Anonymous Referee #1
Received and published: 28 February 2019

General comments

The manuscript applies a scaling and multi-fractal analysis to sea-ice deformation fields simulated with neXtSIM and derived from RGPS satellite observations. It shows that the spatial and temporal scaling of the observed sea-ice deformation is well reproduced by the model. The paper also explores the multi-fractality and spatio-temporal coupling of the scaling, but whether these behaviors are significant is debatable considering the very large error bars on the values compared.

The manuscript is generally well written but the wording used is often too strong or too conclusive for what the results are showing. I think the paper would be greatly improved if the authors would include a discussion of the error to support their affirmations. I also disagree with the choice of method for analyzing the RGPS data and would also like more information on how the scaling analysis is performed. For these reasons, I recommend that is this paper be reconsidered after major revisions and I provide more details about points to be addressed in the revisions below.

Dear reviewer, thank you for your in-depth review of our manuscript, and all your suggestions to improve it. We hope your numerous concerns about the methodology will be cleared after reading our responses and the modification we made to our manuscript. We think these concerns were mostly our fault and we thank you for having spotted incoherency or inaccuracy in our wording.

The review process also allowed us to find a bug in our analysis script, which mainly impacted our results of the space-time coupling of sea ice deformation that we propose to look through an actual change in the degree of multi-fractality of the scaling with respect to the temporal or spatial scale considered. We therefore re-performed the whole analysis and generated new figures with updated results.

Also, we found that our values of curvature of the structure functions calculated from the temporal scaling were incorrect.

Please find below our answers (in red) to your comments/suggestions/questions.

Major points to be addressed:

1. I have reservations about the choices of methods used to analyze the data in this study. By using a nearest-neighbour interpolation on the RGPS trajectories, the authors artificially set all initial temporal scales in the RGPS data to 3 days, although they vary strongly from a few minutes to up to 10 days. A filtering that keep the original RGPS temporal scales, for example, keeping only the trajectories that are updated more or less every 3 days, would be more appropriate. As it is now, it is not clear to me that the temporal scaling and spatio-temporal coupling the authors are reporting here is not an artifact of the method used.

Moreover, it would be necessary to include more details on the scaling (or "coarse- graining") procedure used in this study so that the results can be reproduced by others. As of now, it is also unclear what are the effects of using a sub-sampling of the trajectories (as I understood is done) instead of spatio-temporal averaging as usually done for the scaling analysis. The differences in the method and also the justification for choosing a different method need to be clearly stated.

2. I am also not convinced of the significance of the multi-fractality and spatio-temporal coupling behavior that the authors affirm is present in the results. The error bars on the values used to infer these behaviors are sometimes used to confirm that values overlap and therefore are equal, but are elsewhere ignored to affirm that the values are different. Also, the "error bars" as defined in this study rather represent the goodness of fit on the data, than an actual error on the values calculated. Proper error estimates are needed to claim that the multi-fractality or the coupling are significant.

3. The discussion of scaling in sea-ice models is limited to (Maxwell) Elasto-Brittle (EB/MEB) papers. There are several studies using the viscous-plastic (VP) rheology that are worth mentioning. Especially, the claim that for the first time spatio-temporal scaling is shown for a model, is not true (see Fig. 7 in Hutter et al., 2018 - see reference below).
4. The conclusion is mostly a summary of what the neXtSIM model is capable of rather than overall conclusion that can be drawn from the presented work for other studies or model development. The overall conclusion that is drawn (that is, that multi-fractal scaling analysis should be the prerequisite validation step before analysis of any other variable that might be related to sea-ice dynamics) is clearly application-dependent and needs to be modified. I would wish that the authors come up with a conclusion that is more useful for the scientific community than just promoting the model. There is clearly enough good material in the paper to do so.
5. I would appreciate if the model physics and the configuration of the simulation (e.g. wind forcing, rheology parameters, Lagrangian grid, etc.) would be separated more carefully when drawing conclusions from the simulated results. It feels like when there is agreement between the model and observations, the authors attribute this agreement to their choice of model physics, while if the model results disagree with the observations, the authors note that it could be due to the model configuration. Maybe a change in wording would help to reduce this impression.

Below are more specific comments to help the authors address the general comments above.
== SPECIFIC COMMENTS

Page 2:

- Line 16: There is a new paper about filtering LKFs in the entire RGPS data- set that also shows are wide range of intersection angles of LKFs (Hutter et al., 2019, see References below) and is worth to citing here.

Reference added.

Page 3:

- Line 4: Again, please add Hutter et al. (2019) (Linow and Dierking only studied 10 RGPS snapshots, whereas in the above mentioned paper, the LKF length was studied for the entire RGPS data-set).

Reference added.

- Line 7: "These events also sustain deformation, maintaining the LKFs “active” for many days Coon et al. (2007)" This is not clear. If the deformations are of short duration, how are they responsible for "sustaining" deformation rates over many days? Please clarify.

There was indeed a typo in the previous sentence: We wanted to say “short-duration *fracturing* events of…” instead of “short-duration *deformation* events of…”. The text has been changed accordingly.

- Line 15: To me, it is the distribution that can be in or out of the Gaussian basin of attraction, not the values themselves. I would remove "that are out of the Gaussian basin of attraction" or rewrite something like "...i.e., dominated by extreme values and out of the Gaussian basin of attraction"

Yes, we agree. The text has been changed following your suggestion, i.e. removing “that are out of the Gaussian basin of attraction”.

- Line 25: Please shortly explain what a "coarse-graining" method is.

We prefer to refer to the methodology section of the manuscript.

Page 4:

- Line 6: I agree that beta=0 is homogeneous deformations. But if one imagines the scaling exponent to be something similar as the fractal dimension (see Weiss, 2003), then beta=1 would correspond to deformations concentrated in one line, and for beta=2, all deformations would be localized in one single point. This is also what one would expect from averaging in two spatial dimensions. Please clarify.

Yes this is correct. We thank the reviewer for spotting this typo. We changed the text accordingly: “In the space domain, $\beta = 0$ characterizes the homogeneous deformation of an elastic solid or viscous fluid, i.e., a deformation that does not depend on the spatial scale, while $\beta = 2$, i.e. the topological dimension for the 2D-like sea ice cover, corresponds to a single "point" concentrating all of the deformation in an otherwise undeformed material (Rampal et al. 2008)”.

- Lines 21-24: Two paragraphs above you state that the distribution of the sea-ice deformation rates is out of the Gaussian basin of attraction (i.e. the decay has a slope <= 3). For those power-law distributions it is known that the higher moments (variance, skewness, etc.) do not converge, due to the presence of extreme values, to a real number but to infinity. Please clarify how those ill defined moments help better describe the distribution?

