The authors would like to thank Dr. Véronique Dansereau (the referee) for the invaluable comments. The following are the replies for each comment, together with specific revisions that are made. The original comments are in *green italic* font, and the revisions are highlighted in the revised manuscript in red.

Reply to comments:

This study compares EVP sea ice simulations at different spatial resolutions and with different level of convergence (i.e., number of subiterations of the solver) of the model solution. The comparison is made on the basis of the simulated deformation rates, which are analyzed in terms of their spatial distribution (fields), probability density function, cumulative probability density function and scaling properties, in space. Unlike recent studies which have used these metrics (e.g., Rampal et al., 2019, Hutter et al., 2018 and others), the authors use a climatology as their atmospheric forcing and analyze the simulated deformation rates after spin-up and stabilization of their modeled seasonal cycle. Daily and 3-days mean deformation rate fields are used as the basis of their analyses. Two days are compared, which correspond to different AO scenarios and hence circulation patterns in the Arctic.

Analyses of the statistical properties of the simulated deformation rates (i.e., the shape of the PDF) are performed for different level of convergence of the model solution and demonstrate that simulation at higher resolution require a larger number of subiteration of the EVP solver to obtain heavy-tailed PDFs that are indicative of spatial scaling properties in sea ice deformation.

While the climatological approach is different than previous studies and perhaps eases the comparison of the modeled dynamics under different atmospheric forcing scenarios, it also precludes a direct, quantitative comparison between the model and observations, which I believe is a weakness of this study, especially considering that the physical processes that could be responsible for the difference in the results between the two atmospheric scenarios analyzed are only vaguely discussed. It also makes it hard to put the study into a temporal context (e.g., sea ice thickness and extent fields cannot be related to a specific time period and especially do not seem representative of recent sea ice conditions).

Reply to the major comment: the authors thank the referee's comment on the comparability aspects of our study. We agree with the referee on the comparison of model output with observations. We would like to make the following clarifications. First, the purpose of the experiment methodology, including using atmospheric NYF dataset as well as coupling to slab ocean model, is to increase comparability across the resolution range. Second, since the NYF dataset is used to force the ice model, there is no direct comparable observation available. In turn, the comparison we have carried out are mainly between different (grid) resolutions, including deformation, scaling properties, and EVP convergence. Third, regarding the limited results and analysis as pointed out by the referee, for revisions, we extend the analysis beyond the two representative days to the whole winter, and carry out attribution study of the scaling properties and differences among the different resolutions. Lastly, this work is a first step of our effort in using the grid hierarchy with the ocean-ice coupled settings and inter-annually forced experiments. Both aspects (coupling to dynamic ocean and IAF experiments) would require a much more higher computational overhead (especially for the TS005 grid) which we look forward to exploration in the future.

I note that a lot of care has gone into building the grid and chosing the model (atmosphere and ocean) components. These choices are clearly explained and justified. However, while a lot of information is given on the model grid, surprisingly little information is given on the dynamics part of the sea ice component (thickness resdistribution scheme parameters, rheology/mechanical parameters), which is obviously of high importance in determining the simulated dynamics. References should be included to redirect the reader towards the EVP parameter values used. I also suggest including a table with these values (P, ellipse ratio, etc.) so that to avoid having to dig for these values into other papers and ease eventual comparisons to other similar scaling analyses.*

Reply to the major comment: regarding the referee's comment, we now add a separate appendix (Appendix B) to include all the parameterization schemes and the specific values of parameters we have adopted in the experiments. All the schemes, especially thermodynamic processes are kept the same as much as possible across the 3 resolutions, for the sake of improved comparability.

Importantly, no information is given on the method used for the scaling analyses. A subsection to 3.2 that explains the steps taken towards these analyses should definitely be included in a revised version of the paper. Information on the impact of the choice of the region, period of time, the exclusion or not of grid cells close to coasts, the exclusion or not of scaling data points to evaluate the structure functions, etc., should also be given, as all of these factors can have a significant impact on the results. Also, how do your results differ if other days than Dec 10 and Feb 6 are chosen?

Reply to the major comment: regarding to comment on the lack of the details of the methodology for scaling analysis, we now include another appendix (Appendix C) to cover these details. Specifically, the method involves line integrals that cover different spatial scales in the model's grid system, which follows Marsan et al. (2004) and Rampal et al. (2019). Besides, we update the region of scaling analysis and remove those close to land, and the results are also updated accordingly. Last but not the least, besides Dec. 20th and Feb. 6th, we extend the analysis of daily deformations to winter months, and in Sec. 3 the new contents are added.

Overall, section 3.2, which presents the scaling and PDF analysis, is very hard to follow. It includes some contradictions, uses of wrong words, some important misunderstandings, etc. I make several specific comments to this effect below. The figures associated with this section are in my point of view incomplete, which makes the appreciation of the results difficult. I also give suggestions below on how to improve them.

Reply to the major comment: the authors thank the referee for the careful examination of the manuscript and detailed comments on Sec. 3.2. We have made specific replies to each comment, and the following revisions in general. First, we update the scaling analysis of Cumulative Probability (C-CDF in previous manuscript) to be more accurate, including a more formal introduction of the effective resolution. Second, the scaling analysis for multi-fractality and structure functions are now revised. Third, we include the analysis of other wintertime days for the sake of completeness and further analysis of contributing factors.

