We thank the referee for reading thoroughly and performing a second round of reviews of the paper. We have incorporated most of the suggestions made. We comment on the exceptions below.

1- p.6, line 19: I think the reference for Hutter et al. (2019) should be Hutter and Losch (2019) instead:
Hutter, N. and Losch, M.: Feature-based comparison of sea-ice deformation in lead-resolving sea-ice simulations, The Cryosphere Discuss., https://doi.org/10.5194/tc-2019-88, in review, 2019.

Thank you, we have corrected this reference.

2- p.6, line 20: "They report inconsistent temporal scaling with a reasonably good temporal scaling"??? and later: "and no temporal scaling in the region covered by the EGPS data they compare to." There is a temporal scaling, however the scaling exponent is smaller than in observations. I would correct this sentence to:
"They also report a reasonably good temporal scaling, however the scaling exponents found for the model simulation in the same region covered by the EGPS data are smaller than in observations."

We agree that the formulation used was indeed not clear and we therefore rephrase this sentence as:
"The results on temporal scaling show some inconsistencies: the authors report a reasonably good temporal scaling when considering the full domain (but do not report on multi-fractality), however, in the smaller region covered by the EGPS data the estimated scaling exponants are significantly smaller than for observations."

3- p.6, line 24: "but as this paper is still under review further detailing of their results is premature." The results shown in Hutter and Losch (2019) (reference above) are pretty conclusive. Please remove and add instead: "Hutter and Losch (2019) present further results confirming the ability of viscous-plastic sea-ice models run at very high resolution to reproduce the observed spatial and temporal scaling and multi-fractal behaviour of the ice."

This is a subjective comment: the "conclusive" character of these results, as in any other study, should be assessed by peer-reviewing. Hence the sentence remains as is.

4- p.7, line 14: "atmosphere–ocean interaction" --> "atmosphere–ocean interactions"

Thank your for catching this.

5- p.9, line 23: "The provided fields surface height fields" --> "The provided surface height fields"

And again.

6- p. 10, line 2: "The final ocean currents forcing" --> "The final ocean current forcing"

Ok.

7- The methodology for the scaling analysis is much clearer! Thank you. However, to avoid confusion, I would change "polygon" to "triangle" everywhere in this section since you only are considering triplets of points, i.e. triangles.

We will keep polygons since it is a generic term for the method used: indeed, similar analysis (I.e., using contour integrals) could be performed on different types of polygons (e.g., squares).

8- p. 13, line 6: "A is the encompassed area of the polygon equal to L^2." --> "A is the encompassed area of the polygon approximately equal to L2." Not all triangle areas will be exactly equal to L^2 if L is a mean for all triangles.

Correct. We have corrected the text as suggested.

9- p.14, line 26: "This effect is even more important..." This was already mentioned above at line 23 in the sentence starting with "In the time domain,..." Please remove one or the other.

This last sentence was indeed redundant and we have removed it. We have modified line 23 slightly to convey the idea of line 24 and following.

10- p.15, line 4: "total, shear and absolute deformation rates" --> "total, shear and absolute divergence deformation rates"

Yes, thank you.

11- p.15, line 6: "all simulated or observed deformation rates for the period of 7 days" --> "all simulated or observed 3-day deformation rates for a period of 7 days"

Ok.

12- p.15, line 23: "However, the first, second moments" --> "However, the first and second moments"

Ok.

13- p.16, line 3: The reference to Bouchat and Tremblay (2017) should be inserted after point (2) instead of after point (3) in this sentence.

No: our mechanical parameters are not the same as in Tremblay and Bouchat, who used a VP rheology. A correct reference here would be Weiss and Dansereau, 2017, who performed a sensitivity analysis of the MEB model to the value of its parameters.
Here, the Bouchat and Tremblay citation indeed refers to the drag coefficient.

14- p.20, line 20: "We note that using a Lagrangian mesh then helps preserving such features, once formed, but plays no role in their formation." I still think it does matter in their formation as well. It is easier to resolve discontinuities with a Lagrangian mesh compared to an Eulerian one, and therefore it will be easier for those features to appear with a Lagrangian mesh. Please remove this sentence.

This is not true: the constitutive and dynamical equations solved are the same in a Lagrangian and a Eulerian frame, except the advective terms, which do not create velocity or stress gradients. The equations are well-posed and the choice of numerical scheme is independent of the physics. A Lagrangian scheme helps “resolving” gradients, but does not create them. This sentence stays as we believe it is in fac very important to make the distinction between the equations (and physics behind) and the numerical scheme used to solve them, as it seems to be a source of confusion.

15- p.21, line 26: "that is about to be submitted." --> "in preparation."

Agreed.

16- Figure 1: Why do you have to mask the model field? If what you are plotting are the triangles that correspond to the ones in RGPS, then you should show all of them even if they cover different regions since they are entering your analysis.

We are not masking the model fields. This was an incorrect sentence that we forgot to remove from the caption. We have therefore removed it now.