We sincerely thank both reviewers for their insightful comments and constructive feedback. The suggestions have significantly improved the quality of our manuscript. We appreciate your time and effort. Additionally, we thank both reviewers and the editor for their patience in waiting for our response. Due to several circumstances, we needed to delay the work on this review. The review has also led to some additional changes in the manuscript, which we want to summarize here:

- *We have increased the amount of simulations to address the reviewer ones concerns about the limited number of simulations. Every initial ice thickness h_i features now 5 simulations, which have different initial conditions regarding the inhomogenity.*
- *During the review, we realized that the description of the initial setup in HiDEM did not fully represent the simulations, but showed some outdated ideas from initial simulations. The whole ice in HiDEM has inhomogenity (there is no boundary region!) and the beam probability was between 0.4 and 0.6.*
- *A new section is added to the beginning of the discussion about the simulation setups and their potential implications on the results.*
- *A new discussion paragraph about how the resolution of the results of HiDEM influence the ITD as well as what implications this result has on comparability with observations.*
- *A new section is added to the appendix to better illustrate the variability of both the HiDEM and neXtSIM simulations. This appendix includes a Figure of the final deformed ice field for each simulation and a comparison of the ITDs of each simulation ensemble.*

All answers to the specific comments are written in cursive blue next to the original comments in black.

General Comments

In this study, a Discrete Element Model is used to assess how triangular and trapezoidal sea ice ridges contribute to the ice thickness distribution (ITD). Results are compared to the output of a continuum sea ice model and two commonly used ridging functions. The study consequently suggests how the new learnings can be translated into an analytical redistribution function which captures the effect of ridge shapes on the ITD.

Overall, the manuscript gives relevant, interesting and new insights into modeling of sea ice ridges, which makes it well suited for publication in The Cryosphere. The manuscript is for the most part well structured and clearly presents the new findings. The English language is used in a proficient and efficient way, with only minor mistakes or typos. I do, however, have some criticism regarding the interpretations and conclusions of the study.

I recommend publishing the manuscript once the issues mentioned below have been addressed, in particular the following three:

1. There are several instances throughout the manuscript where you claim that the DEM improves representation of ridges compared to neXtSIM or the two ridging functions. This claim

is missing its foundation because it is never defined what the baseline for such an improvement would be. How do you determine which is better than the other? The hypothesis that the much more realistic and detailed model setup and the ability to simulate different ridge shapes improves overall ridge representation is of course very valid and logical, but needs to be proven or at least properly discussed. *The main idea of our study is to compare ridges produced by HiDEM with ridges represented in typical continuum sea-ice models. As HiDEM simulates the ridging process explicitly, we compare the results from the other models to HiDEM, with HiDEM representing the "highest resolution" of the process as well as the highest spatial resolution. We agree, that some areas in the manuscript need rephrasing and have adjusted the text. Additionally, the discussion has a new subsection about the model setups and their potential influence on the results.*

Are the results from the DEM closer to observations (see also my comment #2)? *This will be answered in more detail below. Briefly, the ridge shapes observed in HiDEM are ridge shapes commonly found in nature; thus, HiDEM appears to have the ability to represent ridges as they can occur in nature.*

Similarly, how do you determine that the new redistribution function $n(h)$ is "better" than previous solutions? *$n(h)$ was developed as none of the previous solutions were able to fully describe the ITD from HiDEM. Further, we could connect the shape of the ITD to the shape of the ridges. Previous solutions did not account for different ridge shapes. The ridging function based on HI80 accounts only for triangular ridges, while LI07 is based on observations over larger areas of the deformed ice cover. Determining whether $n(h)$ would be more accurate in large-scale continuum models would require it's implementation into them. Here, we can only say that it gives the most accurate representation of HiDEMs ITD, while also accounting for two ridge shapes. Thus, $n(h)$ is connected indirectly to the ridging process, which makes it an extension of HI80 and following a different approach than LI07. We will adjust the discussion to clarify some of these statements.*

This comment relates to several instances in the paper:

- Line 11-12: "Our results demonstrate that an improved representation of ridging is needed within continuum models ...": In the abstract, such a statement is acceptable if backed by argumentation in the paper's results or discussion and a definition of what "needed" means. However, I don't think this is sufficiently demonstrated in the manuscript. *This sentence is reformulated.*

