The authors would like to thank Dr. Frederic Dupont (the referee) for the invaluable comments. The following are the replies for each comment, together with specific revisions that are made. The original comments are in *green italic* font, and the revisions highlighted in the revised manuscript in cyan.

**Reply to comments:**

*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.*

**Reply to the general comment (1)**: the authors fully acknowledge the pioneering work on ocean model grid generation in Madec & Imbard (1996), as well as the similarity with our study. Specifically, in Madec & Imbard (1996), the details of the grid generation method with embedded ellipses are only briefly mentioned by the end of the paper. The resulting grids are adopted further in NEMO with various grid resolutions in different applications.
However, there are three major reasons that we design and implement the grid generation method for our own use. First, we use CESM (v1.2.1) and its component models for all the experiments. Both POP and CICE use Arakawa-B staggering with a U-fold for the tripolar grid. On the other hand, as far as I know, NEMO uses Arakawa-C staggering with a T-fold tripolar grid. Although a seemingly minor issue, it does greatly affect all the numbering with many associated code changes to existing grid generation method.
Second, we want to have more flexible control of all the grid generation method and tool for the grid. The requirements include the control of grid cell size anisotropy in the polar regions, the grid size transition to the two grid poles on Eurasia and North American continents. For the TS grids in study we have adopted polynomial form (Fig. A1.b and Appendix A), but other options are possible, including exponential function and sinusoidal function. Different forms result in differences in grid scales in the Arctic region. Therefore we are yet to explore the various options of the grids, including resolution differences in CAA. Utilizing a fully-fledged grid generation toolkit enables aforementioned tasks.
Finally, the proposed tripolar grid generation method, including its implementation, serves as the basis for a comprehensive toolset for future development. In specific, we look forward to combining several grid generation methods into a complete toolset, including tripolar grids

(as in this study) and coastline/bathymetry-following techniques with complex conformal mappings (Xu et al., 2015).
In summary, we attain more flexibility with the proposed grid generation method for our ongoing model development. The similarity between Madec & Imbard (1996) and this study is that both adopt embedding ellipses, but the specific methods differs including in the construction of these ellipses as well as the numerical method to construct the grid lines.

*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.*

**Reply to the general comment (2)**: the authors would like to reemphasize that the purpose of using Normal-Year Forcing based experiments, as also stated by the referee, is to align the experiments across the different grids. The NYF dataset includes synoptic signals from the atmospheric reanalysis, but lacks inter-annual variability as well as long-term trend. Therefore, NYF is also used in Ocean Model Inter-comparison Project (OMIP) to study climatological and equilibrium status of the ocean-sea ice coupled system. In response, in Section 3.1 and 4, we add further discussion of the methodology of using NYF in our study, and potential extension to Inter-Annual Forcing (IAF) dataset in the future.

*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.*

**Reply to the general comment (3)**: regarding this comment, the authors have made the 2 revisions and an add-on experiment for the attribution of extra-thick ice in Beaufort Gyre. First, we have carried out comparison of Arctic basin sea ice drift for winter months. In specific, we have compared DJF sea ice drift from the model output against the climatological sea ice drift of NSIDC (corresponding months for 1980-2000). Second, we re-evaluate the modeling results against comparable sea ice volume by PIOMAS. The annual cycle of PIOMAS sea ice volume for the year from 1980 to 2000 is used to compute the climatology.
The add-on experiments mainly dealt with the sensitivity of modeled Arctic sea ice volume (especially in Beaufort Sea) to ice strength parameterization. In our experiments, as well as those in standard CESM settings, the ice strength parameterization follows Rothrock (1975) and Flato & Hilber (1995), instead of Hibler (1979) which is arguably more widely used. In specific, in the strength parameterization adopted by CESM (Lipscomb et al., 2007 JGR), the sea ice strength is assumed to be proportional to the change in ice potential energy with respect to compressive deformation, which in turn relates to the ridging (with exponential redistribution) and the specific ITD settings. For contrast, in Hibler (1979), the ice strength is directly computed with prognostic status including sea ice thickness (in linear relationship), concentration, as well as prescribed parameters of which Pstar is treated as a tunable constant. As a result of the choice of strength parameterization, the equilibrium sea ice thickness across the basin is considerably different, with thick sea ice in Beaufort Sea absent when using Hibler (1979). The figure below (Fig. 1) shows the comparison with TS045 grid.

