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Geoscientific Model Development Discussions

Interactive comment on "Assessment of the Finite VolumE Sea Ice Ocean Model (FESOM2.0), Part I: Description of selected key model elements and comparison to its predecessor version" *by* Patrick Scholz et al.

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Interactive comment on "Assessment of the Finite VolumE Sea Ice Ocean Model (FESOM2.0), Part I: Description of selected key model elements and comparison to its predecessor version" by Patrick Scholz et al.

Anonymous (Referee #2)

This paper gives a careful account and documentation of the development of FESOM. The comparisons between the impact of different vertical coordinate/free surface al-

gorithm choices and aspects of the Gent-McWilliams parameterisation will be of great use to the future users of FESOM2.0. The documented speedup of FESOM2.0 with respect to FESOM1.4 is impressive. My comments are generally minor and only concern aspects of presentation, rather than the science itself.

We thank the reviewer for his efforts and constructive comments. We tried to thoroughly include all of his comments or answer his concerns. Further, we have to add that some months after the submission of the manuscript we discovered a bug in the code of FESOM2.0 that only affected the zlevel and zstar part of the model. This bug made it necessary to redo only these runs. That means that the figures 2, 3, 4, and 5 are new, which also required to rewrite their descriptive part in section 3.1.

Things to consider

-The abstract doesn't mention the switch from a finite element to finite volume algorithm. I think it should because anyone familiar with FESOM will expect it to be finite element and it isn't covered until page 2.

–> We clarified in the abstract with an additional sentence that the new version of FESOM works on Finite-Volume, while the predecessor version performed on Finite-Elements.

-In the opening parts of Section 3 there are only comparisons between different model configurations. These will be very useful to users of FESOM2.0, and potentially to users of other models developing new configurations. However, there isn't any comparison to observations, even though there is later in the paper. It would be a good idea to at least tell the reader why such a comparison has been deferred. The observational comparison that is included, e.g. Fig. 6, is also carried out using WOA05. There are much more recent climatologies, such as WOA18, available.

–> We neglected at this point the comparison to observations since the anomalies between linfs and zlevel as well as linfs and zstar are much smaller than the anomalies

with respect to the observed climatology, that is why we limit ourself to only showing the comparison between linfs and WOA05. We have chosen the WOA05 to be comparable with the results shown in the first FESOM2.0 publication of Danilov et al. 2017 and to enable the reader to see the achieved improvements since Danilov et al. 2017.

–> ...ÂăThe new version of the model takes advantage of the Finite-Volume approach, whereas its predecessor version, FESOM1.4 was based on Finite-Element approach. ...

-lines 214-234 : On first reading this section I was lead to believe that the authors hadn't included the bolus overturning in their calculation of the MOC. Largely because the discussion mentions the Deacon cell, instead of in terms of Eulerian and bolus overturning (see Marshall & Radko (2003), Viebahn & Eden (2010), and Abernathey et al. (2011), etc), and because of the noted lack of connection between AABW and UCDW cells. Later in the paper the inclusion of the bolus overturning is explicitly mentioned (Section 3.2.2) and so I suspect that it has been included. This should be made clear at this point in the paper. Splitting the overturning circulation into Eulerian and bolus components may also be helpful, i.e. is the similarity between different versions of the model due to compensating changes in the two components? On the other hand, if both components are largely the same between models simply stating so would be sufficient.

–> We agree with the reviewer comment and added the information to the manuscript:

–> . . . The MOC contains the contribution from the Eulerian and eddy induced circulation (bolus velocity). ...

-End of Section 3 & Fig. 6, etc. There are very deep mixed layers in the Weddell Sea, which can be seen in all the mld figures. The Southern Ocean mixed layer depths look like a poor match to observational estimates as a result, possibly because the colour bar extends to 3000m. Are the deep Weddell Sea mixed layer due to deep convection and/or is there a persistent polynya in the Weddell Sea?

–> The deep Weddell Sea mixed layer seems to be due to deep convection in this model configuration, there are no polynyas in the sea ice data

At the end of Section 4.1 I was expecting a statement to the effect that FESOM2.0 is an overall improvement with respect to FESOM1.4. Is this the case? If not, can the authors speculate as to what they would change in order to exceed the performance of FESOM1.4?

–> We agree with the reviewers comment and added the following statement to the manuscript:

...In summary, one can say that FESOM2.0 and FESOM1.4 simulate the ocean with a comparable magnitude in the hydrographic biases, although FESOM2.0 tends to have warmer biases, while FESOM1.4 fields are dominated by colder biases. Nevertheless it should be kept in mind that FESOM1.4 was optimized, improved and tuned over a period of ten years while with FESOM2.0 we just stand at the beginning of that process. ...

