Anonymous Referee #2

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General comments

This paper aims at validating the basic behavior of deformation rate in the neXtSIM sea ice model which is based on the Maxwell-Elasto-Brittle rheology, focusing on the scaling properties in space and time. The model domain was the whole Arctic Ocean and the coarse graining method was used for scaling analysis with the drifters' data in the model. For validation data, the Lagrangian displacement data produced from RADARSAT Geophysical Processor System (RGPS) were used. Through scaling analysis, it was shown that the multi-fractal properties can be reproduced for the first time in the numerical sea ice model. Besides, the statistical properties of the first, second, and third moments of deformation rates at temporal scales of 3 days to 96 days and spatial scales of 7.5 km to 700 km were shown to be mostly consistent with the observations. In conclusion, since the fundamental properties were validated, they suggest that the neXtSIM model could be used as a proper tool to further study the physical meaning of the processes related to deformation. Considering that it is still a big challenge to reproduce the rapid thinning trend of ice thickness distribution in the Arctic Ocean in the numerical sea ice model and the need to improve the deformation processes in the model has been recognized for a long time, the topic of this paper is timely, and the results of this paper will provide insightful implications. Overall, I feel that this paper is an elaborate and nice work, and this approach is indispensable to improve our understanding of the dynamic behavior of sea ice. Therefore, I believe this work will contribute to the development of sea ice dynamics, especially for the parameterization of the model, related to the deformation.

First of all, we would like to thank the reviewer for this very positive evaluation and minor, though important/well spotted comments/points. Please find below (in red) our answers.

My comments, which might come from the lack of my knowledge about mathematics, are limited to minor points as follows:

1) Regarding the description of exponents, α and β (Eqs. 4 and 5), could you please explain more about why these exponents can be expressed as a quadratic equation of the moment parameter (q)? To be honest, I could not follow the subsequent paragraph (P5L4-11) completely. To my understanding, multi-fractal means the geometric properties that contain various dimensions of fractals. If this is correct, why can the curvature of the exponents as a function of q be an indicator of multi-fractal which discriminates from mono-fractal? In my mind, if I could accept this concept, the manuscript would have become much more understandable to me.

In order to make the description of the concepts of mono versus multi-fractal scaling hopefully more easy to follow by the reviewer or any other reader of our article, we slightly reformulated the paragraph picked up by the reviewer as follows:

"In the case of a linear structure function, i.e., no curvature, the amount of localization of large and small deformation events is the same and the scaling is said to be *mono-fractal*.

When both coefficients *a* and *b* or *c* and *d* are positive the structure functions are quadratic and convex, meaning that the higher order moments of the distribution 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. 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 of the structure function, the stronger the *degree of multifractality* of the scaling."

2) Regarding the methodology of analysis, it is stated that you used the coarse-graining approach (P10L12). Is this the method described after P11L12? If so, it might make it readable when you insert "(shown later)" at the end of the sentence (P10L12).

No, we apologize for this confusion but we made a mistake saying that we used a coarse-graining approach. We instead used a buoy dispersion method, using triplets. This has been corrected in the revised version of our manuscript.

Besides, regarding the statement, "Only the trajectories that are common to both the simulation and RGPS dataset are considered in the calculation of the deformation and their statistics" (P10L22-24), I am a bit concerned whether this approach might affect the results by setting a bias in the calculation. I mean the data consistent with observations might have preferentially selected. If you can add some description about how much fraction of data were discarded by this method and show that this selection did not affect the result significantly, it would be appreciated.

Very few portions of the trajectories are discarded by this method, i.e. representing only about 2% of the total dataset. This selection does not affect in any case the results obtained, but was made in order to make our comparison between model and observations as much consistent and clean as possible. We slightly changed the sentence picked up by the reviewer by the following one:

"Only the trajectories spanning the same time periods in both the simulation and RGPS dataset are considered in the calculation of the deformation and their statistics. This selection lead to discarding about 1% only of the total trajectory dataset, and does not affect the results of the analyses presented in this paper. However we apply this selection in order to make our comparison between model and observations as much consistent and clean as possible."

