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Comment on gmd-2021-94

Luke Van Roekel (Referee)

Referee comment on "Assessment of the Finite-VolumE Sea ice–Ocean Model (FESOM2.0) – Part 2: Partial bottom cells, embedded sea ice and vertical mixing library CVMix" by Patrick Scholz et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2021-94-RC3>, 2021

The manuscript of Scholz et al documents a large number of sensitivity tests related to new features for FESOM2 (partial bottom cells, sea ice coupling, and vertical mixing). I was extremely impressed by the vast number of simulated years done for this manuscript and the quality of FESOM developments and the writing. While I found the paper sound and clear for the most part, I have a couple major concerns and more detailed minor comments that I would ask the authors to consider.

First and foremost, I struggled in the manuscript to see the scientific impact to the broader community. This manuscript reads as a FESOM technical report and is no doubt very beneficial to that community. However, it was very difficult for me to draw out points of interest to the broader modeling community. A few examples, I felt the discussion and results around partial bottom cells (PBCs) and sea ice coupling did not come across as of interest to the broader community, but it is quite possible that having all these results in one place will be a useful go to reference. You could consider trying to address causes of biases more directly and what the role of the change was in the circulation change, or perhaps consider making a recommendation of best practice configuration for FESOM at the very least. These could be ways to improve the broader impact of this manuscript. As of now, for the most part biases are simply noted and then moved on. There are a few exceptions, it is mentioned the way FESOM computes the bulk Richardson number causes the changes in some biases, but plots of boundary layer depth or surface layer average velocity and buoyancy for each method were not plotted.

Second, I think there were issues with the discussion of vertical mixing with regards to CVMix. It is stated that FESOM_kpp is configured in a different manner from CVMix, with the surface layer averaging noted as a key difference. However, this is not correct. CVMix leaves a number of choices up to the calling model, amongst them is the velocity and buoyancy difference for the bulk Richardson number. Griffies et al 2015 recommends using 10% of the boundary layer depth as the surface layer average, but this is not within CVMix itself. We further discuss the dependence on model choices in Van Roekel et al 2018. As an example, POP chooses the largest shear between a depth and the surface cell making this one step further than what FESOM chooses. It would be interesting if FESOM made other choices in configuration relative to default CVMix, e.g. Monin obukhov/Ekman limiters, matching at the boundary layer base, shape function parameters, etc... and what the impact of these choices might be.

Relatedly, I think it is important to note that the FESOM KPP choice of the first layer being the surface layer is not physically consistent with KPP (even Large et al 94). Throughout KPP there are built in assumptions regarding the depth of the surface layer (default to 10% of the boundary layer depth) and assuming the first layer as the surface layer is inconsistent. While it is a valid assumption it is important to point out this issue and the consequences of it. It would be interesting to explore the impact of this choice, but that is likely beyond the scope of this paper. Here it shows basic plots of T/S, but would be good to know a finer scale view too. Have you conducted a simulation that uses the CVMix library but the FESOM_KPP choice for the numerator and denominator of the bulk Richardson number? This could clearly show differences associated with the choice. My expectation is that the shear is the more dominant term and using the surface value will deepen boundary layers, but it would be interesting to see clear evidence of this. Again, this is a possible place where you could make broader impacts.

To help clarify and grasp the broad points of the paper, I would suggest perhaps having bullet point take summary somewhere (perhaps the discussion/conclusions) with call outs to key figures. A few other suggestions to help with clarity: (1) a table of differences in KPP vs PP and in CVMix / FESOM versions would help maintain clarity (2) a table of the FESOM KPP configuration, e.g. is matching utilized?

Minor comments

Throughout, you write CVMIX, the acronym is CVMix.

In numerous places you have things like "southern hemisphere September" which reads odd to me. I'd suggest parenthesis around the month.

There are also a number of references that need proofing, e.g. Griffis 2015 and Ilicack2006.

There are also many places with subscripts that didn't typeset correctly.

Line specific Comments

L27 - Delete "The"

L29 - suggest adding "southern hemisphere" to "sea ice melt season mixing"

L40 and L41, remove "one" after "first" and "second"

L50-57 - by the word "embedded" I expected the sea ice code to be in the ocean code as in MOM6/SIS, is this the case for FESOM? It is not clear. It may be better to say "non-levitating" or "pressure exerting" for clarity, but no need to change the word if clearly defined if the ice code is in the ocean model or uses a coupler.

L57 - you mention that you must compute sea ice at every ocean time step to "embed", this doesn't seem desirable and is not actually required in our experience in MPAS/E3SM. Embedding is more dependent on the fidelity of the ice-ocean coupling in our experience. Can you clarify what you mean by this statement?

L81 - It isn't clear to me what "prime vertical mixing" means, is this default?

L82 - the phrase "deliver a usable mixing scheme" is confusing to me. Do you have a meaning in mind?

L129 – delete the comma and which

L145 – It is unclear to me why Pacanowski and Gnanadesikan 98 is discussed. It seems FESOM uses Schepetkin 2003 instead.

L151-153 – Have you tested FESOM without the requirement that the bottom thickness be greater than $\frac{1}{2}$ the layer thickness? As an example, MPAS-O runs stably without this requirement, but I have not looked in depth at the possible biases that may exist even though it is stable. It could be interesting to further examine this choice.

L157 – vertice -> vertex

L165 – biasin -> bias in

Section 3.2 – In this section you discuss a dependence on sea ice thickness in configuration choices but then only present comparison to sea ice concentration. It would be helpful to plot thickness. This is also more consistent with what is actually measured by satellite.

L281 – while MOM6 does have a branch with CVMix the original implementation was designed to reproduce the POP formulation, so I would change MOM6 to POP

L315+ - the strong similarity between KPP and PP in the analysis was surprising to me. These schemes are quite different, especially in the near surface. Have you examined boundary layer differences? E.g. Mixed layer depth? I wonder if perhaps the similarities are due to the fields presented, you show averages over fairly thick layers and below the boundary layer I imagine FESOM uses the shear instability induced mixing of Large et al. 1994, it is possible your analysis is only highlighting more deep ocean impacts and the similarities in the LMD SI induced mixing and PP81 mixing make the results seem similar. A simple test would be comparison of MLD between the schemes.

L398 – ando -> and

L450-453 – Why not test different background options? Seems like a very easy test to do.

L486-488 – this sentence is very confusing to me. When you use 'except' but discuss freshening in one part and temperature in the other it doesn't read easy to me.

L527-529 – have you tested combinations of changes? it seems possible (perhaps likely) that some changes have nonlinear interactions and is not as simple as just adding biases.

L549-552 – any ideas why you see a large change in the gulf stream for the MOMIX + KPP? Is this related to changes in transport (AMOC maybe?)

L582-583 – As an MPAS-O developer I confess I agree with your statement here, I'm always deeply impressed by the pace and quality of FESOM developments.

L701-702 – add commas around "and to a decrease in the high-latitude"

Fig 14 and 18 – the plot titles seem wrong in most panels here. Also in panel (c) of both there is an odd high salinity bias 40N. It is interesting it is identical in both Fig 14 and 18. Is this a plotting or analysis artifact?