Reviewer Comments on:

Evaluation of a coupled ocean and sea-ice model (MOM6-NEP10k) over the Bering Sea and its sensitivity to turbulence decay scales
Author(s): Vivek Seelanki et al.
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This manuscript is focused on demonstrating the performance of an implementation of MOM 6 sea-ice ocean model in the Bering Sea. The authors provide a detailed comparison of the model with available observational data from 1993-2018. The manuscript is well written and organized. The figures are clean and easy to interpret. As a report on 'Development and deployment of MOM 6 in a regional configuration' the authors have done a good job, but as a manuscript on geophysical model development, it is not as clear. The principal model 'development' is an adjustment to the vertical mixing parameterization, but no physical justification for the change is provided. Numerous model/observation comparisons are performed, but for several of them where the model doesn't perform well, the authors just note that it is unresolved, rather than offer an explanation. I think for someone interested in using MOM6 in the Bering Sea with their configuration all the content is well presented, but a general reader would get frustrated particularly in the latter half of the manuscript, where a lot of detail is given mostly to say that in some places the model does quite well, in others it doesn't and we don't know why. In addition to my specific comments to address below, I would suggest the authors streamline the presentation by simply summarizing where more work is needed, maybe putting some of the figures that show unexplained, disfavorable comparisons in an appendix, (if they really want to keep them). But, I leave it to the editor's discretion to give guidance for this particular special issue, on how much of this content should be in the body of the paper.

Scientific questions/comments

As mentioned above, my principal concern with this paper relates to the tuning of vertical mixing to better reproduce stratification and bottom temperature on the Bering Sea shelf. The law-of-the-wall scaling that is used to specify the mixing length near the bottom has quite a robust basis in observation and theory, The authors really need to point to some physical process that could cause a factor of 5 deviation from this and offer some supporting evidence. Altering the slope of the profile of shear driven vertical mixing at the bottom boundary impacts the profile throughout the water column (Figure 3) so the authors need to justify the change over the whole profile.

A related question is, how does altering the vertical mixing scheme impact model performance beyond the eastern Bering Sea shelf? What impact does this have on the Aleutian throughflows or even along the US and Canadian NorthEast Pacific coasts? If the change only improves things on the Bering Sea shelf, is it sensible for the modified parameterization to vary spatially?

A second scientific concern I have relates to the discussion of circulation. The entire section only seems to say that currents qualitatively agree with what we expect to see. There is no comparison with observation so I think this should require only a sentence or two at most, rather than a whole subsection. Additionally I question the robustness of the Alaska Coastal Current on the Bering Sea Shelf. The model mean circulation shows no clearly identifiable currents flowing northward over the 50 and 100m isobath in the only figure in the paper related to currents (figure 2b), nor do I know of any recent observational datasets that clearly delineate such currents.

My third scientific concern relates to the sea ice concentration comparison with observations. The authors offer a detailed description of sea ice comparison at two mooring locations. But as there are no sea ice concentrations measured at those moorings, why not provide a more comprehensive spatial analysis of model error and biases over the domain? Does the behavior at the two moorings exemplify the range of variation found over the domain?

Particulars and clarifications (by line)

Line 30: More than 10% of the word's fish, half US.. etc. This statistic references publications from over a decade ago, is there a more recent assessment that confirms it is still the case?

Line 32: 37 million poinds Same reference as line 30?

Line 43: more oceanic in its vertical structure. What does 'more oceanic' mean?

Line 45 'but the domains exist…' I don't understand what this means.

Line 53 Western passes exit to the Pacific I think, not the Gulf of Alaska

Line 60 (Stabeno and Cheng, in prep) I would list this as 'personnel communication' rather than in prep., at least until their manuscript is submitted and accepted for review.

Line 93 A paragraph describing briefly (not necessarily comprehensively) what regional modeling has been done previously for the Bering Sea seems necessary in this section, so that the reader can put into context how this work adds to that.

Line 110 maybe mention the distance the not-open boundary is from Bering Strait to further justify why it's not of great concern for this study?

Line 137 How are GLOFAS river discharges implemented? Presumably they could provide a freshwater flux at every coastal point. But one might also only consider major rivers.

Line 143, The switch from ERA5 to JRA55 could use further explanation. What is it that makes JRA55 'specifically tailored' for driving ice-ocean models?

Line 143. Does Large and Yeager specify buoyancy and momentum fluxes over both water and sea ice?

Line 208 Provide a few more words describing the Jackson et al 2008 scheme. It's a scheme designed for climate models. Why is it deemed appropriate at these scales? Are other schemes available in MOM6? Were they tried?

Line 216 unnecessary sentence.

Line 227 Ric hasn't really been introduced. Presumably it's a parameter in the Jackson scheme. Why not decrease Ric?

Line 235 A zoom in to the pycnocline depth might be informative. Often the slope of the mixing coefficient profile here plays a critical role, even though the absolute magnitude of the coefficient is smaller. It matters more because the T and Z gradients(curvature) are larger.

Line 265. I can only discern a few data points above 13 deg..is this correct?

Line 315 clarify why those particular days in March were used for comparison. Were they the days of maximum sea ice extent?

Line 315. There are so many years squeeze onto this figure (and later ones) that it's hard to really distinguish the model and obs lines in panel b. Perhaps stretch over two rows split into first and second half of years modeled?

Line 352 the 'discrepancy' as determined by what measure?

Line 356 M8 simulations ?

Line 360. Wasn't 'older' ice in Hunke's paper referring to multiyear ice in the arctic up to 6 years old? Is this applicable for seasonal? Also a high ice year, doesn't necessarily mean an early ice onset year, so it's uncertain that the ice would be older.

Line 368 ...Cold Siberian Chukchi maybe related to lack of sea ice OBCs to flush sea ice?

Line 375. Discrepancies seem to suggest that the Yukon and Anadyr river outflows may be underestimated?

Line 391. Looks like a 'warm' bias south of the Yukon outflow, maybe clarify where is being referred to.

Line 455. If Observations are more well mixed, does that mean that the adjustment of Beta parameter makes the solution worse here?

Line 456 ..leftover comment from internal review probably

Line 483. Examples of specific times or places the model didn't do well but the authors don't know why. I don't know that such discussions provide the reader with anything compelling.

Line 501: A sentence fragment maybe meant for the conclusions? I'm not sure why it's here.

Line 508 Temperature plays ... a tautology?

Line 548- 552 This isn't really demonstrated anywhere in this paper.

Line 566 The mention of comparisons with Kearney appear a bit out of place in the conclusion. There weren't any quantitative comparisons with Kearney's results in the body of the manuscript to support these statements. Earlier biases in MOM6 freeze and melt were mentioned but I don't know how those compared quantitatively to the 2 weeks for Kearney referred to here. [

Line 576. There hasn't been any quantification of flow through Bering Strait vs. Local production, and presumably even if there were significant flow through the strait, the open boundaries may be far enough away not to matter.

Line 583: There are several differences between this and Drenkard, based on my reading here. Is it clear that the differences are more likely caused by the difference in atm forcing than by the difference in mixing parameterization (beta)? - or did I misunderstand and Drenkard used the same adjustment to Jackson's scheme?

Final comment: Careful review takes time. With the number of established authors on this manuscript I would expect that numerous issues I found in my reading should have been identified and addressed by the main author or their co-authors before submission.