3) Regarding the interpretation of the scaling analysis (Fig.5&6), it is stated that "We find that the estimated spatial scaling exponent, β, decreases with increasing T (Figure 5 and 6, left panels)" (P15L15-16). To my understanding, β corresponds to the slopes of the graphs. As far as looking at the left panels, however, the slopes appear not to be significantly different for all the values of T (3 days to 96 days) at least for q = 1. When looking at right panels, there certainly be a decreasing trend with the increase of T for q = 2 and 3. Thus, unless there is a physical meaning in the decreasing trend of β with the increase of T, it might be one idea to focus on the decrease of the multifractality of the spatial scaling with the increase of T. The similar discussion may apply for the last paragraph in section 4.2 (P17L11-21).

You are right. The statement of this sentence should have been more accurate. We changed it to the following one in the revised manuscript:

"We find that the estimated spatial scaling exponent, β, decreases with increasing T, although this behavior is only obvious for the moments of order 2 and 3"

We also lowered tone the next statement we made in the original manuscript by reformulating the text as follows:

"This seems to correspond to the existence of space-time coupling of the scaling properties of sea ice deformation. This property was originally suggested in Rampal(2008) from the result of their scaling analysis of buoy pairs dispersion, and was further explained in Marsan(2010) as being a possible characteristic of brittle deformation at geophysical scales."

Besides the fact that a dedicated and more throughout analysis is deserved to conclude on this point, we decided to keep mentioning this result in the revised manuscript because (i) we do not know of any other sea ice modeling study showing such result, (ii) we know this property has been already observed and documented in previous studies focusing on Earth crust dynamics, which are likely to be an analogue of sea ice dynamics. The text in the revised manuscript is now as follows:

“To our knowledge, this is the first time such result is shown from a sea ice model simulation. The origin of this coupling has been previously proposed to be linked to the complex correlation patterns related to chain triggering of ice-quakes. Further study is however needed to explore this hypothesis, which is out of the scope of this paper.

Besides, the additional description about the physical implications of the decrease of the multi-fractality would be appreciated if it is possible.
We guess that the reviewer is referring here to the fact that the model does not reproduce the observed heterogeneity of the sea ice deformation at large time scale. As we mentioned in the manuscript, this means that the largest deformation events are too evenly distributed over the Arctic basin in the model compared to the observations, and therefore the spatial “localization” is lost when considering statistics over large temporal window. One could blame the atmospheric forcing to not represent properly the extremes, or at least the presence and trajectories of polar lows in the Arctic region. Or one could also relate this to the healing mechanism (applied on the damage variable in the model) we currently use in the MEB rheology and that may be inadequately parameterized or tuned.
In order to stress these point more clearly, we added the following text in the revised manuscript:
“This could either be attributed to inaccurate position or lacking of extreme events in the atmospheric forcing, or to an inadequate or insufficiently tuned parameterization of the damage healing in the model.”

Specific comments:
*(P2L19-20) “Rothrock and Thorndike, 1984; Matsushita, 1985” & “Rothrock and Thorndike, 1980” are missing in the reference lists.
Thank you for spotting this. There are now in the reference list
*(P3L7-8) “Coon et al. (2007)” should be “(Coon et al., 2007)”
Corrected.
*(P12L18) Is there any meaning in the selection of 30 degrees?
The value itself has been taken arbitrarily so that every triangle selected for the analysis is not too much “distorted”, in the sense that when calculating the deformation of these triangles at a given spatial scale L=sqrt(surface area of the triangle), the homogeneity assumption we make about the deformation is actually making sense.
*(Figure 1&2) Considering the order of appearance in the manuscript, it would be preferable to exchange Figure 1 and 2.
You are right. We changed the order of these two figures.
*(P15L4) I think “0.2” should be “-0.2”.
Well spotted. This has been corrected.
*(Figure 8) It is stated that “The dashed lines are extrapolation for the smallest scales” in the caption. However, I could not see the dashed lines. Besides, “L=7.5km”, which appears in the upper left corner of the figure, is misleading. Please take it if not necessary.
Absolutely correct. This sentence mentioning the dash lines has been removed from the caption.

That is all. Faithfully yours.