REVIEW #1

"Assessment of the Finite VolumE Sea Ice Ocean Model (FESOM2.0), Part II: Partial bottom cells, embedded sea ice and vertical mixing library CVMIX" by Scholz et al., 2021.

The presented manuscript describes and evaluates the application of the recently developed unstructured-mesh Finite-volumE Sea ice-Ocean Model version 2.0 (FESOM2.0). The current state of the art FESOM2.0 model include multiple options and the authors analyze the impact of the partial cell, embedded sea ice-ocean model coupling and several mixing parameterizations from the Community Vertical Mixing (CVMIX) project library on the performance of the coarse resolution global sea ice FESOM2.0 model.

During the last decades the FESOM model was actively developed and applied for sea ice modeling in multiple publications. Therefore, the detailed analysis of different FESOM2.0 options is a necessary step and will be extremely useful for potential FESOM users. The manuscript is relatively well written and provides many Figures which illustrate the impact of different options on the model solution. Meanwhile, I find that the authors provide only "technical and qualitative comparison" and that makes this manuscript to be very similar to a technical report but not a research paper.

Thus, I definitely may recommend the manuscript for publication in the Geoscientific Model Development Journal, but recommend adding some *quantitative* comparison between different FESOM2.0 options and provide at least a *qualitative physical explanation* of the revealed differences. This is especially important towards understanding the impact of the partial cells and embedded sea ice options.

Below, I provide my comments and remarks which I would suggest should be taken into account.

We thank the reviewer for his efforts and constructive comments. We tried to thoroughly include all of his comments or answer his concerns.

<u>**Line 54-55:**</u> - *"Embedded Sea ice … "*

As I remember, the first understanding of the importance of the Embedded Sea ice was provided by Hibler et al., 1998, and after that, was used in several publications (e.g. Hutchings et al, 2005). I guess Hibler's embedded ice models are different from FESOM2.0, but these publications are also related to the embedded sea ice and should be at least cited.

We thank the reviewer for the hint to the publications of Hibler et al. 1998 and Hutchings et al. 2005 and will cite them in the manuscript.

__Line 57:__ *"implementation of embedded sea ice relies on the zstar vertical-coordinate option in FESOM2 and also on the fact that the sea ice component is called on each time step of the ocean model"*

As I understand FESOM2 uses an EVP solver (or its modification) and this suggests the application of two time steps. Which time steps do you mean for the ocean and sea ice models? Also, the modern sea ice model (e.g. CICE6, MIT ocean model) typically includes explicit and implicit solvers: EVP, different VP solvers (GMRES, Newtonian..)? Do you plan to include an implicit VP solver? If so, it could be expensive to use a VP solver for each time step of the ocean model.

We refer here to the time step of the ocean model, not the sub cycled time steps of the sea ice model. The shown model results use the standard EVP method of Hunke and Dukowicz, 1997 using N_{EVP}=150 subcycles. We will consider using a VP solver, but only if we manage to make it as efficient as the EVP solver.

__Line 151:__ *"we limited the thickness of the partial bottom cell to be at least half of the full cell layer thickness to reduce the possibility of violating the vertical Courant–Friedrichs–Lewy (CFL) criterion."*

It looks like a strong limitation: usually the CFL is defined by dz near the surface, so it looks strange to not allow the near bottom dz to be about 0.25 from the full dz. Did you observe any instability in the FESOM numerical scheme?

The limit of 0.5 was chosen for two reasons: 1st., we wanted to prevent the bottom layer in shallow shelf areas from becoming too small especially in the vicinity of strong boundary currents. It has been found that these regions lead to instabilities of CFL type, which reduce the throughput of the model. Second, in the abyssal ocean we were willing to limit pressure gradient errors. The threshold of 0.5 helps to achieve this. Nevertheless, this limit is a parameter in the model that users can specify depending on their application.

__Lines 159-184:__ **"***In order to demonstrate the effect of the partial cells on the simulated ocean state we performed two model simulations using the full cell and partial cell approaches, respectively. We investigate, first, the temperature biases of the full cell approach with respect to the data of the World Ocean Atlas 2018 (WOA18, Locarnini et al., 2018; Zweng et al., 2018, in the left column of Fig. 1) and, second, the temperature differences between partial cell and full cell (partial-full) averaged over five different depth* *ranges 0-250m, 250-500m, 500-1000m, 1000-2000m and 2000-4000m (in the right column of Fig. 1)"*

When you compare the FESOM2 performance in this (or similar) figure, you provide only a qualitative comparison ,which sometimes looks subjective. You need to provide some quantitative criteria (e.g. STD from WOA) for each (0-250m, 250-500m, 500-1000m, 1000-2000m and 2000-4000m) layer and include these numbers into the Figures. This will provide real estimates of the positive impact of the partial cells and/or other FESOM2 options.

