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Reply on RC2

Torge Martin and Arne Biastoch

Author comment on "On the ocean's response to enhanced Greenland runoff in model experiments: relevance of mesoscale dynamics and atmospheric coupling" by Torge Martin and Arne Biastoch, EGUsphere, https://doi.org/10.5194/egusphere-2022-869-AC3, 2022

We appreciate the overall positive and constructive comments by the reviewer. We will provide a detailed response together with the revised manuscript and will here just briefly touch upon the main points of the criticism. In our revision we will also try and tighten the manuscript as suggested here and also by Reviewer #1 and clarify the technical issues.

Salinity restoring is required to stabilize the AMOC in the forced configurations. The atmospheric forcing of the ocean-only model tends to create a fresh bias in the subpolar North Atlantic, which otherwise sends the AMOC on a declining trend. The restoring of sea surface salinity as applied here is standard procedure and we note that we apply a relatively weak correction, which moreover is neither applied under sea ice nor at grid nodes with runoff. This being said, the restoring flux does indeed oppose part of the freshwater perturbation on basin-scale. Integrated over the subpolar North Atlantic between 45°N and 80°N, the restoring adds a negative freshwater flux of approx. 0.02 Sv with similar magnitude in both high and low resolution configurations but steeper trend in the latter.This means that about 40% of the freshwater being added along Greenland's coast is withdrawn over the broad scale of the subpolar seas. Nevertheless, the forced experiments yield a significantly enhanced decline of AMOC strength compared to the coupled ones—despite the restoring. In fact, we may speculate that the restoring actually prevents a runaway positive feedback loop with northward salinity advection declining as the AMOC weakens. We will discuss this aspect in the revised version.

As in our first response to Reviewer #1, we note that running the ocean-only experiment with historical (instead of repeated year) forcing but the coupled ones under pre-industrial control conditions was a compromise to have sufficient internal variability in the former and to isolate the impact of fresh water from other global warming signals in the latter. This will be further discussed in the detailed response and argued for in the revised version of the paper.

The effect of the averaging period on the response of the AMOC is, for example, included in Figure 4 and Table 2, where we also show the distribution and mean response for the 20-year period of 43-62 years after onset of the perturbation in the coupled runs. We discuss that this result is more prone to be influenced by multi-decadal variability. We argue against an expansion of the averaging period of the forced experiments to 50 years using Figure A1, which shows a clear trend in AMOC decline prior to year 40 of the simulation. The comment of having more freshwater added and allowing it to spread further by selecting a slightly later and longer averaging period for the coupled experiments is an interesting aspect. This certainly would be an issue in identifying time scales of the responses, which we refrain from doing, and focusing on the large-scale response patterns, it is again the coupled configurations with the later/longer averaging period, which show the weaker responses (despite having more freshwater added in the end). Figure A2 compares the SST response for different averaging periods (e.g. 20 and 50 years), and while there are local differences, the large-scale patterns are robust. Based on such investigations, we concluded that it is rather internal variability than the timing of the averaging period that causes the larger uncertainty and decided for a longer averaging period for the coupled experiments. Nevertheless, we will improve this discussion in the manuscript using the concerns expressed by the reviewer as guidance.