

Response to reviewer comments for manuscript: “South Atlantic overturning and heat transport variations in an ocean reanalysis ensemble and observation-based estimates”

I thank the authors for addressing my previous comments carefully. The revised manuscript has improved and the new detailed comparisons between reanalyses and observation-based estimates are helpful. I recommend the manuscript for publication after the following minor points are addressed.

We are happy our changes have satisfied the reviewer’s main comments and that this has improved the paper. We thank them greatly for their valuable feedback and for the additional comments they have provided below that help improve the paper’s clarity. We respond to their comments and questions below in blue font.

Title: It is awkward to include ‘other estimates’ in the title. Is it referring to those ‘observation-based estimates’? Please rephrase and it would be better to be specific.

Thanks, we have changed the title to “observation-based estimates” to make it clear we are referring to estimates of the MOC and MHT and to be more specific. We have not included details of the specific observations used to prevent the title becoming too long.

Lines 141-146: What velocity is used at the reference level depth? Is it assumed to be zero all the time? Is it the time-mean or time-varying velocity taken from the model at that reference level depth? Some details on this calculation are currently missing, making it hard to understand how the authors obtain their baroclinic velocities (and thus the baroclinic component of the MOC).

When calculating the baroclinic MOC anomaly in the reanalyses, we set the velocity at the reference level depth to zero throughout the timeseries and calculate the baroclinic velocity above this level using the thermal wind balance equations. We make this choice because we use these velocity estimates to calculate the baroclinic MOC anomalies, so we do not include the barotropic velocity in this calculation. Since we calculate anomalies of the baroclinic MOC from their time-mean values in each reanalysis rather than the absolute magnitude of the baroclinic MOC, our estimate would be the same if we set the velocity at the reference level depth to the time-mean velocity at this depth.

We now make this important detail explicit in the methods by adding a sentence to lines 146-147: “The reference velocity is not required to calculate the baroclinic MOC anomalies, so we set the baroclinic velocity to zero at the reference level depth”

Lines 151-155: I cannot follow the justification of the reanalyses-SAMBA comparisons given their known incompatibility.

We agree this sentence was confusing. The reanalyses and SAMBA estimates do not use the same reference levels, so some of the differences between the estimates of the baroclinic and barotropic MOC anomalies could arise from the different reference levels. However, given both estimates have reference levels at depths below the depths over which the baroclinic velocity varies greatly (at least based on the velocities in the reanalyses), most of the differences between their baroclinic and barotropic MOC anomaly estimates are likely due to differences in the geostrophic MOC (i.e., what we are trying to understand) rather than differences in the reference levels. The reanalysis estimate of the baroclinic MOC anomaly is also not very sensitive to the choice of reference level. We note these points in the method section.

We think adding “and methodologies” to this sentence was confusing because the fact the methodology (besides the reference levels) is different does not mean comparisons cannot be made between the estimates. Instead, the methodology differences likely cause some of the differences in the geostrophic MOC that we are trying to understand. We have now removed “and methodologies”, which hopefully makes this clearer.

Lines 263-266: Same question as above. Their incompatibility seems to make the objective of this section impossible (that is to ‘investigate possible causes of the differences in variability between SAMBA and the ensemble’). Please elaborate.

Thanks, we agree this was also confusing and made the comparison seem inappropriate.

As above, we now only mention the reference levels being different. We remove “and method of computing the reference level velocity; nonetheless, major features can be inferred from each dataset.” We now state in lines 264-266:

“The baroclinic and barotropic components of the MOC are not directly comparable between the ensemble and SAMBA due to differences in the reference level depth, but this probably has little impact on the differences between these estimates (see Section 2.2).”

While the method of computing the reference level velocity (and other differences in the methodologies) cause some of the differences in the baroclinic and barotropic MOC anomalies between the estimates, they also cause differences between the estimates in the geostrophic MOC that we are trying to understand. Thus, the impact of these differences on the MOC anomalies should be included in our estimates of the baroclinic and barotropic MOC anomalies. While the different reference level depths prevent direct comparison between these estimates, they likely only cause small differences between the estimates as described in the previous comment.