The problem here is that improvements due to partial cells or also other options are mostly local (e.g. limited to the regions of zonal fronts), whereas other regions also can show an increase in bias. Therefore improvements in STD or RMSE on a global scale due to partial cells are rather marginal or even not visible. However we could provide STD or RMSE estimates on a more regional scale e. g. for the North Atlantic (-80<lon<5, 35<lat<70) Gulf Stream and North Atlantic Current region.

North Atlantic (-80<lon<5, 35<lat<70)	***STD (respect to WOA18)***		***RMSE (respect to WOA18)***	
	PC:0	***PC:1***	***PC:0***	***PC:1***
0-250m	***1.42***	***1.35***	***1.27***	***1.19***
250-500m	***1.31***	***1.28***	***1.18***	***1.12***
500-1000m	***0.84***	***0.82***	***0.75***	***0.71***
1000-2000m	***0.59***	***0.61***	***0.53***	***0.56***
2000-4000m	***0.48***	***0.50***	***0.48***	***0.49***

There, it can be seen that for the upper and intermediate depth ranges in the North Atlantic partial cell leads to an improvement of the STD and RMSE, while the very deep depth ranges indicate a rather marginal increase in STD and RMSE when using partial cells. In the revised manuscript we provide this table in supplementary material.

Lines: 168-174 + *Figure 1*

This is a very interesting figure, could you please explain the intensive zonal features in Full Cell cases? Note, sometime these features change the sign between 0-250m and 250-500m.

I see some “correlation” with figure 12, from Adcroft et al., 1997, so, some analysis would be very useful.

We are not able to answer this question with the model configuration used here. A simplified test case as the one in Adcroft et al. 1997 would be required for that. Also, the output frequency does not allow us to analyze the excitation of possible gravity waves on the topography steps.

Figure 4: Could you please provide the reference related to the intensive convection in the Weddell Sea and south from Greenland?

We will refer in the paper to the classical review paper of Marshall and Schott (1999), “open-ocean convection: observation, theory, and models“. Intensified deep convection south of Greenland is a known feature in modeled (Danabasoglu et al. 2014) but also observed mixed layer depth (de Boyer Montégut et al. 2004). Mixed layer depth in FESOM is in the range of other ocean models (see Danabasoglu et al. 2014). Intensive convection in the Weddell Sea is also a common feature especially in coarser ocean models (e.g. Sallée, J. B., et al. 2013) although here it is often overestimated due to an underestimation of summertime surface mixing in the southern ocean (Timmermann, R. and Beckmann, 2004).

Line 216-217: “*One can summarize that partial cells lead to a clear improvement of the circulation pattern, especially regarding the branch of the Gulf Stream and NAC even in rather coarse resolved configurations.*”

Where can I see that? Maybe an additional plot?

We originally wanted here to refer especially to the reduced zonallity of Gulf Stream and NAC when using partial cells. This can be seen in Fig. 1 in the negative temperature biases along the American east coast but also in Fig 3. in the norm of horizontal velocity profiles which indicates, especially for the upper two depth ranges, an enhanced meridionality at 30°W when using partial cells. We changed the text in the revised paper to avoid confusion.

Line 222: (*linfs*)

(linfs)-> LinFS?

We would like here to stick with the notation of the previous FESOM2 paper of Scholz, P. et al. 2019.

Line 234: *"… reality sea ice and ocean velocities are rarely identical especially in the presence of high frequency wind"*

Not accurate: even if wind is 0, there is a turning angle in the ocean forcing, so, ice will be flowing in a different direction.

we change the text to "...reality sea ice and ocean velocities are not identical." to avoid confusion.

Line 236: *variability especially near the ice-edge where ice divergence/convergence is large (Campin et al., 2008).*

Are you talking about Marginal Ice Zone (MIZ)? In the miz, the concentration is low and ice is especially thin. Not sure that may provide significant impact into the overall sea ice dynamics.

