Dear Prof. Dienes,

We like to thank you and the reviewers for investing the time to provide constructive feedback to this latest round of reviews.

We believe that your comments so far have led to an improved version of the registered report, and we are confident that this latest version addresses the comments that have been raised by yourself and the reviewers.

Below, you can find our point-to-point response to each one of the raised comments. Yours and the reviewers' comments are presented in **bold font**, followed by our responses.

Yours sincerely,
Phivos Phylactou

Comments by the Editor:

Sorry for the delay in getting back; due to one of the original reveiwers being busy, I asked another, Rob Mcintosh, to judge how well you had addressed the original reviewers' points. Both the revewiers are very happy with how thoroughly you have revised the manuscript. Mcintosh has some very useful points of clarification that I ask you to address.

I have on further point of my own, concerning your scale factors. Note that when the meta-analysis you base you scale factors on reports an efect size of e.g. 0.8, that implies the true effect size may well be larger than 0,8. Let us say the 0.,8 was signficaint just p < .05; then the meta-analytic 95% confidence interval would go to 1.60. Your logic of using small scale factors so that the plausible upper limit is about the mean meta-analytic effect size does not take this fact into account. One more thing: Can you simulate assuming either the population effect size is zero or that it is the meta-analytic mean, what proportion of times your BF would exceed 6 or 1/6, given your maximum N of 40? In other words, assuming the theory is false (effect = 0) what is the probability that you would find evidence against the theory (BF < 1/6)? Or given the theory is true, what is the probability you would find evidence for it? This is a check that your maximum N is reasonable given your models of H0 and H1 and the requirement of test severity.

Response to comment:

We thank you for clarifying the issue with our scale factors, which indeed failed to take into consideration higher effects that could be plausible. To address this issue, we will now use the reported meta-analytic effect size as the scale factor of our Cauchy priors. Given this change, we have made the appropriate amendments throughout the manuscript, including Table 1, and the *Analysis Plan* section on page 21, paragraph 2, as you can also see below:

"Each prior for the paired t-tests is described by a Cauchy distribution centered around zero (see Rouder et al., 2009). Each prior was based on the results of a recent meta-analysis on the topic (Phylactou et al., 2022), which reported the standardised differences (Hedge's g) of accuracies and signal detection estimates between sensory visual cortex TMS and control

conditions. These standardised differences will be used to inform the width parameter of each Cauchy prior. In detail, by considering the overall effect size (g = .58), the effect size for early TMS (up to 200 ms; g = .80), and the effect size for late TMS (after 200 ms; g = .50) from our previous meta-analytic work (Phylactou et al., 2022), the width parameter of the Cauchy distribution will correspond to 0.58 for the 0 ms condition, to 0.8 for the 200 ms condition, and to 0.5 for the 1000 ms condition, respectively."