- Line 193-195, in particular the words "underestimate" and "benefits" are evaluative without an argumentation and with the implicit assumption that HiDEM / the sub-grid parameterizations do a better job. Please convince me that they do. *In this paragraph, we have estimated the depth of deep ridges by using the 99^{th} percentile $P99$. If we investigate the values from neXtSIM more closely, $P99$ is between 1.5 and 3 times the initial ice thickness h_i, which would be quite shallow ridges. The sub-grid parametrization based on Hibler (1980) (HI80) estimates the maximum ice depth based on an empirical formulation which is comparable to values from observations, see for example Amundrud et al. (2004), who found a maximum depth of ridges of of $20\sqrt{h_i}$. These observations are also more discussed in the discussion of the manuscript. We have reformulated this line to highlight that neXtSIM underestimates the thick ice compared* *to the other methods.*

- Implicitly, the same assumption is made in lines 340-349 and 359-361. *Similarly as the answer above, this statement refers to the observation that the two-level representation of the ice thickness, as done in neXtSIM, shows rather shallow ridges under compression compared to the other methods as well as to observations. We have made adjustments to this paragraph to tie it more to the arguments before.*

2. It is clear that an extensive comparison of your results to observations is neither feasible nor the point of your study. However, the manuscript in its current state feels quite detached from reality and would strongly benefit from at least a better qualitative overview of the difference between simulated and observed ridges and ITDs. A suitable partner for comparison would be observations of landfast young/first-year ice which is constrained in its motion and has not experienced many deformation events.

Such a comparison would be useful in several instances throughout the paper:

- Figure 3 shows the cross-sectional shapes of the simulated ridges. How close are these to observed ridge shapes? *We have significantly extended the first paragraph on ridge shapes and the ITD within the discussion. Therefore, we used the results from the recent study by Eilers et al. (2026), which, for example, showed that ridges with a depth from 2 to 8m are mainly trapezoidal. All ridge keels from HiDEM for $h_i = 0.5m$ and most of the keels for $h_i = 1.0\,,m$ are below 8 m depth and demonstrate a high fraction of trapezoidal ridges. They observe a different ridge shape for deeper ridges compared to the trapezoidal ridge shape for HiDEM ridges for $h_i = 2.0\,m$, but we argue that this discrepancy can be explained with our setup. More importantly, we argue that this discrepancy does not devalue our observation of triangular and trapezoidal ridges at the same time for the thinner ice categories.*

- The beginning of section 3.1 / lines 153-160: Please describe qualitatively which of those better meet your expectations in relation to observations. Optionally, you could include an comparable observational example, e.g. from airborne laser scanning, in Fig. 2. *We do not plan to include an observational example in Fig. 2 as observations are conducted over an ice cover, which might have experiences several deformation events and, thus, are very hard to compare. The spatial distribution of ridges for HiDEM and neXtSIM both contain features, which are expected from observations. In HiDEM, the ridges are clearly localized. In neXtSIM, the ridge distribution shows intersection angles, which are commonly measured in larger scales. We will add this information to the paragraph. Nevertheless, the failure pattern in neXtSIM is also clearly dominated by the Mohr-Coulomb failure pattern. The relationship between these angles and different rheologies is also discussed by Ringeisen et al. (2019), which highlights the intricacies of simulating these patterns accurately. This topic will also be included in the discussion of the simulation setups.*

- Line 256: "Field observations have shown ...": This is a sentence that I would welcome much earlier in the manuscript, ideally already in the introduction, or where you introduce triangular and trapezoidal ridges. *We have added this information briefly into the results, where the classification of ridges is described.*

- Section 4.1 includes some limited comparison to observations. You bring up the difference between the modeled ITD and observations and give reasons for these differences. However, the comparison is rather shallow and I am missing an evaluation of your result against observations that are better comparable than the mentioned studies: *Overall, we agree with your sentiment that a better comparison to observations would be beneficial. To our knowledge, there are not that many studies which analyze the ITD over rather smaller area with very similar sea ice thickness, which would make the conditions comparable to our simulations. We hope that the added knowledge from observations at other stages in the manuscripts (e.g. the ridge shape) helps adding more comparison to observations to this study.*

- If possible, please show the ITD of a comparable observational study in one of your figures (this could, for example, be the one by von Albedyll which you already mention in line 285) and describe and explain the difference using this direct comparison.