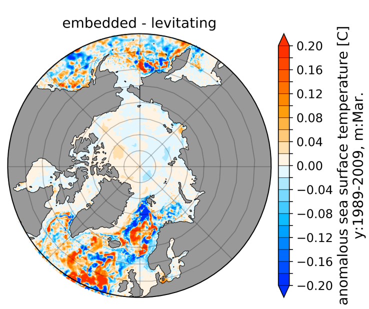
We refer here to the seasonal dynamically changing ice-edge which also includes the MIZ. Fig.8 showing the difference in the Arctic ocean circulation with and without embedded sea ice reveals quite some impact on the overall ocean dynamics.

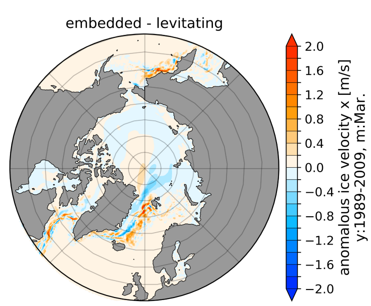
Line 247: Figure 6f.

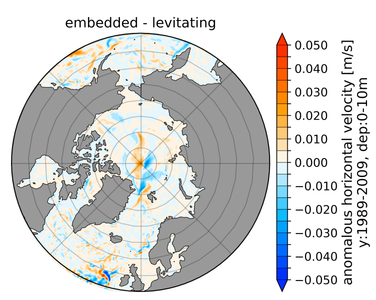
As I see, the embedded ice model provides less ice in the deep Beaufort Sea and more ice in the shallow ESS sea. Could you please provide any explanations?

The same for the Figure 6e – anomaly in the Greenland Sea in March. Why?

a) Greenland Sea March:

<table>
<tr>
<td>


</td>
<td>


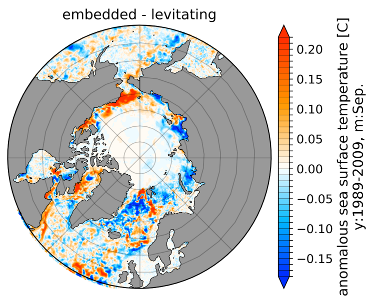
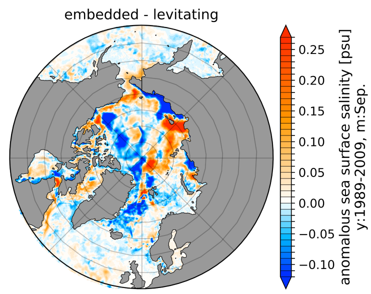
</td>
<td>


</td>
</tr>
<tr>
<td>anomalous sst</td>
<td>anomalous norm of ice velocity</td>
<td>anomalous norm of surface ocean velocity</td>
</tr>
</table>

It is difficult to analyse this from our data, since we did not save seasonal information for the 3d ocean variables of temperature, salinity, diffusivity and velocity. But from the data we have it looks like that more Sea ice in March in Greenland Sea when using embedded sea ice is related to colder surface ocean, slightly stronger sea ice export from fram strait along sea ice edge and stronger east greenland current out of the Fram Strait.

b) East Siberian Sea in September

For the dipole-like structure in September, with less ice in the deep Beaufort Sea and more ice in the shallow East Siberian Sea the message is less conclusive also due to the fact that we can not provide seasonal information for the 3d variables. The anomalous SST only tells us that there is a colder ocean in the East Siberian Sea and Laptev Sea that allows for more sea ice production and transport within the transpolar drift.

		
anomalous sst	anomalous sea surface salinity	

<u>Lines 261- 263</u>: *The temperature and salinity differences reveal that a significant warming of up to 0.5°C and a salinification of up to 0.10 psu occurs in almost the entire AO due to embedded sea ice, except in a thin stripe along the eastern continental shelf of the AO that shows negative anomalies in the depth ranges of 0-250 m, 250-500 m and 500-1000 m*

Question: I always suggested that embedded sea ice provides a stronger divergence/convergence due to stronger ocean forcing, and that the embedded sea ice should provide more “open” water or polynyas and that should cool the near surface layer. Am I wrong?