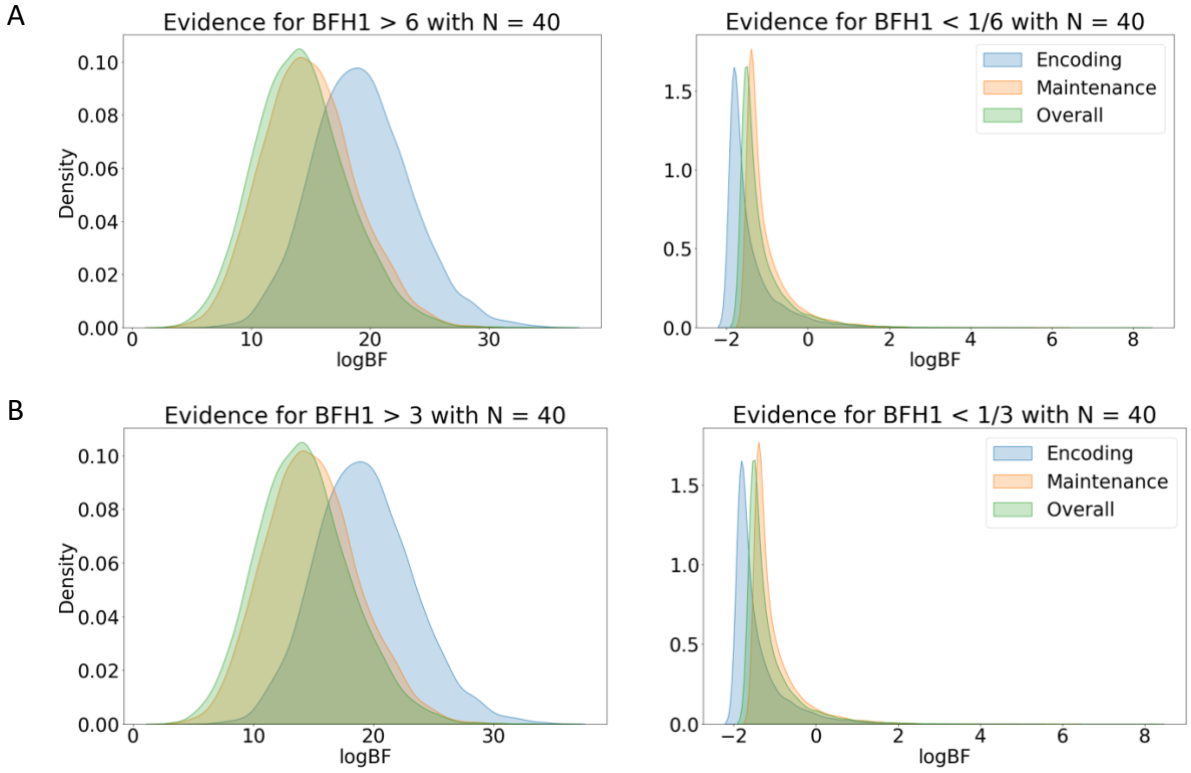
Similar edits were made on page 2, paragraph 2:

"The priors which will be used for the paired t-tests are described as a Cauchy distribution centered around 0 with a width set to 0.8 for the 200 ms condition and 0.5 for the 1000 ms condition, as estimated by the results of recent meta-analytic evidence (Phylactou et al., 2022), which reported a standardised effect size for early TMS (up to 200 ms; g = .8) and for late TMS (after 200 ms; g = .5)."

Regarding the concern of simulating a true effect of 0 or a true effect equal to the meta-analytic mean, we have prepared some custom code and run 10,000 simulations for each of our three Cauchy Priors (r = 0.8, r = 0.5, r = 0.58), for both a null effect or a true effect equal to the meta-analytic standardized difference (g = 0.8, g = 0.5, g = 0.58). Unfortunately, the simulation indicated that 40 participants can only provide a BF > 6 or BF < 1/6 only 50% of the times for the g = 0.5 and g = 0.58 conditions (see Figure 1A below). Therefore, we have now reduced our BF threshold to 3, for which the simulations indicated that a BF > 3 or BF < 1/3 is evident 90% of the times for the g = 0.8 condition, 81% for the g = 0.5 condition, and 85% of the g = 0.58 condition (see Figure 1B). All adaptions reflecting the reduction of the BF threshold have been made throughout the registered report manuscript. The simulation code and results can be accessed through the study's repository (https://osf.io/d9bqk/), which will be publicly available after stage 2 acceptance. We have now added a new paragraph under the *Sampling Plan* section on page 20, paragraph 1, describing the details of the simulation, as we also present below:

"In order to confirm the adequacy of our proposed sample size, we simulated each of our registered t-tests 10000 times. The simulation results indicated that for the outcome neutral condition of BF > 3 (median BF = 12.2×10^6) or BF < 1/3 (median BF = 0.252) was evident in 85% of the simulations. For the encoding condition a BF > 3 (median BF = 19.1×10^7) or BF < 1/3 (median BF = 0.189) was evident in 90% of the simulation, and lastly a BF > 3 (median BF = 23.3×10^6) or BF < 1/3 (median BF = 0.285) was evident in 80% of the simulations for the maintenance condition. The results of these simulations are consistent with previous work suggesting that a total of 40 participants is adequate to provide a BF > 3 or BF < 1/3 with a proportion of at least 80% (Palfi & Dienes, 2019)."

Figure 1



Reviews

Reviewed by Evie Vergauwe, 03 Mar 2022 18:24

The authors have done a great job in revising the manuscript. Regarding the concerns I had raised, they are all adequately and convincingly addressed either by revising specific sections in the mansucript, or by providing a reasonable justification as to why it was decided not to modify the manuscript. I have only one minor comment, and that is that some of the newly-added sentences are very long (and difficult), e.g. end of p.8 - begin of p.9, there is a 6-line sentence. Other than that, I have no further comments and look forward to the results and the publication!

