The authors would like to express sincere thanks to the two referees and the editor for their invaluable comments and efforts in helping us to improve the revised version of the manuscript. We have made replies to the comments and corresponding revisions, covering those from referee #1 on page 1 through 4, and from referee #2 on page 5 to 10. Besides, a marked up version of the revised manuscript is provided to highlight the specific revisions (yellow for the replies to the comments from referee #1, cyan for those from referee #2).

Following up are the replies to the comments from Dr. Nils Hutter (the referee #1). The original comments are in *green italic* font, and the revisions are highlighted in the revised manuscript in yellow.

Reply to comments of Referee #1:

Review of "Comparison of Sea Ice Kinematics at Different Resolutions Modeled with a Grid Hierarchy in Community Earth System Model (version 1.2.1)" from Xu et al. (2020)
Nils Hutter

The authors have done a good job in answering to all reviewer's comments and revising the manuscript. Thank you for that. The focus of the paper remains as the original but the robustness of the presented analysis and the clarity of the manuscript have been improved significantly by the authors. Major changes include new simulations with the Hibler (1979) ice strength that lead to a more realistic ice distribution and the presented scaling analysis of sea ice deformation has been extended to a 3-month period. With the modifications the paper is a valuable contribution to the ongoing scientific discussion and I recommend the paper for publication after my minor comments are addressed:

General comments:

1. Scaling analysis: The authors perform a scaling analysis for three winter months (Dec to Feb) and include the new section 3.3 discussing the variability of the scaling exponents during that period, which I appreciate. Why do you not present these new results in Figure 9 (lower row) and Figure 10 (lower row) as a substitute of the analysis of three day intervals. As a reader, I would find this results more interesting. Nevertheless, with Figure 11 you showed that the scaling behaviour is persistent throughout the 3-month period, which answers my main concern raised in the first review.

Reply: the authors thank the referee for the comments on the replies and the revisions we have made to the original version of the manuscript. We consider the wintertime, long-term analysis an independent and outstanding issue, which is beyond the analysis for the typical days (as well as the 3-day analysis surrounding these two typical days). We thank the referee for appreciating our extension of the scaling analysis to the whole winter.

2. Future work plans: In "Summary and discussion" you state at many places your future research plans (P25: L 9-10, L12-13, L20-23; P27: L1-6,…). I understand that these plans are the logical *next steps from the results presented in this study. However, I recommend to generalise these plans to recommendations to the community where further research is needed. Right now, some parts of the summary reads more as project proposal, which is inappropriate.*

Reply: the authors have made extensions to the 3 part of Sec. 4 pointed out by the referee, in order to improve relevance to the community's interests in related topics. Specifically, for P25:L9-10 and P25:L12-13, we revise the sentences as: "*Although with large-scale, coarse atmospheric forcings such as CORE-2, sea ice models can produce multi-fractal sea ice deformation events, how these events are governed by the multi-scale atmospheric processes remain unclear. With different atmospheric forcing datasets such as CORE-2 and JRA-55, we plan to carry out comparative study by using higher versions of the coupled model (i.e., version 2 of CESM). Besides, the dynamical and thermodynamic feedback of the sea ice deformation can also be studied with an atmosphere-sea ice coupled modeling framework, with multi-resolution setting for both the atmospheric and the sea ice component model, as in CESM*".

For P25:L20-23, we revise the sentences as: "*Another issue with modeling sea ice at very high resolution (such as TS005) is the prominent anisotropic characteristics. In order to explore its effect, the anisotropic rheology models such as EAP (Tsamados et al., 2013) can be utilized for a comparative study with standard EVP (or VP) with a very high resolution setting (1 to 2 km in the Arctic Basin). This is planned in our future work with the updated version of the sea ice component (version 5 of CICE) in CESM*".

For P27:L1-6, we make the sentences more concise, as follows: "*For future work, long-term, inter-annually forced simulation of the coupled ocean-sea ice system is planned, under the multi-resolution framework and the spin-up strategy as adopted in this study. Specifically, the comparison with coinciding satellite observations can be carried out such as the RGPS dataset*".

Specific comments:

P1 L3 "sea ice kinematics"
add small-scale. Basin-scale kinematics are also simulated at coarse resolution.

Reply: revised as: "…, small scale sea ice kinematics is the most prominent … ".

