We thank the reviewers for their helpful feedback on our work. Please find our itemized, point-by-point replies below each comment (reviewers' comments are in Italics).

Review by anonymous reviewer 1, 04 Feb 2025 13:22

I would like to thank the authors for their detailed and informative response letter. They have carefully addressed most of the reviewers' concerns and further improved the report significantly. On the other hand, some issues remain unresolved even though they could be addressed.

Major points

Interval of interest:

Comment
The IOI for the LOC analysis is now clearly defined. However, how exactly is the IOI for the MA analysis "based on mean RTs"? Is it based on each participant's mean RT, or on the grand average? Moreover, how is the interval defined? For example, does it include a specific interval around, before or after the peak?

Response
We thank the reviewer for giving us the possibility to better specify how the IOI for MA analysis will be defined. Indeed, it will be based on the average of RTs across participants and it will encompass a time-window ranging from -1,5 sd and + 1,5 sd around the mean value. We added this information in the Study design table.

Negative results:

Comment
Concerning evidence of absence and Bayesian statistics, the authors responded that "EROS statistical analyses are "limited" to the use of […] Opt3d, which does not allow computing Bayesian statistics." However, the Opt3d manual (https://www.nitrc.org/docman/view.php/868/2031/opt3d%20Manual) says on p. 1 and 27 that the data can be exported into standard text files (File > Save), including the option "ROI Analysis: Provides data from the average across all voxels defined in the ROI". In the case of time courses, one would only need to average the samples across time. Bayes factors could easily be computed using, for example, the GUIs of jamovi (which the authors have used for their behavioral data) or JASP (https://jasp-stats.org/) and default priors without previous data. For details and tutorials, see Keysers et al. (2020, Nat Neurosci). Since the PCI RR guide for authors (https://rr.peercommunityin.org/PCIRegisteredReports/help/guide_for_authors) specifically encourages Bayesian tests for evidence of absence, they might want to consider this option.

Response
We thank the reviewer for suggesting this procedure, following which we modified our manuscript (*3.Study design* and lines 376-377) by adding Bayesian analysis (with default priors) to Analysis plan A2, in which we expect to find no difference between the contrasted conditions. Specifically, as suggested by the reviewer, we will extract averaged data from the ROI of LOC and we will compute the relative Bayes factor using JASP with default priors.

Sample size estimation:

Comment
Concerning the computation of effect sizes, the authors responded that "exporting EROS data would mean extracting a very huge amount of data (i.e., a matrix of data for each voxel and for each time point) which would be too difficult to be managed without a dedicated software. For this reason, EROS analyses are constrained to the use of Opt3d software." However, as mentioned above, Opt3d allows the export of time courses averaged within the voxels of a ROI. Thus, the authors would only get one time course per condition and ROI. Then, they could easily average the samples and compute the effect sizes in, e.g., jamovi or JASP.

Response
Following the reviewer's directions, we exported EROS data from our previous study (Colombari et al., 2024) to calculate the effect size necessary to estimate the sample size of the current study. Importantly, Opt3d does not allow the export of data for a single condition (e.g,, Aware condition or Unaware condition) but only data

resulting from a contrast of conditions (e.g., Aware vs. Baseline, Aware vs. Unaware, and so on). Since the research questions addressed in the present study require contrasting Aware and Unaware conditions, we extracted data contrasting such conditions. Moreover, as the present work aims to investigate the involvement of extra-striate areas in consciousness-related tasks (Q1) and the independence of such areas from response requirement (Q2), we computed the effect size in LOC ROI. Specifically, we averaged the significant time-points and calculated the Cohen's d (0.611) computing a one-sample t-test. We estimated the sample size for the current study using G-power software (v. 3.1.9.7), with a power of 90% and a significance level of 2% (as requested by the guidelines for authors of PCI RR). The estimated sample size resulted in 32 participants (critical t= 2.143; actual power= 0.900). We modified the *Study Design table* and section *2.2.1 Sample size estimation* by specifying how the sample size for the present study is determined.
Importantly, it must be noted that Opt3d only allows exporting data **averaged** across all voxels within the ROI, while statistical analyses are performed **voxel by voxel** within the ROI and corrected for multiple comparisons using the Random Field Theory. Hence, the data used to calculate the sample size are not exactly the same data used for EROS statistical analyses.

ROIS:

Comment
I agree that basing ROIs on the previous study (Colombari et al., 2024) is perfectly fine. However, is circularity / double-dipping (Kriegeskorte et al., 2009) also avoided when investigating the "novel ROIs over areas responsible for visual processing and motor execution" selected based on "visual inspection of functional data" (l. 368)? That is, are these novel ROIs selected a priori or in a data-driven (potentially non-orthogonal) manner?

Response
We thank the reviewer for pointing this aspect out and we agree with her/him that this information could be misleading. We thus modified the text at lines 386-387 by removing the sentence "*by visual inspection of functional data"*.

