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Geoscientific Model Development Discussions

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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The paper describes the creation of a multi-resolution suite of grids for CESM with a focus on their use for sea-ice modelling in the Arctic. The authors study the effect of grid resolution and number of EVP subcycling steps on the statististical properties of sea ice deformation as well as sea ice extent and volume. In particular, the localization of sea ice deformation in shear and failure lines such as leads and pressure ridges is studied. The authors present their model configuration as a starting point for more dedicated studies on sea ice dynamics and climate simulations and share the corresponding code and data. The simulations are analysed without optimising model

parameters to the specific grid resolution and the evaluation of the simulations need to be improved. Therefore, I recommend the manuscript for publication in Geoscientific Model Development after consideration of my general and specific comments.

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General comments:

1) The authors present untuned model runs with biased ice thickness fields (with too thick ice in the Beaufort Gyre) and also the ice volume in the simulations differs with the resolution used. However, the authors describe good agreement of sea ice coverage and volume of all simulations although a comparison with a sea ice thickness product, e.g. PIOMAS, is missing. So first, I suggest a thorough evaluation of the ice thickness and to study the differences in sea ice state between the different resolution simulations. I see two potential ways how to handle these different resolutions simulations that produce different sea ice state:

(a) If you are interested to study the effect of different resolution on sea ice dynamics (which is the topic of Section 3.3), all simulations should produce comparable sea ice distributions (concentration fields thickness as well volume and extent). Otherwise it is not possible to disentangle the effect of the change in resolution and the change in sea ice state on the dynamics. Systematic tuning methods (Massonnet et al., 2014; Ungermann et al., 2017; Sumata et al., 2019) could be used for all three simulations to optimize the parameter choices for each simulations by minimizing the model-observations misfit (for instance concentration, thickness, and drift). To resolve the issue of too thick ice in the Beaufort Gyre, drag coefficients and the ice strength parameterization could be tuned. The tuned simulations are then a good starting point for further multi-resolution studies and also the various parameters determined in the optimization will provide insight in how model physics change with resolution.

(b) If such systematic tuning is not possible due to limited computational resources, the authors should be more cautions with statements regarding the good agreement of ice thickness fields and agreement of all three simulations. The differences between the three simulations should be described and interpreted in details. Possible reasons for the different sea ice distributions should be provided along with guidance what limitations with regard to the kinematic studies originate from the different sea ice distributions.

2) The analysis of deformation rates in the manuscript is limited to two 3-day intervals. Since the scaling properties of sea-ice deformation are highly variable (Stern & Lindsay, 2009) and strongly impacted by atmospheric winds (Herman & Glowacki, 2010), limiting the analysis to such a short time interval does not allow robust conclusions on the model capability to simulate multi-fractal deformation rates. It can not be excluded that the two dates chosen for the analysis mainly highlight the imprint of the atmospheric forcing. Another problem with the too short interval are the CDF of deformation rates that do not show power-law tails due to strong fluctuations (although stated differently by the authors). I suggest to extend this analysis to at least one entire winter. This will reduce the impact of specific wind conditions, smoothen the CDFs, and allow a more robust interpretation of the presented results with regard to the models ability to simulated strongly localized deformation rates along leads and pressure ridges. In addition I recommend to remove all statements on temporal scaling based on these two 3-day intervals from this manuscript, as now temporal scaling analysis is performed by the authors.

3) The good agreement of deformation fields between the different resolutions surprised and impressed me. In your simulations only the degree of detail in deformation feature increases, but the general patterns agree across the different resolutions. Knowing that ice fracture is a chaotic process that is very sensitive to small variations in ice strength these results puzzles me, as I was expecting that the deformation fields diverge very fast due to the different deformation history. At high resolution, a deformation event which is associated with divergence reduces concentration and thickness, and thereby the ice strength, such that deformation is more likely to appear in the same

spot again. This effect should not be so effective in coarse resolution simulations as the reduction in concentration and thickness is much smaller due to the size of the grid box. This different memory should cause different reactions to the same atmospheric forcing. Do you see a reduction in concentration and thickness along the simulated LKFs in all your simulations? Do you see reoccuring deformation lines in all simulations? Your results indicate rather that in general this described feedback is not so strong and that fracture is mainly driven or better prescribed by the forcing, which would be an interesting result. This aspect of your results is definitely worth more discussion in the paper and maybe some additional analysis.

