EGUsphere, referee comment RC1
https://doi.org/10.5194/egusphere-2022-1334-RC1, 2023

Comment on egusphere-2022-1334

Anonymous Referee #1

Referee comment on "Seasonal overturning variability in the eastern North Atlantic subpolar gyre: a Lagrangian perspective" by Oliver John Tooth et al., EGUsphere, https://doi.org/10.5194/egusphere-2022-1334-RC1, 2023

Review of « Seasonal overturning variability in the eastern North Atlantic subpolar gyre: A Lagrangian perspective" by Oliver J. Tooth et al. submitted to Ocean Science.

Using a NEMO-based forced global numerical hindcast simulation, the authors characterize the seasonal variability of the overturning circulation across the OSNAP-east section in the Subpolar North Atlantic. Combining Euleriand and Lagrangian approaches, they found that such seasonality is critically controlled by the transit time of water parcels north of the section, with a 8.5 month threshold beyond which diapycnal transformation is irreversible (i.e. contribution to the mean overturning, not the seasonality). They further describe the pathways and mechanisms underlying the seasonal signal, and show the key role for wind-driven changes in recirculation time of upper water masses in Irminger Sea. Distinguishing the respective pathways and timescales that characterize the seasonal and mean overturning is a key asset of this work.

It was overall a pleasure to read this manuscript. It is well and precisely written, with a rigorous and very comprehensive analysis of well-posed scientific questions. I think it comprises significant findings and will represent, alongside its already-published companion paper on the mean overturning state, a timely contribution to the field. Therefore, I have only a few general and minor comments, which Iist below.

General comments.

- The paper is long, quite dense and detailed, which could give some readers a hard time to eventually extract the key take-home messages. I would suggest shortening the text where possible and only keep the key findings in the main text, and maybe put the complementary diagnostics in supplementary materials. This is more of an advice than

a request.

- It should be made clearer (in the abstract notably) that the results apply to the OSNAP-east section only. In some instance, the findings are presented as relevant for the entire eastern SPNA (e.g. line 13-16). Although the sensitivity of the results to the specific section location is mentioned at line 579-584, I think this should be better emphasized throughout the paper. In fact, I wonder if replacing "in the eastern North Atlantic subpolar Gyre" by "across the OSNAP-east section" in the title would be indeed more correct.
- Although the authors start to elaborate on the possible impact of using higher-resolution runs at the very end of the manuscript, I believe more could be said on their use of a single numerical model to infer general conclusions about the overturning. I think the authors should better acknowledge in their manuscript that their results might be model-dependent, and specifically point out the most sensitive diagnostic/results accordingly. On the same topic, references to previous validation of that particular simulation in the SPNA should be added in Section 2.1 (how well does ORCA025-GJM189 represents the basic features of the subpolar North Atlantic?)
- I was left wondering whether the results could be sensitive to the chosen parametrization of turbulent convective mixing within the mixed layer (random perturbation of vertical velocities)? Could the authors comment on this in their Methods section?

Minor comments.

- 65-69: I am not sure to follow the point made here. Comparing the seasonal AMOC amplitude (4 Sv) with that of its potential surface forcing (20 Sv) makes sense (although the volume term hampers this comparison, as stated), but comparing it to the mean strength (16 Sv) is less clear to me.
- 152: Integrating from the sea-surface (instead of from the bottom) implies that the MOC strength includes the net transport through the section, because one can assume that the northward transport into the Arctic takes place in the shallowest layers. Some studies indeed use bottom-up integration to strictly capture the overturning (in fact the authors here remove this net throughflow to provide the mean at line 190). Therefore, I wonder whether the Eulerian seasonal signal does include a contribution from the seasonal variability of the net throughflow? Was it also removed from the total signal?
- 249-250: What explains the 1 Sv difference between the peak-to-peak amplitude of the seasonal MOC (4.1 Sv) and LMOC (5.1 Sv)? This should be explained.
- 348: Vage et al (2011) is an observational analysis, so it is not obvious whether their definition of boundary-interior limit (500 km) applies in the model too. Are the simulated IG and IC characteristic in line with observed ones ?