

Dear Dr. Dupont,

Thank you very much for your helpful comments. We did the revision according to your comments. The reply details are given below.

1-page 1, line 17: "freshwater" here sounds awkward. Why not just "precipitation"?

reply: Changed to "precipitation" (p1, l17)

2-page 2, line 14: "[...] the changes are further accelerated by processes of Arctic amplification" does not tell anything. Please elaborate or drop.

reply: Dropped.

3-page 4, line 6: "As the first baroclinic Rossby radius is very small in the Arctic Ocean (Nurser and Bacon, 2014) [...]" Please amend. "very small" is not very telling but I assume the authors mean <5km. Then this statement is only true in the shallow parts of the Arctic and around the GIN seas. It also seemingly contradicts the authors goal to nearly resolve the first Rossby radius in the deep parts of the Arctic where it is about 10km or more with at least 2 points.

reply: the range of Rossby radius is added (p4, l3)

4-page 4, line 17: I am not sure what the author meant by "practically optimal": "Almost optimal" or "practical (useful) and optimal"?

reply: Changed to „In practice, however, optimal ...“ (p4, l14)

5-page 7, line 9: "looses" -> "loses"

reply: changed (p7, l22)

6-It would probably be telling if the authors could map an instantaneous field for high- lighting the model capacity to (nearly?) resolve mesoscale activity where resolved.

Reply: We do not consider this model setup as well eddy-resolving with which one can focus on mesoscale processes.

7-Fig 5: please show exact boundaries for domain averaging

reply: Definition is added in the text (p8 l15): „The two basins are defined as the Arctic region where the ocean bottom is deeper than 500 m, and separated by the Lomonosov Ridge.“

8-page 12, line 4: "Different from" sounds awkward. "Contrary to"?

reply: Changed. (p12, l22)

9-Fig10: Given the success of the CAA run to reproduce the same FW pathways as HIGH, I am curious to understand if the CAA run reproduces HIGH in other aspects: profiles, AW layer, SSH... It may be that the eddy-resolving resolution in the deep ocean is not necessary after all, only a realistic throughflow of the CAA (the eddy parametrization seemingly providing sufficient physics for the rest)!

Reply: We had mentioned this aspect in the second last paragraph of the paper. In the revision we added one sentence, „We also found that only better resolving the CAA channels (in the simulation where only the CAA is resolved with 4.5 km) did not significantly impact the representation of the AW layer.“ This additional sensitivity experiment shows the impact of CAA on FW spatial distribution, but not on AW circulation. Further extended studies, if required, can only be done in separate work considering the current paper length. (p33, l5)

10-Fig.12 FWC anomaly relative to which period?

Reply: Relative to the mean of the plotted period. Added to the figure caption.

11-page 14, line 8: definition of FWC from manuscript: "defined as the amount of pure FW that could be taken out of the upper ocean so that the ocean salinity is changed to 34.8[...]". Just for clarity could you provide the exact depth that defines the upper ocean in your calculation of FWC?

Reply: We clarified it now: „In the calculation of the modelled FW content presented below, the integration is taken from ocean surface to the depth where salinity is equal to the reference salinity“ (p14, l8)

12-page 22, line 17: "2D FW content". why 2D here? FWC is assumed implicitly to be a vertical integral. "maps" maybe?

Reply: changed to „vertically integrated“ (p22, l13)

13-Fig 14, maybe a little outside the scope, but given the pattern of thick ice, I suspect that the ice velocity are too slow. Have the authors compared their sea-ice velocity against buoys or derived-satellite products?

Reply: We checked the simulated sea ice velocity. On the contrary, the simulated sea ice drift is higher than observed from satellites. We consulted colleagues working on observations, and they suggested that the observation products might underestimate the drift was well. Further efforts are required in understanding the behavior of sea ice velocity, both in the model and in the observation.

14-page 26, last paragraph. Can the authors comment on the spurious diffusion on LOW. What are the value of the explicit horizontal diffusion in both simulations? For that matter, it would be nice to have background vertical diffusion value as well...

Reply: We did not quantify the spurious numerical mixing coefficients. However, we performed sensitivity simulations, and found that changing the background vertical diffusivity on the order of $1e-6$ m²/s can significantly change the model results. This indicates that numerical mixing, even small, can still significantly impact model results. Lateral mixing coefficients were mentioned in the model setup section. The importance of vertical mixing coefficients was mentioned in line 10 on page 29. The value of background vertical mixing coefficient is added (line 13 on page 6).

15-page 28, line 28: "obtains" sounds ill-chosen in this context. "displays" instead

reply: Changed to „has“

16-page 30, line 25: "Practically" is again a bit ambiguous. "For practical reasons" maybe?

reply: Changed to „in practice“ (p31, l7)

17-page 30, line 30: "Besides, maintaining high resolution measurements of ocean transports is of great importance for model development too." switch to observation-related subject a bit brutal to the reader. Maybe elaborate a bit?

reply: The sentence is removed.

Best regards
the authors