Answers to the editor

Dear Mr. Hutter and Dr. Losch,

Thank you for your details response to the reviewers. I have some additional comments to add after consideration of the reviews and your response. Thank you to the reviewers for identifying points that were not clear in the paper.

I trust you will ensure that the paper is clear especially in regard to its goals and your contribution towards improving assessment of sea ice deformation in models. I am in agreement with you that more metrics, other than spatial and temporal scaling, are needed. It is an important point that scaling relationships are not a unique indicator that a simulated deformation field is physically realistic.

I believe it is important to keep in mind the limitations of the viscous-plastic model. I found that instabilities in the stress-velocity field can follow model grid lines. This might be one reason you find modeled LKF intersection angles cluster around 90 degrees. While I agree that a full sensitivity study is beyond the scope of this paper, such studies have been performed with more idealised models and could help in understanding the persistence and density behaviour of the viscous-plastic model. The fact that there is an increase in instability density with the ITD is not surprising, but does need to be pointed out to people who use these models. As you point out the ITD allows steep gradients in the stress field (ice strength being a function of ice thickness), that may not present with a smoother thickness field. It is very important, from my experience, in idealised sensitivity studies to control for various sources of stress discontinuities: from poorly filtered wind fields due to interpolation procedure, to poorly converging viscous-plastic solution, to the parameterisation of the strength.

Thank you for highlighting these points. Following your observations that instabilities follow model grid lines, we compared the orientation of the modeled LKFs with the orientation of grid lines (Fig. 1 of this answer). There are regions where one mode of the LKF orientation aligns with the grid lines, but we do not observe this behaviour consistently in the entire Arctic. In addition, small LKFs are most likely to follow the grid orientation. For this reason, we limit the analysis to LKFs that are larger than 10 pixel (=125km). We added a sentence to the section of intersection angles and indicate the orientation of the numerical grid in Fig. 6 (showing the LKF orientation):
The peak in the PDF near 90º suggests a dominant LKF alignment with the numerical grid. A close inspection, however, does not show this dominant alignment (Fig. 6b,c,e,f). Therefore, we can assume that the grid orientation has only a small effect on the LKF orientation, but that the rheology itself causes the overestimation of the intersection angle.

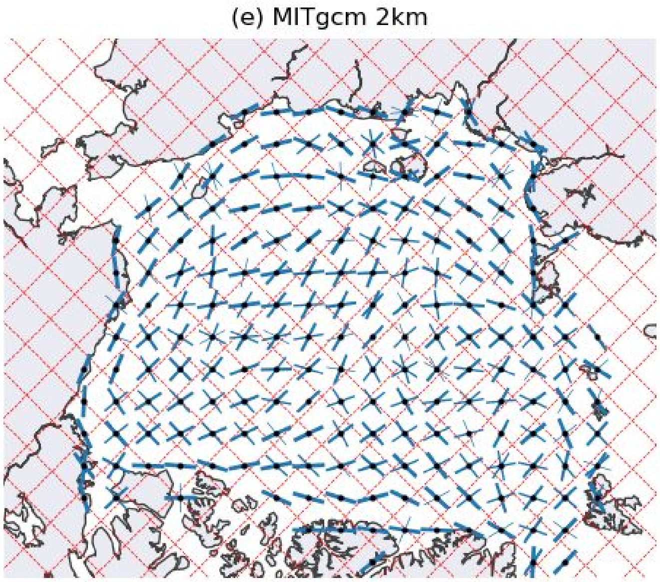

Figure 1: Modes of the LKF orientation in the 2km MITgcm simulation without ITD. Same figure as subfigure (e) in Figure 6 of the manuscript, but including every 100th grid line of model grid in red dashed lines.

We agree that it is important to highlight the increased amount of instabilities in the ITD simulation (we call them inhomogeneities in the manuscript) that initiates LKF formation to the reader. We already do so in the Introduction:
With our analysis we test whether the ice strength parameterization of the ITD model, which mainly depends on the thinner ice classes, accelerates lead formation by a faster feedback between deformation, ice thickness, and ice strength as suggested in Hutter et al. (2018).
and in the Discussion section:
With an ITD, the number and density of LKFs increase significantly. In the ITD simulation shear and divergence have a strong impact on the thin thickness classes which immediately feeds back into the ice strength facilitating further deformation. Therefore, inhomogeneities introduced by deformation in the thickness fields are much stronger compared to the standard VP simulation.

We agree that idealized experiments have a great potential to disentangle the effects of the various drivers of sea ice deformation. We already reference a number of idealized experiments (Hutchings et al., 2005; Hutter, 2015; Dansereau et al. 2016; Heorton et al. 2018, Ringeisen et al., 2019) and discuss how they help to understand the presented results from our Pan-Arctic simulations. In addition, we now encourage the use of idealized experiments in the Discussions Section:
We suggest to disentangle the effects of model resolution and wind forcing, but also ice strength parameterisation and solver parameters, on the formation of LKFs in a sensitivity study. For such a study, idealized experiments appear most suitable as they easily allow for higher number of simulations and to isolate effects.