We would like to thank the reviewer for the positive appraisal of our work.

Regarding the reviewer's concern that "*some of the newly-added sentences are very long*", we have revised the newly added sentence on page 8, paragraph 1, in order to make it shorter and easier to understand. The paragraph now reads:

"Another important shortcoming of the TMS literature relates to the complexity of the stimuli used in the memory array. In a given memory array, there is a minimal representational requirement for VSTM, based on the core features (e.g., color, orientation, shape) of stimuli. A greater combination of stimuli features increases complexity and VSTM capacity requirements (Alvarez & Cavanagh, 2004). Previous TMS studies used various

stimuli in their memory tasks, some of which were complex stimuli such as abstract shapes (van de Ven et al., 2012). However, the evidence leading to the sensory recruitment hypothesis emphasized the selective engagement of the sensory visual cortex in elemental visual features such as orientation, contrast, and direction of movement (Harrison & Tong, 2009; Issa et al., 2008; Konstantinou et al., 2012; Serences et al., 2009). For example, Jia and colleagues (2021), indeed found a strong TMS effect in a VSTM task requiring participants to remember the elemental visual feature of orientation of one grating. However, in a study requiring participants to remember either one (low load) or three (high load) abstract shapes (that are thought to be complex stimuli consisting of a combination of elemental visual features; van de Ven et al., 2012), TMS did not affect performance in the low load condition of remembering a complex shape (TMS effects were evident only during the high load condition). Such findings suggest that when stimulus complexity increases, higher order brain areas, such as the intraparietal sulcus (Xu & Chun, 2006; Xu, 2007) and the posterior parietal cortex (Song & Jiang, 2006), might be more actively recruited for VSTM. Thus, the neural processes required for successful maintenance of complex visual stimuli in VSTM might be more dependent on higher order brain areas than those required for simple stimuli consisting of elemental visual features, given the high selectivity of sensory visual cortex in processing of elemental features (Teng & Postle, 2021). This might explain some of the null effects of sensory visual cortex TMS during the memory delay, since complex representations are likely protected through a more distributed VSTM network (Lorenc & Sreenivasan, 2021; see also Gayet et al., 2018; Scimeca et al., 2018). Hence, it is possible that some of the previous studies failed to find evidence in favor of the sensory visual cortex involvement in VSTM due to using complex, rather than simple, stimuli."

Reviewed by Robert McIntosh, 11 May 2022 22:57

Due to the unavailability of the original Reviewer#2, I have been asked to assess whether this reviewer's comments have been adequately addressed by revisions and accompanying responses. I have read the review history for the paper, as well as the latest tracked version of the paper, and the responses document, with a focus on the responses to Reviewer#2's comments.

Overall, the review comments have been very thoroughly addressed. Points 1-3, and 6-9 all involve clarifications and/or methodological adjustments that the authors have responded to fully and clearly. Where the suggestions made to points 4-5 have not been followed, an adequate rationale has been given for not doing so, and a reasonable case made that it is not essential to the experiment. But I would emphasise strongly, with reference to (4), that the decision not to pre-register any tests of interactions by time-period makes it crucial that the Stage 2 report must drive major conclusions from the pre-registered tests, and not from exploratory follow-up analyses involving interactions by time.

Response to the comment:

We are grateful for the fact that our responses were deemed to have thoroughly addressed the concerns raised during the previous revision. Further, we agree with the reviewer's emphasizing that "*the Stage 2 report must drive major conclusions from the pre-registered*

tests, and not from exploratory follow-up analyses involving interactions by time" and we do share the caution that should be taken during Stage 2, given that the interaction effects have not been pre-registered. As discussed below, we have made adjustments throughout the report, in order to use more careful language.

I will add some minor comments of my own but these are by way of discussion only. At this stage of review, it would probably be unwelcome to bring up too many novel criticisms, and this is not the task that the editor has given me. But in case it is of any use...

