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. This is done by first interpolating the neXtSIM fields onto an Eulerian mesh, such that the interpolated fields are used for the coupling. The study provides a detailed analysis of the modelled ice mass balance in terms of trends, inter-annual variability and seasonal cycles, and investigates both the thermodynamics and dynamical contributions to the mass balance. 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.

The manuscript is very clearly written, well detailed, and presents figures that are appropriate for the analysis. I find this manuscript very well prepared, and that the science (results and discussions) is of high quality. In all, this makes for a very good presentation of the new ice-ocean model, combined with an interesting study on the ice mass balance that will benefit the sea ice community.

I nonetheless have two points that I believe need to be address. First, the manuscript suffers from a couple of subjective statements about the included rheology, which do not relate to the provided analysis. While these statements are few and only found in the abstract, introduction and conclusion, they effectively leaves a first (and last) impression that the authors are pushing their rheology forward. In the context of a scientific manuscript, such subjective statements have a history of distracting readers from the actual analysis and to raise doubt on the transparency. I think it imperative that these statements, listed below, are rephrased or removed. Second, I also believe that more information could be given on the coupling, more particularly about the tuning of the ice drift, thickness and deformations, given that presenting the ice-ocean model is one of the main objectives of the manuscript.

For these reasons, I recommend this manuscript to be accepted for publication, after major revisions.

Major points:

- L7-8: *"Using this rheology enables the reproduction of the observed characteristics and complexity of fine-scale sea ice deformations with little dependency on the mesh resolution."* This implies that one needs the BBM rheology to have performance, which is far from being established. This performance may very well be related to the Lagrangian scheme. The dependency on the mesh resolution is intrinsic to all continuum models, not to a given rheology. This is simply resolved by using a more appropriate and objective turn of phrase, such as “This rheology has been shown to reproduce…”.

- L12: “*The model performs well*”: unless accompanied by some quantifications, this remains vague and subjective.

- L14: “*Benefitting from the model’s ability to reproduce fine-scale sea ice deformations, we estimate that the formation of sea ice in leads and polynyas contributes to 25%–35% of the total ice growth […]*”: This statement made me expect some sort of demonstration of that benefit, but in the analysis, this benefit is assumed but not investigated. This is not that trivial to me, as we do not need fine-scale deformations to have growth and divergence within the pack-ice. Unless this benefit is shown, this should be rephrased.

- L99-100: This tuning is interesting but it is unclear what has actually been done. As this manuscript is presenting the coupled ice-ocean framework, I feel that this needs to be better described. In particular, it would be nice to have a figure that shows how the stress is chosen, and this tuning balance between drift and thickness distribution, and the deformation statistics. This is especially important as these are important parameters for the ice mass balance.

- L395: “*the inability of many models to correctly simulate LKFs*”. This is a bit misleading and should be rephrased. The conclusion of SIREx is actually that all rheologies are able to produce LKFs, but none do so correctly due to a tendency to under-represent them.

- Last paragraph (L453-460) : “*Our results illustrate the interest of using a brittle rheology framework in ice–ocean coupled modelling* […]”. This last paragraph is very subjective and brings conclusions that are by no means discussed in the analysis. Was is shown is that the new ice-ocean model is performing well. Attributing this to the rheology is, to me, not only reductive but inaccurate, as we are discussing a fully coupled ice-ocean model here. The extent at which the portion ice formation associated with pack ice divergence is dependent on the stated heterogeneity is also not demonstrated, and similar results could very well be obtained with other rheologies. I think the authors should focus on contributions demonstrated in the manuscript, which I believe are many and interesting by themselves.

Minor comments:

L5: (<5km) would be more accurate (see Hutter et al. 2022). Same in L30.

L32-34 : *“LKFs are related to the mechanical behaviour of the sea ice, and their absence in models…”*. Too strong: they are not “absent” but under-represented.

L46-52 : This paragraph should be re-worked, I am not quite getting this modifications to the stress state. Is “stress state” used here as a synonym to rheology?

L56: Has this been portion been reported in classical models? If so, this could provide a measure on how much this 30% is being reproduced by (E)VP models, and would perhaps indicates the benefit of representing finer scale deformations.

L90-92: I believe that we could have a bit more information on the thermodynamics, as it is, after all, a significant contributor to the ice mass balance. For instance, is there a melt-pond scheme? How much do we expect results to be affected by the use of more sophisticated thermodynamics (i.e. including brine processes, snow model, etcs)?

L106: I believe that the BBM model has 2 time steps (dynamical and advection). I assume that the 450s time step for the ice model refers to the advection time step? Otherwise, this would mean that the model depends on an elastic component that is largely un-resolved. This needs to be clarified.

L113-120: What about the Lagrangian regridding?

L127: Here it is OPA-neX, but later it is OPE-nex.

L145-150: My understanding is that PIOMAS remains somewhat dependent on model outputs, and not without bias. A work or two on this would be useful.

L215: Not sure why "remarkable" is used. This needlessly adds subjectivity, unless the reason why this was unexpected is specified objectively.

L233-243: This is interesting. May this be related to the representation of old ice? This could explain this 2008 mark for the model performance, given the loss of old ice in observations after the 2007 summer.

L245: "*We first investigate*" I found "first" odd here

L255-156: "*This is consistent with the behaviour of PIOMAS*". This could be clarified. The same underestimation is seen in PIOMAS? What does it mean?

L262-265: I personally don't think that this paragraph is necessary.

L280: Is this similar to previous reports?

L285-291: This is a bit confusing to me. I understand in principle that anomalous melt in spring makes for anomalous surface to grow in Fall, but at the end you seem to say that anomalous growth in the fall also makes for anomalous melt in spring… This is going in circle to me, and if constant throughout the period, how do you get anomalies?

L294: Missing a reference for this dominance of basal melt south of Fram.

L300-301: This may be related to the mass-conserving snow-ice formation scheme. We find that this largely underestimates the snow-ice volume. In Turner et al. (2015) (ref below), the changes in the snow-ice parameterization was the largest contributor to pan-Arctic thickness changes associated with the implementation of the mushy layer in CICE.

L308: This is interesting and a comment could be added about what this implies. E.g, we know that the export of ice is has quite a variability associated with the AO. What this seem to suggest, is that the larger export is compensated with larger divergence and enhanced ice production?

L317: I find unclear what is meant as heterogeneity here. Is it used as a synonym to “leads”? We see the ice formation in leads and its contribution, but how is this a measure of heterogeneity?

Figure 8: I would specify right at the beginning that this only covers the winter, as the lack of melt is puzzling at first glance.

References: There are some errors in the references. For instance, Mehlmann et al., 2021 is incomplete. Some have errors in the DOIs. (E.g., Semtner 1976, Winton 2000, Zhang et al. 2003)

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Refs:

Turner, A. K., Hunke, E. C., & Bitz, C. M. (2013). Two modes of sea-ice gravity drainage: A parameterization for large-scale modeling. Journal of Geophysical Research: Oceans, 118 (5), 2279-2294. Retrieved from https://agupub.onlinelibrary.wiley.com/doi/abs/10.1002/jgrc.20171 doi: https://doi.org/10.1002/jgrc.20171