Section 3.3 contains some intersting results on the effect of the convergence of the EVP model on the simulated dynamics.

Overall, the paper needs some proof-reading to improve the conciseness and accuracy of the formulations used. I also found many grammar mistakes but stopped raising them up at some point. I moreover found that sometimes, jargon-like formulations were used that unfortunately hide the real meaning of the sentences. An important point is the use of the term "multi-scale modeling" for what is really a comparison of model simulations across resolution. This crucially needs to be clarified.

Reply to the major comment: the authors would make thorough check and proof reading on the grammatical usage after all revisions have been made. Specifically, the term of "multi-scale modeling" are replaced with "multi-resolution simulation (or modeling)" throughout the paper. Indeed, as raised by the referee, the approach is multi-resolution with parallel experiments at different grid resolutions, but not multi-scale within a single experiment. We also take care to differentiate the multi-scale deformation of the sea ice cover, and the multi-scale (or multi-resolution) experiments we have carried out.

In brief, I consider that major revisions are required. In my point of view, the points I raise in the specific comments below need to be adressed in a first time. Another review of the paper should be conducted in a second time, in order to better appreciate the results, their meaning and their importance, in the context of this study and provide further suggestions on how to improve the manuscript it its gobality.

Reply to the major comment: the authors thank the referee for these invaluable comments, and the replies (with explanations and revision to be made) are as follows for each specific comment from the referee.

Page 1, title: I find the use of the term ''multi-scale modeling" unfortunately misleading and inapropriate. The paper is effectively about comparision of model simulations performed at various spatial resolutions, while multi-scale modeling refers to codes that can effectively resolve processes occuring different space/time scales by coupling physically and numerically models of these specific processes. A more appropriate title would be "Comparison of sea ice kinematics at different spatial resolutions modeled with a hierarchy CESM..." or "Cross-resolution comparison of CESM sea ice simulations". Please also correct any mention to multi-scale modeling in the text for consistency.

Reply: the authors acknowledge the comment on the use of term "multi-scale modeling". Therefore, we modify the title of the article to: "Comparison of Sea Ice Kinematics at Different Resolutions Modeled with a Grid Hierarchy in Community Earth System Model (version 1.2.1)"

Abstract, line 2 : "Sea ice kinematics is the most prominent feature of high-resolution simulations." There is no need to use high-resolution for kinematics features to be prominent in sea ice simulations. Please sea my comment just below about alternative rheological models, which do resolve the signature of kinematic features at medium and low resolution (> 20 km, Rampal et al., 2019).

Reply: we agree with the referee that with Lagrangian framework and MEB rheology, neXtSIM could model realistic sea ice deformation at lower nominal resolution (Rampal et al. 2019). We would argue, however, that the nominal resolution of the Lagrangian models are not the limitation in resolving multi-scale deformations of sea ice, which is a great advantage of the methodology as compared with Eulerian grid based ones. Therefore, the statement we made apply to these traditional models with non-moving grids. The text is revised accordingly, as follows: "In traditional Eulerian grid based models, sea ice kinematics is the most prominent feature of high-resolution simulations". The introduction to neXtSIM and related works, including references, are now moved to Sec. 1 (from the last section), to give a better background introduction.

Abstract, line 3 : "such as Viscous Plastic" current models are able to reproduce multifractality and linear kinematic features". This is one of my major comments: please be carefull as to make the distinction between multi-fractality in space and in time, throughout the entire text.

Reply: the authors have revised the sentence to be more precise. Since this sentence is to introduce the community's status quo, it is revised as follows: "… with rheology models such as Viscous Plastic (VP) and Maxwell Elasto-Brittle (MEB), sea ice models are able to reproduce multi-fractal sea ice deformation and linear kinematic features that are witnessed in high-resolution observational dataset".

Abstract, line 4 : "we carry out multi-scale sea ice modeling". No, you carry out a comparison of simulations at different resolutions.

Reply: revised as "we carry out modeling of sea ice with multiple grid resolutions"

Abstract, lines 6-7 : "including multi-fractal deformation and scaling properties that are temporally changing". In the light of my other comments below, I would precise "multifractal deformation in space" and not put too much weight on the temporal part. Your abstract should highlight your strong results and the temporal aspect of the scaling analysis is not one.

Reply: we revise the sentence to include spatial scaling only, as follows "… including multi-fractal spatial scaling of sea ice deformation that depends on atmospheric circulation pattern and forcings". The abstract is also revised to be more precise.

Abstract, line 8 : "effective spatial resolution". This effective spatial resolution has not been defined and cannot be understood here. I believe you mean that the model can resolve kinematic features that are 6 or 7 times the width of a model's grid cell? If so, this should be explained clearly and in simple words (i.e., rewrite lines 8-9) in the abstract and redefine later (see my other comment below).

Reply: we rewrite the sentence to be more clear as follows: "By using high-resolution runs as references, we evaluate the model's effective resolution with respect to the statistics of sea ice kinematics. In specific, we find the spatial scale at which the PDF of the scaled sea ice deformation rate of low-resolution runs match that of high-resolution runs. This critical scale is treated as the effective resolution of the coarse resolution grid, which is estimated to be about 6 to 7 times of the grid's native resolution."

