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Geoscientific
Model Development
Discussions

Interactive comment on "Sea-ice evaluation of NEMO-Nordic 1.0: a NEMO–LIM3.6 based ocean–sea ice model setup for the North Sea and Baltic Sea" *by* Per Pemberton et al.

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The paper is based on good and original material, and deserved to be published. Writing is generally good. I found the paper material quite interesting.

Yet I believe it should be improved, following two categories of objections.

1) The conclusions could be more general and interesting.

2) The analysis of results could be more acute and precise

Besides, some sections (3.1, 3.4) are not fully clear and could be sharpened.

I think the manuscript can easily be improved, and I hope my comments will help.

*** General comment #1. The conclusions could be more generic.

I would somehow use the results of the paper paper to question the capabilities of regional modelling ice systems forced by atmospheric reanalysis, considering Baltic Sea ice as a successful example.

The following core of conclusions could be the base of the abstract/

Conclusion 1: The NEMO-Nordic ice modelling system is appropriate to get the mean extent, volume, and geographical distributions of ice concentration and thickness in the Baltic Sea, which all seem rather precisely captured (within 10% of obs?). The ice melts early, which is attributed to XXX.

Conclusion 2: Extreme years, in particular severe winters, are more difficult to simulate.

Conclusion 3: The subgrid scale ice thickness distribution seems challenging.

*** General comment #2. The analysis of results could be more acute and precise

- The SST bias should be analyzed with respect to ice presence or absence. In presence of ice, the SST must be very very close to the freezing point, hence any SST bias has to be attributed to SSS. The warm bias in April is very likely due the early ice retreat, which was barely mentioned in the text.

- Why ice melts too early is not clearly attributed. There are admittedly bits and pieces, but the analysis could be more systematic (air temperature, snow depth, incident solar radiation, surface albedo). The ice thickness bias is neither clearly quantified.

- The ice thickness distribution analysis would be more conclusive if (i) the model ice categories were used for both observations and model, (ii) the exact same time and locations were used to construct the pdf. At this stage, the model looks really bad, but this could be because different ice categories are used.

- The comparison of snow at two stations is not enough to conclude, as blowing snow effects can be locally dominant. As the present analysis is not meaningful, you could remove it and just mention it as being inconclusive. Snow depth could be critical and I'm surprised there are not more snow depth observations to compare with, in such a well studied coastal sea. If available, a more systematic snow depth analysis would be a real plus.

- I think the FDD-model analysis is confusing and adds unnecessary complexity to the paper. I would recommend to compare the winter air temperature bias instead, that would be simpler and actually equivalent.

- The effect of fast ice parameterization is claimed to improve the results, but this is not supported by material. Therefore, I would recommend to be more explicit.

- The analysis of extreme years is quite interesting. It should be stated or shown whether the analysis applies to all extreme ice years. Do all severe years follow a similar ice thickness pattern? Are all mild years realistically captured. In addition, whether forcing or model are responsible should be at least discussed.

- Be more quantitative in the text in general (give numbers instead of "reasonable" or "quite good")

- Revise your conclusions once analysis has been sharpened.

*** Detailed comments

- Introduction can be sharper and a better selection of the required elements could be done.

- p4, l.5 "include"D"

- p4. l.17 give reference, because 0.17m is not the common value in LIM3. In practice, you removed rafting from the model. Why did you do that ?

- p.4 your fast ice parameterization is grid-size dependent because your criterion is based on cubic meter. Why did not you use a volume of ice per grid cell area criterion ?

- p.5 your mean thickness is now referred to as volume per unit area (Notz et al, TC 2016).

- p.5 line 14, why do you use 5 and not 4 in the denominator to compute your level ice thickness ?

- p.6 line 20. Could you describe in one sentence what is the observation based for ice charts ? satellite ? visual ? how was ice thickness quantified ?

- section 3.1 should probably be revised, I found it hard to follow (see general comment as well).

- p. 9, l. 9 "variability" -> "interannual variability". "quite well" -> be more quantitative. Are you sure the units of STD are correct ?

- p. 9 l. 13. What about the trend if you exclude the first 15 years ? May be use ice area to see if that is robust ?

- p.10 I can't reconcile the file that volume could be overestimated with the fact that the ice melts too early.

- p. 11, l 16 "consistS"

- section 3.4. If you keep this analysis, which I don't especially recommend, you may want to explain why you use different sites for FDD and for snow depth. I finally figured why, but it would have been good if you had said it right away.

- acknowledgements: acknowledge NEMO developers :-)

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