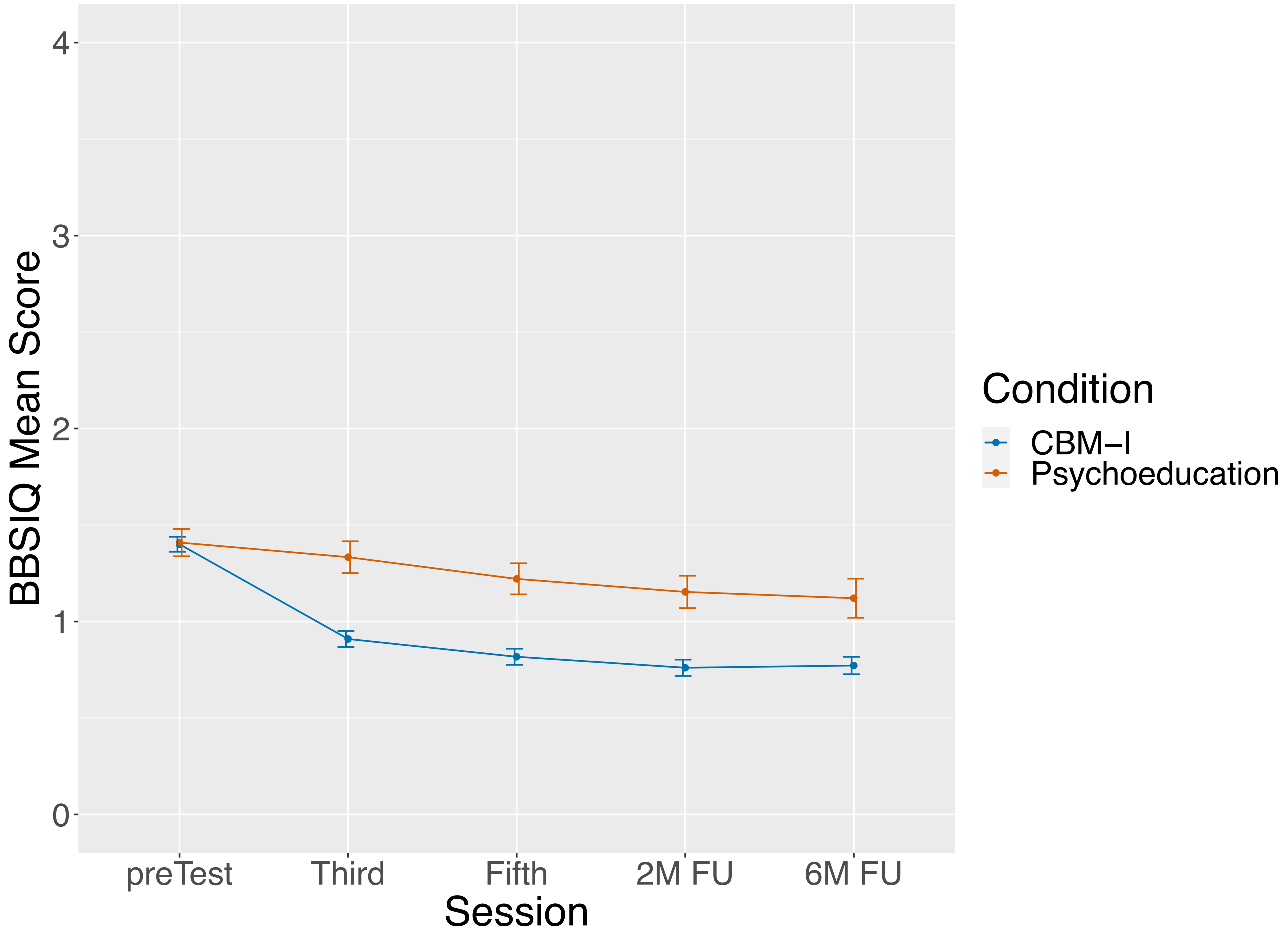
Means with standard errors for DASS-AS Scores across sessions by condition.



Note. The four CBM-I conditions were collapsed to calculate means and standard errors for the CBM-I group shown above; *2M FU* = 2-month follow-up; *6M FU* = 6-month follow-up.

Figure 5.