The comment of the reviewer is interesting. We understand the confusion we may bring here by expressed ourselves the way we did, and in particular saying that the deformation distribution was “out of the Gaussian attraction basin”. This is actually wrong. The distribution of sea ice deformation are indeed heavy-tailed, dominated by extreme values, but with a slope close to 3 (actually slightly larger), meaning that they are still in the Gaussian attraction basin, but requires us to consider higher order moments (in our study up to q=3) than just the mean to be describe them and the associated process of sea ice deformation. In our case both variance and skewness can therefore be defined, and are considered for the scaling analysis along with the mean. In order to correct this mistake and clarify this point as suggested by the reviewer, the text has been changed to the following:

“The fact that sea ice deformation is characterized by heavy-tailed statistical distribution, i.e. dominated by extreme events, also indicates that the mean (moment of order 1) is not a sufficient quantity to describe the distribution of deformation rates at a given time/space scale. Higher moments of the distribution of deformation rates, such as the variance (order 2) and skewness (order 3), should indeed also be explored to better describe the distribution and the associated process of sea ice deformation, and considered in temporal and scaling analyses as proposed in this study.”

Page 5:

- Line 5: "the amount of localization of large and small deformation events is the same" How do you define the localization? I thought the scaling exponents are quantifying the "degree of localization" of deformation (see page 4, line 5)?

Here, even if the curvature is zero, the scaling exponents are still changing linearly with the moment q, so that they are different and then I don’t think you can say that the "amount of localization of large and small deformation events is the same".

Yes! you are right indeed. Thanks for spotting this mistake. The sentence “the amount of localization of large and small deformation events is the same" was put here by mistake. We therefore removed this sentence.

- Line 10-11: Please add references for this.

We added two references: Lovejoy and Schertwer (2007) and Kolmogorov (1962).

The definitions of heterogeneity and intermittency are confusing to me... Isn't saying that a field shows high localization in space/time (ie. beta or alpha > 0) the same as saying that the field is heterogeneous/intermittent?

Not really in fact. The definition of "heterogeneity" and "intermittency" are often related to the characteristics and degree of localization of the process in space and time respectively, i.e. related to the exponent of the structure function.

So, as we said in the manuscript, in the literature the terms spatial "heterogeneity" and temporal "intermittency" are often used when we are in the presence of a field/ or process that can be modeled by *"log-normal cascade"* associated with a multifractal scaling (not mono-fractal). However, we do acknowledge that these definitions of "heterogeneity" and "intermittency" are not consistent throughout the literature and different research communities (for example the one focusing on the study of highly non-linear (e.g. chaotic) dynamical systems differs from the one focusing on geophysical fluid turbulence), which we agree can be a source of confusion.

We changed the text as follows in order to make these definitions more clear:

"In the case of a linear structure function, i.e. no curvature or equivalently a=0 or c=0, the scaling is said to be *mono-fractal*.

In the case where both coefficients a and b or c and d are positive the structure functions are convex, meaning that the higher order moments of the distribution therefore increase much faster than the lower order moments with decreasing scale of observation. In other words, *large* deformation events are *more localized* in time and space than smaller events, corresponding to the definition of a *multi-fractal* scaling (e.g. Kolmogorov 1962, Lovejoy and Schertzer 2007). Note that in the literature multifractality is also called *intermittency* when present in the time dimension and *heterogeneity* when present in the spatial dimension. The largest the curvature (i.e. a or c > 0), the stronger the *degree of multifractality* of the scaling."

Why does a field need to have a (quadratic) change in the localization exponent with the different moments q to define it as heterogeneous/intermittent? What if the structure function is cubic? Please clarify.

As said above, this is a matter of definition, but one may indeed say that a field can exhibit different "*degrees of heterogeneity*" or *"degree of intermittency"* depending on the value of the structure function exponent. So the structure function does not need to be quadratic, but at least diverge from a linear function.

Note that there is another way to look at the degree of multifractality, which is to calculate the moment scaling function defined as $K(q)=C(q^{alpha}-q)/(alpha-1)$. In the framework of the theory of *multiplicative cascade processes*, the exponent alpha (which is related to the so-called "Levy exponent of the generator") is lower or equal to 2. The special case when alpha is equal to 2 (the scaling is then strongly *multifractal*) in fact corresponds to the so-called *"log-normal"* multiplicative cascades (Kolmogorov, 1962), whereas the case when the exponent is equal to 1 (the scaling is then *monofractal*) corresponds to the so-called "*beta-model"* (Frisch et al., 1978).

- Line 20 - "(Olason et al., 2019)" I couldn't find this paper in the Cryosphere Discussion.

This is correct, as this paper is only about to be submitted. We therefore removed this reference and replace it with a conference talk where these results were presented.

- Line 24,25: Downscaling of the modeled sea-ice deformation might be performed if one characterizes the scaling exponents, however, Spreen et al. (2017) have shown that the dependence of the scaling exponent on the sea-ice concentration and thickness has non-trivial effects on the "scaled" deformation rates, so I would suppose that only if you knew the distribution of A and h at a subgrid-scale (which we don't) could you actually perform such a downscaling.

Thanks for this interesting comment. Spreen et al. used a different model with a different dependence of deformation on concentration and thickness. So their results are, a priori, not applicable in our case. Their paper also showed a poor reproduction of deformation scaling and does not explore the multi-fractality of their solution. We make a point of underlining the importance of multi-fractality in the paragraph in question so given that Spreen et al don't consider multi-fractality at all makes it even more difficult to see how their results can be of importance here. Finally, we don't mention concentration and thickness here at all - only deformation. It is true that down-scaling thickness, and concentration (or more generally the thickness and flow size distributions) would be of considerable interest, but this is outside the scope of the paper and we feel that a discussion of this potential application would be distracting for the reader.

Page 6:

- Lines 3-6: It would be worth to include a short summary of studies that used scaling analysis to evaluate sea-ice deformation in simulations and their findings (so far only EB studies are mentioned). For example, Hutter et al. (2018) have shown that the VP rheology can reproduce the observed spatial scaling and also multifractal characteristics in the spatial domain (i.e. quadratic spatial structure function at very high resolution. Please see also Spreen, et al. (2017) and Bouchat and Tremblay (2017).

We now include such a summary.

- Lines 16-19: Please elaborate on why this is needed. What does it mean "to localize the deformation at the nominal scale"?

We've rewritten the paragraph so it should be clear that "localizing the deformation at the nominal scale" refers to reproducing the statistics of deformation from the smallest scales resolved by the model (i.e. the grid spacing and the time step). We have also elaborated on why we expect such localization to be essential to further improve the simulation of ocean-atmosphere interactions modulated by the presence of sea ice.

Page 9:

- Line 2: Please provide the spatial and temporal resolution of the forcing used.

Horizontal spatial resolutions of the forcing used (here 30km for the atmosphere, 50km for the geostrophic current and 12.5km for the ocean temperature and salinity) are now mentioned in the text.

Page 10:

- Line 12: "We use the coarse-graining approach..." Since you already men-
tion both approaches, please justify why you haven chosen the first one.
- Line 13: Why do you choose triplets? It is known that the boundary definition error (Lindsay & Stern, 2003) is larger for lower number of vertices. You have chosen the minimal number of vertices and thus the highest uncertainty. Why? Do you use the smoothing filter as suggested by Bouillon and Rampal (2015) in your analysis to compensate this effect? If not, please mention why not and how you deal with the uncertainties introduced by choosing triangles instead of rectangles as done originally in RGPS.

