Comment on egusphere-2025-311

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In this manuscript, the author analyze sea ice radar images at high temporal and spatial resolution from the MOSAiC expedition. Using a novel method (described in a previous publication), they extract the deformation rates at unprecedented resolution and perform a scaling analysis of these deformation rates. The results show that the scaling laws of sea ice deformation break around 10^2 m. Below this limit, deformation seems not scale and resemble the uniform viscous deformation of a summer sea ice field where the sea ice internal stress is negligible. This results seem to indicate that sea ice rheological model used at larger scales cannot represent the small scale dynamics below that limit.

I am really excited to the see that progress is being done on analysis of the dynamics of sea ice using data from sea ice radars. This is a very interesting study with a huge potential given the novelty of the dataset, I have however a few comments that I think should be addressed before publication.

1. General comments

I have tried to apply a simple MCC method on sea ice radar images during an expedition prior to MOSAiC, without luck. I am happy to see advancement in the treatment of these images, but I am wondering what is the estimation of the error on the deformation rates, and then what would be the error on the scaling exponents. I have searched the method paper to try to find some pointers but did not find a clear idea. In any case, a description of errors would strengthen this paper, maybe the authors can make an analysis inspired or similar as the one shown in Bouchat and Tremblay (2020).

I am questioning myself about the time selection used for the results. The fact that there are periods with no or very small deformation is very interesting, and confirms the concept of scaling: we know that sea ice kind of deforms constantly on the global scale, the MOSAiC expedition was then probably situated in between zones of high deformation, in the viscous/ductile zone. Then, active zones sometimes go through the sea ice radar range, and you then see a lot of deformation. What would be the results of the scaling analysis if the whole timeseries is used? If only the period with small deformation? I think this is necessary and interesting, and would strengthen the paper,

I have always been wondering if the scaling of shear and divergence would be different at very high resolution like here. My reasoning is that as you now see directly the rigid sea ice body floes, and not only the dynamics of the sea ice aggregate, there might be a difference as floes do not converge. Have you looked and seen a difference?

On significance of this study, I would also add that scaling is good metric for sea ice rheological models, but it is not the only one. A model can have a good scaling law but do not represent other sea ice properties well (see the SIREX 1 and 2 papers in refs). There is so much potential in the new dataset presented here, I feel the authors are barely scratching the surface here. I know that this is out of scope of this paper, I would love to see more.

2. Specific comments

1. Introduction

I was bit surprised by the introduction, the author include a lot of description of the novelty of their study and results already within the second paragraphs and onward (Our work, we describe, we employ...).

The paragraphs here seem to follow a structure: {1: New in our study, including results to address a knowledge gap} {2: what was done before}

while the inverse is usually used: {1: what was done before } {2: where is the knowledge gap}; and then in the last introduction paragraph {3: how we address the knowledge/method gap described above}

This is not necessarily wrong but it feels strange to read when we are used to a more traditional structure. I do not think that new/different ways of writing papers should necessarily be cast away, I wanted to just notify the authors, so they can decide if they want to keep it as it is.

2. Methods

p4, L94: "*To further ensure the robustness against the high-frequency noise in the data, described in Uusinoka et al. (2025), we chose 10-minute intervals between sequential images for sufficient displacements. We use 24-hour trajectories to avoid data loss due to artificial rotations in the radar images.*" I am really confused by this, do you use 24h trajectories or 10min trajectories? this is not clear to me.

Results

p5, L121: do you mean: "*Figure 2 shows deformation rate averaged for the 10 km×10 km sea ice area around ...* "

figure 2 and 3: I was wondering if the average deformation rate is the best to show here, wouldn't the maximum deformation be more adapted, we are interested in the localization of the deformation rate?

p6, figure 3: The units are missing on the left side of the plots, I would recommend to put the season names as subplot titles.

p6, figure 3: Is the "core" vertical scale (not the extensions for very high or small values) is at the same scale in all the panels? please check.

p7, figure 4: a video of the sea ice deformation (shear and divergence) as supplementary material would be very interesting to understand better the dynamics and what is happening here. I especially see that for November, there is an alternance of divergence and convergence withing the studied period. One could add the the radar field as well. Also showing how the deformation looks like at a single 10minute period would be nice as it is what is used for the scaling analysis.

p7, figure 4: Zooming on the figure, we can clearly see an grid like pattern in the deformation which I assume arises from the deformation calculations and uncertainty or noise. We see the same in our recent publication (Plante et al., 2025). I feel this should be described and its impact on results assessed.

p8, L156: "*Figure 5a depicts how the scaling exponent β behaved in the case of observations made over various spatial and temporal scales, L and T, respectively.*" respectively of what? something is missing here.

p8, figure 5: The figure caption should state that the deformation rate for B are for the 10 minutes interval

Discussion

p11, L229: like said in the general comment, I feel there is not only sea ice scaling that need to be addressed for sea ice models.

Conclusions

p12, L246: "*this paper*" I would suggest "*Our analysis of high resolution MOSAiC deformation data...*" or something like that to be more precise.

3. References

Bouchat, A., & Tremblay, B. (2020). Reassessing the quality of sea-ice deformation estimates derived from the RADARSAT Geophysical Processor System and its impact on the spatiotemporal scaling statistics. *Journal of Geophysical Research: Oceans*, 125, e2019JC015944. https://doi.org/10.1029/2019JC015944

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