Page 2, line 1 : "scale-invariance properties" Cite Marsan et al., 2004 there and Kwok et al., 2008 after "linear-kinematic features". Also, many other and more recent references can be added to Marsan et al., 2004 regarding scale-invariance, especially scale-invariance in time.

Reply: as suggested by the referee, the order of references is modified, and we have added the references for scale invariance including Rampal et al. (2008) and Weiss and Dansereau (2017).

Page 2, line 6 : "most popular". A more objective term would be "most widely used".

Reply: revised as suggested by the referee.

Page 2, line 7-8 : "a plastic medium for packed ice under shear and pressure". This formulation is vague and unfortunately not accurate: the VP model describes sea ice as undergoing plastic deformation for over-critical shearing and compressive stresses only. Please modify the sentence accordingly.

Reply: the sentence is revised as follows "…, and sea ice undergoes plastic deformations over critical shearing and compressive stresses."

Page 2, lines 15-19 : "In order to reproduce the observed properties of the sea ice kinematics, grids of 0.1 degree resolution or finer are usually required". This is true perhaps only in the VP or EVP rheology cases. The MEB rheology has the capability to localize deformation in space at the nominal grid cell scale, whatever the resolution of the grid (Dansereau et al., 2016, Rampal et al., 2019). Mention of this fact unfortunately come only in the conclusion, whereas an adequate literature review in your introduction should distinguish between the VP/EVP and other existing continuum rheologies (EB, MEB, Elastic-decohesive).

Reply: the authors agree with the referee that the statements we have made apply to models with Eulerian grids. Lagrangian model (neXtSIM) with MEB rheology does not suffer from the resolution limitation of non-moving Eulerian grids. We have also moved the paragraph introducing Lagrangian and novel rheology models in Sec. 4 to Sec. 1.

Page 2, line 20 : "multi-resolution sea ice modeling". I think that "we carry a comparison of sea ice model simulations at different spatial resolutions", or "cross-resolution comparison" would be clearer and more accurate.

Reply: we revise the sentence as "… we carry out comparison of sea ice model simulations at different spatial resolutions with the coupled model …"

Page 3, line 12 : "For the SP (...) For the NP". And the same for the lines below.

Reply: revised.

Page 4, line 4 : "a suite", a series?

Reply: revised.

Page 4, line 16 : This sentence is not clear: is there a repetition of "for TS015" there?

Reply: removed the first "for TS015"

Page 4, line 19 : "within the Arctic Ocean".

Reply: revised

Page 5, lines 2-3 : This sentence is unclear and a bit repetitive.

Reply: the sentence is revised as "We choose shorter time steps for both thermodynamics and dynamics with respect to the resolutions of the grids (Tab. 2)"

Page 5, line 4 : "a series", replace by "different subcycle numbers". Maybe rephrase as "We choose shorter thermodynamics and dynamics time steps for our higher resolution grids"?

Reply: revised "…, different subcycle numbers are chosen for each grid". See also the revision above.

Page 7, line 4 : "Potential compromises of using SOM". This needs to be rephrased, for instance as "Potential compromises pertaining to the use of SOM".

Reply: revised as suggested by the referee.

Page 7, line 6 : "in the Ocean Model Intercomparison Project".

Reply: revised by adding "the", as suggested by the referee.

Page 7, lines 8 to 12 : I understand here that you interpolate the same wind field onto your different (3) resolution grids. Does the interpolation ensures that the input (wind) energy is conserved accross resolutions? If not, this will impact your scaling results. I believe that a clear mention to this effect, in this paragraph, would be a valuable addition.

Reply: for the interpolation between the atmosphere and ocean (or sea ice), we follow the standard protocol of CESM. In specific, interpolation of air-ocean (or ice) fluxes are always kept conservative. For the dynamic coupling (i.e., treatments of winds), high-order method (Patch-Recovery) is adopted. The text is revised accordingly.

Page 7, lines 7-8 : Can you specify to which years corresponds the climatological annual cycle based on NCEP atm. reanalysis that you use? It would help understanding the ice coverage and thickness value that you obtain in your simulations at equilibrium (see my comment about these results just below).

Reply: regarding the CORE2 NYF dataset, the authors make the following statements [details in Large & Yeager (2004)]. First, the fluxes are computed with 43-year NCEP reanalysis. Second, the synoptic signals are mainly from year 1995 of the reanalysis, with transition by the end of December carried out through interpolation with data in December of 1994. We consider this a limitation for future study, and will move to inter-annual forcings in further experiments.
Furthermore, as shown in Stewart et al. (2020) in which JRA55-do reanalysis is used to generate new NYF datasets, the specific year that is chosen greatly affects the model's equilibrium status for simulating Arctic sea ice (Fig. 18 of the reference).
In our opinion, the sensitivity of sea ice climatology to the NYF forcing should be studied further, with the reanalysis's behavior in the Arctic at least a focus point (instead of the current status quo).

Page 7, line 18 : Can you perhaps spell NDTE?