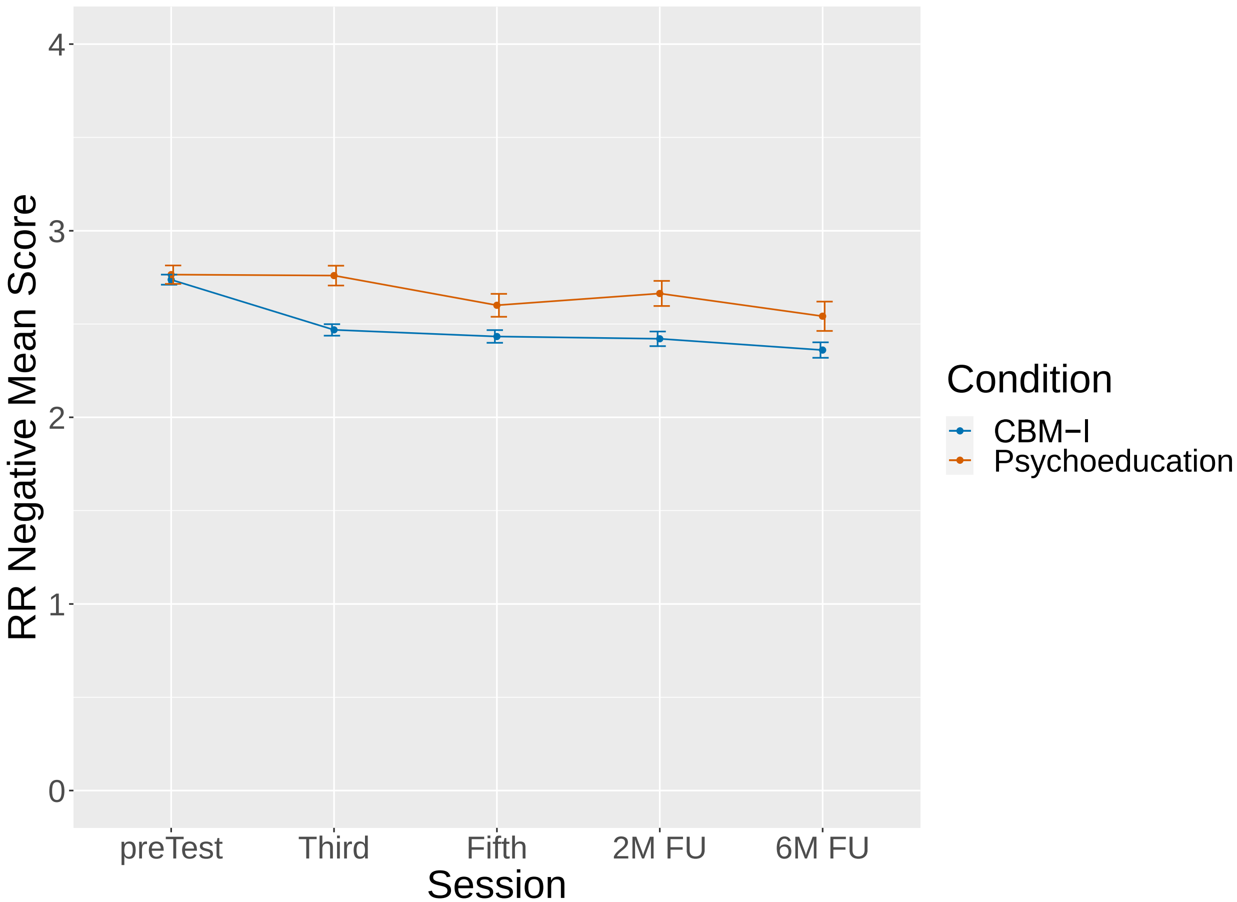
Means with standard errors for BBSIQ Scores across sessions by condition.



Note. The four CBM-I conditions were collapsed to calculate means and standard errors for the CBM-I group shown above; *2M FU* = 2-month follow-up; *6M FU* = 6-month follow-up.

Figure 6.

