I now have two evaluations of your Stage 2 manuscript, kindly provided by the two reviewers who assessed the proposal at Stage 1. As you will see, one of the reviewers is broadly satisfied, asking for minor changes (including some key adjustments to the language). However, the other reviewer (Bonni Crawford) notes a potential error, or at least imprecision, in the ROI localisation method that we need to attend to carefully before proceeding with full acceptance. At Stage 2, is unusual for additional analyses to be required, but in this case I think that the reviewer's suggestion for an additional exploratory analysis is well made, and indeed likely to be essential to ensure that Stage 2 Criterion 2E is met (that the conclusions are based on the evidence). Note that the original analysis should be maintained, and this additional analysis added as transparently *post hoc*. I look forward to your revision and response, which I will return to the reviewer for further consideration. Note also that changes to the Introduction section should be minimal.

We extend our many thanks to the reviewers for the recommendations into improving the submission from stage 1 up until stage 2. We address each point below individually. These revisions have undoubtedly improved the clarity of our presented findings. Our responses to the reviewer's comments are marked in 'bold" and the new added text to the manuscript is marked in 'green'.

It was interesting to read about the results of the preregistered analyses which I had reviewed some months ago. I have no fundamental concerns regarding these analyses and think that the papers provides some new information that advances our knowledge in understanding the psychobiological foundations of intergroup attitudes/behavior. Here are some minor points of mine:

p.3.: "These were certainly not demented nor sadistic individuals who participated." Please eliminate "demented" here, because it could be misunderstood as negative labelling of people who suffer from this terrible disease.

We thank the reviewer for pointing this out and fully agree that this term may be misconstrued and should therefore be omitted from the manuscript.

general comment on Introduction: A lot of effort is put into explaining why SDO and RWA are either unidimensional or separate constructs. May be this could be formulated in a less "black or white" way, because it is highly likely that both constructs own both shared, but also unique variance - which is also represented by the neuroanatomical findings of this paper

We thank the reviewer for highlighting this but we believe that we have sufficiently covered literature that support both perspectives that SDO and RWA could be viewed as unidimensional and separate constructs. We direct the reviewer to the final paragraph in section 1.5. for an explication of our stance in this regard.

Figure 1: please add confidence intervals to the figure

We thank the reviewer for this recommendation and we believe it has improved the utility of this figure.

Table 3: please add information on the direction of the observed relationships (positive vs. negative correlations).

We thank the reviewer for this recommendation and we've incorporated the predicted directions in the table in the manuscript.

Results & Figures: In my opinion, some of the figures could be moved to the supplemental material, and only the main findings should be presented.

We thank the reviewer for this recommendation and agree that some figures could be moved to a separate supplementary materials document. In line with this, we've removed figures 2 and 3 (i.e. uncorrected whole brain analysis glass-brain images) from the main manuscript. Therefore, figures 4-6 (i.e. ROI analysis heatmaps) in the previous iteration of the manuscript will be figures 2-4, respectively.

Stage 2 Review of *Neuroanatomical Correlates of System-justifying Ideologies: A Pre-registered Voxel-based Morphometry Study on Right-Wing Authoritarianism and Social Dominance Orientation*

The authors have now conducted the analyses described in the Methods section reviewed in Stage 1. My feedback on each section of the Results is below:

Section 3.2, lines 2 & 3, where it says "none of the self-report items" and "therefore all items" – should this be 'scales'? i.e. the two self-report scales that were used? Or is this referring to an analysis of all the items within those scales? Also, with regards to testing the assumption of normality, was any statistical test of normality conducted (e.g. a Shapiro-Wilk test or Kolmogorov-Smirnov test)?

We thank the reviewer for seeking clarification on this. The analysis does reference individual items within the scales. That is, all items were subjected to analysis of skewness and kurtosis scores. We refrained from using any statistical tests of normality as the conditions to violate the normality assumption in tests such as Shapiro-Wilk test are too strict because they look for theoretical normality. Therefore, any minor deviations particularly in a relatively small sample size may inadvertently remove items worth including in further analysis.

Table 1: It's a bit confusing that summary stats are reported separately for male and female participants, given there were no significant differences between them.

We agree with the reviewer that this may be confusing and therefore we have omitted the separation by gender in this table.

Section 3.3, line 1: Were RWA and SDO both entered in the same model, or were these two separate models?

To clarify, RWA and SDO were entered in the same model for this analysis.

Section 3.3.1: How was the overlapping tested? Was this a conjunction analysis or was this done by overlaying the results from separate GLMs, one for RWA and one for SDO?