Reply: revised as "number of timesteps for elastic wave damping, or NDTE"

Page 7, line 25 to page 8, line 4 : You mention here a minor overestimation in the sea ice extent (cover) in some parts of the Arctic and underestimations in others. What is the basis for this comparision? From figure 5, I understand it is satellite sea ice edge data (from NSIDC), but this should be clearly mentionned in the text as well. Also the year or period of this satellite data should be mentionned with the corresponding years on which the climatology used to force your model is based.

Reply: we revise the text to include the description over the specific region with overestimation of sea ice (mainly during winter in marginal seas). We also add in the texts the years (1979-2000) for the definition of sea ice extent climatology from NSIDC data.

Page 8, lines 4-5 : "consistent with existing sea ice thickness reconstructions by PIOMAS". Also, in the same line as my previous comment, please mention the year for these PIOMAS thickness reconstruction, or insert a figure. It seems to me that there is indeed a lot of ice *stocked into the Beaufort Sea and that such thick ice conditions (up to 5 meters and more than 4 meters over a wide region, in September!) have not been seen at least in the last decade.*

Reply: we add on Fig. 5.b the PIOMAS sea ice volume seasonal cycle (computed with 1979-2000 monthly means) and revise the figure caption and texts to include necessary description. This period (1979-2000) aligns with that for the observational sea ice extent data from NSIDC. They are used as climatological sea ice extent/volume, since we do not have exact match between the model's climatology with existing observations/renanlysis.

Page 8, line 10 : Can you explain in a few words what is a warm start-up?

Reply: by "warm start-up" we mean "starting up the high-resolution simulation with a spun-up status from low-resolution ones". Therefore we revise the sentence as "With the spun-up climatological status of TS015, the experiment with TS005 approaches equilibrium towards year 42."

Page 8, line 15 : "a minor decrease" of what? Please specify "both sea ice extent and volume" or merge this part of the sentence within the next one.

Reply: according to the referee's suggestion, we have merged the two sentences as one: "Similar to TS045 and TS015, the experiment with TS005 produces reasonable sea ice climatologies, but with a minor decrease in both sea ice coverage (mainly in summer) and sea ice volume (all season) with respect to TS045 and TS015."

Page 8, line 30 : "two years' daily mean sea ice fields (...)". Rephrase, eg. "two years (41-42) of daily mean sea ice fields for all three TS grids".

Reply: rephrased as "two years (41-42) of daily mean sea ice fields for all three TS grids".

Page 8, lines 28–30 : This is one of my major comment/concern. In this paragraph, you mention computing the deformation invariants from the daily mean sea ice drift speeds. This is the time scale set throughout your scaling analysis of daily deformation rates. You do not however mention how deformation rate components (du/dx, dv/dy, du/dy, dv/dx) are calculated, in particular at space scales larger than that of the cells of your Eulerian grids (with Arawaka-B staggering). Because you use Eulerian grids, I am guessing that your are following a coarse-graining method such as the one used in Marsan et al., 2004, but what are the details of the method? Do you, for instance, define square boxes and use a contour integral calculation to estimate each of the deformation rate components? Or just sum the components over each box? Most importantly, in estimating deformation rates at a given space scale, do you effectively sum (i.e., average) the deformation rate components and then calculate the corresponding invariants at that scale or do you sum (i.e, average) the deformation invariants themselves over that space scale? Also, why do you choose the region outlined in Figure 7 for your scaling and PDF analyses especially? How do you deal with the presence of coasts? Do you to eliminate data within a margin of the coasts? How do you deal *with coarse graining boxes that might contain land regions? All of these details will most probably affect your scaling results and should be mentionned.*

Reply: the authors would like to clarify that the computation of scaled deformation rates are strictly following the coarse-graining algorithm of line integration for the specific scale (for our case a square region of the grid), as carried out in Marsan et al. (2014) and Rampal et al. (2019) (Sec. 3 of the reference). In specific, a region of a certain area (in our case, a number of adjacent cells that form a square), we compute its scale (L) as well as the deformation rates by computing line integration around its outer walls. The values of u_x, u_y, v_x and v_y are computed and then used to compute the deformation rates. We have added an appendix to specify the computations we have carried out in more detail.
The region we chose (as shown in Fig. 7) covers the majority of the Arctic basin, and due to its square shape, it facilitates the computation of aforementioned scaled deformation rates.
The authors agree with the referee that the treatments on the outer rim of the sea ice cover (including coastal regions) affects the analysis results. For our study, in the case of adjacency to coast, since we use Arakawa-B grids in CICE, we omit any T-points that have any single vertex on land.
In the revised version of the paper, we carry out the analysis without any points close to land. We confirm that there is slight change, but no qualitative difference in the new results.

Section 3.2, pages 8-9 : I my point of view, readability would be improved if the second and third paragraphs were included after the first sentence of paragraph one of section 3.2. Then, after paragraph 6, you a paragraph or sub-section describing your method for the estimation of deformation rates at different space scales, and of the scaling exponents, is necessary.

Reply: according to the suggestion of the referee, revisions are made to re-arrange the contents of these paragraphs. First, the characteristic of the forcing data and the choosing of the representative days are covered. Second, a general description of the modeled deformation of the two days. Third, the analysis of the PDF and scaling properties, including the methodology, consistency checks, scaling analysis results.