We indeed chose to consider triplets because it was the best compromise between the number of samples (or triangles) we could consider in our analysis, while still getting robust power law fits through the moments of the distributions. Using triangles has more uncertainties than using quadrangles if the absolute values of deformation rates are of interest (as mentioned already in Bouillon and Rampal (2015)). Here however, we focus on the scaling properties of sea ice deformation rates and on the comparison of a model with observations. So what matters is to make sure one applies the same methodology on both to derive sea ice deformation rates, which we do by considering and tracking over the entire winter season the exact same set of triplets in the model and the RGPS dataset. Note also that using triangle allows to decrease the actual minimum spatial scale considered in the scaling analysis from 10km to 7.5km.

We found that applying the filtering suggested in our previous study is therefore not needed for the sake of comparing model and observations as done in this study.

That being said, we do agree that using quadrangles would be a possible way to go, but this would reduce the number of samples in the distributions strongly, and therefore would imply to consider more than one winter dataset to perform this analysis, which we consider to be beyond the goal of the present study.

- Line 15: The value of 7.5 km is an average for the triangulation over the 2006/2007 season? Otherwise, it is not clear to me how you get that number analytically. Please specify.

7.5 km is the number obtained when averaging the square root of surface area of the triangles obtained from the delaunay triangulation through the initial (i.e. on December 3 2006) RGPS drifter's position.

- Line 25: Do you use the same triangulation for both the RGPS and model trajectory sets?

Yes, the exact same one, meaning that we follow the exact same set of triplets of trajectories (or triangles) in the model and in the observations, defined from the Delaunay triangulation performed on the initial set of RGPS/model drifter points. We added a sentence in the text to clarify this.

Also, how do you handle the different streams of the original RGPS Lagrangian ice motion dataset?

This specific points are explained in Bouillon and Rampal (2015), see section 3, 3rd paragraph. We now refer to this paper in the text, since repeating the whole procedure here would be too long and may contribute losing the reader.

Page 11:

- Line 5-8: Not clear. What is the "subsampled cloud" of positions? How do you select the triangles to add up to a certain spatial scale? Please add details of the subsampling procedure!
- Line 9: "The number of triplets available for the statistical analyses decreases as the space scale increases." Why? Because you use a filter to discard larger scales if they are not filled up to a certain percentage? Please clarify.
- Line 9-11: "Coarse-graining in time..." Are you re-sampling the trajectories at larger time intervals and then computing new estimates of the strain rates at these larger time scales, instead of averaging multiple 3-day strain rates together? To be consistent with the spatial scaling analysis as it is usually done (e.g. as in Marsan et al. 2004), the strain rates should be averaged and not re-sampled.
- Line 10: "The number of available triplets also decreases as the time scale increases."
Again, please indicate why this is the case.
- Line 18: "...around the boundary of each polygon associated to a given space scale L" Again, it seems like you are saying that you are re-calculating the strain rates at different scales instead of averaging multiple triangles of the original data set together to add up to a certain time/spatial scale. If you are really recalculating the strain rates instead of averaging them, then please show what are the effects of doing this vs averaging on the scaling analysis, as I don't recall other studies that have used this method. Or maybe simply re-calculate your strain rates by averaging the triangles instead.

We realized that the reviewer misunderstood our methodology, and we acknowledge that the confusion may have come from the fact that we said we were using a so-called "coarse-graining" method in our study in order to explore the scaling properties of sea ice deformation. Our statement was wrong. We in fact use the "buoy's" dispersion method, using triplets, as in Oikkonnen et al. 2017. This consists in mapping the Arctic with contiguous triangles of different sizes (i.e. 7.5; 15; 30; 60 ... 700 km that corresponding to the spatial scales we want to consider for the scaling analysis) and to look at how the

triangles with the same given size deform over different time period (i.e. from 3 days to 100 days in our study). The above mentioned set of triangles are defined once, at the starting time of the RGPS trajectories, i.e. 2 December 2006 in our case, and then are followed throughout the winter season.
So, in order to address all the above comments (related to page 11), we rephrased some parts of the first paragraph of section 3 that describes our methodology, trying to make it more clear. We also send the reader to Oikkonen et al. 2017 for more details on the methodology.

Page 12:
- Line 1: The actual area A of the polygons will differ slightly from the nominal scale L^2. Do you filter the polygons for their area to match the given nominal length scale?
No, see the previous comment

Also, are you using the definition of A with the summation around the vertices of the polygons, e.g. as in Linsday and Stern (2003)? Please specify.
Yes, we do.

I am also confused about which polygons are which... If you are always grouping triangles that have a mean length scale of Li = 7.5 km to make new polygons at different length scales, wouldn't the average length scale defined as "the mean of the square root of the polygon surface areas", as written on page 11, i.e. what I understand as L = (L1 + L2 + L3 + ... + Ln)/ n (with Li, i=1,...,n being the length scale of the individual triangles), also equal about 7.5 km regardless of the number of triangles you average together? I feel like it would make more sense to define the spatial scale L of the new polygons (i.e. the aggregation of triangles) as the square root of the sum of all the triangle areas in the polygon, i.e. L = sqrt(A1 + A2 + A3 +...) where Ai is the area of the i-th triangle that is averaged together. Or maybe I just don't understand your scaling procedure. Please clarify.
See our comment above. The polygons at a larger scale L2>L1 are NOT the aggregation of smaller triangles of scale L1. They are independent triangles, defined once from the triangulation performed through the points corresponding to a subset of the original RGPS points positions. So the surface areas of the L2-triangles are directly derived from the summation around their vertices, e.g. as in Lindsay and Stern (2003), the spacing between their vertices being just larger compared to the L1-triangles.

Page 13:
- Line 1: I strongly disagree with this procedure. A nearest neighbour interpolation will artificially set all initial temporal scales in RGPS data to 3 days, although they vary strongly from a few minutes to up to 10 days. Why do you not use the original temporal scale of the observations for the scaling analysis?
How much of the method is therefore responsible for the temporal or spatio-temporal scaling you are showing afterwards?
Wer agree with the reviewer that an interpolation like the one he/she thinks we have performed would be physically wrong and likely impact the results.
In fact, we suspect here a misunderstanding of the reviewer of what we meant by "interpolating" the RGPS data, probably due to our fault, i.e. using inaccurate wording in our submitted manuscript. We in fact applied a nearest neighbor "interpolation" at a regular 3 days frequency only when a RGPS observation is available within + or - 6 hours around the target time. All the RGPS position records that are not satisfying this condition are discarded, which does not mean that the entire trajectory is discarded, but truncated instead. Therefore, the smallest temporal scales for which deformation rates are calculated from the RGPS data is 2.5 days. Or in other words, the smallest temporal scale considered for the scaling

analysis of the RGPS data is not an exact time i.e. 3-days but rather a temporal bin between 2.5 and 3.5 days.

From the model simulation, positions of the drifters are saved every 6 hours (from midnight to midnight). Therefore, the above-mentioned problem does not exist in this case. Our method allows for an accurate comparison (over a quasi equal time window) of the simulated and observed deformation rates at a given temporal scale T.