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Specific comments:

P1, Line 3, "multi-fractality": of what? Please add scaling of sea-ice deformation

P1 Line 19, "kilometer-scale" satellite observations: SAR images have a resolution in the range of tens of meters. The drift and deformation products derived from consecutive SAR images have a kilometer-scale resolution. Please be more specific.

P2 Line 1, "Linear kinematic features": You have not described what these linear kinematic features are. Please describe once what they are (failure and shear lines where deformation is localized).

P2 Line 7: In the VP framework, the transition between viscous and plastic deformation depends on the stress states and not the concentration. The concentration influences the stress states by scaling the ice strength, but there is no direct link as suggested by your description. Please clarify.

P2 Line 14: CMI -> CMIP (here and elsewhere in the manuscript)

P2 Line 15: This is true for VP/EVP models. For other rheologies that include memory of past deformation, as the Maxwell elasto-brittle rheology, also coarser grid resolution might produce similar deformation statistics.

P2 Line 18: The continuum assumption is part of all continuum sea-ice models regardless what rheology they use. Please consider not explicitly mentioning the rheology here.

P2 Line 22, "main driver": It is not clear to me what you mean with main driver. Please clarify this sentence.

P4, Table1: Please be more specific with the grid descriptions in the “Notes: column, such that the table is understandable without reading the text. There is enough space for that.

P4 Line 21-22: at the grid location and 60 vertical layers,. . .

P4 Line 25: Please rewrite sentence.

P6 Figure 3: Please think about using the same limits for the contour plots for both grids. This would make it easier to see the difference between them. The contour lines are also hardly visible, you might also want to use a brighter red instead.

P8 Line 6-8: The thickness anomaly in Beaufort Gyre could also be caused by too weak ice and not properly tuned ice strength parameterization. The thick ice north of CAA and Greenland is then advected by the ice drift and accumulates within the Gyre.

P8 Line 13, "With the warm start-up, the experiments with TS005 approaches equilibrium towards year 42.": Only for the extent, the volume is still decreasing. Please clarify.

P8 Line 17, "The overall sea ice coverage and volume of TS005 is also in good agreement with satellite observations and PIOMAS dataset.": I would not describe the strong overestimation of sea ice extent in winter as a good agreement. In addition, I miss the comparison with the PIOMAS dataset in the figure. Please state where to find this comparison.

P8 Line 17-19: I do not understand why using the same parameterizations for all three

grids is a reason for reasonable results. It is known that model parameters need to be adapted to different grid resolutions to show similar physics (e.g. Williams & Tremblay, 2018). Please clarify or rewrite.

P9 Line 8, "removed of seasonal cycle": -> and the seasonal cycle is removed

P10 Figure5, "satellite-observed": Please state which satellite product is used for this comparison

P10-11 AO index analysis It is not clear why this analysis is needed here. As the corresponding explanation is rather complex, please consider to remove them from manuscript for clarity.

P11 Line 8: sybcycle count -> subcycles

P11 Line 17-18: "The kinematic features with TS005 are richer and much narrower, such as the network of shearing in Beaufort Sea." Do you want to say that in TS005 more and finer features are simulated?

P11 Line 35: The region for the analysis you have chosen is problematic as it mixes pack-ice regions with coastal regions. In coastal regions stable deformation features, like flaw lead, are found that show nearly constantly very high deformation rates, which impacts the presented CDFs. I suggest to use the entire Arctic Ocean as study region and filter all grid points that are closer than 150-200km to the coast as done in other scaling studies.

P12 Line 3: Please be cautious for two reasons: (1) just because the PDF/CDF of sea ice deformation shows a power-law tail does not mean it is multi-fractal. To show mulit-fractality a scaling analysis of the moments of sea-ice deformation need to be performed that shows a non-linear convex structure function (you do this analysis but it is described later). (2) The distributions shown in Figure 9 show hardly power-law distributions. I suggest to use the methodology of Clauset et al. (2009) to test for power-law distributions.

