

Geosci. Model Dev. Discuss., referee comment RC2
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Comment on gmd-2022-23

Anonymous Referee #2

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-RC2>, 2022

General comment

Comments to this manuscript is based on the original manuscript, thus there may be some changes according to the review by the first reviewer that I have missed. I have tried to avoid repeating the same points. The manuscript describes a new nested modeling system for ROMS and CICE. Resolution is important and computational expensive, thus nesting of high resolution areas into large scale models with higher resolution is of interest as well as a nesting capability within CICE. That being said I agree with reviewer 1 that some work still needs to be done in order to accept this manuscript both on the structure of the paper and on the My comments follows below:

The "ideal" boundary conditions are mentioned. Correct these are not available from remotely sensed data. Most likely this is not the case for any model output from a "none in-house" model system either. For instance Topaz4 only deliver ice concentration/ice thickness (and maybe a few more parameters) if downloaded from Copernicus. Some estimates have to be made here which also applies when assimilating data. This discussion and maybe also the result of "bad choices" should be at least mentioned and maybe discussed. In addition what would be needed in order to expand this to be a "cheap" assimilation scheme

Some of the conclusion would have been easier to diagnose if the model systems were run at the same time and not in different time periods.

Mayor

I would focus the abstract more on the results and challenges instead of listing all boundary condition parameters used and which models are used for boundary conditions.

Line 34 – 41 I think that this belongs to a model description, which makes the introduction very short. I would like to see a section of other attempts to use ice models in a nested configuration. Both CICE and other models.

More complex boundary conditions are mentioned. Have you considered the effect of relaxing towards a value in the halo zone only compared to relaxing in a larger area or a flux representation? This could be brought up in the discussion just as the “perfect” availability of data is.

The location of the N-ICE cruise would be nice. Maybe in figure 1.

I would maybe skip one of the figures 9 or 10 as the information is somewhat redundant.

S4K vs TOPAZ ice thickness. Could you elaborate a bit on why you think that S4k has a better ice cover (atm forcing, higher resolution ocean or?)

Minor

Line 19: “sea ice size categories”. I would change to “sea ice thickness categories”

Line 27 Norwegian Meteorological Institute shortname is MET here and Met Norway a few lines down. This should be consistent.

Line 50 please reformulate “We have chosen to use these two models because, whereas...”

Line 62 “(for details on coupling refer 2.1.3)” add “to” after refer

Line 63 vertical coordinate following the bathymetry = terrain following?

Line 69 TPXO reference, which version and reference? The same is the case for nve data (mentioned further down) and IBCAO. Reference should be mentioned first time they are used. There are likely more that

Line 79 S4K is 4km in resolution. What is the resolution of the A4 model. 4 as well? If this is the case what is the motivation for nesting? Is A4 only ocean or what is the reason for not using this for both ocean and sea ice conditions

Line 87. Why use ocean boundary conditions from A4 and ice from TOPAZ. This seems to be a place where inconsistencies can be found.

Line 170: This assumption will influence the result and the strength of the ice (depending on the choice of kstrength)

Line 184 depth of layer ... Should this have been the fractional depth of each category?

Line 192 Microwave -> Passive Microwave

Line 193: The ice charts are drawn manually as polygons. The resolution is the gridding resolution not the actual resolution.

Line 320 ocean results

Is the improved results due to the A4 ocean boundaries, the ice boundaries or? It would have been easier to distinguish if Topaz4 were used all the way through. Line 320 states that the bias is larger for depths between 100 and 300m. It does not say compared to what. I assume that this is other depths.

On a general note I would say that three to four lines of text is very short for 3 big figures. I would evaluate some more on this result or leave it out and maybe add it to supplementary work.

Line 330 (and 407) Any suggestion why the ice has a larger extent in S4K? I think that the assimilation is probably the cause as mention, however the S4K should have a resolution that improves the front. Maybe fresh water

Table 2: I would put this in an appendix.

Figure 3 b and c. I am not entirely sure based on the text. Was figure 3.b just after starting from Topaz4 or did it run for a while and then it is just "luck" that they start so close to each other in figure 3b. Furthermore line 305 (figure 3 caption should be updated to four instead of three).

Line 394 refer 2.3.2 . Is this in this manuscript?

Figure 4 (a)

I agree with the comments from reviewer 1 here. Another thing that puzzles me is why does the RMSE for the AMSR2 drop at the green line and start to increase again in winter time?

Figure 11. Header of figure a,b and c,d seems to be swapped unless it is the caption text that is swapped..