Clarifications:

- Abstract L8 "with little dependency on the mesh resolution", as well as at P2, L38-39: "regardless of the horizontal resolution used". This should be rephrased: I believe that the authors here refer to the production of LKFs and heterogeneity at relatively coarse resolution, but not that the model is insensitive to the choice of resolution (which I doubt any model could claim to be). I suggest changing for (at both L8 and L38) "at relatively coarse resolutions".
This is a good point. We followed the referee's suggestion.

- L26-28: This seems self-contradicting (they are expected to have a strong impact but their importance is unclear). To me, the impact of LKFs and heterogeneity in the real world is quite clear, but the necessity to resolve the very fine scale to represent this impact in a sea ice model is not. E.g., what is the benefit of resolving the smaller-scale heterogeneity, versus representing its impact via sub-grid parameterizations?

We agree with the referee that this sentence is a bit self-contradicting. However, the question suggested in the comment ("what is the benefit of resolving the smaller-scale heterogeneity, versus representing its impact via sub-grid parameterizations?") is not exactly the one we address in the manuscript (and as pointed out by the other referee, this is not one we can address with this setup), it might therefore be misleading to mention it in our introduction. Instead, we suggest rephrasing this sentence to insist on the fact that we use our model to quantify the impact of LKFs at a Pan-Arctic scale.

L26: These ubiquitous features, particularly leads, are expected to have a strong impact on ocean-ice-atmosphere interactions in polar regions (Lüpkes et al., 2008; Marcq and Weiss, 2012; Steiner et al., 2013), but this impact at a pan-Arctic scale has not yet been quantified.

- L49: Verify reference to Steele et al. 1997: I am not sure it is an accurate ref to use here.

We agree that this reference is not accurate for this sentence (as the impact of rheology on mass balance is not discussed in their article), but it supports the following sentence (about the fact that internal stress matters for sea ice transport as it affects motion and thickness). We therefore moved this reference to the end of the next sentence.

- L340-349: This paragraph is more confusing than enlightening: it meanders around the unclear relation between deformation rates and ice growth in leads, while trying to present the small-scale dynamics as an advantage for the representing this ice growth. I believe that this should rather be brought as a question of interest, with neXtSIM as a new means to answer it. For instance, what do we expect, in terms of ice growth, from a localised lead vs. a non localised lead, especially considering the use of an ITD? What does it change, in terms of ice growth, that a model has the right rates of divergence (or not)?

We have rephrased to make more explicit the question of interest, but are reluctant to put too much emphasis on things (i.e. the impact of explicitly resolving leads or not) that are beyond the analysis presented in this paper, as it was one of the main negative comment of the other referee. We are also a bit confused by the point between divergence and the ITD.

To represent the effects of leads with an ITD in the absence of divergence, there is a need to constrain the ITD shape to ensure the presence of open water or very thin ice (which would be the effect of divergence). The simplest way would be to, for instance, cap the concentration of thick ice, so that each cell includes at least a small fraction of thin ice or open water to represent the leads (this is a possibility in the LIM3 model). Just "using" an ITD and letting it evolve prognostically is not sufficient. This may be what the referee means with the expression "non-localized leads" (as they are not related to any physical process, and therefore their spatial distribution is likely to be uniform). The paragraph has been rewritten with these comments:

L340: *This is because the highest values of divergence rates (and deformation rates in general) in Arctic pack ice are very localised (Figure A1a,b), which would not be the case if the ice cover was homogeneous (e.g., Stern and Lindsay, 2009). For instance, Bouillon and Rampal (2015) found that in neXtSIM at 10 km resolution, 50% of the divergence in the Central Arctic was associated with only 5-10% of the surface area in the domain used for the analysis (this surface ratio would be 50% in the case of a homogeneous ice cover). Divergent ice motion, therefore, results primarily in the formation of localised leads in the central pack or of polynyas near the coast. An underestimate of divergence rates, which "standard" sea ice models run at resolutions coarser than 5km tend to do (Hutter et al., 2021), would imply a subsequent underestimation of ice production in winter if there is not a sufficient parameterization to represent the effect of leads. This parameterization can be done using, for instance, a minimum value for the lead fraction in each grid cell, resulting in a more uniform distribution of lead growth over the domain (as this can be done in the LIM3 model, Rousset et al., 2015). The importance of resolving leads versus using parameterizations to represent the ice growth in leads in numerical models has not been assessed to our knowledge. This would likely require a model comparison between a model which captures divergence rates well and another one using a parameterization for leads, which is out of the scope of this study. Instead, we focus on estimating the importance of ice production in leads in our simulation, as this has not been estimated at a Pan-Arctic scale before. The advantage of using neXtSIM in our analysis is that its ability to reproduce small-scale sea ice dynamics has been thoroughly evaluated before (see Ólason et al., 2022, and appendix A). In addition, it has been shown that the model is able to capture rates of divergence consistent with observations and relevant statistics of the observed lead fraction in the Central Arctic at spatial resolutions like the one used here (Ólason et al., 2021, 2022, and Figure A1).*

Minor typos:

- L76: Typo, remove "make"
Done
- L101: Typo, add space between the point and the new phrase
Done
- L125-128: This probably means that NEMO sees a smoothed version of neXtSIM?
The triangles of the neXtSIM mesh are constrained to have side lengths within 10% of the side lengths of the exchange grid cells. Interpolation of the fields is inevitable since the two models are running on different grids, and the term "remapping" may therefore be more adequate than "smoothing". The interpolation approach also ensures that any smoothing is minimal

and very unlikely to impact the dynamics discussed in the paper. As interpolation is inevitable and smoothing minimal, we think that mentioning the word “smoothing” in the manuscript would be more confusing to the reader than the current way we describe the interpolation in L125-128.

- Table 1: 3rd row, I think the convention for units should be kPa m-3/2
Done
- Figure 5: units convention, I think it should be [km day-1] instead of [km/day]
Done