As a general evaluation, I would say that this is a complex pair of experiments, and that the inter-relationship between them is not easy to follow from the text alone. The Design table is completely essential to make sense of the logical structure, but this table does not cover the full range of possible outcomes. For instance, it is not clear what should be concluded if the alternative hypothesis is supported for H2 and/or H4, but not for H5; or if the equivalent situation were to arise for H3/H6 and H7; or if the alternative hypothesis is supported for key hypotheses in either study but the outcome neutral H1 has not been supported. Similarly, the multiple testing of related hypotheses presents the possibility that some comparisons will find evidence for a conclusion whilst other tests of the same hypothesis will not, and it is not clear how conclusions will be drawn under these circumstances (the attitude to multiple comparisons for these Bayesian tests, in which inferential thresholds have been applied, has not been explicitly described). In short, the design of the experiment does not nail down all the interpretative degrees of freedom. Every effort should be made to do so if further modifications to the Stage 1 manuscript are required.

Response to comment:

We thank the reviewer for raising the issue that "*the inter-relationship between [the two experiments] is not easy to follow from the text alone"*. To make this relationship clearer to the reader, we have now elaborated on page 11, paragraph 1, as provided below:

"In short, the objective of the current study is to provide causal evidence for the role of the sensory visual cortex during early (200 ms) and/or late (1000 ms) VSTM maintenance using TMS, while ensuring monocular vision. In the proposed experiments, stimuli will be presented in the center of the visual field to be viewed monocularly. Therefore, based on the neuroanatomy of the visual pathway (Joukal, 2017; Tong et al., 2006; Wichmann & Müller-Forell, 2004), it is expected that visual information will initially be processed solely by the ipsilateral (to the eye receiving the information) sensory visual cortex. As a result, and contrary to past experiments, the contralateral sensory visual cortex will be the control condition. To explore our main question of whether the sensory visual cortex is involved in VSTM maintenance, our hypotheses focus on testing differences in detection sensitivity (Stanislaw & Todorov, 1999) for a VSTM task in two experiments. In Experiment 1, detection sensitivity will be compared between the ipsilateral and contralateral conditions when stimuli are presented monocularly and TMS is applied (1) during perceptual processing (outcome neutral condition; 0 ms after stimulus onset), (2) during early information maintenance (200 ms after stimulus onset), or (3) during late information maintenance

(1000 ms after stimulus onset). More specifically, Experiment 1 would enable us to replicate previous, similar, TMS studies, at two different temporal points during the memory delay period, at an early (200 ms condition) and late (1000 ms) maintenance timepoint. Given the established role of the sensory visual cortex during perceptual processing (0 ms condition), the outcome neutral condition in Experiment 1 (ipsilateral vs contralateral d' in 0 ms TMS condition; see H1 in Table 1) will be employed to evaluate the sufficiency of our methods to successfully manipulate sensory visual cortex activity with TMS. However, as discussed below, it is likely that a comparison between the ipsilateral and contralateral conditions alone, is inadequate to explore the effects of TMS, for example, due to feedback and/or feedforward processes or due to TMS interference affecting both sensory visual cortex hemispheres (see Experimental Design). Therefore, in a second experiment, further to the ipsilateral versus contralateral comparison, we will test whether VSTM performance differs between a TMS and a sham TMS condition (1) during early information maintenance (200 ms after stimulus onset) and (2) during memory late information maintenance (1000 ms after stimulus onset). Table 1 presents a detailed description of the main research hypotheses for each experimental condition. Following testing of the preregistered hypotheses, exploratory analyses will investigate any temporal differences between the proposed timing conditions."

Further, regarding the reviewer's concern that *"this table does not cover the full range of possible outcomes"* and that "*the design of the experiment does not nail down all the interpretative degrees of freedom*". To address this concern, we have now updated our Design Table, on page 31, in order to incorporate the full range of possible outcomes.

My strong impression iis that Experiment 2 seems to be the critical one, and that a simpler and stronger Stage 1 report would be possible if Experiment 1 had already been run as a first (non-preregistered) step, or even if an incremental preregistration approach were taken to the two experiments. However, this has not been done, and I do not suspect that the authors wish to change course at this point.

