Efficacy of an Attention Guidance VR Intervention for Social Anxiety Disorder

Statistical Analysis Plan

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Data Analysis Plan

The primary aims of this study were to 1) examine whether an attention guidance augmentation enhanced VRET compared to VRET alone and 2) test whether changes in gaze behavior following the intervention mediated the effects VRET. To test our primary hypotheses regarding the influence of the intervention on fear of public speaking (PRPSA), we conducted Bayesian multilevel models using the *brms* package version 2.15 (Bürkner, 2018). For aim 1 we examined the interaction between assessment and group predicting the outcome post-treatment and at 1-week-followup. For aim 2, we examined the indirect (i.e., mediating) effect of proportion of fixations to uninterested (socially threatening audience members) at the post-treatment assessment, on the relationship between intervention group and the post-treatment assessment of PRPSA at the 1-week-followup (see Figure 3). To facilitate interpretation of the mediation analysis, we partially standardized the model coefficients after completing the analyses using unstandardized variables following recommendations in the literature regarding indirect effect sizes when X is dichotomous (Hayes & Rockwood, 2017; Preacher & Kelley, 2011). In all models, we included average proportion of fixations to audience members during intervention sessions as a covariate to control for variation in treatment adherence. We completed the same analyses to evaluate our secondary outcome of general social anxiety symptoms measured with the LSAS. As integrity checks on the efficacy of the attention augmentation condition we tested whether there were group differences for average number of fixations on audience members during the intervention trials, as well as whether there were differences in proportion of fixations to uninterested audience members post-intervention and at 1-week follow-up.

We computed Bayes Factors (BFs) using the Savage-Dickey Density ratio (Wagenmakers et al., 2010) for all models where we set priors using the *hypothesis* function in *brms*. The Savage-Dickey Density ratio was calculated in the current context by dividing the posterior density by the prior density at zero (a null effect). Given that priors exert a large influence on the posterior estimates with small samples sizes, we used BFs to provide a sense of the influence of the priors on the study data rather than using them to provide a measure of confidence in the posterior estimates themselves as they are sometimes used (in *brms* if the *hypothesis* function is directional, it provides the latter estimate rather than the Savage-Dickey Density ratio). A BF equal to one means there is equal support for the null and alternative hypotheses while smaller BFs reflect greater support for the null and larger BFs reflect greater support for the alternative, with commonly accepted guidelines for the magnitude of the support (e.g., 1-3 is anecdotal evidence in favor of the alternative; 1/3-1 is anecdotal evidence in favor of the null).

For each result we report the beta estimates, 95% highest posterior density interval (HDI), and BFs of the model estimated with our original prior. We also provide the range of BFs as well as the sensitivity of the beta estimates based on our sensitivity analyses (see below).

Prior Estimates. We set informative priors based on expected effects that were based on a literature review of brief exposure-based intervention for social anxiety, as well as based on expert review. On a theoretical level, use of priors aligns with the idea that all available data should be used to draw inferences – including data from previous findings (Kruschke & Liddell, 2018). On a more practical level, there is substantial evidence to support to the use of informative priors to address the 'winner's curse' – which is where significant effects in an initial study are not subsequently replicated (Altoè et al., 2020). Moreover, Lemoine (2019) conducted simulation analyses showing that weakly informative priors can regularize results in small samples ($n < 50$), providing a more conservative estimate.

We largely followed the WAMBS (When to worry and how to Avoid the Misuse of Bayesian Statistics) checklist (Depaoli & van de Schoot, 2017). This checklist provides a step-by-step approach to ensuring that a model estimation procedure is acceptable and that the influence of the priors is well delineated. We tested the sensitivity of the priors by using less informative (smaller effects) parameter estimates as well as uninformative default (flat) priors centered on zero to determine the influence of different priors on the posterior estimates.

Priors for the effect of the intervention on fear of public speaking (PRPSA) were primarily based on a relatively recent study which also used a two-session public speaking exposure model (testing affect labeling as a potential mechanism; Niles et al., 2015). In terms of the efficacy of the augmentation for which there is no previous research, we used a prior that reflected a moderate effect (Δ 10 on PRPSA score). It was impossible to predict whether there would be a greater gain at post-treatment followed by a 'rebound' (i.e., regression towards the mean) or whether the gains would continue and we tested the sensitivity of our priors by varying the magnitude of the effects as well as their direction (to a certain degree). We also tested a model with uninformative priors, centered on zero. Similarly, for the LSAS we used previous research to determine the priors. One challenge was that most studies addressing fear of public speaking did not have a clinical sample of socially anxious individuals – we predicted greater severity of social anxiety symptoms in the current study – and so drew from other studies as well (Lazarov et al., 2018). We primarily based our estimates of the efficacy of the intervention using a recently published single-session feasibility VRET study (Lindner et al., 2021). The estimate of the effect of the intervention provided a good starting place to inform our priors – based on the likelihood that 2 sessions would be more potent, we also used estimates from other longer intervention studies.

Priors on the effects of the intervention on proportions of fixations to audience members were not based on previously collected data because there is no literature that clearly delineates expected changes in gaze during public speaking challenges. We chose priors that reflected modest but meaningful effects, indicating changes in gaze behavior substantial enough to have potential implications for treatment outcomes as a causal mechanism. As with the other analyses, we tested several priors reflecting smaller effects as well as an uninformative prior centered on zero.

Power analysis. We did not conduct a power analysis that reflected the sample size for the current pilot study. We had initially conducted a power analysis through a simulation study prior to COVID-19, which indicated a sample size of 60 would be sufficient to detect a meaningful effect for both aims 1 and 2. However, due to COVID-19 enrollment ended before we could meet our recruitment goals. Given that research was necessarily stopped, we decided to rely on the strengths of the Bayesian approach highlighted above to investigate whether it would be worthwhile to conduct a more extensive RCT with a larger sample.

Data and syntax are available at https://osf.io/un92m/.

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