The changed the text of the manuscript, which now reads: "The RGPS trajectories are not sampled at regular time intervals, contrary to the model, due to the irregular interval between two satellite orbits. The mean sampling is of about 3 days, and 90% of trajectories are sampled with a frequency between 2.5 and 3 days. Because sea ice deformation depends on the time scale (see results of section \ref{sec:temporal_scaling}) one should make sure to use similar sampling times for the observations and the model when computing and comparing deformation rates estimates. To deal with this issue, we performed a sub-sampling of the RGPS trajectory dataset using a nearest-neighbour interpolation of the original positions at 3-day intervals, but only when the RGPS drifter's position is available within plus or minus 6 hours around the interpolation target time. The positions simulated by the model, that are outputted every 3 hours from midnight to midnight each day, are taken to match the sub-sampled RGPS time series obtained as described above."

- Lines 10-15: Because trajectories are eventually removed from your analysis by filtering? Or why else is this the case?

No, this is only because we analyse one single winter period (2006-2007), and we therefore face with a limited number of synchronous trajectories that are forming the triplets we then track over time and use for the deformation calculations. We acknowledge though that using all the years covered by the RGPS dataset would resolve the potential lack of robustness of the statistics. However, we found it was not necessary to do so in order to obtain significant and robust power law scaling of the simulated/observed deformation rates.

- Line 17: "the 3-day shear [...] for the same period of 7 days" How do you get the strain rates on a 7-day period if they are the 3-day strain rates?

It seems like there is a misunderstanding here. What we do here is to pick up all the calculated 3-day strain rates that are observed/or simulated within the time period of 7 days, and we plot them all together to build the map shown on figure 1, assuming they are synchronous in time. This assumption is not strictly exact but allows to improve the spatial coverage of the data shown on the map, and thus for illustration at least, improve visually the field of deformation rates shown on the map.

We modified the text as follows "Note that to obtain a better spatial coverage, these maps are showing all simulated or observed deformation rates for the period of 7 days centered on 4 February 2007" and changed the caption accordingly.

- Line 19: Technically, what you are showing is not the cumulative probability distribution (CDF), but the complementary cumulative distribution function (CCDF), i.e. the probability of having a value greater than a given strain rate. Please correct.

We now show PDFs instead

Also, why choose to show the CCDF instead of the PDF as in previous studies? It would be interesting to show here the PDFs of shear and divergence since it is the first time it would be shown for the MEB rheology in this configuration.

We now plot the distributions as PDFs as suggested by the reviewer for the total, shear and absolute divergence rates.

- Line 20-21: Please discuss the fact that the probability distribution for your model is always greater than that of RGPS. What does this imply?

The distributions of total deformation rates that we show in figure 2 suggest that our model rather slightly under-estimate the largest values compared to the RGPS.

- Line 21: If we assume a power law probability distribution function (PDF) that goes like P(x) -> xˆ{-alpha}, then the CDF (or CCDF) would decay like C(x) -> xˆ{-alpha+1}. Hence, if you find a slope of -3 for the CCDF of both your model and RGPS, it means that the PDFs for both data sets decay with a slope of -4, which according to Sornette (2006), implies that the PDFs slowly converge to Gaussian distributions (or that they are in the "Gaussian basin of attraction") and therefore your argument following in the text does not hold... Please address this.

This is correct. We made a mistake in comparing the slope of the tails of the CCDF to -3, which is supposed to be meaningful value when looking at PDFs, not CCDFs. As said in our response above, we now show in figure 2 the PDFs instead of the CCFD, and we therefore still compare the slope of the tail with the power law with a -3 exponent.

We corrected the text accordingly and removed any mention to the Gaussian basin of attraction. However, we still state that higher order moments than the mean (variance and skewness) can be calculated and are necessary to better describe the distributions of sea ice deformation rates. We stop at q=3 though, because a transition is observed around q_{critic}=2.5 to 3: indeed, because the PDF decays as x^{-3}, moments of order $q>q_{critic}$ diverge (Schertzer and Lovejoy, J. Geophys. Res. **92**, 9693, 1987)

Page 14:

- Line 3: Stern et al. (2018) suggest to use Maximum Likelihood Estimators to determine power-law exponents and test those with a goodness-of-the-fit test (Clauset et al., 2009).

We probably wrongly expressed ourselves here. Indeed, Stern et al. (2018) recommend the use of MLE as the best solution to estimate power-law exponents, but also they also say that already using binned data in log-log space allows to get “reasonably accurate estimates”. We therefore changed the text in the revised manuscript to make this clearer as follows: “We use logarithmically spaced bins and applied an ordinary least square method to the binned data in log-log space to get reasonably accurate estimate of the power-law fits (Stern et al., 2018)”.

- Lines 18-19: Defined as in Bouillon and Rampal (2015), these bars are rather representing the "goodness of the linear fit" rather than an actual error on the values you are comparing. It would be much more useful (in terms of comparing the model to observations) to compute the error on your observed and simulated deformation rates given the known error on the trajectory positions (see for example Lindsay and Stern, 2003) and then the ensuing error on your scaling analysis. Only then can you conclude that the structure functions are "equal within their margin of error".

Yes, you are right that this would allow us to be more conclusive. This is something we will probably do in the future when performing such analysis. Also, one may note that putting “error” bars on the structure functions is something very rarely done in the literature. As long as the compared curves are very close to each other and/or showing clear deviation from the linear model, these “error” bars. We removed the term error bars and replace it with just saying that “the plotted *bars* represent the goodness of the power law fit”.

- Line 21: "... the scaling is clearly multi-fractal, as no linear function can be contained within the error bars." I can pass a line through the origin and through all the "error bars" for the model values. Please remove.

Your remark is valid. However, please note that we discovered a bug in our analysis script calculating the deformation rates and the moments for the model. The updated figure 3 now shows the results after correction. Our statement about saying that no line can pass through the origin and through all the “error bars” is consistent with this new result. But as commented by the reviewer above, the “error bars” are not derived statistically and should rather be called “bars” representing upper-bound estimates of the goodness of the power law fits. So we decided to remove “as no linear function can be contained within the error bars” from our sentence to be more accurate.

- Line 22: "applying a quadratic fit" Please provide the quadratic fit parameters for both RGPS and the model, either here or on the figure.
Good suggestion. Done.

Page 15:
- Line 12-13: Mean curvatures of 0.07 and 0.08 seem quite low to consider this a "clear" signature of multi-fractality... Again, it would be necessary to have the error on these values to know if it is significant or not.
This very low values obtained for the curvature of the structure functions were in fact due to the mistake we found in our analysis script and that we mentioned above already, affecting the spatial scaling in particular. We find that the values of the curvature are now 0.11 and 0.13 (averaged over the winter period) for the model and the RGPS respectively. This means that the model is still lower on average compared to the RGPS. We chose to remove the plot showing the time series of the curvature values as we realized that it does not add any substantial information to the paper.
We note also that such fairly low values of curvatures for multi-fractality of a field in the spatial domain are typical in geophysics (e.g. in the range 0.05-0.15 for the wind, cloud radiances, topography...), although it can be much larger (0.25-0.7 for rain and turbulent fluxes) (See e.g. Lovejoy, S., and D. Schertzer (2007), Scale, Scaling and Multifractals in Geophysics: Twenty Years on, in *Nonlinear Dynamics in Geosciences*, vol. 59, pp. 311–337, Springer, New York, NY)

- Line 16: "beta decreases with increasing T" This is not very clear from figures 5 and 6... In fact, from the right panel in figure 5, it looks more like beta is increasing with increasing T for q=1. Please add a log-log plot of beta vs T for the different values of q for both RGPS and the model, similar to what is done in figures 5 and 7 in Hutter et al. (2018).
The dependance is now clearer on our new results. We do agree though that the scaling exponents for the mean are very weakly dependent on the temporal scale considered.