Page 11, line 22 : "There is good agreement". I think it would be more accurate to say that the results are consistent accross spatial resolution, since the comparison here is not done on the basis of observational data.

Reply: we revise the sentence as "The simulation results are consistent across the three grid resolutions …"

Page 11, line 23 : "large shearing belts accross the basin". There are indeed large shearing belts and diffuse regions of shearing rates seen at all model resolutions. Are these diffuse shearing belts physical? How do they compare, for instance, to shearing rates fields inferred from RadarSAT data? To what process, physical or numerical, do you think they are related?

Reply: the authors agree that the comparison and validation with observational dataset (such as SAR-based deformation) is important. But because we use a NYF dataset from CORE2 for the experiments, there is no direct correspondence of the modeled sea ice field to any observations. In this paper we mainly focus on comparing the simulation of various

resolutions with certain consistency among them, but we do acknowledge this a limitation of our current work. We are looking forward to work with IAF dataset that improve comparability with observations such as RGPS.
In our opinion, very large deformation structure (such as shearing belt) is an aggregate response of the sea ice cover to large-scale forcings. Especially, there is very large variability of the (sea ice) transpolar drift on the daily scale (i.e., within synoptic scale), regarding both direction and strength. There is a wide spatial scale involved surrounding the either side of the transpolar drift. Another possible mechanism is that low-pressure systems entering the Arctic, causing large-scale deformation events.

Page 11, line 25 : "There is a clear".

Reply: corrected.

Page 11, line 26 : Please change "more well-defined" by "better defined".

Reply: revised.

Page 11, lines 31-33 : The last sentence of this paragraph is not clear. Please define clearly what you mean by "effective" resolution of the model. I guess it corresponds to the width of the simulated LKFs?

Reply: the authors would like to clarify that the model's native resolution serves as the basis of the simulation of certain phenomena (such as waves in geofluid dynamics or sea ice kinematic features), but these phenomena are not realistic on the spatial scales of the native resolution. Rather, the "effective resolution" of the model, which is usually coarser than the native resolution, is the spatial scale on which the model could realistically produce these realistic phenomena. Here we do not focus on the widths of the LKFs, but rather the shape of the modeled LKFs do not agree well with observations, in which the both ends of the (relatively) larger LKFs should feature even smaller LKFs. On the contrary, the ends of the many modeled LKFs end in a larger region with a spread-out, smaller deformation rates, with no clear structure.

Page 11, line 34 : "the distribution of total deformation rates follow power-law distributions". What distribution? Please be precise here, e.g., "the statistical (or probability density) function of total deformation rates follow a power law".

Reply: revised as "… the probability density function of total deformation rates follows a power law distribution"

Page 12, line 1 : I find the accronym C-CFD to be confusing here. You are calculating the cumulative probability density function of both daily and 3-days mean deformation rates. This term and the accronym cumulative PDF is used in most published scaling analyses within the sea ice community. I suggest for clarity that you use similar terms.

Reply: the authors would like to clarify that we use complementary cumulative distribution function (or C-CDF) for the cumulative PDF (which is more widely used by the sea ice community, as mentioned by the referee). In response to the comment, we replace the use of C-CDF with Cumulative PDF or Cumulative Probability as revisions.

Page 12, line 3 : "For both daily and 3-days cumulative distributions, we attain multifractality accross the three resolutions". I am confused here: how do you conclude that deformation rates are multifractal from the cumulative distributions in Figure 9? This information is rather given by a scaling analysis based on different moments of the distribution of deformation rates and the estimation of the convexity of the quadratic function describing the dependance of the scaling exponents on the moment. Please clarify or remove.

Reply: the authors remove this statement of multi-fractality, since this is actually the result drawn from the scaling analysis later in this sub-section.

Page 12, lines 7-21 : This paragraph is very confusing and I do not understand your method here. First you say that you carry out the spatial scaling of one grid onto another? How do you do that? Do you mean that you interpolate deformation rates from one grid to another? Line 7 : you mention that the slopes become steeper for the higher resolution grids for one given day but not the other. I suspect you mean the slope of the "CFDs/C-CDFs" in log-log space? How do you compute these slopes from figure 9? Can you please show these slopes on the figure so that one can evaluate at least qualitatively the goodness of fit? Lines 9-10 : "the slopes of C-CFDs from scaled rates". Do you mean interpolated doformation rates? Also, putting all of the curves on each panel of figure 9 makes it very difficult to read the figures. I would suggest separating the "non-scaled" or non-interpolated and interpolated results on different figures, or use different levels of opacity for the non-interpolated and interpolated results. Lines 13-14 : what is a realistic shape for the distribution? On what data do you base your evalution of a "realistic shape"? Also, a realistic shape for a "power-law distribution" is by definition a power law! Hence I suggest you write simply "a realistic shape of the distribution of deformation rates". Also see my previous comment about defining the "effective" resolution of the model. Line 16 : "we attain the same slope" Lines 18-19 : "the CFD of sea ice deformation rates", not kinematics. Also, please explain how you evalute this effective resolution, which is 6-7 times higher than that of the TS015 grid and what are the different days that you are analyzing.

