Review of gmd-2017-136, version 2

Recommendation: minor revision

General comments

The authors have updated the manuscript in light of the comments provided. In particular:

1. It is now possible to access source files for the FESOM 1.4 code used in the simulations. The error in the tar archive previously provided has been fixed.
2. Figure 1 has been fixed. Agree that figure 2(a) is good as is (except for a question over the higher resolution seen in the Baltic Sea – see below).
3. Labelling of the 3 cases has been made consistent and introduced earlier, before first use.
4. Meshes are provided, enabling setups to be run (although more could be done to improve ability to make comparisons in the future – see below).
5. Model performance table added, which is very helpful, and essential for future comparisons.
6. Explanation in 5.1(a) improved.
7. FW variability and over-estimation of LFW clarified.
8. Some of the highlighted repetition has been removed.

As highlighted in the initial review, the research is timely, investigating an interesting question of model spatial resolution in the Arctic. This is directly relevant to current developments in coupled climate models, as flexible resolution models enter challenging inter comparison studies such as CORE-II. Whilst it is somewhat unsatisfactory to take a single mesh choice, run the coupled model in this configuration and analyse the output there is still the potential to learn significantly from this type of exercise.

There are, however, still significant concerns over the verbosity and unnecessary length of the paper. The length has not changed. It still stands at 41 pages. A more concise report would help make the paper accessible. There are still many sentences that are too general, lack specifics or quantitative backing. I disagree that so much repetition is required, which acts to dilute the key messages of the paper. Some of the structure has been changed, but this could be further improved also. On these first two points I defer to the topical editor as to what level of repetition and verbosity is appropriate for GMD.

Aside from this, there are a few other remaining issues to address regarding a discrepancy between text and figure on resolution in the Baltic, and why passive tracers are not fully plotted (and again the text here does not appear to describe what appears in the figure). See below for further details.

Specific comments

1. *Focus.* Requests were made to improve lucidity, removing general statements that do not add to the report. Focusing and reducing verbosity would make the paper more accessible. Whilst some repetition and verbosity has been reduced, as suggested, the authors have chosen to keep a large proportion of the content which appears superfluous, and does not add to the paper. The paper is still at 41 pages in length, despite these requests to shorten and make concise. There are still many sentences that are too general, lack specifics or quantitative backing. e.g. referring to 'common issues" with no specifics that are not helpful. Another:

"with a very reasonable thickness" – how good is the AW modelled? A quantitative statement gives a basis for others to compare to (and improve upon) in the future. Further examples were highlighted in the initial review.

On the advection scheme: Discussion of the FCT scheme still remains. Nothing profound, since it would be unusual to use an advection scheme which does not reduce numerical smoothing as resolution is increased! The discussion here again seems unnecessary. It is accepted higher resolution gives more accurate representation of mixing processes. This is not specific to the FCT scheme mentioned. Moreover, "numerical mixing" is possibly better termed "numerical smoothing" or "diffusive solution"?

2. *Structure.* The reasons to apply high resolution has been repositioned earlier as suggested, which helps lead a reader through the motivation.

   Points raised on section 5.3 and a request for context within FESOM efforts (since the 4.5km mesh has been used elsewhere in another publication) have not been adopted.

   This would be helpful for not only those following FESOM, but unstructured mesh model contributions to CORE-II.

3. *Figure 1.* Figure 1 has been fixed. Agree that figure 2(a) is good as is.

4. *Figure projections.* Fixed, such that comparisons are easy to make by eye.

5. *Definition of HIGH and LOW.* Labelling of the 3 cases has been made consistent and introduced earlier, before first use. Clear definition of the CAA case also now made.

6. *Eddy resolving considerations.* A quantitative statement has now been included.

7. *Reproducibility and Zenodo archive.* It is now possible to access source files for the FESOM 1.4 code used in the simulations. The error in the tar archive previously provided has been fixed.

   A suggestion was also made to provide the model output to facilitate future comparisons. This again would help model intercomparison efforts.

8. *Mesh set-up and reproducibility.* Meshes have now been provided.

   This is good, but a more general prescription of mesh generation, as suggested, would provide a more rigorous foundation for other comparative studies. These provided raw mesh descriptions can only be used by a limited number of other models that are similar to FESOM1.4. For example, it would be useful to include a more thorough description of how mesh size changes over Long-Lat space to enable generation in general – i.e. the element size metric. Currently, others will need to analyse the meshes to infer how this was chosen.

   This again would help improve possibilities for model intercomparisons.

   On meshes for intercomparison studies, the authors comment: "...and people often forget who did the mesh and how it was done." Why not provide a more complete description and lead the solution to this prevalent problem in the field?

   Also, the point regarding the higher resolution that seems to appear in the Baltic Sea in figure 2(a) has not been addressed.

9. *Spin up and simulation time frames.* This has been explained, and justification for selected time frames usefully included.

10. *Atlantic Water core temperature prediction.* Points raised have been clarified in text.

11. *Over-estimation of liquid freshwater content.* Explanations accepted. On the second reply discussing HIGH-CAA, the point was to make the context now provided in the response clear in the paper. This is a key point but is somewhat lost in the paper.

12. *Passive tracer implementation.* Before, the implication was that it was set to zero in the simulation, so it is helpful this has been clarified – although to avoid ambiguity, why not just say the passive tracer is not plotted below the Fram and Davis Straits?

    This does lead the reader to wonder why the passive tracer is not plotted in these regions. Why not plot the tracer over the entire domain?

    Referring to the location of the Davis Strait marked on Figure 1, it does not look to be true that the passive tracer is not plotted there in Figure 11 (a) and (b), both (middle) and (right).

    Do the authors mean it is not plotted in the region bounded by the Fram Strait and the Barents Sea Opening? Please correct. Again, why is not plotted there?

13. *FW variability.* Thank you for the explanations and clarifications in the paper.

14. *Outcomes.* Again, thank you for the explanations and clarifications in the paper.

15. *Other small points.* Explanations and text changes help clarify.

Technical corrections

All complete – good.

A few other minor points seen in the updated version:

1. Page 1, line 13: "independent on" better replaced by "independent to"?

2. Page 14, lines 6-9: Two sentences in a single parenthesis. Better to remove and start a fresh sentence: "content (Defined " $\rightarrow$ "content. This is defined "

3. References: JGR Oceans referred to as: "J. Geophys. Res. - Oceans", "Journal of Geophysical Research-oceans", "J. Geophys. Res. Oceans"

4. Page 37: "Mcwilliams" $\rightarrow$ "McWilliams"