Review of the first revision of "The tidal effects in the FInite-volumeE Sea ice-Ocean Model (FESOM2.1): a comparison between parameterised tidal mixing and explicit tidal forcing" by Song et al

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There are some significant improvements in the revised manuscript, but the main problem remains. Over most of the global ocean, the resolution of the model is insufficient to resolve the internal tides. It is therefore obvious from the beginning that the simulation LSTIDE will not capture the increased vertical mixing caused by breaking internal tides. Hence, it is pointless to compare it to CVTIDE, which is constructed to capture precisely this effect. I therefore do not recommend publication.

Nevertheless, there are some interesting results, in particular the strengthened upwelling in the north Pacific in LSTIDE, and its effect on the global overturning and the hydrography. In the new version it is shown that this is not caused by increased vertical diffusivity K_v, but by increased resolved vertical buoyancy flux $\overline{w'b'}$ in the north Pacific. However, it is still not clear how the tidal motion leads to this buoyancy flux. It is speculated that it is caused by trapped tidal waves at the Kuril-Aleutian Ridge, but no specific support for this is presented. For example, an alternative hypothesis is that propagating internal tides with semidiurnal frequency are involved, since they are in fact resolved in at least part of this region, according to Fig. 15. It should be easy to check this by studying the tidal motion in detail, since the trapped internal tides have diurnal frequency. But even if it shown what kind of tidal waves are involved, it remains to understand why whey increase the vertical buoyancy flux.

A manuscript concentrating on these aspects might be publishable. The simulation CVTIDE should then be omitted. It cannot be expected to capture the effect of trapped waves, since its parameterization of the vertical diffusivity is constructed from the scaling of the generation of propagating internal tides.

Such a manuscript would be so vastly different from the present one (with a very different title) that it should be considered as a new submission, not a revised one.

Detailed comments:

1. The language is sometimes incorrect, and needs to be checked. Some corrections are listed below.

2. There are many lengthy descriptions of figures that add little to what the reader can see for himself. Such descriptions are boring to read, and should be shortened strongly. There are also too many figures. Concentrate on the features that have a clear connection to the main conclusions!

3. Page 5, line 128: 'rate of dissipated mechanical energy' should be 'fraction of dissipated mechanical energy'.

4. Page 5, line 144: 'consists with the Prandtl number' should be 'agrees with the Prandtl number'.

5. Page 7, line 207: 'temperature biases' should be 'temperature differences'.

6. Page 9, line 265: 'the depth where the density over depth differs by 0.125 sigma units'. I don't understand this.

7. Page 9, line 279: 'enhanced for 4 Sv' shuld be 'enhanced by 4 Sv'.

8. Page 9, line 281: 'upwelling for 5 Sv' shuld be 'upwelling by 5 Sv'.

9. Page 10, line 308: 'surface pressure power' is better described as 'conversion from kinetic energy to barotropic potential energy'.

10. Page 10, line 310: 'the horizontal distributions show differences', and Page 12, line 356, 'higher buoyancy flux than NOTIDE': According to Fig. 14 h,i the differences are a factor 10^{-6} smaller than the values for NOTIDE, and hence insignificant.

11. Page 12, line 359 and Fig. 16: Since the buoyancy flux has different sign in different locations, the 'equivalent diffusivity' is negative in many places. Therefore, the interpretation of the buoyancy flux in terms of equivalent diffusivity does not make sense. However, this

interpretation is not needed, and can simply be omitted. What matters for the overturning is the diapycnal buoyancy flux, and the importance of the diffusivity is just that it causes such a flux.

12. Page 12, line 367: 'Trapped internal tides ... can be simulated with LSTIDE'. Check this! Trapped waves have a well defined spatial scale that can be compared to the model resolution, thus extending the analysis in Fig. 15 to the gray areas.

13. Page 13, lines 391-392: 'the upwelling in the North Pacific and Indonesian Archipelago links to the North Atlantic'. I don't think that this has been shown clearly, even though I agree that the alternative explanation by the Atlantic mixing can be ruled out. If there is such a link, why does it not hold in CVTIDE?

14. Page 13, line 395: 'the barotropic streamfunction has an offset effect in the vertical direction'. I don't understand what is meant by this.

15. Page 16, lines 481-482: The text seems to indicate that the increased ITF is caused by the increased upwelling in the north Pacific. A similar impression is given on page 12, line 362. However, the connection has not been shown. Perhaps the increased ITF is mainly caused by increased buoyancy flux and upwelling in the Indonesian Archipelago?

16. Figure 12: The units of the color scales should be given. It is also very unclear to me what is meant by 'difference of scaled vertical diffusivity'. It is strange that the differences seem to be several orders of magnitude larger than the values in NOTIDE.

17. Figure 14: The units of the color scales should be given. The sign of the buoyancy flux seems mostly positive in the plots, which contradicts the negative value in Table 3. Does the viscous dissipation include both horizontal and vertical viscosity?

18. Figure 15: Give the explicit expression for the plotted ratio.