Reply: the authors apologize for the lack of clarity of this paragraph. The central role of the analysis is to compute and compare the scaled C-CDFs (or cumulative probability) from different grids. For example, we can scale the model output from TS005 to the grid resolution of TS015, and directly compare the C-CDF with that of TS015. Another example is that we scale the model output from TS005 to the resolution of TS045, and scale the model output of TS015 to the resolution of TS045. Then, we can compare the tail slopes of the Cumulative Probability. Suppose that we want to evaluate the effective resolution of TS015, by using the results from TS005 as a reference, we can find the spatial scale at which the slopes of scaled C-CDF from TS015 matches that of TS015. This spatial scale is then viewed as the effective resolution of the model output of TS015.
This analysis inherently relies on high-res. (TS005) model outputs as references. Also, we compute the slope as the slope of the linear least-square fitting between 0.05 and 0.25 for the

cumulative probability (y-axis). We have re-written the whole paragraph to increase clarity. Besides, Fig. 9 is also revised to include necessary information of statistical fittings.

Page 12, lines 22-26 : Why do you think the (absolute) slopes you are estimating are smaller than that of Marsan et al. 2004 at a similar time scale? It would be relevant to offer possible explanations here.

Reply: the authors would like to clarify that the absolute values of slopes we have in the analysis (-1.6 to -2.7) are indeed smaller than that of Marsan et al. (2004), which is -2.5 at the spatial scale of 13~20km. This is only to cite these values, since there is inherently no comparability among them (different date, different time duration, models not forced with realistic atmospheric forcings). We conjecture that this value of slope is time-variant and changes with forcing and ice conditions, but this could be investigated in the future.

Page 12, line 26 : "to evaluate the structure function".

Reply: revised (on line 28).

Page 12, line 31 : "At q = 3, the structure function is in the range...". Do you mean beta instead of the structure function?

Reply: corrected to be beta(q), according to the referee's suggestion.

Page 12, last paragraph : I do not think it is relevant to cite the differences in the values of beta(q) or in the shape of the structure functions between the two analyzed days if you do not try to explain these differences physically.

Reply: in the revised version the authors extend the scaling analysis of structure function to the potential contributing factors including the atmospheric forcings.

Page 12, line 35 : "the average deformation rate". It would be more specific to refer to the mean deformation rate or to the moment of order 1.

Reply: revised to "the mean deformation rate".

Figure 10 : On the scaling figures (left panels) please indicate the moment order corresponding to each set of curves and insert a legend for the different colors/model resolutions. Also, you label the y-axis with epsilon for the total deformation rate, whereas in equations (1) to (3) you use dot(varepsilon) (indeed not available in MATLAB) for this variable and the other deformation invariants. Please use consistant symbols accross the text and figures. On the x-axis of the same figure, you use the label "space scaling", which would rather be appropriate as a title for these figures. I believe you mean "space scale". Your structure function is estimated using the moment of order 0.5, but it is not shown in the scaling analyses (left panels), why? Your structure function results could also be appreciated

more objectively if you included error bars for beta for each of the moments (see e.g., Rampal et al, 2019 for the definition of the error bars on beta(q)).

Reply: the authors apologize for the missing of legend for different (grid) resolutions. Actually the starting points of different resolutions differ, since only TS005 (purple) reaches down to the spatial scale of 2km, and TS015 (red) reaches down to 7km. For the sake of clarity, we add a legend to these panels and corresponding texts in the figure caption. Symbols for deformation rates are also revised to be consistent across the article, as suggested by the referee.
“Space scaling” are replaced with “Spatial scale” in the figures. We have also included the moment-order of 0.5 in the scaling analyses. Besides, error bars are added for the panels for structure functions, by using formulations in Rampal et al. (2019).

Most importantly, for the December case in particular it is apparent from the scaling figures (left panels) that the slope (beta) of the moments of order 2 and 3 is calculated by leaving out at least the two last points of the scaling analysis, corresponding to the largest space scales. Why is that and how considering/rejecting these points affects your results? If some data points are left out or attributed less weigth in the analyses, this should be definitely be clearly mentionned and argued for in the text.

Reply: the authors thank the referee for the careful examination of the figures. Indeed at very large spatial scales, we are potentially hit with the problem of lacking samples. Actually across all the spatial scales, the effective sample counts change (or decrease) dramatically. The confidence level on the mean value at these largest scales are much wider, as a result. We further examine the results in the revised version of the manuscript with the exclusion of the scales larger than 200km, which corresponds to the last points mentioned by the referee.

Page 13, line 4 : Please specify that your result support multifractality of the simulated sea ice deformation in space (i.e, not in time), hence not “multifractality of sea ice kinematics”.

Reply: revised to be more precise by only mentioning spatial scaling.

Page 13, line 5 : How does the inclusion of the deformation rates at the two larger space scales for the moments 2 and 3 change the value of the estimated curvature? (see my previous comment on figure 9).

Reply: We further examine the results in the revised version of the manuscript with the exclusion of the scales larger than 200km, which corresponds to the last points mentioned by the referee (also replied above).

Page 13, line 6-8 : This sentence is a generic comment and does not offer a satisfactory explaination for the difference in results between the two time period analyzed. Either offer some physical hypotheses or refrain from comparing the two results.