Besides, we have also confirmed that this phenomenon is qualitatively similar with CESM's native grid (e.g., GX1V6, shown in Fig. 2), as well as TS015 (not shown). Given that Lipscomb et al., (2007, JGR) is used by default in scientifically checked cases of CESM relevant to our study, we consider this choice a reasonable choice for our experiments and analysis here. However, we consider this an important issue that should be further explored in future studies.

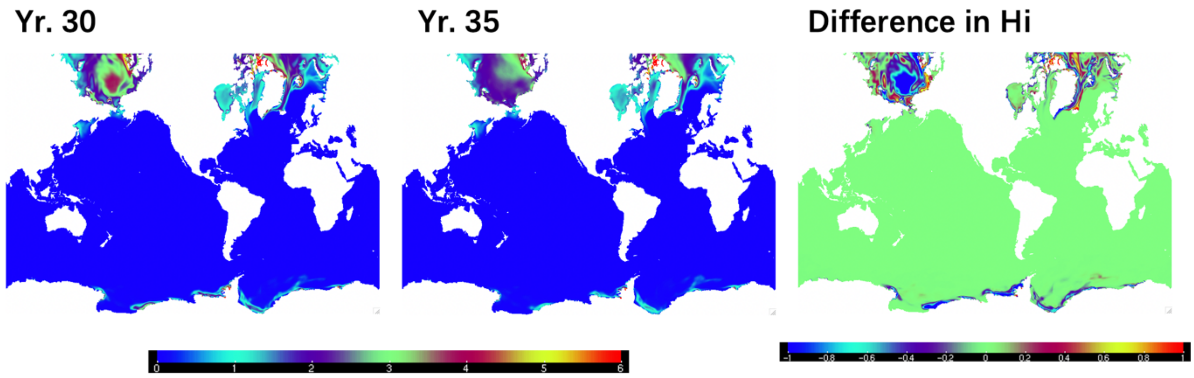


Fig. 1. March sea ice thickness difference for TS045 grid at equilibrium states under CESM D-type experiments. Left: Rothrock (1975) scheme; Middle: Hibler (1979) scheme; Right: difference (middle *minus* left). All values are in meters.

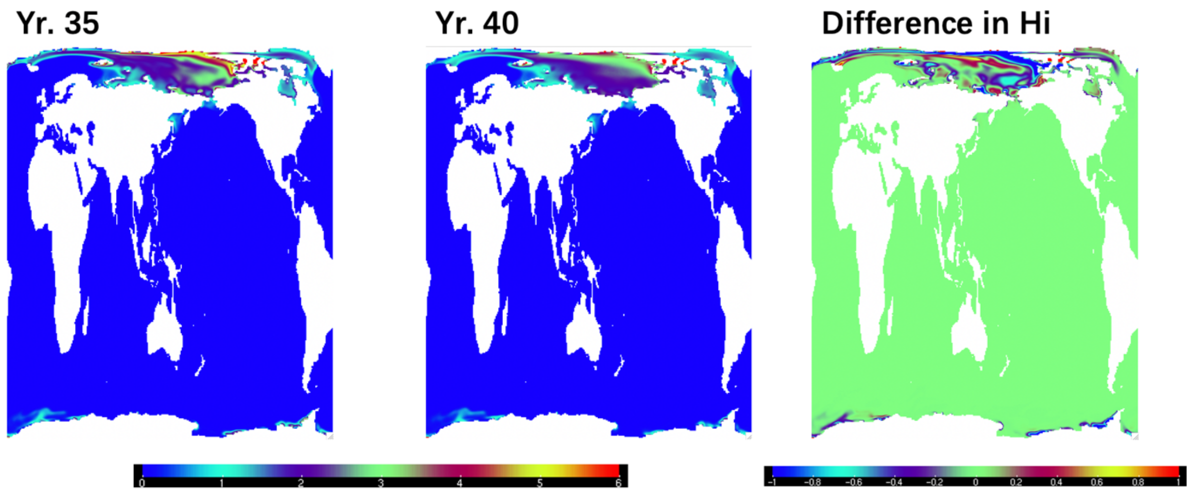


Fig. 2. March sea ice thickness difference for GX1V6 grid at equilibrium states under CESM D-type experiments. Left: Rothrock (1975) scheme; Middle: Hibler (1979) scheme; Right: difference (middle *minus* left). All values are in meters.