Response to the comment:

We agree with the reviewer that "*stronger Stage 1 report would be possible if Experiment 1 had already been run as a first (non-preregistered) step*". Unfortunately, and as also mentioned by the reviewer, this has not been done and we believe that a change of course at the given moment will not be possible. However, we would like to emphasise that the motivation for registering both of these experiments in a single report, was to enable reproducibility, if a true effect indeed exists in both cases. Specifically, this has a twofold purpose; (1) it allows reproducibility for similar experiments that preceded (Experiment 1), and (2) it allows reproducibility of our own experimental design (Experiment 2). We also agree with the reviewer that an incremental preregistration approach could have made the report simpler and stronger, nevertheless, we believe that registering both experiments together has some benefits of its own (e.g., reproducibility).

Other than these general points, I note only a couple of minor clarifications:

Abstract: "Behavioural effects in the ipsilateral occipital hemisphere to visual hemifield will indicate a causal involvement of the sensory visual cortex during a specific temporal point in VSTM." >> Here, and elsewhere in the manuscript, care must be taken not to imply that the specificity of any effect to a given time point will be tested, because this is not part of the preregistered analysis plan. (The last two paragraphs of the Introduction run the same risk, and more careful language may be needed to make sure the reader knows clearly what the pre-registered components will test, and to keep this distinct from subsequent exploration.)

We are thankful to the reviewer for raising this concern. To address this issue, we have made several adjustments throughout the preregistered report, as we indicate below:

Page 2 (abstract):

"The role of the sensory visual cortex during visual short-term memory (VSTM) remains controversial. This controversy is possibly due to methodological issues in previous attempts to investigate the effects of transcranial magnetic stimulation (TMS) on VSTM. This study aims to use TMS, while covering previous methodological deficits. Young adults will be recruited to participate in two experiments using a VSTM orientation change-detection under TMS. Monocular vision will be ensured using red-blue goggles combined with red-blue stimuli. Double-pulse TMS will be delivered at different times (Experiment 1: 0 ms, 200 ms, or 1000 ms; Experiment 2: 200 ms, 1000 ms) during a 2 s retention phase, on one side of the occipital hemisphere (right hemisphere for 50% of the participants). In experiment 2, a sham-TMS condition will be introduced. Behavioural effects in the ipsilateral occipital hemisphere to visual hemifield will indicate a causal involvement of the sensory visual cortex during early (200 ms) and/or late (1000 ms) maintenance in VSTM."

Page 10, paragraph 1:

"Previous TMS studies, stimulated the sensory visual cortex at various timepoints during VSTM maintenance, with variable results (e.g., Rademaker et al., 2017; van de Ven et al., 2012; van Lamsweerde et al., 2017; for reviews see Phylactou et al, 2022; Xu, 2017). For example, Rademaker et al. (2017) interfered with sensory visual cortex TMS at 0 ms and 900 ms into a 2 second delay period, after the offset of a memory array presented for 200 ms. Similarly, van Lamsweerde et al. (2017) stimulated at 0 ms, 100 ms, and 200 ms during a 1 second delay period, which followed a 100 ms memory array. In another study, van de Ven et al. (2012) induced TMS at 100 ms, 200 ms, and 400 ms of a 1.5 second delay period, after the presentation of a 150 ms memory array. Some studies indicated that TMS effects were stronger for earlier stimulation (up to 200 ms; Rademaker et al., 2017; van Lamsweerde et al., 2017), compared to later stimulation at 400 ms (van de Ven et al. 2012), and 900 ms (Rademaker et al., 2017), however other studies indicated that TMS after 200 ms was stronger (van de Ven et al., 2012). Based on a recent meta-analysis examining the effects of TMS on VSTM performance during the maintenance period, most studies differentiated between earlier (up to 200 ms into the maintenance period) and later (after 200 ms; usually halfway into the maintenance period) stimulation (Phylactou et al., 2022). The meta-analysis provided evidence for a strong TMS effect (g = 0.8) during earlier TMS, and a moderate effect (g = 0.5) during later TMS; however, further analyses indicated that the TMS effects

were not significantly different between the two timing conditions (overall effect *g* = 0.58). In the current work, we also differentiated between early and late TMS, by considering the outcomes of previous studies (Rademaker et al., 2017; van de Ven et al., 2012; van Lamsweerde et al., 2017), and thus, to test our main question of whether the sensory visual cortex is involved in visual short-term memory we aim to examine the effects of TMS on behavioral performance separately for stimulation induced at 200 ms and 1000 ms (halfway) into the delay period. Further to our main hypotheses, exploratory analyses will be performed in order to replicate and explore any similar findings concerning a different TMS effect size for earlier compared to later stimulation."

