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Interactive comment on "Multi-Scale Sea Ice Kinematics Modeling with a Grid Hierarchy in Community Earth System Model (version 1.2.1)" *by* Shiming Xu et al.

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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).

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.

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?

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.

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.

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.

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.

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).

Abstract, line 3 : “such as Viscous Plastic” current models are able to reproduce multi-fractality 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.

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

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).

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.

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

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.

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).

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.

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

Page 4, line 4 : “a suite”, a series?

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

Page 4, line 19 : “within the Arctic Ocean”.

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

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”?

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”.

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

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.

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).

Page 7, line 18 : Can you perhaps spell 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.

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.

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

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.

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”.

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.

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.

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.

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?

Page 11, line 25 : “There is a clear”.

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

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?

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”.

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.

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.

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.

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.

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

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

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.

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.

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)).

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.

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”.

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).

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.

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).

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

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.

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

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

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

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.

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

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.

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

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

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

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.

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

Page 15, line 2 : “an ice arch”

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.

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

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

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

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

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.

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

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.

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

Page 22, lines 23-27 : Scaling analyses of modeled deformation fields and their com-

parison 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.

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.

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.

Page 23, line 6 : “we have witnessed that”.

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.

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.

Véronique Dansereau

Please also note the supplement to this comment:
https://gmd.copernicus.org/preprints/gmd-2020-160/gmd-2020-160-RC3-supplement.pdf

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Interactive comment on Geosci. Model Dev. Discuss., https://doi.org/10.5194/gmd-2020-160, 2020.