P12 Line 6: What do you mean with "spatial scaling”? Are you coarse-graining the high resolution simulation to coarser grid resolution? Please clarify.

P12 Line 6-21: (1) The CDF in Figure 9 hardly show power-law tails and deviate from strait tails. It is not clear how you determine the power-law slopes. I recommend to use larger time intervals for the analysis to reduce the imprint of certain atmospheric forcing conditions and second to use the methodology presented in Clauset et al. (2009) to test for power-law distributions.

(2) It is unclear to me how you relate the slopes the CDF-tails of coarse-grained deformation rates to the nominal resolution of the grid. Please describe this concept more in detail.

P12 Line 22-26: Given the limitations of your analysis (short-time interval, no clear power) I do not recommend a direct comparison with observations or at least mention these limitations.

P12 Line 32, "about 1.3 on Feb. 6th for all three grids": I see values from 1.2 to 1.3.

P13 L1-2, "Furthermore, no positive value of β is detected at q = 0.5, which is consistent with Marsan et al. (2004) (Fig. 4 of the reference).": Please clarify. In Marsan et al. (2004) beta is positive for q=0.5. Also in your Figure 9 beta seems to be positive for q=0.5. What would be the physical interpretation of negative scaling exponents if you find them in your model?

P13 Line 6-8: Please be more specific. Do you mean that with increasing resolution, deformation rates are more localized with yields to more pronounced scaling?

P13 Line 8-9: Temporal scaling is indicated by the decrease in β for the daily field and 3-day field for Dec. 20th, and not evident for Feb. 6th. This could be also caused by just smoothening of deformation fields due to advection. To test for for temporal scaling a Lagrangian analysis is needed that follows the ice deformation with the drift. Please remove this sentence or add analysis.

P13 Line 14, "indicating less dominant large-scale 15 features on Feb. 6th.": or a more heterogenous distribution of deformation rates along the LKFs.

P13 Line 15-16, "Furthermore, there is more effective temporal scaling on Dec. 20th than Feb. 6th, as shown for C-CDFs in Figure 9 and structure functions in Figure 10.": Please remove this sentence since no temporal scaling analysis is done.

P14 Line 4, "Figure 7": Do you mean Figure 12?

P14 Line 6, "noisier": It is really hard to spot the noise in Fig. 12 except you zoom very strongly in certain regions. Could you find better ways to show this? For instance, plot or average the difference between the deformation rates in a grid cell and its local surrounding (couple of grid cells). This would shift the focus on the noise. Or just zoom on a certain subdomain where the noise is seen.

P17 Figure 9: This figure needs more explanation in the caption: What do the colors refer to (NDTE?)? Are 0.05°, 0.15°, and 0.45° the grid resolution and why do you not use the names T005, etc. here?

P17 Line 5, "equilibrium in sea ice thickness and volume": But volume is still increasing, please clarify how this fits to the claimed equilibrium.

P22 Line 17: MITGcm -> MITgcm

P22 Line 20, "with initial study with temporal scaling analysis with 3-day mean drift fields": Please remove, since you have not done a temporal scaling analysis.

P23 Line 22-23: Remove one “in our study”.

P22 Line 28 - P23 Line 3: This paragraph is rather a summary of on going research in the sea ice modelling community and your future plans and not a conclusion of your study. Please remove it here or move to the state of research in the introduction.

P23 Line 5: Which efforts? Please add citations.

P23 Line 30 is -> are

P24 Line 3-4, "Given that the modeled sea ice climatology is reasonable and consistent among the three resolutions": In the high resolution run, the sea ice climatology is distinctively different from the two coarser runs, which indicated that parameters of the sea-ice model need to be tuned for each specific resolution to reach the same climatology. I agree that using a slab-ocean in this study is fine, but further tuning of sea ice model parameters would be required to obtain runs with comparable sea ice climatology. Please elaborate on this.

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