P2 L6-8 "and quasi-linear kinematic features by visual inspections (Kwok et al., 2008), including local deformation regions of sea ice failures and shearing."
a verb missing in the sentence

Reply: the sentence is revised as: "*and quasilinear kinematic features are observed through visual inspections (Kwok et al., 2008), including local deformation regions of sea ice failures and shearing*".

P2 L29-30: "Purely Lagrangian models such as neXtSIM (Rampal et al., 2019) are potentially 30 free of the resolution issues for resolving small deformation features."
This is not correct. First, neXtSIM shows deformation features as small as the resolution of the

grid but it is not resolution independent. Second, the reason for this is likely to be the combination of the MEB rheology generating these LKFs and the Lagrangian advection scheme preserving them. The Lagrangian advection scheme is hardly the only reasons like it sounds in your sentence.

Reply: the authors would like to clarify that by the sentence we mean that Lagrangian models do not suffer from the traditional resolution limitation of Eulerian grid based models, since there is a hard limitation (grid resolution) of these models due to the smallest grid size is fixed. So this sentence only refers to the resolution aspects, not the rheology part. On the other hand, even with MEB rheology, we would argue that Eulerian grid-based models still suffer from the resolution limitation in resolving very small deformation events.

P2 L33 "(Hutter and Losch, 2020),"
This paper does not comment on potential violations of continuum assumption in high resolution simulations. The issue with deformation features in VP models is also in my opinion rather the missing memory of past deformation (as discussed in the cited paper) than the continuum assumption.

Reply: the whole sentence is revised as follows: "*Traditional VP rheology is limited due to the lack of memory for past deformation events (Hutter and Losch, 2020). Besides, there are also efforts in improving the rheology model in simulating observed anisotropy in sea ice floe shape and associated deformation (Tsamados et al., 2013).*"

P7 L10 "(Lipscomb et al., 2007; Hibler, 1979)"
I recommend the following to clearly state which ice strength definition you are using: ridging/rafting scheme (Lipscomb et al., 2007) and ice strength model (Hibler, 1979)

Reply: the references are differentiated for the two schemes in the revised sentence.

P12 L32: "deformation caused by numerics"
What is this? Do you mean noise generated by the EVP solver? Please be more specific.

Reply: to be more specific, we revise the term as: "*the deformation caused by numerical issues, such as limitation of grid resolution and non-convergent solution*".

P8 L33: "As a consequence"
The effective resolution is coarser than the grid resolution because a few grid cells are needed to represent steep gradients in the drift fields, not due to numeric noise. At least in converged solutions. Please clarify.

Reply: we revise the previous sentence to include the two major aspect we intend to include, the limitation of grid resolution and non-convergent solution to the VP problem.

P20 Figure11: Please use the same limits for both y-axis. Since you are showing the running mean of the same data in the second y-axis, different limits are confusing and misleading.

Reply: we have revised the figure to align the y-axes, as indicated by the referee.

P21 L28-29: "multi-fractal sea ice deformation is accurately modeled by all three resolutions" Rephrase to: "the modelled sea ice deformation is characterised by multi-fractal scaling". Since you did not compared to observed multi-fractal characteristics "accurately" is misleading.

Reply: rephrased as: "*..., the modelled sea ice deformation is characterized by multi-fractal scaling for all three grid resolutions*".

P22 L8: is -> are

Reply: corrected.

P22 L9-10: "Specifically, the process dependent scaling 10 properties were found for representative days for sea ice drift and AO index."
Unclear what process dependent properties are. Please rephrase and clarify.

Reply: we revise the sentence to improve its clarity: "*Specifically, we have observed time-varying scaling properties, which the scaling coefficient correlates with the leading mode of atmospheric forcing (i.e., Arctic Oscillation)*".

P25 L27-28: "Based on a traditional implementation of EVP in our model, we have witnessed asymptotic and converging behavior of modeled kinematics by the increasing EVP subcycle count."
Two times the same sentence, please remove one

Reply: revised by removing one instance of the sentence.

Reply to comments of Referee #2:

General comments:

This is the revised manuscript accompanied by responses to the reviews of the original manuscript. The authors present the results of one component of a new earth system, namely the sea-ice. They first introduced a new grid generator for a tripolar grid. Then, they show initial results of the sea-ice component under normal-year-forcing (a kind of climatological forcing with synoptic scales sampled from a specific year) and different resolutions, including multi-scale analysis of the sea-ice deformation. Finally, they show the sensitivity of the dynamics of the model to the grid resolution and a long-debated parameter in the dynamics, namely the number of subcycling used in solving the viscous plastic equations.