The overlapping was tested using conjunction analysis such that two contrasts in the model (i.e. RWA and SDO) are highlighted

Section 3.3.1, line 2: in addition to looking at regions in which grey matter volume was positively associated with both RWA and SDO, did the authors look to see if there were any regions in which grey matter volume correlated negatively with both RWA and SDO?

We thank the reviewer for seeking clarification on this. We did not report these correlations because this direction of correlation with both RWA and SDO was not specified in our preregistered hypotheses. However, it is worth noting that we did explore this possibility but as with the GMV positively associated with both RWA and SDO, no clusters survived FWE correction.

Section 3.3.1, line 2: Why was the minimum cluster extent of 20 voxels used? Did the authors consider employing threshold-free cluster enhancement (see tr08ss1.pdf (ox.ac.uk)) to eliminate the need for imposing a cluster-forming threshold?

We thank the reviewer for seeking clarification on this. We selected the minimum cluster extent of 20 voxels following the methodology of Nam et al. (2017). However, we nonetheless re-ran the analysis using the TFCE (threshold-free cluster enhancement) toolbox in SPM for both spherical ROIs and atlas-based ROIs and reported in the manuscript under section 3.7. Exploratory Analysis.

Section 3.3.2, line 1: in addition to looking at regions in which grey matter volume was negatively associated with only one of RWA or SDO, did the authors look to see if there were any regions in which grey matter volume correlated positively with only one of these?

We thank the reviewer for seeking clarification on this. We did not report these correlations because this direction of correlation with RWA and SDO separately were not specified in our preregistered hypotheses. However, it is worth noting that we did explore this possibility but as with the GMV positively associated with both RWA and SDO, no clusters survived FWE correction.

Figure 3a: Should the legend say negatively associated with RWA, rather than positively?

We thank the reviewer for identifying this error. We have made the amendment in the manuscript.

Section 3.4.1: As this is an ROI analysis, the subheading here should refer to the question of whether grey matter in the amygdala (as opposed to 'any region/s) is associated with RWA and SDO.

We thank the reviewer for pointing this out. We fully agree with the reviewer in that the subheading should be more specific to the region. As such, we've made the necessary amendments to the manuscript.

Section 3.4.1, line 2: Apologies that I did not feed back on this when I reviewed the methods, but this ROI sphere (radius of 20mm) is much too big for the amygdalae. The amygdalae are small structures, with a volume of around 1.5 – 2 cm^3 each. A sphere with a diameter of 4 cm would have a volume of around 30 cm^3 so these ROIs are going to cover a lot of brain that is not amygdala.

Section 3.4.2: As this is an ROI analysis, the subheading here should refer to grey matter in the vmPFC (as opposed to 'any region/s).

We thank the reviewer for pointing this out. We fully agree with the reviewer in that the subheading should be more specific to the region. As such, we've made the necessary amendments to the manuscript.

Section 3.4.3: As this is an ROI analysis, the subheading here should refer to grey matter in the left anterior insula (as opposed to 'any region/s).

We thank the reviewer for pointing this out. We fully agree with the reviewer in that the subheading should be more specific to the region. As such, we've made the necessary amendments to the manuscript.

Figure 4: These blobs do not look like they are in the amygdalae.

Figure 5b: In these images, it looks as though the blob is outside the brain?

Figure 6: These blobs do not appear to be in insular cortex.

Overall

Given my feedback above, I would suggest that the spherical ROIs have not been successful in isolating the desired brain regions. I have never used the sphere approach to making ROIs, so perhaps the problem is simply that the spheres were too big. However, I would suggest that the authors consider using a probabilistic atlas to construct the ROIs. This eliminates the problem of working out what sphere radius is appropriate for an ROI for a small region like the amygdala versus a larger region like the vmPFC. Also, using a probabilistic atlas to create the ROIs accounts for the fact that brain structures are not necessarily spherical.

We thank the reviewer for this recommendation. Hence, we've included a new section under results, "3.7. Exploratory Analysis" that includes both atlas-based ROI analysis and TFCE analysis. We've also included changes in phrasing of our interpretation of the findings in the discussion section to reflect any discrepancy in results between the preregistered and exploratory analyses.

I would also like to ask the authors to clarify what correction for multiple comparisons was applied in the ROI analyses? Was it SPM's small volume correction?

We thank the reviewer for seeking clarification on this. Yes, small volume corrections were used for all ROI analyses.

Regarding the section of 3.4 that is underneath Table 3, I suggest the authors remove this. It is not necessary, as the T-statistics provide this information. Extracting the volumes and then correlating these with the original measures could also arguably be called double-dipping.

We thank the reviewer for highlighting this circular analysis and have removed this from the results section.

I hope the authors find this feedback helpful. In case the results are different following analyses with more localised ROIs, I am not going to review the Discussion at this time. I will, however, be very happy to review an amended Results and Discussion section in future.