- Line 20: "This property is for the first time shown from a sea ice model simulation." This is not true. See Figure 7 in Hutter et al. (2018).
Yes, indeed Hutter et al. (2018) are showing some coupling as well. We in fact meant that this coupling is obtained from sea ice model simulation ran at a relatively coarse resolution. We changed the sentence as follows: “To our knowledge, this is the first time such coupling is obtained from a sea ice model simulation ran at such relatively coarse spatial resolution.”

Moreover, is this coupling really significant, as the "error bars" overlap for all temporal scales (for each moment respectively)? If you say that the structure functions for both RGPS and the model are equal in Figure 3, then I would also say they are equal here in Figure 5 for all time scales, and we therefore cannot conclude to a significant coupling.

We think that our new results are now showing more clearly that this coupling is significant, especially if we consider as the reviewer suggests that our “error bars” should not be considered as statistical error bars, but more as upper bound estimates of the goodness of the power fits.

Page 16:

- Lines 3-4: A few more words might be helpful here to understand this offset in the model curvature: Is MEB leading to damage in the ice cover everywhere and, therefore, evenly distributed events with no preferred regions of deformation?

The offset in the model curvature is not readily explained. Indeed we state in lines 5-6: “The reason for this discrepancy should be further explored but is out of scope of the present paper”. To underline this, and to make clearer the last statement of the paragraph we have removed the sentence “This may come from the fact that the highest deformation events are too evenly distributed over the Arctic region in the simulation compared to the observations”.

- Line 20: "This means that the proportion of extreme deformation events compared to lower ones is too small or that their values are too low in the simulation." The CCDF for the shear deformations in Figure 1 actually shows that the probability of having larger deformations is higher in the model than for RGPS, no? If you show the PDFs of shear and divergence, it would probably help to clarify this.

We now show the distributions of total deformation rates in Figure 2 and these show that the model actually under-estimate the largest values, which is consistent with the statement above that the reviewer is picking up: “This means that the proportion of extreme deformation events compared to lower ones is too small or that their values are too low in the simulation.”. The previous inconsistency spotted by the reviewer was in fact due to the fact that we were looking at the shear deformation rates distribution in Figure 1, whereas the scaling analysis is performed on the total deformation rates.

Page 17:

- Line 12-15: It is not clear from Figures 9 and 10 that alpha is decreasing for increasing L. Please add a plot of alpha vs L for q=1,2,3. Again, the question of whether this coupling is significant if all alpha(q=1,2) lie within the errorbars of alpha(q=1,2) arises.

We think our new results show this more clearly now. It is however true that the coupling is not as strong for the mean, compared to the second and third moments.

- Line 23: "reproduces correctly the distribution of sea ice deformation rates" Please show PDFs of shear and divergence to affirm this.

We now show the PDF of shear and divergence as well as the total deformation rates. We think it shows clearly enough that the model reproduces correctly the distribution of all invariant of sea ice deformation. We kept the sentence in the revised manuscript.

Page 18:

- Line 9: "a threshold mechanism" Is that the damage parametrization?

Yes, the fact of relating the large scale motion and deformation of the ice to a variable (here the damage) which evolves at the scale of one single element of the mesh in a highly non-linear way only and only when a criterion is fulfilled (i.e. sort of step function/discontinuous behavior).

- Line 14: You show that your model reproduces some of the observed scaling characteristics, but you have not shown that it does because your model includes the "ingredients" mentioned above. The configuration of the model (i.e. forcing, strength parameters, etc.) as well as the Lagrangian mesh instead of an Eulerian grid also have the potential for generating/influencing these behaviors, and it is not clear yet to which model parametrization or configuration ingredients these behaviors are due.

Yes, we have not shown causality between including these ingredients in the MEB rheological framework and the reproduced scaling characteristics. So we rephrased this paragraph to avoid confusing the reader and to not let him/her think that we say these ingredients are the only way to generate such scaling properties..

That said, we would like to stress that a quite large community of geophysicists have been working on the deformation of the Earth crust over the last 30 years, on its scaling invariance properties and their physical origin. Today's understanding in this community is that the ingredients we list here, and that the MEB framework is using, are i) playing a role in the observed Earth crust dynamics, and ii) are all making physical sense as they can be related to the brittle nature of a geophysical solid, where earthquakes/icequakes allow the system to release energy when a critical internal stress state is reached, where the associated elastic waves allow to redistribute this energy within the system and over long distances, where this redistribution results in propagation of fractures associated with local "mechanical damage" of the material that is keeping the memory of this critical process in the system over long time scales, etc… .

So part of these scaling are of course probably inherited from e.g. the atmospheric/oceanic forcing (although this has never been proved so far), but the large range of spatial and temporal scales over which these scaling are observed for the sea ice (down to a couple of minutes according to Oikkonnen et al. 2017) let us think that, at least at the small time scales, some of the ingredients mentioned in our paper and present in the MEB rheology may play a role if not to be the main source.

What we therefore aim at with this study (and more generally with the development of neXtSIM) is to show that a rheological framework including meaningful (in the physical sense) ingredients in a context of geophysical solid dynamics can reproduce some non-trivial characteristics of the observed sea ice dynamics, and therefore that these may be reproduced in the model for reasons that are mechanically relevant.

- Line 16: "the spatial scaling [...] holds down to the nominal resolution of the mesh" and just after "It means that neXtSIM does not need to be run at higher spatial resolution in order to resolve the presence of linear kinematic features..." I am not sure that the first sentence justifies the second one... For example, Spreen et al. (2017) and Bouchat and Tremblay (2017), both show that VP models at 9 km and 10 km can also have a spatial scaling that "holds down" to 10 km (ie. the nominal resolution), but you would still need to run the model at higher resolution if you want to resolve finer structures in the sea-ice fields because the models are represented on Eulerian grids. In the case of your model, you might better resolve LKFs because you are using a Lagrangian mesh, which represents discontinuities more accurately, not necessarily because of the scaling of deformations.

We don't agree that the examples of Spreen et al. (2017) and Bouchat and Tremblay (2017) show that their scaling holds down to the nominal resolution. In the case of Spreen et al they show very weak scaling, with exponent of -0.08 +/- 0.05 for winter time shear for the 9 km resolution run. Their figure 8 can even be interpreted such that there is even less, or no scaling from 10 km to about 100 or 200 km and a stronger scaling after that. The results of Bouchat and Tremblay suffer much the same problem, with very weak scaling and a large scatter of the points. Bouchat and Tremblay don't provide error or uncertainty estimates for their exponents, but one could again argue that there's very weak scaling in the 10 to 100 km range and a slightly stronger scaling above that. The point here is that with such weak scaling as Spreen et al and Bouchat and Tremblay have it is very difficult to see if it flattens out towards the nominal scale or not. What is worse, one could argue that their scaling actually starts flattening out around 100 km, not when nearing 10 km. The only publications showing a reasonably strong scaling with a model other than neXtSIM is that of Hutter et al (2018, 2019), but unfortunately the authors chose not to show how their model scales below the 10 km scale, even if the model is run at a much higher resolution.

