Reply to RC2 comments on

A probabilistic seabed-ice keel interaction model by
Frédéric Dupont, Dany Dumont, Jean-François Lemieux, Elie Dumas-Lefebvre and Alain Caya

Comments are reproduced in italic text and followed by our response in normal text and in blue.

The manuscript describes extended version of grounding scheme by Lemieux et al., 2015. Authors provide theoretical description of the method, apply it for short term sea ice-ocean simulations and describe the results.

The paper is very well written, and enjoyable to read. Figures are also of a good quality. I have only several very small comments, and in my view, paper can be accepted after minor revision.

Minor comments:

Line 30. It would be nice to see a paragraph about other attempts to add fast ice in the Arctic Ocean simulations, like Lieser et al., 2004, Itkin et al., 2015 and Olason, 2016.

Done, the three references are discussed in the introduction:

"Various attempts at representing landfast ice in Arctic simulations have been conducted in the past with a mix of these processes, starting from the crude zero velocity condition using an ice thickness-to-depth ratio of \citet{Lieser2004}, an increased maximum viscosity in \citet{Olason2016}, an artificially large tensile strength in \citet{Itkin2015} or the seabed stress parameterization of \citet[][hereafter referred to as L15]{Lemieux2015}."

Lines 38-39: Please comment on computational efficiency as well.

The additional computational costs associated with the LKD method is very small compared to the rest of the sea ice model computations (thermodynamics, dynamic solver, transport, ridging, etc...) as the main calculation is the maximum seabed stress (computed outside of the EVP subcycle) and the remaining operations during the subcycling are minor. We feel though that this explanation does not need to appear in the manuscript.

Line 57. You probably mean then --> than.

Thanks, corrected.

Line 82. While it became obvious from the rest of the paper why you represent bathymetry as random variable, a simple additional sentence giving the motivation *for it would be useful for ocean modelers like me, who often just take bathymetry as something that is well defined.*

Done. Adcroft (2013) shows actually an interesting use of the subgrid scale bathymetry information to restrain the bottom flow. Reference added in the manuscript.

Adcroft, A. (2013). Representation of topography by porous barriers and objective interpolation of topographic data. Ocean Modelling, 67, 13-27.

Line 118. …here (see Section 3) --> in this section

Following the first reviewer, we went for "section 3.2", for more precision.

Line 119. "The following SUBsections".

Thanks, done

Line 163. You mean Subsection 3.3.1 here, I guess.

Yes, thanks for the correction

Line 242. Why so many EVP cycles? The standard value for CICE is around 120, if I am not mistaken?

Ah, thanks for bringing this topic up. There is in fact mounting evidence that the 120 is way too low (Lemieux et al., 2012; Kimmritz et al. 2015, Xu et al., 2021) if you want to approach the viscous-plastic solution, which is the underlying assumption behind the EVP solver, although the number seems to be also a function of resolution. Note that the standard value in CICE6 is now 240 (which we believe is still too small!)

Lemieux, J. F., Knoll, D. A., Tremblay, B., Holland, D. M., & Losch, M. (2012). A comparison of the Jacobian-free Newton–Krylov method and the EVP model for solving the sea ice momentum equation with a viscous-plastic formulation: A serial algorithm study. Journal of Computational Physics, 231(17), 5926-5944.

Kimmritz, M., Danilov, S., & Losch, M. (2015). On the convergence of the modified elastic–viscous–plastic method for solving the sea ice momentum equation. Journal of Computational Physics, 296, 90-100.

Xu, S., Ma, J., Zhou, L., Zhang, Y., Liu, J., & Wang, B. (2021). Comparison of sea ice kinematics at different resolutions modeled with a grid hierarchy in the Community Earth System Model (version 1.2. 1). Geoscientific Model Development, 14(1), 603-628.

Line 247. Please comment on what is the advantage of this forcing, which seem to be popular in regional ocean modelling, but is quite exotic for global modelling.

This forcing has indeed become the reference for all the experiments done in our group. One interesting feature is the enhanced resolution in space and time while maintaining a fair degree of precision (see the provided reference, Smith et al., 2014, for more precisions). We feel though that this explanation does not need to appear in the manuscript.

It would be good if you mention computational efficiency of the scheme in Section 3.4. Just if it decreases the model speed to a noticeable amount.

Indeed, both methods do not decrease speed to a noticeable amount. As explained above, for both methods, the computation of the maximum sea bed stress is done outside of the EVP subcycling, while the rest of the operations inside the subcycling are very minor. We feel though that this explanation does not need to appear in the manuscript.

Line 325. "… a factor OF two".

Thanks corrected.

Discussion

The resolution in the model setup is around 12.5 km in the Arctic. Please comment on how well, you think, this grounding scheme will be working in higher resolution setups (e.g. ORCA12 and higher).

Indeed 12 km is not considered anymore a high resolution, but it is sufficient to resolve well enough the Siberian Shelves and to a lesser degree the Alaskan and Canadian Shelves. The proposed probabilistic scheme has the advantage that the subgrid topography is taken into account. Therefore, the actual resolved topography has less importance than its subgrid distribution. With higher resolution, we expect that the mean depth will be better represented but, given a realistic subgrid distribution and that ridges are not represented explicitly (i.e., the continuum approximation of sea ice), the sea ice dynamics and landfast ice position should be relatively invariant to the resolution. We feel though that this explanation does not need to appear in the manuscript.

Please add to the discussion comparison to other studies, that try to simulate fast ice.

Done in the introduction (see above response).

References

Lieser, J. (2004), A numerical model for short-term sea ice forecasting in the Arctic (Ein numerisches Modell zur Meereisvorhersage in der Arktis), Rep. Polar Mar. Res. (Berichte zur Polar Meeresforschung), vol. 485, 93 pp.

Einar Olason, A dynamical model of Kara Sea landâ   fast ice, Journal of Geophysical Research: Oceans, 10.1002/2016JC011638, 121, 5, (3141-3158), (2016).

Itkin, P., Losch, M., and Gerdes, R. (2015), Landfast ice affects the stability of the Arctic halocline: Evidence from a numerical model, J. Geophys. Res. Oceans, 120, 2622– 2635, doi:10.1002/2014JC010353.
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