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Response to Morina et al.'s criticisms of Horigome et al.'s recent report on the efficacy of virtual reality exposure therapy for social anxiety disorder

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While research on virtual reality exposure therapy (VRET) for social anxiety disorder (SAD) has been conducted, large-scale studies remain scarce. We found that there were more studies than previously reported in meta-analyses, and we have included these newly identified studies in our analysis to understand the effectiveness of VRET interventions (Horigome et al., 2020).

In response to our paper, Morina, Kampmann, Emmelkamp, Barbui, and Hoppen (2021) published a paper stating that 'Horigome and colleagues misleadingly imply that there is a larger body of literature on the efficacy of VRET for SAD than there actually is.' We would like to thank the authors who have been working in this area for bringing our paper to their attention and starting a discussion. Reading the criticisms by Morina et al. (2021), we can assume that their goals are different from ours. While they tried to examine the effects of VRET as purely as possible using only studies comparing the effects of VRET and *in vivo* exposure, we wished to perform a broader examination of whether interventions using VR can have a therapeutic effect on SAD and public speaking anxiety, whether the effect is sustained, and whether VR is effective compared with other treatments. The points that Morina et al. have made are probably all a result of this difference in stance, and we hope that the following explanation will aid understanding.

First, they pointed out that Table 1, which lists 22 papers that met our eligibility criteria, counts Anderson et al. (2013) and Safir, Wallach, and Bar-Zvi (2012) twice each. It is true that each of these studies published follow-up studies on the same population, and we agree that it would have been more appropriate to say 22 publications reporting 20 studies, rather than 22 studies. However, the data from these studies were not included twice in our analysis. For example, in our comparison of the baseline and post-intervention symptoms, only data comparing symptoms at these two time points were included; data from the two follow-up studies were only included in the analysis of the long-term outcomes. Since the data were not duplicated in our analyses, we believe that our procedure is not problematic.

Second, Morina et al. argue that it is wrong to consider two of the included studies as VRET. The first was a study by Lister, Piercey, and Joordens (2010). Morina et al. claimed that in this study, VR, and not VRET, was used for the assessment. However, in this study, participants were exposed to a virtual audience created using shutter glasses and cathode-ray tube television, and anxiety was induced by reading; so, we believe that VRET was used for therapeutic purposes. The second study was by Yuen et al. (2019). Morina et al. once again argued that it was incorrect to determine that the treatment was VRET because less than 50% of the treatments consisted of VRET, so the potential treatment effect could be attributed to factors other than VRET. However, exposure to SAD inevitably occurs outside of sessions as well, and treatment sessions were only held once a week for an hour in most studies. Consequently, it is practically difficult to control for these factors and to ensure that exposures other than those produced using VR do not occur outside of sessions. On the other hand, we were concerned about the fact that Yuen et al. (2019) also conducted exposure exercises in the VR group session. However, our eligibility criteria did not exclude *in vivo* exposure in the VR intervention group, and all other criteria were met; consequently, we decided to include the study in order to obtain a broader picture of the effectiveness of VR interventions.

Next, Morina et al. criticised our eligibility criterion of 'study participants numbered 10 or more people.' In clinical trials using new technologies such as VR, it is ethically and scientifically correct to conduct feasibility studies first. As Morina et al. argued, it is true that studies with fewer subjects may not reflect a true effect, and caution regarding this issue is needed. We set the number of participants at ten per study to exclude case studies that may have reported only cases in which an effect had been obtained. It would have been safer to include only studies with ten or more participants per condition, but we believe that an inclusion

criterion of ten or more participants per study was sufficient to exclude case series. We also agree with Morina's assertion about the possibility of over-reporting effects in studies with smaller numbers of participants, but their reanalysis, which excluded studies with smaller numbers of participants, seemed to show even stronger effects (Morina et al., 2021).

Finally, Morina et al., stated that we are using Wallach, Safir, and Bar-Zvi's (2011) cognitive therapy (CT) as a TAU; however, as explained in Table 1, the CT comparison was different from that used in the *in vivo* exposure and waiting list groups and was therefore not included in our analysis. For the study by Wallach et al. (2011), we compared the VRET group with data from the waiting list. However, the waiting list of Wallach et al. (2011) group used waiting list data from Wallach, Safir, and Bar-Zvi's (2009) study, and the subjects overlapped. Therefore, we compared the VRET group of Wallach et al. (2011) with the waiting list of Wallach et al. (2009).

As mentioned above, VR is being used in various ways. We believe that it is significant to include many of these studies in our meta-analysis to examine not only the effect of VRET, the changes in effect over time, and differences in the effect size relative to a comparison arm, but also to examine the differences in effect according to study design (randomised controlled trial or not, with or without structured psychotherapy). Of course, as we mentioned in the limitation section, the studies examined VR in diverse ways, and many studies had problems with the quality of the reported research.

We would like to express our respect for the pioneering work of Morina et al. VR is effective against SAD, as they have acknowledged, and yet it is safe to use. We hope to see increasing numbers of high-quality, large-scale studies in the future.

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