Reply: regarding to this comment, we extend the analysis to daily deformation fields during the winter and carry out attribution study by relating the scaling to circulation pattern and forcing data.

Page 13, line 8-9 : If there is a slight drop between the values of beta or the curvature of the structure functions between the daily and 3-days deformation rates and this difference is not evident for the Feb. 6 case, my opinion is that this is no sufficient evidence that the model can reproduce temporal scaling. Such assumption should rather be based on a proper temporal scaling analysis that spans several orders of time scales, not a comparision between 1 and 3-days fields. This comment in my point of view should be removed because not supported from your results (and mention to it should be removed from the abstract as well, which should only state your strong results).

Reply: the authors agree with the referee that the method (based on Eulerian means) and the ensuing analysis do not strictly correspond to temporal scaling analysis. We have revised it to include the results above, but removed related statements on temporal scaling.

Page 13, line 12 : "existing studies with observational datasets and modeling results". Please put some references here.

Reply: references are added, as suggested by the referee, including Marsan et al. (2004) and Rampal et al. (2019).

Page 13, line 14-15 : Why is there a comparison here? This does not make sense. Do you mean "more convex on Feb. 6 than Dec. 20"? Also, why would this support less dominant large-scale features on Feb. 6? Please clarify.

Reply: the authors have revised the manuscript to remove this statement. Attribution to the various circulation pattern and forcing data are made in relevant part of the paper instead.

Page 13, line 15 : What is a ''more effective temporal scaling"? How does the results on figure 9 and 10 support temporal scaling?

Reply: (according to previous comment and reply related to temporal scaling) during revision we remove statements involving temporal scaling.

Page 13, lines 16-24 : I do not understand the link between these sentences and the previous sentences of this paragraph. What point are you trying to make? Please explain. Also, I think it would be more accurate not to refer to Lindsay et al., 2003 as current RGPS observations. RGPS is currently not running. Maybe just drop the reference.

Reply: as noted by the referee, there is missing link in the logic linking the analysis of typical days to atmospheric forcings. We make the following revisions that potentially improve the inherent logic in the argument. We extend the analysis to winter time daily deformation fields, and carry out attribution study of the deformation statistics (scaling, PDF, etc.) to various factors including circulation and forcings.

Page 13, line 26 : "we evaluate the sensitivity? of the modeled kinematics".

Reply: revised as "… we evaluate the sensitivity of the modeled sea ice kinematics to the EVP subcycling …"

Page 13, line 27 : "the probability density function (PDF)".

Reply: corrected.

Page 13, line 28 : and figure 11 : I suggest adding the estimated slope of the tail of each PDF on the graphs of figure 11 to illustrate how you estimate it from your results. It would also help putting a Gaussian distribution on each graph to visually identify the fat tails of the PDFs.

Reply: as suggested by the referee, we add the estimation of slopes from the fittings, the range (on the y-axis) that is used to estimate the slopes, as well as the theoretical slope on Fig. 11.

Page 13, line 28 : "for the total deformation rate".

Reply: corrected.

Page 13, Section 3.3, 1st paragraph : To support your claim that insufficient convergence of the model solution (i.e., nb of subcycles) is responsible for the absence of convergence between the PDFs of the simulated distribution rates at high resolution, I think you could also mention that the tail of the log-log PDF at the higher resolution and lowest number of sub-iteration does not seem linear as in the other cases.

Reply: according to the referee's comment, we add another 2 sentences in the first paragraph of Sec. 3.3 (specifically, on line 1 of page 14), as follows "Besides, the PDFs of high-resolution runs (TS015 and TS005) show better tail structure, and furthermore, the slopes are also better characterized (i.e., closer linear fittings at -3) with larger values of NDTE. For TS045, the tail of the PDF suffers from the lack of samples for large deformation events."

Page 14, line 6 : I think you mean figure 12, not 7.

Reply: corrected.

Page 14, line 8 : I think you mean figure 11, not 12.

Reply: corrected

Page 14, line 8 : What is the "physical" deformation rate? Please explain more clearly.

Reply: the authors would like to clarify that by "physical deformation rate" we are referring to the inherent deformation when the EVP convergence is attained. Actually for regions outside LKFs, the deformation rate drops (without the noise), and the structure of LKFs becomes more defined. Therefore this sentence is less accurate, and we revise it as follows "With larger values of NDTE, the noise level decreases and the deformation rate around the linear kinematic features becomes smaller. As a result, a convergent PDF and linear feature maps are attained."

Page 14, lines 10-12 : Instead of convergence of the kinematics, it is the convergence of the model solution, or simulated kinematics. Also, instead of "deterioration of simulation speed", I would write "increase in simulation time or cost" for more clarity.

Reply: revised as indicated by the referee.

Page 15, line 1 : "in the Canadian Arctic Archipelago". Line 3, "the CAA" again. Same mistake in other places on page 18.

Reply: adding missing word of "the" for this case and all other cases.

Page 15, line 2 : "an ice arch"

Reply: corrected.

Page 15, lines 8-10 : It would be helpful for the reader if you could further explain how you can separate these contributions in your model, by showing the term of the the dynamics/thermodynamics equation associated with each of them.

