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Geoscientific Model Development Discussions

Interactive comment on "A 1/16° eddying simulation of the global NEMOv3.4 sea ice-ocean system" *by* Doroteaciro Iovino et al.

Anonymous Referee #2

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This manuscript outlines the first results from a new global 1/16° implementation (GLOB16) of the NEMO-LIM ocean-sea ice model. The manuscript outlines some key metrics from the model and compares some metrics to a lower resolution implementations of the same model (GLOB4).

The model described in this manuscript is close to the leading edge of global ocean-sea ice models. It's important to document these models as they develop, and thus there are good reasons for GMD to want to publish this paper. However, there are a number of areas in which the manuscript could be improved.

My primary query is whether this manuscript is here simply to document the existence of a viable model (that is, the model works and is sufficient) or whether the aim is to make the case that the model is an improvement over previous, lower resolution ver-

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Discussion paper

sions. I strongly recommend following the latter path, but I found on reading the paper that the case for the GLOB16 model being an improvement on GLOB4 was somewhat tenuous. For many metrics the GLOB4 results were not shown, and in some areas GLOB4 looked slightly better! If one is to justify the move towards eddy-resolving models then a stronger case that the additional computational expense is worthwhilemust be built. (Alternatively, perhaps the conclusion may be that eddy-resolving is not worth the expense until models improve!) There are more details on these issues in the following list of suggested improvements that the authors may want to consider:

1. The use of acronyms (e.g. NEMO, CMCC) should be avoided in the abstract. In fact, CMCC is never defined in the text of the paper, and it seems unnecessary to list the affiliation of authors within the manuscript.

2. There is an ambiguous phrase on line 79: "... all (most of) the domain..." I suggest being more explicit.

3. In section 2.3, it's important to list more details about the magnitude of biharmonic viscosity, diffusivity, etc. If it's complicated, then a figure can be justified.

4. The SST restoring timescale seems very strong ... This value needs justification.

5. The first part of 2.7 should be shifted to 2.8. It also refers to an appendix which isn't present?

6. I don't understand the phrase bi-Laplacian (line 217). I'm used to either Laplacian or biharmonic.

7. On line 233 and beyond, replace TKE with simply KE (as many fields use TKE to represent Turbulent KE).

8. In 3.1, I'm not convinced that the mean surface biases mean anything in the presence of such strong restoring.

9. What is called AIW here is usually referred to as AAIW.

10. I found the results to be somewhat out of order. I suggest putting the global SSH variance maps second, right after the EKE results. In addition, I would put the global transport values before the AMOC results.

11. In 3.2, the depth-space overturning means very little in the Southern Ocean. The global MOC should be calculated in density space. The Deacon cell (line 350) is not a physically relevant cell and it would be better to estimate the size of the lower overturning cell in density space.

12. In Figures 2, 3, 5, 6, 8a,b, 9 and 10 there was no information on the GLOB4 results. However, occasionally, there were references in the text. As noted above, this manuscript will be much stronger if we know where and how improvements between GLOB4 and GLOB16 are manifested, so these results should be included wherever possible.

13. In Fig. 5 I would also like to see a line indicating estimates and errors of each quantity from observations. (Some are listed, some are not. In particular, the Mozambique Channel transport is stated as being "within the range of observed estimates" without a reference!) Also, the ACC transport, listed as the average over all years, is steady and very low for the last 6 years âĂŤ it is this equilibrium value that should be listed, not the average over all years.

14. On line 444, it is ambiguous as to which "two transports are out of phase".

15. One open question which deserves more investigation is the lack of mesoscale variability in the Southern Ocean. There is a suggestion (in the Conclusion) that this is due to viscous parameterisations, but no quantitative information on what those parameterisations are. The Southern Ocean is one of the key locations where one might expect this resolution to make a dynamical difference, but the very low variability and ACC transport indicate that something is missing here. I suggest a deeper quantitative comparison with other high resolution models is in order.

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