I would like to point out an incorrect definition that is in the title of your paper. Two kilometers is not lead resolving. True, some leads may be two kilometers or more in width, but there is a distribution of lead and crack widths from sub-meter to many kilometers. You need to be careful about this, in the title to your paper and elsewhere where you state the models are lead resolving.
We agree that the 2-km simulations in the manuscript do not resolve the entire spectrum of lead width. Nevertheless, simulations at these high resolution are unique in a way that leads (although wide ones) are simulated as features of reduced ice thickness and concentration as well as localized deformation. In analogy to simulating eddies in ocean models, we now describe our simulations as lead-permitting in a sense that the simulations resolves leads with a width larger than the grid resolution. The new title reads as,
Feature-based comparison of sea-ice deformation in lead-permitting sea-ice simulations
and we replaced “lead-resolving” by “lead-permitting” in the entire manuscript.

While I agree with much of your response to the reviewers there are some points I feel you should address.

I also on my first read of the paper questions if the two simulations had been cherry picked. Your description of how the runs were set up was very helpful and should be included in the paper.
We added a brief description of the model performance to the text, as already suggested in the answers to the reviewers. In addition, we decided to add the full description and an additional figure that shows time series of the modeled sea ice volume and extent (same as in the answers to the reviewers) as an appendix to the paper.

Regarding your upper limit in angle between sections that are pieced together into LKFs, have you considered the high curvature tidal cracking can form? I realise this is outside of the scope of your paper, but I do caution you to consider that this impact of the cut off will change with model resolution and also

the kinematics a model can resolve. Does your cut off length of 125km preclude inclusion of features with high curvature?
The detection algorithm has limitations in detecting short high-curvature leads. Larger LKFs can show higher curvatures, which we also observe as wider spread in curvature in Fig. 8 of the manuscript. The parameters of the algorithms (also the accepted difference in angle that weighs the impact of the difference in orientation of two segments in the reconnection instance) are optimized to fit the majority of LKFs. As most of them are quasi-linear with little curvature, the small number of short high-curvature leads might not be picked up by the algorithm with the same reliability. Nevertheless, Linow & Dierking (2017) studied the curvature of LKFs from hand-picked data and automatic detected LKFs and found low curvatures with close to linear LKFs in both cases. Thus, we are confident to describe the LKF shapes as scale invariant, but add an additional sentence to the LKF curvature section:
The spread in curvature decreases at smaller LKF length (<200km) for RGPS and both simulations, because the LKF detection algorithm does a poor job of detecting short high-curvature LKFs.

The limit of 125km is only used to determine the intersection angle of LKFs. The curvature analysis runs on all detected LKFs and thus has a lower limit for LKF lengths of 37.5km. We could limit the curvature analysis to LKFs larger than 200km to preclude the underestimation of short high-curvature LKFs by the detection algorithm. However, we prefer to show Figure 8 as is and comment on this limitation given the small amount of these features at the resolution used in our study.

Some specific comments:

page 16, line 27: constraining your analysis to LKFs that form in the same time record may not be a strict enough constraint to ensure LKFs formed at the same time. RGPS images can be days apart. While this is the best you can do, I feel you need to acknowledge the limitation.
We agree and added:
Therefore, we limit the analysis of intersection angles to pairs of LKFs that form in the same time record. **We note that with this restriction the maximum time between the formation of both LKFs is determined by the temporal resolution of RGPS of 3days. Therefore some LKF pairs may not have formed simultaneously.**

If the mechanism creating LKFs in the viscous-plastic model is an instability in the model itself, how does this compare to actual quasi-brittle or granular failure? I ask this question because I feel it is important that we understand how this instability is controlled by noise in the ice strength (or stress) field and I wonder if we can parameterise this to produce realistics LKF densities. The use of a damage parameter and brittle failure is perhaps shifting this to a more physically tractable representation, yet you may still need to control for ice age in some way in what ever parameterisation you use.
The plastic failure that creates LKFs in the high resolution VP simulations is similar to brittle failure with the difference that the information of past deformation is stored in the ice thickness and concentration and not in a separate damage parameter. By varying the dependence of the ice strength on the ice thickness and concentration, we can amplify the effect of inhomogeneities in the thickness and concentration fields on the ice strength and triggering failure. In the ITD simulation exactly this is happening with an increasing number of LKFs and a realistic LKF density. However, we observe that the model needs too much time to close a lead and level out the associated inhomogeneities. The benefit of an additional damage parameter it that it allows to adjust this closing rate with a healing parameter separate from the thermodynamic processes associated with ice growth.

Finally, density and number is an interesting parameter to compare between LKFs extracted from 10km (roughly) resolution RGPS and 2km resolution model. Perhaps I missed this, but how did you control for this?
Due to the Lagrangian nature of the RGPS data set, the RGPS deformation rates cumulate the deformation occurring all LKFs within a grid box. Thus the RGPS deformation rates also include imprints of LKFs at smaller scales than the grid resolution of 10km. In the model, multiple grid points are needed to resolve a lead, so that the nominal resolution for leads is lower than 2km. To compare the density and numbers of LKFs on grids with different resolution, we normalize these quantities according to number of observations and horizontal grid spacing. The parameters of the detection algorithm that depend on the horizontal grid spacing are also adjusted accordingly.

Best regards,
Jenny