Minor Comments lines 95-103 : repeated use of resolution. Do they mean resolution of grid/node spacing?

–> The resolution in our case is the mean distance between the vertices within a triangle. We try to clarify this in the manuscript.

lines 129-130: The authors later cite Adcroft & Campin '04 and use zstar as a label. I'd suggest introducing it here.

–> We followed this advice.

lines144-155: choosing zlevel as the label for the nonlinear free surface method is potentially confusing, given that zlevel is a common term for a geopotential coordinate system. Why not just use nonlin?

–> We share the reviewers concerns and are aware of the problem but wanted to stay

consistent with the notation in Ringler et al. 2013.

line 170-174: One of the big improvements that you'd also expect moving from a linear free surface to full z* via nonlinear free surface is a general improvement in tracer conservation. Have the authors investigated this?

–> Of course tracer conservation was our motivation during the development process, however we did not explicitly compare the tracer conservation between linfs, zlevel and zstar.

lines 211: Its probably worth noting that it isn't that surprising that the largest differences between mixed layer diagnostics are in the Southern Ocean, given how notorious the region is for biases, etc.

–> Here we disagree with the reviewer, since two mixed layer diagnostics of our choice although based on the same hydrography/density still lead to different results for the Southern Ocean. Since various definitions are used in different ocean models, a comparison of mixed layer depth across different models is prone to errors when the MLD definition do not match.

line 284: eddy counteraction, are they referring to eddy compensation? Again, use of Deacon cell, better to refer to Eulerian and bolus overturning.

–> The eddy counteraction here is referring to the existence or absence of the GM bolus velocities. We clarify this in the manuscript

Typos, etc line 32: "the" global ocean and climate → changed in manuscript

line 67: allows to utilize plenty of different - > allows the utilization of different vertical → changed in manuscript

line 105: an medium -> a medium → changed in manuscript

line 249: whereby skewness formulation as suggested as Griffies et al (1998) is used.–> where the skew flux formulation of Griffies et al (1998) is used. → changed in

manuscript

line 265: within same density class -> within the same density class. → changed in manuscript

line 267: consistent with what? → Without Redi diffusivity there is no consistent way for the model to mix Temperature and Salinity along isopycnals, there will be predominantly spurious mixing effects.

line 271: Fig. 10 being referenced before 8 or 9, maybe just reorder them. → changed in manuscript

line 282: without GM -> without the GM → changed in manuscript

line 306: Align -> Aligned → changed in manuscript

line 423: brackets around MPI→ changed in manuscript

line 462: had -> has, plenty -> large amount? → changed in manuscript

line 555: of the -> the → changed in manuscript

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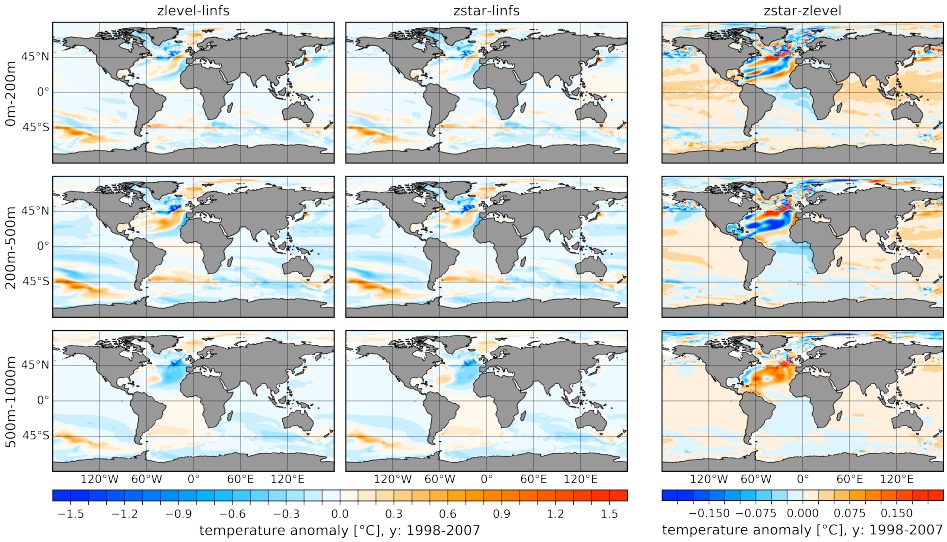

