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Geoscientific Model Development Discussions

Interactive comment on "Multi-Scale Sea Ice Kinematics Modeling with a Grid Hierarchy in Community Earth System Model (version 1.2.1)" *by* Shiming Xu et al.

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General comments:

The authors present the results of one component of a new earth system, namely the sea-ice. They first introduced a new grid generation for a tripolar grid. Then, they show initial results of the sea-ice component under normal-year-forcing (a kind of climatological forcing with synoptic scales sampled from different years), including multi-scale analysis of the sea-ice deformation. Finally, they show the sensitivity of the dynamics of the model to a long-debated parameter in the dynamics, namely the number of subcycling in order that allows for artificially slow elastic waves in order to

solve for the viscous plastic equations. This is to my mind the most interesting bit.

The manuscript is well-written. The experiments are well described. The results are well presented and analyzed using existing diagnostic tools.

1-One comment about the grid generation, it looks very similar to the ORCA grid we use in NEMO, but I see only a short mention in Appendix (p25) to Madec and Imbard (1996). It would be important to mention how your method differs and improves on existing ones, otherwise it sounds like you are reinventing the wheel. One additional suggestion would be give more context on why you are doing this.

2-One general concern would be that the experiments are done with climatological atmospheric forcing and slab ocean, which means that, while it helps standardizing the experimental framework, it is not necessarily realistic as it lacks the increasing spectrum at high wave numbers present in the atmospheric and oceanic fields as one increases the resolution of the model.It is not a major problem but it would be worth discussing.

3-One important metric which seems to be missing is the ice drift error (only Fig.6 shows it for two different dates). Given the experimental framework chosen here, would it possible to add one? It is important to support that overall sea-ice volume, ice export out of the Arctic ocean or the convergence in the Beaufort gyre are reasonably modelled.

4-Another is that as Lemieux et al. (2012, JCP) showed, one cannot claim a true convergence of the EVP solver (it does not convergence in the numerical sense). Please also add a discussion on this and define what you mean by "convergence".

5-Since the paper goes relatively in depth in analyzing sea-ice deformation –which is usually an prelude for intense discussion on rheology models– I recommend that you broaden a bit more your discussion at the end (p.22-23). For instance, about the effect of the form of the yield curve in viscous-plastic models, Bouchat and Tremblay (2017, JGR) for instance claims that decreasing the ice strengh and eccentricity improves their simulation (thickness, drift and deformation), while Ringeisen et al. (2018, cryosphere) claim that the angle in intersecting fractures from viscous plastic models is nowhere realistic...

Minor comments (given in the order of appearance in the text and figures):

1-any other changes to the ice physics except for ndte? There is only a reference to CESM D-type experiments and a short list of default schemes (p3 line 25 to p7 line 14), but it would be good that those are listed somewhere with chosen parameters.

2-line 14: operational forecast BASED on Dupont et al. (2015), i.e. we were not reporting on the operational implementation in this paper but on the general long hindcast prior to it.

3-line 22: main driver OF

4-line 32: NCEP CORE: requires a reference and likely mislabed (CORE 2 might be more accurate)

5-page 5, line 4-5 (and last column of table 2), giving the number of subcycling per hour is misleading as in CICE the restoring time for the elastic waves is function anyway of the larger transport+thermodynamic timestep. I see no argument in reporting this (#/hour) except artificially increasing the cycling number when resolution is below 1 degree. Table 2 in fact shows that you did not go above 1000 cycles per timestep. Please remove.

6-page 5, line 12, "ocean status" might be "ocean processes"?

7-Fig.5a: missing what years are used in the NSIDC climatology.for the comparison

8-the text refers to PIOMAS but Fig.5b does not show the comparison. Can you add it please?

9-p11 line 1, Nice analysis of NYF Artic Oscillation. I was always concerned of a

particular bias with repeatingly using NYF. So at least the winter wind pattern is mildly neutral. What about summer though? Can we say it is also neutral?

10-p13, line 17 "geostrophic" is mispelled

11-Fig.7: TS005 appears too smooth in the central Arctic (this is also noted in the text). Could it be an issue with the "convergence"?

12-Fig.9 has an unclear color key (it seems to be function of the run and spatial filter) and Fig.10: is missing one altogether. Please elaborate so that the figure is self-readable.

13-Fig.11: values are getting noisy past 1e-1 for TS045, sounds like a lack of resolution compared to TS015 and TS005.

14-Fig.13: interesting that ndte has such an impact on thickness even for the lowest resolution (TS045), whereas pattern of deformation are equivalent (top row of Fig.12). [please check that the top row is indeed showing different ndte results!]. I suspect this is because most of the changes in thickness are insise the Canadian Arctic Archipelago (CAA) where the deformation is not plotted. I suspect that the ice in the CAA is referred as "landfast", but it would be nice to have a more rigorous definition (is it in terms of some velocity threshold?).

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