The papers of Spreen et al and Brouchat and Tremblay aside, the point here is that a correct scaling down to the nominal resolution means that the correct statistical behaviour at a length scale of 10 km is reproduced when the model runs at a 10 km resolution. So in order to study the statistics at 10 km we don't need to run the model at e.g. 1 km. This is irrespective of the advection scheme as it relates strictly to the formation of features, not their preservation over longer time. Indeed, Girard et al. (2011) ran their model for only 72 hours with no advection. It was not accurate to say that since the spatial scaling holds down to the nominal resolution then we can better resolve linear kinematic features, however, and we have modified the text to make clearer the distinction between reproducing the statistical behaviour at the nominal scale (which we do) and reproducing related features (such as LKFs - this is not explored here).

- Line 20: Add reference to Hutter et al. (2018)? You seem to be indirectly referring to this study.
Done

- Lines 21-28: This should be moved to the results section.
We argue that these lines should stay where they are. Lines 28 of page 18 to 8 of page 19 are a discussion of the results. Lines 21-28 are indeed more descriptive, but they nonetheless bring together various elements of our results necessary for the discussion that follows. Lines 21-28 could as such be moved to the results section, but lines 18-8 would then not make any sense. To make this point clearer we have made lines 21 to 8 a paragraph of their own.

Page 20:
- Line 12-13: I disagree. See comment for Page 15, Line 20.
Fine, if you want. We removed "for the first time" in the sentence.

- Lines 21-25: This is a too strong statement that depends a lot on what the model is used for. There are applications where heterogeneity and intermittency of deformation are important (i.e. regional and short range forecasting of ice conditions) but there are also larger scale applications where other parameters are more relevant. Either remove this statement or give specific application areas where this is needed.
We slightly rephrased this statement to make it weaker. Here is how it reads now: "As the mono versus multi-fractal character of the scaling of deformation rates is the discriminating factor for the heterogeneity and intermittency of the deformation, we suggest that a multi-fractal scaling analysis could be considered as a meaningful validation step before further analyzing sea ice model outputs that could be influenced by sea ice dynamics."

Page 22:
- Line 10: In Dansereau et al. (2016), d=1 for undamaged and d=0 for completely damaged ice. Please indicate that you use the reverse definition.
Done

Page 23:
- Lines 11-13: g(H) is not defined.
- Equation (A13) and (A14): The prime variables have not been defined. in (A14), shouldn't it be sigma_1_prime and sigma_2_prime instead?

g(H) is superfluous here and we've removed it. The primed variables were supposed to indicate values after updating the damage, but this was neither clear nor correctly done. We've reformulated the text to make it clearer and remove the primed variables.

Page 24:

- As it seems that the implementation of this 3-thickness categories differs from Stern and Rothrock (1995), I would like to have a bit more details on how it is done/defined and how it is different from what was suggested Stern and Rothrock (1995).

Yes, there are some differences. Things in common:

- 3-layer model
- Thin ice is ridged first, before thick ice.

Differences:

- No additional open water source terms.
- Thick ice cannot return to being thin ice - our thin ice is perhaps more correctly described as young ice.
- We do not use the formulation of Gray & Morland (1994) to keep our total ice concentration <= 1, but only redistribute ice volume if concentration > 1.

-For example, please explain the addition of the divergence term in the evolution equations and a term for ridging for the thin ice category as well.

The divergence term is fairly standard, $\partial\phi/\partial t + \partial/\partial x(u\phi) + \partial/\partial y(v\phi) = D\phi/Dt + \phi(\partial u/\partial x + \partial v/\partial y)$; in our Lagrangian framework it represents the fact that ϕ should increase if the area of the mesh element decreases and vice versa.

The thin ice ridging term is covered by (A16) and implicitly by the mechanical redistribution procedure.

- Please also clarify if A and H are the total ice concentration and volume per unit area? i.e. A = A_thin + A_thick and H = H_thin + H_thick?

We clarified this in the appendix and table 2: total conc = A + A_t, total volume per unit area = H+H_t, total snow volume per unit area = h_s+h_{s,t}.

- Lines 16-18: "Thin ice thickness is considered to be uniformly distributed between hmin and hmax", do you mean linearly distributed? Why does that put a maximum bound on the total ice volume per area? Maybe here it should be "H_t_min = A_t * h_min" and "H_t_max = A_t * (h_min+h_max)/2" instead?

The reviewer is correct we originally meant to say it was linearly distributed. However we have since realised that there is a simpler, equivalent, formulation with a uniform distribution and added some text and some equations to elaborate on the thermodynamic transfer of ice from thin to thick ice category.

Page 26:

- Equation (A23): Please explain why you introduce this delta_A variable and what is the purpose of the "aspect ratio parameter" zeta, and what a value of 10 implies.

Beginning with the other variables, we treat ice and snow volume in a conservative manner. ΔA will be used to increase the concentration of thick ice A in a similar way, but non-conservatively: by increasing A by an amount less than $A_t - A'_t$ (through the parameter ζ), we are preferring to increase the absolute thickness of the thick ice more than it would be if area was conserved - the factor of extra increase is approximately $1 + (1 - \frac{1}{\zeta})\frac{A_t - A'_t}{A + A_t - A'_t}$.

- Line 8: Shouldn't more ridging also affect the value of H^{n+1}?

No, the ice volume should not change (only the absolute thickness would increase).

Figure 1: - I would switch for PDFs and also add a panel with divergence distributions.
We prefer to keep showing the Probability of exceedance (or CCDF) as it makes the comparison of the tails of the distribution clearer, as in Marsan et al., 2004. Note that we have now switched figure 1 and figure 2 (the fields of total deformation rates first in figure 1 and the corresponding distributions in figure 2), following the suggestion of Reviewer 2.

Figure 2: - Please also show the divergence fields.
We are now showing the **total** deformation rates for the model and the RGPS, instead of the **shear** deformation rates. For consistency reasons with the rest of the paper presenting statistics and analysis for the total deformation rates, as well as for simplicity, we chose to keep considering and showing the total deformation rates here, as throughout the paper.
Both the captions and the text are now corrected accordingly.

Figure 3: - The left panel y-axis shows the scaling for eps_tot, however, in the caption it is written that you are showing the scaling and the structure for the shear deformation rate... Please show the scaling and the structure function for eps_tot instead.
Thanks for spotting this. It was in fact a typo in the caption. What we showed in this figure was already for the total deformation rates.

Figures 5 & 6: - You could group these two figures for ease of comparison between the model and RGPS observations.
We tried this but it was at the cost of lack of visibility. We therefore chose to separate these two figures.

