Geosci. Model Dev. Discuss.,
https://doi.org/10.5194/gmd-2018-329-RC2, 2019

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.

Anonymous Referee #2

Received and published: 23 July 2019

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

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.

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.

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.

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 matcg 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?

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?

Minor Comments

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

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

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?

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?

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.

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

Typos, etc

line 32 : "the" global ocean and climate

line 67 : allows to utilize plenty of different - > allows the utilization of different vertical

line 105 : an medium -> a medium

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.

line 265 : within same density class -> within the same density class.

line 267 : consistent with what?

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

line 282 : without GM -> without the GM

line 306 : Align -> Aligned

line 423 : brackets around MPI

line 462 : had -> has, plenty -> large amount?

line 555 : of the -> the

References

Abernathey, R., J. Marshall, and D. Ferreira, 2011: The dependence of Southern Ocean meridional overturning on wind stress. J. Phys. Oceanogr., 41, 2261–2278.

Marshall, J. and T. Radko, 2003: Residual-mean solutions for the Antarctic Circumpolar Current and its associated overturning circulation. J. Phys. Oceanogr., 33, 2341–2354.

Viebahn, J. and C. Eden, 2010: Towards the impact of eddies on the response of the Southern Ocean to climate change. Ocean Modell., 34, 150–165.

Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2018-329, 2019.