Minor points

Comment
The authors adequately introduced previous no-report NCC studies using EEG and fMRI (l. 54-75), but only mentioned results focusing on the temporal domain (i.e., VAN and LP in EEG studies) while neglecting spatial results (e.g., LOC effects in fMRI studies). In their introduction of spatial aspects (l. 76-107), the authors highlight that EROS can provide "accurate information […] both from the temporal and spatial point of view" (l. 106), but later mention the „relatively low signal-to-noise ratio of EROS" (l. 182). Thus, it would be worth mentioning that previous no-report studies using both EEG and fMRI (e.g., Dellert et al., 2021; Kronemer et al., 2022) have also found awareness effects in the lateral occipital complex (LOC) and linked it to the VAN, which is the focus of the present study.

Response
We thank the reviewer for this suggestion. We revised our manuscript accordingly in lines 77-79.

Comment
L. 108: In line with the abstract, „peculiar" should also be changed to "distinctive" here.

Response
We thank the reviewer for noticing this oversight. We modified the manuscript text as suggested (line 106).

Comment
L. 188: I appreciate the clear new exclusion criteria. On a minor note, "will not also be included in the analyses" should be fixed.

Response
We thank the reviewer for this suggestion, following which we modified the manuscript (line 206- "[…] will not ~~also~~ be included in the analyses as well").

Comment
L. 211: "the left portion of the primary visual cortex, which is known to be anatomically closer to the skull compared to the right one". Could the authors provide references for this?

Response
We agree with the reviewer that references supporting this statement should be provided. For this reason, we modified our manuscript by adding the following reference (lines 231-232):

- Zhao, L., Matloff, W., Shi, Y., Cabeen, R. P., & Toga, A. W. (2022). Mapping complex brain torque components and their genetic architecture and phenomic associations in 24,112 individuals. Biological psychiatry, 91(8), 753-768.

Comment
L. 340: The authors have clarified in their response that the baseline correction will not be conducted using the whole 486 ms prestimulus interval of the segment (which the current report would suggest) but only 204 ms. This is good due to the reasons stated in my previous review, but the actual 204 ms baseline correction interval should also be stated in l. 340.

Response
We thank the reviewer for noticing this oversight. We modified the text as suggested. Importantly, the baseline correction performed by p_pod (i.e., the one the reviewer is referring to, mentioned at line 357) it's not the actual baseline correction described in the previous version of the manuscript. Indeed, the correction performed by p_pod is a demeaning, while Opt3D performs the proper baseline correction. We corrected our manuscript at lines 357 and 367.

Review by anonymous reviewer 3, 23 Jan 2025 08:19

The authors clearly improved the manuscript. All my comments have been addressed, and passages of the manuscript that were lacking certain details have been supplemented with information.

I believe that if the study is carried out as planned, it will be a valuable contribution to the field.

Comment

We thank the reviewer for her/his appreciation of our work.

Review by anonymous reviewer 2, 04 Feb 2025 12:05

I would like to thank the authors for thouroughly addressing my concerns. However, I have two important points where I think the registered report could still improve:

Comment
1 Use of Bayesian statistics. I understand that the EROS data might not be easily tested using Bayesian statistics given constraints of the Opt3D software. However, as written in the Opt3D Manual (https://www.nitrc.org/docman/view.php/868/2031/opt3d) data can be exported. Given this option and the fact that the behavioral data is independent from Opt3d constrains I strongly recommend including Bayesian statistics in the analysis plan (for the behavioral data as well as for the EROS data) for testing null hypothesis. If no prior knowledge exists default priors can be used. One option would be e.g. JASP (https://jasp-stats.org/) as it is similar to Jamovi or any other statistics program like R.

Response
We thank the reviewer for this helpful suggestion, and we agree with her/him that Bayesian statistics represents a useful tool to test and interpret null effects. Following the reviewer's directions, we thus modified our manuscript at lines 376-377 and in section *3. Study design* by adding Bayesian hypothesis testing to Analysis plan 2, in which we expect to find no significant difference between the contrasted conditions. Specifically, as suggested by the

reviewer, we will extract averaged data from the ROI of LOC and we will compute the relative Bayes factor using JASP with default priors.
Concerning the behavioral analyses, they are not listed in our design table as they do not address our research questions and thus we do not make any prediction based on them.

Comment
2 Interpretation of non-significant t-tests concerning response times: I still think the description of the behavioral pilot data is misleading. The results (e.g. "Paired sample (two-tailed) t-test performed with Jamovi (version 2.3.28) highlighted that there was no significant difference between the two conditions (t(4) = 1.88, p = .134, Cohen's d = .839), suggesting that they are comparable.") do not indicate that the values are comparable. While it is true that there is no significant difference with N = 5, only Bayesian statistics could offer the conclusion of evidence for the absence of an effect. One option would be to delete the 'suggesting that they are comparable' and 'This indicated that there was no difference in the responsiveness between the two conditions.' or to include Bayesian evidence (which given the t-values would likely be inconclusive, thus leading to a more fitting bottom line).

Response
We thank the reviewer for pointing this aspect out. Indeed, we agree with her/him that the description of the behavioral pilot results could be misleading, as the performed analyses cannot actually test the absence of an effect. For this reason, we modified the manuscript by deleting the sentence *"suggesting that they are comparable"* at line 439 and the sentence "*This indicated that there was no difference in the responsiveness between the two conditions*." at lines 442-443.