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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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Review of Pemberton et al.: "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 F. Massonnet

The study presents the sea-ice component of NEMO-Nordic, a new regional configuration based on of the European community ocean-sea ice model NEMO. The authors have adapted the Louvain-la-Neuve Ice Model LIM3 to match the specificities of the Baltic Sea, and run a control hindcast of 45 years (1961-2006). The model output is compared to data from ice charts. The model is found to be in overall good agree-

ment with observational data, but to underestimate the volume of level ice and the sea surface temperatures.

To my point of view the study is adapted for Geoscientific Model Development, as it presents the final product obtained after certainly a lot of developments (I acknowledge the work of creating a new configuration). On the scientific side, I find the paper very descriptive, and sometimes speculative, and would therefore welcome more discussion or experiments to understand the origins of specific results (I'm giving a few suggestions below)

I appreciate the effort to develop this configuration as part of a community effort (NEMO) and believe that the results are satisfactory enough to publish this study. However, I have two main points that I would like the authors to make clearer, possibly by running one or two additional short experiments:

1. By prescribing a constant sea ice salinity of 0.001 PSU in the model, the authors essentially switch off the halo-dynamics in the sea ice model. What is the reason that led them to that choice? Did the authors conduct tests with the full interactive halo-dynamics module of LIM active, and compared the results? I know that the Baltic sea is a particularly fresh sea, hence the ice is mostly non-salty, but why not try to let the model figure it out itself?

2. The authors diagnose the proportion of ridged ice as the ice concentration in the thickest category (p. 5, lines 16-21). The authors correctly write that this is an approximation. I'm wondering if the authors could go a bit more quantitative here. For example, by running the same simulation but deactivating the ridging/rafting scheme, they could measure the amount of thermodynamically-grown ice in the fifth category. If that amount is very small (say, < 5 %) then indeed that category in the reference run contains most of the deformed ice.

Other comments: - How did the authors come to the specific values of LIM for table 1? How did they tune this configuration? Providing a bit of background, with lessons learned, would be very helpful to those authors that may want to run their own configuration (e.g. in Hudson Bay).

- Given the high resolution of $\sim$4 km, the continuum hypothesis for the sea ice medium is not necessarily true. The (elastic) viscous-plastic rheology used in LIM3 heavily rests on that assumption, though. Did the authors experience numerical instabilities? Did they check the sea ice velocity/deformation fields? What would be their advice in terms of rheology at that high resolution?

- What are the ocean and sea ice model time steps?

- Fig. 12 suggests that the simulated volume overestimates the observational reference BASIS. However, as explained in the text, BASIS is essentially a measure of level ice volume. Given the fact that the atmospheric forcing is already cold, the authors conclude that the simulated volume is definitely too low. I have two questions here: 1) Are there uncertainties available around the BASIS estimates? 2) What does the mismatch in simulated and observed volume tell about the sea ice model biases? If I understand, this is more a thermodynamic issue since the deformed ice was not considered. How would the authors do to investigate the origin of this bias? Is it possibly related to the conductivity of snow for which could be significantly different from the one used for the Arctic? See also my next comment

- In the conclusion, the authors note that the cold bias in SST in the model is somehow contradictory with the negative bias in sea ice volume, and point towards the possible role of snow. I assume that the snow conductivity was set to its default value. However, is there a good reason to assume that snow conductivity in the Baltic Sea is the same as that of the polar regions?

Minor comments

p. 1, l. 16: "... the ice extent can reach a coverage of almost 100% during severe winters". Ice extent has usually units, which makes this sentence confusing. Consider

"ice coverage reaches 100% during severe winters"

p. 3, l. 19: To how many vertical levels does the configuration correspond? Please specify.

p. 4, l. 5: "include" –> "included"

p. 4, l. 8: "uses" –> use

p. 4., l. 26. I don't understand the sentence saying that the scale of most sea-ice models is $\sim$ 1 km. Most large-scale sea ice models currently run at $\sim$10 to $\sim$50 km, which is at least one order of magnitude larger than what is written.

p. 5, l. 27: "show" –> shows

p. 7, l. 25: An extra "the" is present.

Fig. 4. The color bar is misleading because "100% ice" and "no ice" have essentially the same colour, i.e. white. Consider a more adapter colormap.

Fig. 8a. The figure 8a seems to suggest that the sea ice model grows too much ice in the thin categories. Did the authors try to run sensitivity tests with difference ice thickness boundaries? It would be interesting to at least identify possible candidates to explain the striking differences seen in Fig. 8a. This is somehow done in the text (p. 10, first paragraph) but in a very descriptive and speculative way.

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