

Response to the Reviewer Comments on Manuscript “Scale invariance in kilometer-scale sea ice deformation”

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First, we'd like to apologize for the delay in submitting the revised manuscript and response letter. The delay was due to the authors' summer holidays occurring at different times, followed by the corresponding author completing and submitting their doctoral dissertation. In addition, the generation of new deformation data for the error estimates required considerable time.

We'd also like to thank the reviewers for their important constructive comments. All authors agree that the presented comments have strengthened the manuscript quite significantly. This is especially important given that the paper presents new observations where the reliability of the results is absolutely necessary. In the following, we've tried to address all the presented comments. In the revised manuscript, modifications are highlighted in red, with the exception of Appendix B, which is presented in standard font for readability.

Changes made to the revised form of the article:

- In Appendix B1 we provide the error propagation over different scales similarly to [1] and [2] based on the displacement estimate errors from [3]. We also provide estimation on the noise floor based on deformation distributions over a inactive subregion in the radar coverage during both an active and a quiescent period.
- In Appendix B2 we provide spatial scaling estimates during a quiescent period in both the full radar coverage as well as in a inactive subregion showing the checkerboard structure.
- We've applied the suggested text edits, softened the claim of the lower bound slightly for better robustness, and added a short mention of the complementary results from [4].

We hope that these edits sufficiently address the concerns of data quality. We find that the new appendix section strengthens the paper considerably.

Reviewer #1

General comments

We thank the reviewer for the constructive follow-up. We fully agree that a major issue with noisy field data always is distinguishing signal from noise. We've now expanded the manuscript with a new section in the appendix to address this. Since the observations and the methodology are new, we've softened the claims of the article slightly to highlight the fact that further observations are needed to verify the existence of such lower bound. To address the concerns about data quality at higher resolutions in the new appendix section, we

- 1) *quantify propagated errors and the noise floor. We use the error-propagation framework of [1] and used by [2] with error estimates from our synthetic tests in [3]. This provides explicit estimates of relative strain-rate errors as a function of spatial and temporal scale similar to previous work.*

- 2) *compare noise distributions during quiescent and active periods. We estimate the deformation noise level to be in the order of $\sim 10^{-3} \text{ h}^{-1}$, which is roughly an order of magnitude smaller than mean deformation rates during active events. On top of the signal-to-noise perspective, we justify concentrating the analysis to active periods by showing that the assumed noise distribution is narrower and thus more predictable during active periods.*
- 3) *test scaling during a quiescent period both with full radar coverage as well as with an inactive subregion in the radar data that is dominated by the checkerboard. The scaling seems to remain power-law-like with $R^2 \geq 0.95$ with a slight increase for $L \geq 10^2 \text{ m}$. In the inactive subregion we do not see this increase.*

We hope that these additions address the concern that noise could affect the scaling analysis. While we believe we cannot fully resolve the question without independent work with other datasets and deformation estimation methods, these error estimates are assumed to give a clearer picture of the presence of noise in the analysis. We've revised the main text slightly to highlight that future work using complementary data sources will be needed to fully establish the robustness of the lower bound at $\sim 10^2 \text{ m}$.

Specific comments: Text

- 2.1 *Page 2, line 50: There is some logic missing in this sentence. The fact that the study area has major and smaller deformation periods does not relate to the first clause in the sentence "We present three key findings that are all important to account for in modeling of sea ice". This is a description of your data, not a general finding.*

We've now rephrased this paragraph to better distinguish between data description and general findings.

- 2.2 *Page 3, line 60: Hutchings et al. (2024) found a transition between 5 and 10km, which is close to the edge of the range you can resolve in your study. They also had a lower resolution limit of more than 100m. So your findings are not incompatible with those of Hutchings et al. (2024), you would need a larger region to coarse grain over to capture the 1-100km range where a transition at around 10km is apparent. I understand that you explain this better in the discussion of the paper (page 12, lines 236-245), but feel that on page 3 the reader could be misled by your phrase.*

The sentence has now been changed to emphasize the complementary nature of our paper and Hutchings et al. (2024) already in the introduction.

- 2.3 *Line 71: "the radar signal" → "a radar signal"*

This has now been fixed.

- 2.4 *Line 75: check punctuation, a full stop is doubled.*

We've removed unnecessary punctuation to increase readability.

- 2.5 *Page 8 line 168: Should $T \geq 10 \text{ min}$ be $T = 10 \text{ min}$?*

We've now changed it to " $T = 10 \text{ min}$ ". Thank you for noticing!

2.6 Figure 5: Explain what the dashed line is (L_c ?) in the caption.

We've now added a description for the dashed line in Figure 5.

Reviewer #2

Specific comments: Text

2.1 Thanks for this addition, it addresses by comment, I would only recommend writing " [...] and presented in Uusinoka et al. (2025, Supp. S2) –demonstrate that localized [...]" to help the reader find the information, using `\citet[][Supp. S2]{bibkey}`.

This is a good point and something we should've already added earlier. Thank you for pointing this out!

2.2 Following comment 2.3 of reviewer 1, I also wondered about the units consistency of Equation A2 with the Einstein notation. After careful reading, I think the notation and units are correct because $\mathbf{u}$ is not the velocity (as one would expect from the $\mathbf{u}$ usual convention) but the displacement vector in [m], hence $\frac{\partial u_i}{\partial x_j}$ is unitless, and so is $\frac{\partial u_k}{\partial x_i} \frac{\partial u_k}{\partial x_j}$. I am not sure your answer points this out. However I am not sure how one would do to compute directly the strain rate by using the velocity instead of displacement.

To encompass the reader better, we've added a small extension to the verbal description of strain tensor to highlight that $\mathbf{u}$ is the displacement vector: "The components of $\mathbf{E}$ defined as a function of the displacement gradients are given by ...".

REFERENCES

- [1] J. Hutchings, P. Heil, A. Steer, and W. Hibler III, "Subsynoptic scale spatial variability of sea ice deformation in the western weddell sea during early summer," *Journal of Geophysical Research: Oceans*, vol. 117, no. C1, 2012.
- [2] A. Oikkonen, J. Haapala, M. Lensu, J. Karvonen, and P. Itkin, "Small-scale sea ice deformation during n-ice 2015: From compact pack ice to marginal ice zone," *Journal of Geophysical Research: Oceans*, vol. 122, no. 6, pp. 5105–5120, 2017.
- [3] M. Uusinoka, J. Haapala, and A. Polojärvi, "Deep learning-based optical flow in fine-scale deformation mapping of sea ice dynamics," *Geophysical Research Letters*, vol. 52, no. 2, p. e2024GL112000, 2025.
- [4] M. Uusinoka, A. Savard, J. Åström, J. Haapala, and A. Polojärvi, "Threshold domain sizes for multifractality in sea ice deformation," *Geophysical Research Letters*, vol. 52, no. 16, p. e2025GL116833, 2025.