Figure 8:
- Caption, Line 3: Normalized moments have not been defined in the text.
Indeed. We are not showing the normalized moments, but the moments. It was just a typo in the caption. This is now corrected.

Figures 9 & 10: - You could group these two figures for ease of comparison between the model and RGPS observations.
Same answer as for the figure 5 & 6 (see above)

== TECHNICAL CORRECTIONS

Page 2:
- Line 7: - Delete "for the first time"
Done.

- Line 16-17: "e.g.," should come before enumerating the references.
Corrected

Page 3:
- Line 2: Replace "kinematic linear features" with "Linear Kinematic Features"
Done

- Line 6: Delete "levels of"
Done

- Line 7: "Coon et al., 2007" should be in parenthesis
Corrected

- Line 9: Add a coma after (Kwok, 2001), ie: "(Kwok, 2001), and permanent..."
Done

- Line 13: Delete "such as" and replace with "... of the deformation rate invariants (i.e. shear and divergence) and of the total deformation rates, which..."
Done

- Line 26: I think there is a part missing in this sentence. Maybe add "applied to observed deformation fields derived from satellite imagery" before "(e.g. Lindsay..." or something like that?
We changed the sentence with the following one: "Estimated using coarse-graining analysis (e.g. Lindsay 2003, Marsan et al, 2004, Bouillon and Rampal 2015b) or dispersion analysis of pair of buoys (Rampal et al. 2008), the *mean* sea ice deformation rate has been shown to vary with the spatial scale, *L*, and temporal scale of observation, *T*,..."

- Line 27: Replace "or pair of buoys dispersion analysis" with "or by dispersion analysis of pair of buoys"
Done (See previous response)

Page 4:
- Line 4: Add "The scaling exponents..."
Done

- Line 9-10: Rewrite "...to a homogeneous deformation, and alpha=1 to a single, temporally isolated deformation event."
We instead rewrote the sentence as follows: "In the space domain, beta = 0 characterizes the homogeneous deformation of an elastic solid or viscous fluid, i.e., a deformation that does not depend on the spatial scale, while beta = 2, i.e. the topological dimension for the 2D-like sea ice cover, corresponds to a single "point" concentrating all of the deformation in an otherwise undeformed material (Rampal et al., 2008)"

- Line 16: "approximated" –> "modeled as"
We changed with "assumed" instead
and "relevant for Arctic system simulations"?
Done

- Line 18: Delete "out of the Gaussian basin of attraction" (see specific comment for Page 3, Line 14).
Done (see which changes we applied in our response to the corresponding "specific comment" of the reviewer listed above)

- Line 25: Replace "beta" with "the scaling exponents beta and alpha"
Done

- Line 27: Delete "indeed"
Done

Page 5:

- Line 4: "... linear structure function, i.e., no curvature, ..." replace with "... linear structure function, i.e. no curvature or equivalently a=0 or b=0, ..."

Done

- Line 6: Replace "For both coefficients..." with "In the case where both coefficients..."

Done

- Line 7: Add "therefore" between "distribution" and "increase", i.e "...of the distribution therefore increase..."

Done

- Line 12-13: Replace "have shown" with "show"

Done

Page 6:

- Line 7: "... in the deformation and related characteristics of sea ice" Not clear. Please reformulate.

Sorry, we could not find which sentence the reviewer is referring to.

- Lines 21-23: This sentence is not clear. Please reformulate.

We reformulated the sentence as follows: “In the absence of a characteristic space/time scale for the sea ice deformation and with the knowledge that the scaling invariance holds beyond the space/time nominal resolution of typical regional and global model's grids, perhaps the best a continuum framework for sea ice modelling can do is to correctly reproduce the statistics of deformation from the smallest available (or *nominal*) scales that can be resolved, i.e. at the resolution of the grid in space and for the model time step in time, to the largest scales as possible, i.e. the size of the Arctic basin and the time scale of a season.”

Page 7:

- Line 1: Replace "the first part of the paper" with "Section 1 and section 2 of the paper" and delete "(section 2)" in line 2.

Done

- Line 2: Replace "The second part" with "Section 3" and remove "(Section 3)" in line 4.

Done

- Line 10: Replace "(Amitrano et al., 1999)" with "Amitrano et al. (1999)"

Done

- Line 17: Rewrite "In particular, it was shown that the simulated deformation rates..."

Done

- Line 18: Add "... in space only".

We disagree with the reviewer here. A temporal scaling was also performed in this paper (see figure 12) showing good agreement of the temporal scaling exponent.

Page 8:

- Line 8: Replace "entering" with "of"

Done

- Line 9: Replace "Appendix" with "appendices"

Done

- Line 15: Replace "length of the vertices" with "distance between the vertices"

We rather changed for: “i.e. mean length of the edges of the triangular elements”

- Line 24: Remove "the applied", and all of "the" in front of the quantities enumerated
Done

Page 10:
- Line 1: Replace "displacement" with "ice motion"
Done

Page 12:
- Line 18: Add "... 30 degrees or less".
Done
- Line 19: Replace "as the model is" with "contrary to the model"
Done
- Line 23: "affect" should be "affects"
Done
- Line 23: "sub- sampling" Do you mean interpolation?
Yes. We slightly change the text with the following sentence: “The positions simulated by the model are taken to match the sub-sampled RGPS time series obtained as described above.”

Page 13:
- Line 6: "we therefore chose to..." You do not chose, you can’t go below 3 days given that this is the smallest time scale you have for your dataset.
Yes. We changed the sentence with: “we therefore restrict ourselves to time scales equal or greater than 3 days”.
- Line 7: Remove "on the whole"
Done
- Line 17: Maybe relabel Figure 2 to Figure 1 since you are referring to it first?
Yes. Done
- Line 17: Replace "3-days" with "3-day"
Done

Page 14:
- Line 2: Add "... spatial scaling analysis for a T= 3 days temporal scale..."
Done.
- Line 12: Add "our choice of mechanical parameters values (e.g. Bouchat and Tremblay, 2017)"
Reference added.
- Line 22: Replace "a quadratic fit to the data (in the least squared sense)" with "a least-square quadratic fit to the data"
Done
- Line 25: Add "... simulated deformation fields is consistent..."
Done
- Line 27: Add "... the value of the spatial scaling exponent beta..."
Done
- Line 27: "for the mean obtained for the successive and contiguous snapshots throughout the winter" This is not clear. The mean = mean deformations , i.e. q=1? Please rewrite.
Yes, you understood correctly. The sentence has been rewritten as follows: “Using successive and contiguous *snapshots* throughout the winter, a time-series of the value of the spatial scaling exponent beta obtained for the mean deformation rates is calculated, and shown on Figure…”.

Page 15:

- Line 1: Replace "the scaling exponent varies" with "the spatial scaling exponent varies"

Done

- Line 4: Replace "for the mean" with "for the mean deformation rates (i.e. q=1)"

Done

- Line 5: Add "... which is also the value..."

Done

- Line 10: Add "...that the observed and simulated curvature values..."

Done

- Line 21: "The origin of this coupling has been previously proposed to be linked to the complex correlation patterns related to chain triggering of ice-quakes." Please add reference for this.

