2nd Review of:
"*Arctic sea ice mass balance in a new coupled ice-ocean model using a brittle rheology framework*" by Guillaume Boutin, Einar Ólason, Pierre Rampal, Heather Regan, Camille Lique, Claude Talandier, Laurent Brodeau and Robert Ricker.

This manuscript presents a new coupled ice-ocean model (with neXtSIM for sea ice, OPA for the ocean) and discuss its performance in representing the Arctic sea ice mass balance based on an 18 years long simulation (2000-2018). They describe their methods for coupling neXtSIM, a Lagrangian model, with OPA, an Eulerian model. They provide a detailed analysis of the modelled ice mass balance in terms of trends, inter-annual variability and seasonal cycles of each thermodynamics and dynamics contributors. They show that the ice-ocean model captures the amount (25-35%) of ice growth occurring in leads and polynya, and that this portion has a positive trend mostly attributed to the coastal polynyas.

I commend the work made by the authors to clearly and genuinely address all of my previous comments in the manuscript. All pointed statements have been either corrected or removed, and details about the model tuning has been added as an appendix. Altogether, this makes for a strong and interesting analysis that we interest the ice modeling community.

I therefore recommend its publication, after addressing a few remaining 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".

- 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?

- L49: Steele et al. 1997: I am not sure if this reference still appropriate in this modified phrase. They look at the contribution of the internal stress terms in the sea-ice force balance, but not the ice mass balance nor do they discuss the choice of rheology.

- 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 changes, in terms of ice growth, that a model has the right rates of divergence (or not)?

Minor typos:

- L76: Typo, remove "make"

- L101: Typo, add space between the point and the new phrase
- L125-128: This probably means that NEMO sees a smoothed version of neXtSIM?
- Table 1: 3rd row, I think the convention for units should be kPa m$^{-3/2}$
- Figure 5: units convention, I think it should be [km day^{-1}] instead of [km/day]

Congratulations to all the authors for this nice analysis,

Mathieu Plante