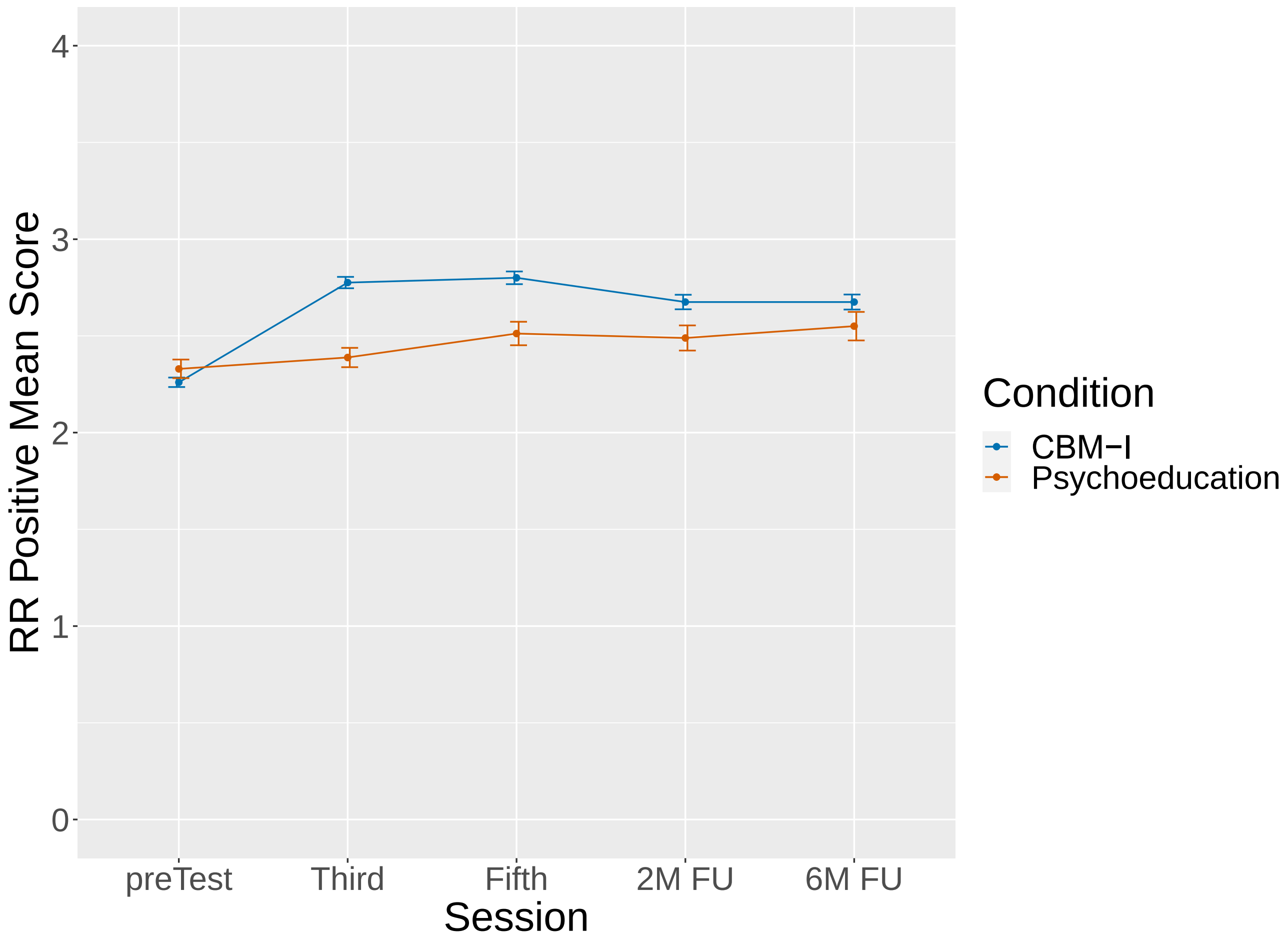
Means with standard errors for RR Negative Interpretation Bias Scores across sessions by condition



Note. The four CBM-I conditions were collapsed to calculate means and standard errors for the CBM-I group shown above; *2M FU* = 2-month follow-up; *6M FU* = 6-month follow-up.

Figure 7.

Means with standard errors for RR Positive Interpretation Bias Scores across sessions by condition



Note. The four CBM-I conditions were collapsed to calculate means and standard errors for the CBM-I group shown above; *2M FU* = 2-month follow-up; *6M FU* = 6-month follow-up.