

Interactive comment on “A 4.5 km resolution Arctic Ocean simulation with the global multi-resolution model FESOM1.4” by Qiang Wang et al.

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Recommendation: minor revision

General comments:

in general well written although with some ill-posed expressions at times (see details below). Not groundbreaking but offers some insight into the state-of-the-art Arctic ice-ocean modelling. The unstructured model used here, FESOM, displays a good level of maturity enabling the authors to run sensitivity experiments with different resolutions at an acceptable cost and no particular negative impact on the scientific goals. Thus,

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the authors conclude that the higher resolution model is indeed capable of resolving smaller topographic-related details and better the intrinsic variability than the coarser version. In particular they emphasize the role of the Canadian Arctic Archipelago throughflow in impacting the Arctic-wide freshwater pathways. Below I even suggest further comparison between the 3 runs presented.

Detailed comments:

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

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

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.

4-page 5, line 17: I am not sure what the author meant by "practically optimal": "Almost optimal" or "practical (useful) and optimal"?

5-page 6, line 9: "looses" -> "loses"

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

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

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

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

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ocean is not necessary after all, only a realistic throughflow of the CAA (the eddy parametrization seemingly providing sufficient physics for the rest)!

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

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?

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

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?

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...

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

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

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?

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