Page 11, Paragraph 1:

"In short, the objective of the current study is to provide causal evidence for the role of the sensory visual cortex during early (200 ms) and/or late (1000 ms) VSTM maintenance using TMS, while ensuring monocular vision. In the proposed experiments, stimuli will be presented in the center of the visual field to be viewed monocularly. Therefore, based on the neuroanatomy of the visual pathway (Joukal, 2017; Tong et al., 2006; Wichmann & Müller-Forell, 2004), it is expected that visual information will initially be processed solely by the ipsilateral (to the eye receiving the information) sensory visual cortex. As a result, and contrary to past experiments, the contralateral sensory visual cortex will be the control condition. To explore our main question of whether the sensory visual cortex is involved in VSTM maintenance, our hypotheses focus on testing differences in detection sensitivity (Stanislaw & Todorov, 1999) for a VSTM task in two experiments. In Experiment 1, detection sensitivity will be compared between the ipsilateral and contralateral conditions when stimuli are presented monocularly and TMS is applied (1) during perceptual processing (outcome neutral condition; 0 ms after stimulus onset), (2) during early information maintenance (200 ms after stimulus onset), or (3) during late information maintenance (1000 ms after stimulus onset). More specifically, Experiment 1 would enable us to replicate previous, similar, TMS studies, at two different temporal points during the memory delay period, at an early (200 ms condition) and late (1000 ms) maintenance timepoint. Given the established role of the sensory visual cortex during perceptual processing (0 ms condition), the outcome neutral condition in Experiment 1 (ipsilateral vs contralateral d' in 0 ms TMS condition; see H1 in Table 1) will be employed to evaluate the sufficiency of our methods to successfully manipulate sensory visual cortex activity with TMS. However, as discussed below, it is likely that a comparison between the ipsilateral and contralateral conditions alone, is inadequate to explore the effects of TMS, for example, due to feedback and/or feedforward processes or due to TMS interference affecting both sensory visual cortex hemispheres (see Experimental Design). Therefore, in a second experiment, further to the ipsilateral versus contralateral comparison, we will test whether VSTM performance differs between a TMS and a sham TMS condition (1) during early information maintenance (200 ms after stimulus onset) and (2) during memory late information maintenance (1000 ms after stimulus onset). Table 1 presents a detailed description of the main research hypotheses for each experimental condition. Following testing of the preregistered hypotheses, exploratory analyses will investigate any temporal differences between the proposed timing conditions."

Introduction: "For example, Rademaker et al. (2017) interfered with sensory visual cortex TMS at 0 ms and 900 ms into the delay period, van Lamsweerde et al. (2017), at 0 ms, 100 ms, and 200 ms, and van de Ven et al. (2012) at 100 ms, 200 ms, and 400 ms." >> This paragraph needs a more definition to make sure the reader knows what the zero point of the delay period means (is it the moment of stimulus offset?), and how long the typical memory delay is.

Response to the comment:

We agree with the reviewer that "*this paragraph needs a more definition to make sure the reader knows what the zero point of the delay period means (is it the moment of stimulus offset?), and how long the typical memory delay is"*. To address this, we have elaborated on each of the reported experiments on page 10, paragraph 1, as follows:

