

Geosci. Model Dev. Discuss., referee comment RC1
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Comment on gmd-2022-23

Anonymous Referee #1

Referee comment on "Implementation and evaluation of open boundary conditions for sea ice in a regional coupled ocean (ROMS) and sea ice (CICE) modeling system" by Pedro Duarte et al., Geosci. Model Dev. Discuss., <https://doi.org/10.5194/gmd-2022-23-RC1>, 2022

Duarte et al.: Implementation and evaluation of open boundary conditions for sea ice in a regional coupled ocean (ROMS 3.7) and sea ice (CICE 5.1.2) modelling system

The authors implement a new coupled configuration of the ROMS regional ocean model and CICE sea ice models for two regional modeling domains around the Barents Sea and Svalbard for operational (Barents-2.5km) and research (S4K) purposes. The authors present results showing that using time varying sea ice boundary conditions for the CICE sea ice model improve regional model results. It does appear that there are improvements to the model results with the time varying boundary conditions. However, the manuscript does not fully explain some of the model set up that would be necessary to replicate results, nor do they fully explain results and in some cases seem to make conclusions not supported by the figures. I think the authors need to expand their descriptions and discussions in the paper before the manuscript should be accepted.

Major concerns

- The authors do not provide some crucial details about the model assumptions:
 - Line 174: What slightly negative value did you use? -1.8C?
 - Line 175: What sort of interpolation? How did this work if there was snow?
 - Line 260: Did you test the mixing of initial conditions to see the impact on the solution?
 - Line 401: I disagree with the statement that the lower resolution model needs the doesn't necessarily need the same number of categories and layers, but you do need information to interpolate from the original to the new model.
- Impact of time varying Boundary Conditions:
 - Line 287/Figure 4: You state that the time varying boundary conditions lead to lower RMSE. However, in Fig. 4a it looks like the drop in RMSE happens BEFORE the

change in boundary conditions. What is going on here? It is also impossible in Fig 4b to see the difference in M1 and M2. Can you use different colors or shapes for the markers?

- Line 393: Is it possible that some errors are caused by the discontinuity at the boundary? Many models - e.g. WRF for the atmosphere ([https://doi.org/10.1175/1520-0477\(1997\)078%3C2599:ATOLBC%3E2.0.CO%3B2](https://doi.org/10.1175/1520-0477(1997)078%3C2599:ATOLBC%3E2.0.CO%3B2)) or RASM in the ocean (<https://doi.org/10.3189/2015AoG69A760>) – have some sort of buffer zone to help with spurious boundary issues. Did you investigate this at all? Is it worth investigating?
- Line 320-322: Your description about the biases is missing a lot of the detail from Figure 5 and in some cases contradicts it. For example in Figure 5 shows that S4k biases are largest from the surface to 100m whereas your description states the opposite. You also need to explain how the agreement shown in Figure 7 is better – larger range, warmer temperatures, fresher waters?
- Figures 8, 9, 10: Can you zoom in on the relevant domain rather than show pan-Arctic figures. It's hard to see anything meaningful in the full Arctic scale when the important details are at the transition/boundary of the regional model. Also, making difference figures might help as well to highlight the differences you describe because at present the differences are challenging to see. Adding arrows or other markers may help too.
- Figure 9: the boundary looks smoother, but why is there so much less ice in S4K (9d) than Topaz (9c)? This seems important and worth explaining. It seems the S4K model is performing worse with so little ice.
- Figure 11: c/d. Why do the frequencies for HEM/EM31/Magna vary between the columns? Are they on the same domain, because if so they should be identical frequencies. Is it possible to put all three PDFs on one figure for the same domain?

Minor concerns

- Figures 2 and 3: Please provide more descriptive, standalone captions.
- Introduction: You should probably address some of the points from this paper on whether sea ice models from climate models should be used for forecasting (<https://doi.org/1007/s40641-020-00162-y>). Also at line 386-388 you mention relevant operational reasons this model improvement has been useful. It seems this should be expanded on more in the introduction.
- Line 35: You should provide a DOI for whatever version of CICE you are using. See the table here: <https://github.com/CICE-Consortium/CICE/wiki/CICE-Release-Table>.
- Line 37: You may want to clarify that CICE6 and later have Icepack separate from the dynamics but CICE5 and any earlier versions they were combined. Use the specific version numbers here.
- Line 40: You should provide more information about other modeling systems that use CICE, even older versions, beyond the CMIP6. E.g. RASM (<https://doi.org/10.3189/2015AoG69A760>), CESM (<https://doi.org/1029/2019MS001916>), Canadian Operational Center (<https://doi.org/10.5194/gmd-2020-255>), Danish Meteorological Institute (<https://doi.org/10.1002/2017JC013481>), etc.
- Line 68: It would be helpful to have 1 sentence about TOPAZ4 and why it's optimal for this set up since not all readers will be familiar with it.
- Line 71: AMSR2 link doesn't work. Also maybe mentioning here that in section 2.4.1 you will describe how the satellite product at 6km is downscaled.
- Line 87: Is this run continuously or with restarts (as reanalyses are usually run)?
- Line 133-135: Do you have an estimate about the extra computational cost for using

more memory? 10% or 90% increase?

- Line 161: there are two periods at the end of the sentence.
- Line 166: use "internal" instead of "inner"
- Line 294: Remove "likely" as you have not yet shown this to be the case. The sentences below do show this, but at this point it seems unproven.
- Line 406: There is no figure 10e.