Reply: according to the suggestion from the referee, this paragraph is extended to include a formal description of the different contributing terms to sea ice thickness, as well as the details of how to compute in the model (we adopted CICE).

Page 18, line 2 : "due to the fact that the thermodynamic growth".

Reply: revised according to the referee's suggestion

Figure 13 : Please increase the fonts of the figure titles and colorbars.

Reply: this figure is modified to improve readability.

Page 22, line 1 : Please rephrase that sentence, which is not clear at all, both in the meaning and construction.

Reply: this sentence is revised as: "In this paper we carried out sea ice simulations with a multi-resolution framework with Community Earth System Model."

Page 22, line 2 : Change "grid stepping" for "grid resolution".

Reply: revised according to the referee's suggestion.

Page 22, lines 6-8 : "Multi-fractal sea ice deformation is accurately modeled by all three resolutions": please see my comment above on the inclusion or not of the all the points in your scaling analysis for the case of Dec 20. "with good agreement with observational works in terms of scaling properties" : have you try to compare the slopes of the scaling analyses (beta) for the three moments and the curvature of the structure functions with observational analyses at equivalent spatial resolution, e.g., based on RGPS data? If not, this comment should be revised.

Reply: the authors revise this sentence according to the referee's suggestion, as follows "As shown in the spatial scaling analysis on the representative days, multi-fractal sea ice deformation is accurately modeled by all three resolutions."

Page 22, line 12 : "multi-scale modeling studies". Please consider my previous comment on the meaning of "multi-scale modeling".

Reply: we correct this case, along with all other cases, of "multi-scale modeling" to the more precise statement of "multi-resolution simulations"

Page 22, line 19-21 : This sentence is incomprehensible, what is the "initial study with temporal scaling analysis with 3-day mean drift fields"? I did not see these results. If you are referring to the comparison of the daily mean and 3-days mean results, see my previous comment about how a much larger range of timescales would be necessary to conduct a meaningful temporal analysis.

Reply: the authors agree with the referee's comment on temporal analysis. We remove the statements of referring the analysis of 3-day mean drift as temporal scaling.

Page 22, line 21 : Repetition of "in our study".

Reply: removed the second instance of "in our study"

Page 22, lines 23-27 : Scaling analyses of modeled deformation fields and their comparison with equivalent analyses of observed deformation fields date from around 2010. Such *analyses and model-observation comparisons have been made by only a few sea ice research groups and besides, techniques for comparing accurately Lagrangian/Eulerian model outputs to observational deformation data, especially in the context of time scaling analyses, are complex and have been recently developped and applied. Hence I would not qualify scaling analysis as a "traditional" tool for evaluating sea ice kinematics. For further validating sea ice deformation properties simulated with your multi-resolution framework, I also suggest a comparison of simulations to observations of sea ice deformation.*

Reply: the authors agree that the scaling analysis involving modeled sea ice kinematics, including validation with observed high-resolution kinematics is a recent development in the community. Therefore we remove the term "traditional" from our statements. We also look forward to carrying out historical simulations with the multi-resolution grid system and coupling with a fully dynamic ocean component. We plan to start this work by using inter-annual forcing (IAF) dataset and comparing with historical observational dataset.

Page 22, lines 31 to 33 : It is clearly stated in the paper by Rampal et al., 2019 that the MEB rheology of Dansereau et al., 2016 is used, not the EB rheology of Girard et al., 2011. Please read the paper and correct your sentence. Also, replace "which are shown" by "which is shown" and for a demonstration of the MEB model capability to localize deformation at the nominal grid cell scale, whatever the grid resolution, which explains the fact that neXtSIM does not encounter "effective" resolution issues and does not require using a sub-LFK spatial resolution to simulate adequately these features, therfore reproducing the scaling properties accross model resolutions, see Dansereau et al., 2016.

Reply: the authors apologize for the imprecise statement of the rheological model in neXtSIM. Revisions have been made for both corrections to Maxwell Elasto-Brittle (MEB) rheology in neXtSIM and the addition of reference (Dansereau et al., 2016). Other errors including grammatical ones are also corrected.

Page 22, line 6 : It is hard from the figures shown here only to witness the asymptotic convergence of the simulated modeled kinematics. Please remove that sentence or include a figure that shows this clearly.

Reply: as suggested by the referee, we revise this sentence to be more precise: "Based on a traditional implementation of EVP in our model, we have witnessed asymptotic converging behavior of sea ice kinematics fields with increased EVP subcycle count."

Page 23, line 6 : "we have witnessed that".

Reply: corrected.

Page 23, line 15 : What is your choice of ice strength parameterization scheme? See my previous comment about the importance of including at least a reference to the dynamic equations of the model and a table listing the EVP rheology parameter values.

Reply: the ice strength parameterization scheme is based on Rothrock (1975) with details of implementation in Lipscomb et al., (2007). This scheme is used by CESM in its scientifically validated experiments. We have now included a new appendix to include a complete list of the specific parameterization schemes and parameters that are used in the experiments.

Page 23, line 29 : "Multi-scale simulations". I belive you mean that the comparison of simulations across spacial resolutions is becoming common in the climate modeling community.

Reply: revised as "Simulation with multiple spatial resolutions".

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