"Previous TMS studies, stimulated the sensory visual cortex at various timepoints during VSTM maintenance, with variable results (e.g., Rademaker et al., 2017; van de Ven et al., 2012; van Lamsweerde et al., 2017; for reviews see Phylactou et al, 2022; Xu, 2017). For example, Rademaker et al. (2017) interfered with sensory visual cortex TMS at 0 ms and 900 ms into a 2 second delay period, after the offset of a memory array presented for 200 ms. Similarly, van Lamsweerde et al. (2017) stimulated at 0 ms, 100 ms, and 200 ms during a 1 second delay period, which followed a 100 ms memory array. In another study, van de Ven et al. (2012) induced TMS at 100 ms, 200 ms, and 400 ms of a 1.5 second delay period, after the presentation of a 150 ms memory array. Some studies indicated that TMS effects were stronger for earlier stimulation (up to 200 ms; Rademaker et al., 2017; van Lamsweerde et al., 2017), compared to later stimulation at 400 ms (van de Ven et al. 2012), and 900 ms (Rademaker et al., 2017), however other studies indicated that TMS after 200 ms was stronger (van de Ven et al., 2012). Based on a recent meta-analysis examining the effects of TMS on VSTM performance during the maintenance period, most studies differentiated between earlier (up to 200 ms into the maintenance period) and later (after 200 ms; usually halfway into the maintenance period) stimulation (Phylactou et al., 2022). The meta-analysis provided evidence for a strong TMS effect (g = 0.8) during earlier TMS, and a moderate effect (g = 0.5) during later TMS; however, further analyses indicated that the TMS effects were not significantly different between the two timing conditions (overall effect g = 0.58). In the current work, we also differentiated between early and late TMS, by considering the outcomes of previous studies (Rademaker et al., 2017; van de Ven et al., 2012; van Lamsweerde et al., 2017), and thus, to test our main question of whether the sensory visual cortex is involved in visual short-term memory we aim to examine the effects of TMS on behavioral performance separately for stimulation induced at 200 ms and 1000 ms (halfway) into the delay period. Further to our main hypotheses, exploratory analyses will be performed in order to replicate and explore any similar findings concerning a different TMS effect size for earlier compared to later stimulation."

Introduction: "Based on a recent meta-analysis examining the effects of TMS on VSTM performance during the maintenance period, most studies differentiated between earlier (up to 200 ms into the maintenance period) and later (after 200 ms; usually at the middle of the maintenance period) stimulation (Phylactou et al., 2021)." >> It would be helpful to

know what this meta-analysis found regarding the size of the effects of early and late TMS.

Response to comment:

We thank the reviewer for bringing this to our attention and we apologise for omitting this information in the previous versions. We have now added the results of the meta-analysis on page 10, paragraph 1, as also shown below:

"Previous TMS studies, stimulated the sensory visual cortex at various timepoints during VSTM maintenance, with variable results (e.g., Rademaker et al., 2017; van de Ven et al., 2012; van Lamsweerde et al., 2017; for reviews see Phylactou et al, 2022; Xu, 2017). For example, Rademaker et al. (2017) interfered with sensory visual cortex TMS at 0 ms and 900 ms into a 2 second delay period, after the offset of a memory array presented for 200 ms. Similarly, van Lamsweerde et al. (2017) stimulated at 0 ms, 100 ms, and 200 ms during a 1 second delay period, which followed a 100 ms memory array. In another study, van de Ven et al. (2012) induced TMS at 100 ms, 200 ms, and 400 ms of a 1.5 second delay period, after the presentation of a 150 ms memory array. Some studies indicated that TMS effects were stronger for earlier stimulation (up to 200 ms; Rademaker et al., 2017; van Lamsweerde et al., 2017), compared to later stimulation at 400 ms (van de Ven et al. 2012), and 900 ms (Rademaker et al., 2017), however other studies indicated that TMS after 200 ms was stronger (van de Ven et al., 2012). Based on a recent meta-analysis examining the effects of TMS on VSTM performance during the maintenance period, most studies differentiated between earlier (up to 200 ms into the maintenance period) and later (after 200 ms; usually halfway into the maintenance period) stimulation (Phylactou et al., 2022). The meta-analysis provided evidence for a strong TMS effect (g = 0.8) during earlier TMS, and a moderate effect (g = 0.5) during later TMS; however, further analyses indicated that the TMS effects were not significantly different between the two timing conditions (overall effect g = 0.58). In the current work, we also differentiated between early and late TMS, by considering the outcomes of previous studies (Rademaker et al., 2017; van de Ven et al., 2012; van Lamsweerde et al., 2017), and thus, to test our main question of whether the sensory visual cortex is involved in visual short-term memory we aim to examine the effects of TMS on behavioral performance separately for stimulation induced at 200 ms and 1000 ms (halfway) into the delay period. Further to our main hypotheses, exploratory analyses will be performed in order to replicate and explore any similar findings concerning a different TMS effect size for earlier compared to later stimulation."

Overall, this is an interesting and generally well-motivated and well-specified experiment, addressing a topic of pretty wide interest to Cognitive Neuroscience. I wish the authors well in conducting the experiments.

We thank the reviewer for the overall positive appraisal of this work, and we appreciate all the issues and concerns discussed by the reviewer, which we believe had led to an improved revised version of this registered report.