We added the Marsan and Weiss (2010) reference at this exact location in the text:

Marsan, D., and J. Weiss (2010), Space/time coupling in brittle deformation at geophysical scales, *Earth Planet. Sci. Lett.*, *296*(3-4), 353–359, doi:10.1016/j.epsl.2010.05.019.

- Line 24: Add "... the multi-fractal character of the spatial scaling (i.e. the curvature of beta(q)) for both RGPS and the model when..."

Done

Page 16:

- Line 9: Remove "robust" and "very similar" since you then discuss how it differs for the q=3.

We instead changed the text to make clear the scaling are very similar for the two first moments of the distributions:

"We see a robust and very similar power-law scaling for the two first moments ($q=1,2$) for both the model and observations…"

And we kept the adjective "robust" in the sentence because the power law fits are indeed very "robust" in a statistical sense.

- Line 16: Replace "in this recent study" with "by Oikkonen et al. (2017)"

Done

- Lines 17-18: Remove "(gray, dark and cyan top curves in the left panel of Fig. 8)"

Done

Page 17:

- Line 4: "virtually perfect" please change to a more sober wording. For example, the values for q=2 are not "perfectly" matching.

We changed the wording as suggested, with "remarkably good".

- Line 4: Rewrite "The curvature of the quadratic functions alpha(q) are 0.11 for..."

Done

- Line 7: "This seems to argue that..." Weird wording. Please rephrase.

Yes indeed. We changed the beginning of this sentence with "This suggests that…"

Page 19:

- Line 18: Replace "." after "distribution" by a coma, and change "A proper..." for "a proper..."

Done

- Line 24: Replace "concurrent" with "parallel"?

Done

Page 20:

- Line 2: Add "from RGPS observations..."

Done

- Line 13: Remove "for the first time" and "by a model"

We followed reviewer's suggestion here, and removed "for the first time".

However, we guess that the reviewer is making reference to the paper by Hutter et al. 2018, which is indeed showing a coupling of the scaling in space and time. But we would like to stress that this coupling is "very weak" as the authors acknowledge themselves in this paper, which may be the consequence of the very weak scaling reproduced by their model in the temporal domain.

Page 21:

- Line 16: Replace "thick ice thickness" with "thick-ice thickness"

Done

Page 22:

- Line 15: Why not write -c* with c* =20 as done in Hibler (1979)? And you could put (A6) back in (A5) to save space.

We followed the reviewer suggestion here, changing c* to -c* in equation A6. We however prefer to keep A5 and A6 separated.

Figure 1:

- Please add legend in the figure for ease of comparison - Add the fit exponents on the figure or in the caption, for both model and RGPS.

Done

- Caption, Line 2: Add "...and the RGPS observations"

Done

Figure 2:

- There seems to be a plotting issue since some of the triangles are touching the land boundaries (e.g. on the Alaskan coast), but you mention in the manuscript that you filter out trajectories that are 100 km or closer to land. Please correct.

We indeed filter out the trajectories that are 100 km or closer to the land, but only for the scaling analyses we performed. However, this figure is mostly for illustration, and therefore we show the whole dataset available from the RGPS (except if the triangles are too deformed/large), and masked the model data to match spatially those observations. We added the following sentence in the caption: "The model field is masked to match spatially with the RGPS data coverage."

- Please add "RGPS" and "Model" on top of the panels.

Done

- Caption: Add that the green lines are the model's open boundaries.

There removed the green lines from the figure.

Figure 3:

- Caption, Line 2: Replace "than in the RGPS dataset" with "and RGPS dataset"

Done

- Caption, Line 7, "local scaling exponents" Not clear. Use a similar wording as in Bouillon and Rampal (2015).

We changed the caption accordingly using the same wording as in Bouillon et al. 2015, as suggested.

- Caption: Use the same wording for the caption as for Figure 8 (with the suggested corrections).
Done

Figure 4:
- Please add legend in the figure for ease of comparison
Done
- Caption, Line 1: Replace "power scaling exponents" with "spatial scaling exponents for the average total deformation (i.e. q=1)"
Done
- Caption: Add something like "calculated for individual snapshots, i.e. at a temporal scale of T = 3 days"
Done

Figure 5:
- Please use same y-axis for left panels in Figures 3,5,6,8,9,10,12 for ease of comparison. - Please use same y-axis for right panels in Figures 3,5,6,12
We thought about doing as the reviewer suggested and tried it when preparing the manuscript but although it eases the comparison between figures, it also significantly degrade the visibility of each individual figure. We thus prefer to keep the axes as they are.

Figure 7:
- Caption: Add "..for the RGPS observations..."
Done.

Figure 8:
- Caption, Line 3: Replace "distributions of the deformation rate" with "distributions of the total deformation rate"
Done
- Caption, Line 4: Switch "for the observations" with "for the model" later in the sentence, and indicate that the values for T=3hrs to 1 day are taken from Oikkonen et al. (2017) in the caption as well.
Done
- Caption, Line 6: Remove "The dashed lines are extrapolation for the smallest scales" There are no dashed lines.
Done
- Caption, Line 7: Replace "observation" with "RGPS observations"
Done

Figure 10:
- Caption: Add "..for the RGPS observations..."
Done

==

REFERENCES

Bouchat, A., and B. Tremblay (2017), Using sea-ice deformation fields to constrain the mechanical strength parameters of geophysical sea ice, J. Geophys. Res. Oceans, 122, doi:10.1002/2017JC013020.
Clauset, A, Shalizi, CR and Newman, MEJ. 2009. Powerlaw distributions in empirical data. SIAM Rev 51(4): 661–703, https://doi.org/10.1137/070710111
Hutter, N., Zampieri, L., and Losch, M.: Leads and ridges in Arctic sea ice from RGPS data and a new tracking algorithm, The Cryosphere, 13, 627-645, https://doi.org/10.5194/tc-13-627-2019, 2019

Hutter, N., Losch, M., & Menemenlis, D. (2018). Scaling properties of arctic sea
ice deformation in a highâĂŘresolution viscousâĂŘplastic sea ice model and in satellite observations. Journal of Geophysical Research: Oceans, 123, 672–687. https://doi.org/10.1002/2017JC013119

Lindsay, R.W. and H.L. Stern, 2003: The RADARSAT Geophysical Pro- cessor System: Quality of Sea Ice Trajectory and Deformation Estimates. J. Atmos. Oceanic Technol., 20, 1333–1347, https://doi.org/10.1175/1520- 0426(2003)020<1333:TRGPSQ>2.0.CO;2

Sornette, D.: Power Law Distributions, pp. 93–121, Springer Berlin Heidelberg, Berlin, Heidelberg, doi:10.1007/3-540-33182-4_4, https://doi.org/10.1007/3-540-33182-4_4, 2006.

Spreen, G., Kwok, R., Menemenlis, D., and Nguyen, A. T.: Sea-ice deformation in a coupled ocean–sea-ice model and in satellite remote sensing data, The Cryosphere, 11, 1553-1573, https://doi.org/10.5194/tc-11-1553-2017, 2017.

Weiss, J. Surveys in Geophysics (2003) 24: 185. https://doi.org/10.1023/A:1023293117309