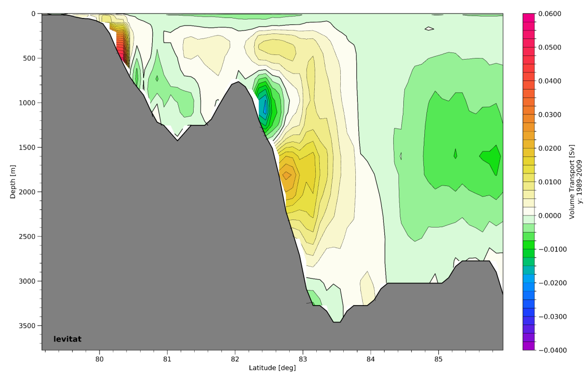
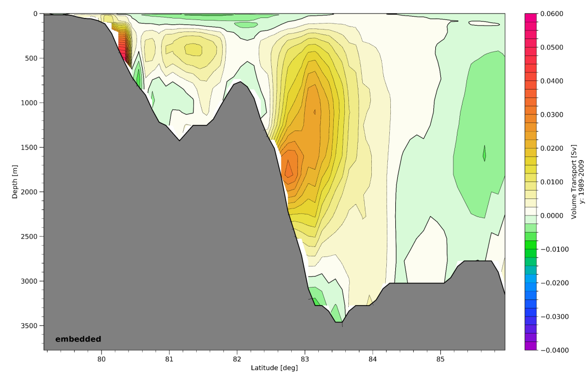
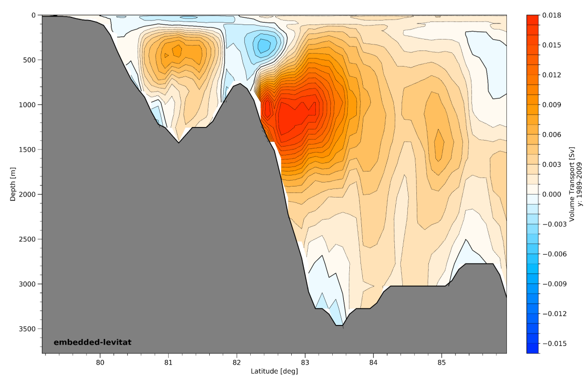
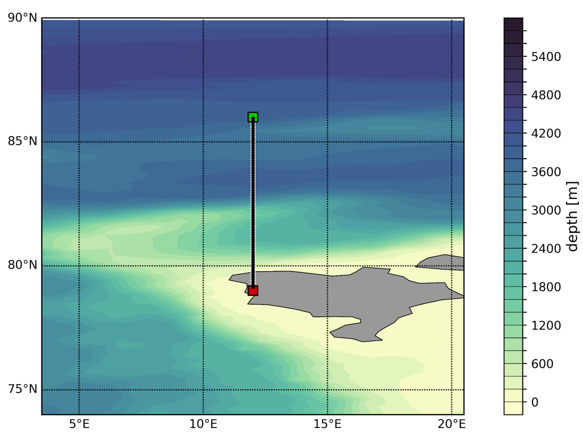
Did you estimate the “embedding” impact on the sea ice divergence/convergence?

In this part we did not focus on the effect of embedded sea-ice onto the sea ice itself, we focused on the effect for the ocean.

<u>Line 265-… :</u> *The changes in temperature and salinity can be explained by the changes in ocean currents.*

Do I understand this correctly: you explain the temperature/salinity increase due to stronger inflow of the Atlantic water into the AO? If so, you need to estimate this inflow through the Fram strait and provide the numbers.

The inflow current into the AO gets up to 4 times stronger in a depth range of around 500m and up to 2 times stronger in a depth range of 2000m when using embedded sea ice. We will include these numbers in the revised manuscript.

<table>
<tr><td>
</td><td>
</td></tr>
<tr><td>Transport in Sv through section for levitating sea ice (positive eastward transport)</td><td>embedded sea ice</td></tr>
<tr><td>
</td><td>
</td></tr>
<tr><td>anomalous volume transport through section (embedded-livitating)</td><td>section position</td></tr>
</table>

<u>Figure 7:</u>

You analyze the impact of the embedded sea ice in the AO and Southern Ocean. Why do you need the area between 60S and 60N? It is hard to see any features in the AO. Could you please re-plot this figure and remove the area 60S-60N and increase the AO /Antarctic region?

We agree with the reviewer and will replace the figures with a north polar projection from 30°N-90°N. We do not show the Southern Ocean since the

effect of embedded sea ice onto the SO is marginal, which will be stated in the text.

Line 393: "… all topographic features which is induced by the tidal vertical mixing parameterization"

Which topography features? What does it mean ALL? Besides, you may mention the increase of diffusivity mixing in the Indonesian region where internal waves are well-known features of the local dynamics.