*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".*

**Reply to the general comment (4)**: the authors apologize for using "convergence" in a less strict sense. The convergence we mentioned for EVP is not in the strict numerical sense. Rather, there is asymptotic behavior of the sea ice deformation and kinematic features with respect to EVP subcycling count. Fig. 7, 8 and 11 show the asymptotic convergence. For example, the PDFs of the deformation rates (Fig. 11) attain a good match when the NDTE is large enough especially for TS045 and TS015. Also with larger NDTE values, the kinematics features become more refined in structure and match well when NDTE goes beyond a certain value (e.g., comparing NDTE=480 and NDTE=960 for TS045). Here we revise the manuscript of Sec. 3.3 by adding extra text to formally explain what we 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...*

**Reply to the general comment (5)**: following the referee’s comment, the authors have added extra discussion in Section 4 on rheology models and related dynamics-related parameterization schemes, especially regarding related works. In particular, we consider the ice strength parameterization a key discussion point here. As many works on tuning the model have adopted the scheme of Hibler (1979) [including Bouchat & Tremblay (2017) which is also mentioned by the referee], the most straightforward tuning parameters are Pstar and eccentricity of the yield ellipse. But for the scheme we have adopted, we have not observed much tuning work especially regarding basin-scale simulations. One particular parameter that can be subjected to tuning is the empirical parameter of C_f that is used to characterize frictional energy dissipation, which can be scale-dependent (details in CICE documents).

*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.*

**Reply**: we confirm that the thermodynamic parameterizations are kept the same across all 3 grids and different NDTE values. In our original design of the experiments, the parameters are kept the same in order to improve the comparability of the simulation results. To revise, we have now added a separate appendix (Appendix B) that includes all parameterization schemes and related parameters that are used in the experiments, including both thermodynamics and dynamics.

*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.*

**Reply**: the text is corrected to be more precise, as “the hindcast experiments for operational sea ice forecasts in Dupont et al. (2015)”.

*3-line 22: main driver OF*

**Reply**: corrected.

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

**Reply**: corrected to be CORE 2.

*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.*

**Reply**: according to the referee's suggestion, the authors have removed the last column which showed the total subcycling count per model hour. Besides, we confirm that the maximum subcycle count per timestep is 960, which is indeed under 1000 cycles.

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

**Reply**: revised as indicated, by changing "ocean status" into "ocean processes".

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

**Reply**: the climatological sea ice extent is computed as the annual cycle of monthly mean sea ice extent for the year from 1979 to 2000. The text is also revised to include this information.

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

**Reply**: the authors have added the climatological sea ice volume annual cycle from PIOMAS on Fig. 5.b. The same years (1979-2000) from PIOMAS are used to compute the annual cycle.

*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?*

**Reply**: we have extended the analysis of AO to the summer months of NYF dataset, which is now incorporated into Fig. S1. In general, the summertime AO index pf CORE2 NYF is also neutral.

*10-p13, line 17 "geostrophic" is mispelled*

**Reply**: corrected.

*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"?*

**Reply**: we do not consider the relative lower deformation rates in Fig. 7 an abnormal behavior of TS005. As sea ice thickness is generally thinner on the outer rim of the sea ice cover (i.e., marginal seas), the deformation rates tend to be higher as well (Kwok, J-Glaciol., 2010). In the central Arctic, for the two representative days, the seemingly smooth deformation field actually have certain shearing but low convergence/divergence (lower left panel of both Fig. 7 and 8). The convergence we mentioned is related to the concentration of deformation to local regions when NDTE is large enough.

*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 selfreadable.*

**Reply**: both Fig. 9 and Fig. 10 are revised to include more information of the scaling curves for different resolutions. Texts that correspond to these figures are also revised.

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

**Reply**: at the deformation rate of 10%/day for TS045 (shear, div, and total deformation) appear to be noisy, since with lower resolution of TS045, **we do not have sufficient samples** to compute the proper shape (i.e., tail) of the PDF. Therefore, we consider this a sampling issue, rather than the direct issue of the lack of resolution.

*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?).*

**Reply**: the difference in deformation rate between NDTE values for TS045 are mainly in the range of small values (less than 0.5%/day), as shown in the top row in Fig. 12. Note the color difference in CAA. As indicated by the referee, we revise the manuscript adopting the sea ice drift speed criterion of characterizing sea ice as landfast when the 2-week mean drift speed is lower than 5e-4m/s, as introduced in Lemieux et al (2015). The region of landfast ice (with the newly adopted criterion, as a supplementary figure) agrees well with the region of analysis.

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