The manuscript has improved in general but the English needs to be polished once more. The experiments are well described. The results are well presented and analyzed using existing diagnostic tools. More context for what the authors are aiming at would help however the reader as well.

Reply: the authors thank the referee for the invaluable comments and the suggestions in improving the language usage of our paper. We have made the following revisions and replies accordingly.

1-I understand that the motivation in section 3.2 was to claim spatial multi-fractality in CICE which was originally disproved by Girard et al. (2009) => need to refer and discuss their results. Moreover, since the deformation field has not converged yet with respect to the NDTE parameter for TS015 and TS005 (ndte=240 in this section), I am curious to know the sensitivity to the parameter. In passing, I note that the sign of beta varies from negative to positive without clear explanations...

Reply: regarding the study with EVP and CICE in Girard et al. (2009), we would like to clarify that there is distinct differences between the model settings in Girard et al. (2009) and ours, including model resolution, period of study, as well as EVP convergence. We do take extra caution when using the model output on the original grid resolution to study statistics. In Girard et al., (2009), the authors do find better consistency of deformation rate PDFs when using scaled results of the model outputs, which is potentially free from the issues involving limited effective model resolutions. According to the referee's suggestion, we have added extra discussion in Sec. 4, relating to Girard et al., (2009).

Regarding the convergence regarding to subcycling of EVP, we haven't examined the scaling properties with lower NDTE values (i.e., smaller than 960). Currently we treat the two aspect as independent issues. With non-convergent EVP solutions, the scaling properties of the deformation rates are expected to change, but unfortunately we haven't explored this issue yet. Regarding beta values close to or higher than 0 on some days (Fig. 11), we would like to argue that these values are statistically insignificant from 0, which are usually days associated with very large, non-localized deformation events. Also the analysis method (Eulerian instead of Lagrangian) is shown to affect the scaling analysis as well (Hutter et al., 2018), and we are preparing to adopt a Lagrangian framework for a better characterization of these extreme days.

2-About the grid generation, I am grateful that the authors gave their rationale for developing their own generator, however this is done only in their response and is not reflected in the text. The reader is missing that context and is still left in the black. Notably the reference to the pioneered work of others is left to the appendix which leaves the wrong impression that the authors claim for the own the technique in the main part of the text. In their response, when comparing to the tri-polar ORCA grids, the authors imply that POP/CICE use a different and unique "U-fold", which I believe is a misconception. The POP manual (top page 46 of https://www.cesm.ucar.edu/models/cesm1.2/pop2/doc/sci/POPRefManual.pdf) does acknowledges that their tripolar grid is a courtesy of Madec (i.e. the ORCA grid). The only differences between ORCA and POP grids are in fact just technical (ORCA grids include a redundant row of points along the western, eastern and northern edges; ORCA grids accommodate both, what they call, F- or T-folds whereas POP grids exclusively rely on the F-folding technique, which the authors refer to "U-fold"). POP Manual also refers to Murray (1996) given below.

Reply: regarding the referee's comment, we have now added the necessary references during Sec. 2.1 for Murray (1996) and Madec (1996). We would like to clarify for that we do not intend to make the wrong impression of us inventing the grid generation technique, and apologize for any cases for potentially conveying the wrong message.
We do agree with the referee that the treatment of the grid's north poles is consistent between POP grids and ORCA. However, we would also like to highlight that for current POP grids (e.g., TX1 and TX0.1) and ORCA, they are inherently different: on the "turning latitude" that meridional grid sizes in POP grids are NOT continuous, while those for ORCA are.

3-The authors in their response state that they found that their sea-ice state was more realistic with a change of ice strength formulation. I would refer them to Ungermann et al. (2017) where there is a discussion on the subject of Hibler (1979) vs Rothrock (1975).

Reply: the authors thank the referee for pointing out Ungermann et al. (2017), in which MITGcm model is utilized to study the differences between Hibler (1979) (or H79) and Rothrock (1975) (or R75) for ice strength parameterization. Indeed, although a different model is used in our study, qualitatively with H79 our model simulates better ice thickness distribution than R75. The reference to Ungermann et al. (2017), and a reference is added in Appendix B. Besides, we plan to carry out detailed analysis attributing the differences we have witnessed in the future.