Fig. 1. New Fig.2

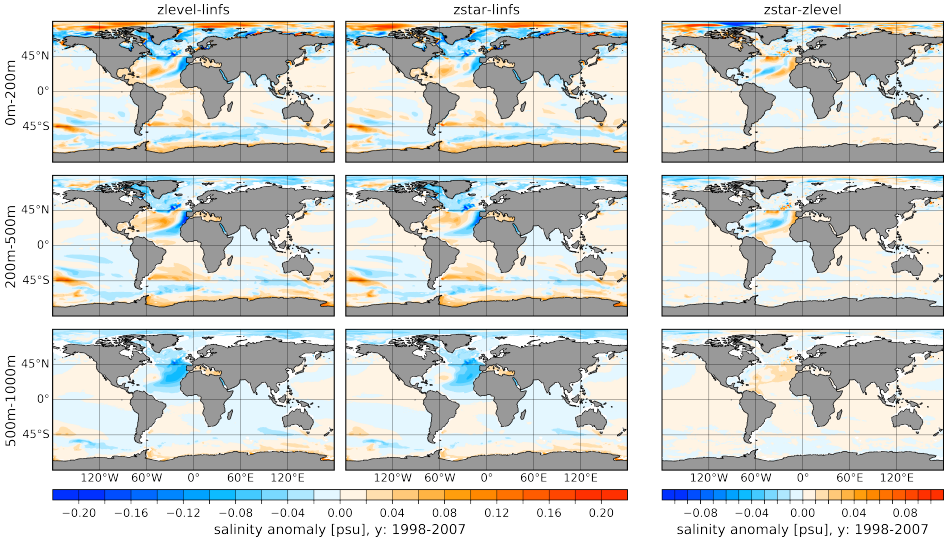

Fig. 2. New Fig.3

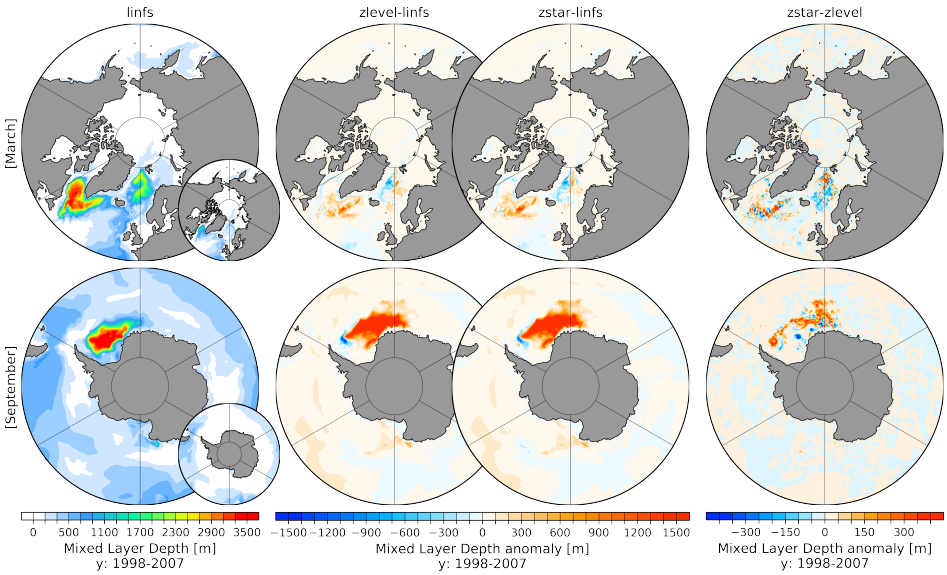

Fig. 3. New Fig.4

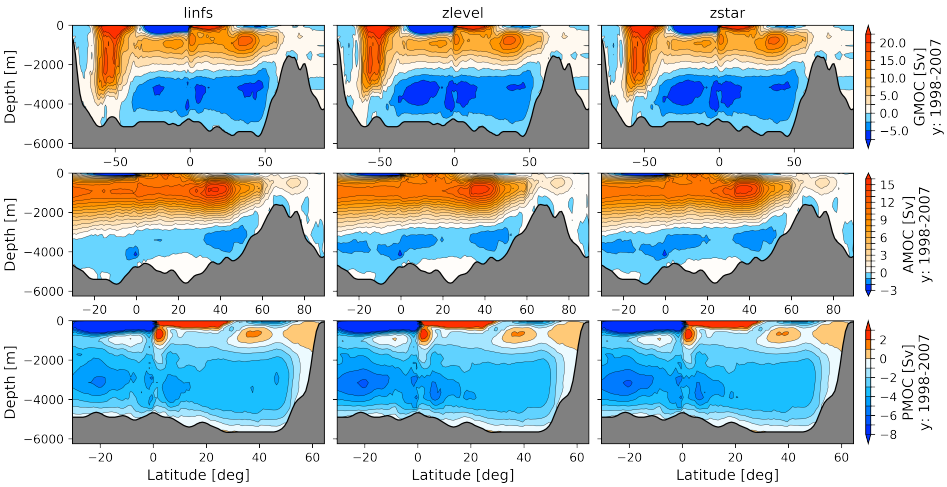

Fig. 4. New Fig.5