We referred here to the difference in vertical Diffusivity of cvmix_KPP with tidal mixing minus without. We wanted to highlight here that due to the tidal mixing parameterisation of Simmons et al 2004, the vertical diffusivity is increased along the sloping bottom topography like the Midatlantic Ridge or the enhanced mixing in the Indonesian region but also along the continental shelf regions. We will try to better clarify this statement in the text.

Figure 14:

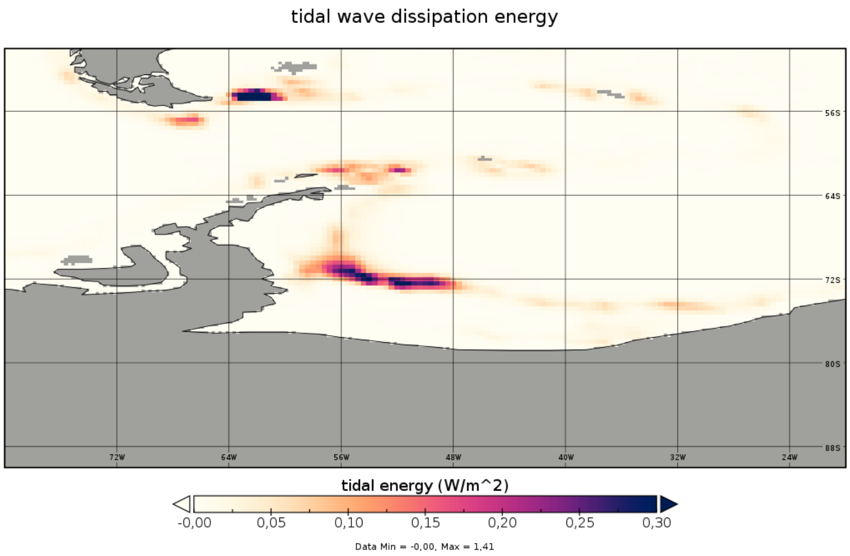
Something wrong with capture… Check it.

We will try to reformulate this caption.

Figure 15:

I see a strong increase in Weddell Sea in the Antarctica> ? Any evidence /references that tides are active in this area?

We show here the tidal energy dissipation flux due to bottom drag and energy conversion into internal waves from Jayne and St. Laurent 2001 in the Weddell Sea which also serves as an input parameter for the tidal mixing of Simmons et al. 2004. This clearly shows that there is tidal activity along the continental slope of the Weddell Sea. Also see A. Foldvik et al. (1990), "The tides of the southern Weddell Sea", https://doi.org/10.1016/0198-0149(90)90047-Y



<u>**Lines 437-439**</u>:

Discussion of figure 18 should be NOT be before the discussion of figure 17.!

Change the order of the figures.

There is a typo here, it must be: ... A closer inspection of temperature and salinity differences between cvmix_TKE and fesom_KPP (Fig. ~~18~~16, 2nd and 4th column) reveals that cvmix_TKE produces an up to 0.5°C colder ocean...

<u>**Line 477 + Figure 17:**</u>

Figure 17 should be Figure 18. See above.

See our reply to the last comment..

<u>**For all figures:**</u>

1)How is it possible that the coastline for 2000-4000m is the same as for 0.50m? Strange.

We always plotted the surface coastline for orientation, the bottom topography for 2000-4000m is left as white (as seen in the absolute Kv plotts), unfortunately white is also part of some of the colormaps which makes the bottom not very well visible. We will try to highlight the bottom topography with a lighter gray to better highlight it.

2) As I mentioned above, the disruption of these figures require some numbers which will allow you to estimate the difference between different mixing and numerical schemes utilized in FESOM2.0. It is reasonable to provide these quantitative differences for each layer.

As mentioned above it is difficult to pinpoint the improvement at a single global number, since the improvements are mostly regional where in other regions biases can even grow. Nevertheless we will try to include some numbers for RMSE either on a global or regional scale.

3) Physical explanations of the observed differences between different schemes/options is necessary, as well, in most of the cases.

A thoroughly physical explanation of all the observed differences in all the schemes and options might exceed the evaluative and descriptive character of this publication. For some of the observed differences, dedicated studies with more process-orientated sensitivity simulations would be necessary.

References:

Hibler W., P. Heil, and V. I. Lytle, 1998: On simulating high-frequency variability in Antarctic sea-ice dynamics models. Ann. Glaciol., 27, 443–448

Hutchings et al, 2005, Modeling Linear Kinematic Features in Sea Ice, Mon. Wea Rev, 3481-3497, 2005.