4-"EVP rheology": Careful with using EVP as it were a different rheology in itself. The rheology remains VP (the constitutive laws) but EVP introduces artificially slow elastic waves for numerical (claimed) convergence. It is actually very akin to the pseudo-compressible method that was once in favour in computational fluid dynamics for solving for incompressible flows. Hunke use "EVP model", Koldunov et al. "EVP solution" and Bouillon et al. (2013) "EVP method".

Reply: we have revised "EVP rheology" into "EVP model", as indicated by the reviewer.

5-ndte=960 does not seem to be enough in TS005 as we still see some blue in the CAA (fig.13), whereas, in the coarser resolution runs, the region is fully white. Same can be said for the central Arctic where it takes an increasing ndte to get the equivalent pattern in TS045, TS015 and TS005 respectively (.e.g the pattern of (TS005, ndte=240) is equivalent to (TS015,ndte=120), and that of (TS015, ndte=960) is in between to (TS045, ndte=240,960)). Same can be said from Fig.12. By the way, I like very much the colour scale in fig. 13, much easier to see patterns.

Reply: As pointed out by the referee, NDTE=960 is arguably not sufficient for the deformation fields to reach (asymptotic) convergence, as evident in Fig. 13 as well as Fig. 12. We have carried out the same experiment with higher NDTE values (i.e., 1920), and indeed the deformation fields are further "cleaner" than that for NDTE=960. Therefore, we argue that with even higher resolution, with the version of EVP solver we adopt, NDTE values should be further enlarged to ensure convergence.
Besides, as pointed out by the referee, we have had intentionally picked a more proper colorbar in highlighting the differences in deformation rates between the range of 0.25%/day and 2%/day.

6-ndte is not indicated in fig.7-8-9. There is only one mention in the text that ndte=240 is used for all comparison of the ice deformation and scaling analysis until the next section. I would appreciate a reminder in the captions for fast browsing readers like me who like to go and forth through a paper and its figures.

Reply: we would like to clarify that, for all the scaling analysis in Sec. 3.2 and Sec. 3.3, we use the model output with NDTE=960. This is pointed out in Sec. 3.2, P12:L5-6. But in Sec. 3.1, since long-term, >30-yr experiments are needed to analyze the spin-up process, for which only NDTE=240 runs are available, the analysis of basin-wide thickness and seasonal cycles are carried out (e.g., Fig. 5 and related texts). The referee is kindly guided to Fig. 4 for the specific experiments and their configurations.
Furthermore, in order to elucidate these details and avoid misunderstanding, we now have emphasized that we have used NDTE=960 results in the captions of Fig. 7 through 11.

7-Missing Figure S4?

Reply: the authors sincerely apologize for the missing of Fig. S4, due to uploading an older version of the supplementary. We have updated the supplementary (which contain updated figures, and newly added ones of Fig. S4 and Fig. S5). For a quick view, we have attached Fig. S4 as below, showing the geolocations in CAA where landfast ice manifest in our model.

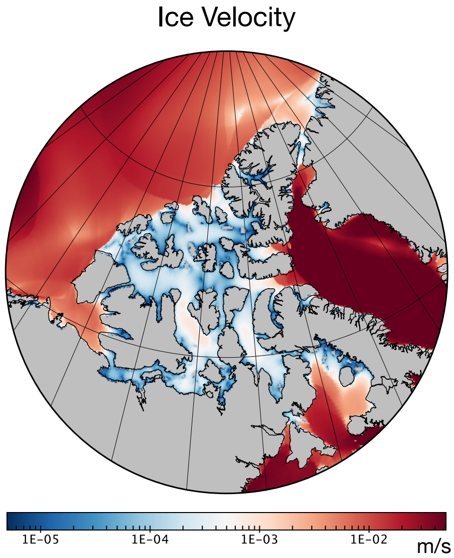

Fig. S4 Two-week mean sea ice velocity in Canadian Arctic Archipelago. First two weeks (Jan. 1st to Jan. 14th) of model output with TS005 is used to illustrate the region of landfast sea ice during winter.

8-Please include drags in Table B1.

Reply: details of the model parameterization for atmosphere/ocean-ice drag are added now in Appendix B and Tab. B1.

9-Appendix B: should you take into account grid deformation in your calculation of deformation?

Reply: we confirm that we have taken into account the different grid scales during the line integrals computations. Regarding the grid deformation, the largest spatial scales during scaling analysis (Fig. 10) is about 200~300km, at which the grid deformation is still very small in terms of isotropy and orthogonality, we do not consider it an important issue in our analysis. However, when the grid is highly deformed, it should be incorporated in the analysis, by adjusting the analyzed region to maintain a generally isotropic and convex shape.

minor comments:

why TS005 on year 31 (or end of year 30) does not match that of TS015 in fig.4?

Reply: we would like to clarify that, the SIE and SIV values in Fig. 4 are annual-mean values. During the transitions from TS045 to TS015 and TS015 to TS005, the model inherently undergoes a shock, for which dynamic and thermodynamic processes are both contributive factors. The sudden jump is also witnessed between TS045 and TS015 (on year 26), which is only less evident than that between TS015 and TS005 (on year 30).
Therefore, these jumps are also of a technical causes. If we had plotted daily mean SIE or SIV, the shock is gone. But we choose to plot annual mean values, to avoid very busy graphs with all the

seasonal cycles of over 30-year's data.

Abstract, line 5: I would "the" before "Community Earth System Model"

Reply: we have added "the" as indicated by the referee.

Abstract, line 8. "In specific" does not sound English

Reply: we revised it into "Specifically".

page 13, line 17-21: are still talking about fig.9? The term "cumulative" has been dropped here.

Reply: revised by adding "cumulative" before "PDF".

page 13, line 23 one instance of "CDF" remains (the others have been converted to "cumulative PDF") I don't think that the figure 9 has been corrected in that regard.

Reply: revised by replacing "CDF" with "cumulative PDF". The caption of Fig. 9 is also revised accordingly.

page 13, line 24-35: "Also, since we witness flatter cumulative PDF slopes in scaled datasets, we expect the "real" tails of cumulative PDFs at 2.4 km flatter than the modeling result from TS005, which have the slope of -1.0 for Dec. 20th, and -0.5 for Feb. 6th." Honestly, I don't understand the sentence anymore.

Reply: we apologize for not conveying the message properly. The sentence is revised as follows: "*Also, since we witness flatter cumulative PDFs for the scaled results of TS005 than TS015, we expect that at the spatial scale of 2.4 km (TS005's native resolution), the physical cumulative PDFs are flatter than the model output of TS005, which have the slope of -1.0 for Dec. 20th, and -0.5 for Feb. 6th.*"

page 13, line 25 "we expect the "real" tails of cumulative PDFs at 2.4 km flatter than..." missing "to be" before "flatter"

Reply: added "to be", as indicated by the referee.

page 13, line 27 "This is due to the temporal averaging on Eulerian grid points attenuates" misses a "which" before "attenuates"

Reply: added missing "which" before "attenuates".

page 14, line 24 "sea ice status" I think the authors meant "sea ice state"

Reply: revised. See also the reply below.

page 14, line 25-36: the whole following sentence "Since the experiments in this study target at climatologlical sea ice states" could be phrased better (these experiment in this study aim at obtaining a climatologlically converged sea ice state?)

Reply: the sentence is revised as: "*the experiment in this study aim at obtaining a converged sea ice state that reflect reasonable Arctic climatology, …*".

CORE instead of CORE2 in appendix B, line 21.

Reply: corrected.

References:

Girard, Lucas, et al. "Evaluation of high-resolution sea ice models on the basis of statistical and scaling properties of Arctic sea ice drift and deformation." Journal of Geophysical Research: Oceans 114.C8 (2009).

Ungermann, Mischa, et al. "Impact of the ice strength formulation on the performance of a sea ice thickness distribution model in the A rctic." Journal of Geophysical Research: Oceans 122.3 (2017): 2090-2107.

Murray, Ross J. "Explicit generation of orthogonal grids for ocean models." Journal of Computational Physics 126.2 (1996): 251-273

Hunke, Elizabeth C. "Viscous–plastic sea ice dynamics with the EVP model: Linearization issues." Journal of Computational Physics 170.1 (2001): 18-38.

Bouillon, Sylvain, et al. "The elastic–viscous–plastic method revisited." Ocean Modelling 71 (2013): 2-12.

Koldunov, Nikolay V., et al. "Fast EVP Solutions in a High-Resolution Sea Ice Model." Journal of Advances in Modeling Earth Systems 11.5 (2019): 1269-1284.