- Please also have a look at Fig. 1b in Sumata et al. (2023). Their ITDs in the Fram Strait seem to be closer to your results than the mentioned negative exponential function in other observations. Can you comment on this? *Thank you for making us aware of Sumata et al. (2023). The ITDs are very similar to ours indeed, with the second modal peak describing ice from the Central Arctic being transported through the Fram Straight. Assuming that this ice experiences a lot of sea-ice deformation throughout its life, it is suitable to compare the second modal peak to our observed distribution of ridged ice. We will include this comparison into the discussion.*

- In discussion and conclusion, I invite you to suggest that a more extensive and also quantitative comparison to observations would be an interesting topic for further research (if you agree). *We agree with this statement and have included a paragraph in the discussion.*

3. The introduction: The pure summary of current knowledge is relatively short (lines 17-45). I don't mind this, as long as relevant background information is addressed and literature is cited, which is well done here. Starting from line 46, further background information, including important references, is mixed with a short summary of the current study. Although a bit unconventional, I enjoy the way this introduces the reader to the research done instead of simply listing one reference after the other. However, this section now also contains results and interpretations which do not belong in the introduction because they are based on methodology, assumptions and limitations that have not been mentioned in details so far and thus cannot be comprehended by the reader at this point. Rather than mentioning these results and interpretations, I encourage you to focus more clearly on your motivation and the aim of your study here, which are both only implicitly mentioned. Hypotheses and a description of how you will approach the research questions are of course welcome in the introduction, as well as the manuscript outline (currently lines 70-76).

In particular, I am referring to the following passages:

- Lines 58-63: Presenting results.
- Lines 63-66: Interpretation and conclusions (see also comment #1).
- Lines 68-69: Interpretation, but this could easily be converted into motivation.

Thank you for the feedback on the introduction. The paragraph in question has now been refor-

mulated. We have reformulated this section to have more emphasis on the aim and objectives of the study.

Specific comments

- Line 19-20 "Overall, ridging has a higher influence on the ice volume via the thickness compared to the ice area." I understand what you mean, but it is not very clearly phrased, especially as it sits right at the beginning of the introduction, where the reader is just getting started and warming up their brain. Please consider rephrasing. *This sentence was rephrased.*

- Line 45/131: "... assuming an exponential distribution of smaller ridges compared to deeper ridges": In one of these instances, please mention that such an exponential decay is known from observations. The references you use later (line 280) are also sufficient if mentioned here. There are of course many more, see e.g. Wadhams (1980) or more recently Rabenstein et al. (2010), ... *The text was adapted and we included Rabenstein et al. (2010) and Wadhams (1992).*

- Line 51: "... demonstrated its capability to simulate ..." : Demonstrated how? In relation to observations? This is very interesting because it has the potential to justify some of your later statements, e.g., regarding your comparison of HiDEM and neXtSIM, and the discussion on which one performs better in certain tasks, see General comment #1. *Yes, in relation to observations. We have extended the information there with the following information: Åström et al. (2024) compared sea-ice fragmentation simulations of the Kvarken area of the Baltic Sea with satellite images, with the model successfully replicating a significant portion of the fracture patterns, fast-ice distributions, and ice drift patterns. Further, they conducted simulations of ice compression in the Gulf of Riga, which showed a floe-size distribution and ridge development aligning with observations.*

- Line 103: "Nevertheless, ..." I suggest to change this to "Therefore, ..." or something alike. Averaging is well justifiable because the ITDs are so similar. *Changed.*

- Line 111: " ... via a mean thickness": Please explain how this mean thickness is defined (over what area, ...?) I would appreciate some more detail on how this works. *In neXtSIM, the sea-ice thickness is represented by one thickness value per grid cell, representing the mean thickness in this area. The sentence was adjusted to "The sea-ice thickness, and changes due to deformation, are represented with one thickness value for each grid cell representing the mean thickness for the area of that grid cell."*

- Line 136: "the sum of A(h)": Why do you here write "the sum of A(h)", and later just "A(h)" when in both times you refer to the same area? *Using "the sum of $A(h)$ is correct in this case as we want to compare the whole area in to the observation area and $A(h)$ by itself is not one value, but the amount of area per thickness category h. This mistake is now adapted in the text.*

- Line 143-144: Please include a definition for the empirical thickness H*, either its physical interpretation (if possible), or describe what role it plays. In this paragraph and throughout the manuscript, please make sure to either consistently indicate the unit for H* (m, according to you and Hibler, 1980) or explain if you have a good reason to omit it. The unit is missing in lines 144, 239 and 309, and possibly other instances. *The unit is added to all mentions of H_*. Within the redistribution scheme, H_* influences the maximum depth of deformed ice, which is the same as the depth of ridges. In practice, H_* is also a tuning parameter for the distribution. We adapted the text introducing H_* to better reflect the practical function as well as its impact on the redistribution.*

- Line 160: "the number of ridges ... increases with decreasing h_i": Can you clarify whether you mean the total number of ridges or just the number of ridges above a certain threshold (e.g. $3h_i$)? In Figure 2 it looks like the total number of ridges is pretty similar across all h_i. *We have tested this statement with different cut-off criteria c for the minimum keel depth k of a ridge. Thus, a ridge was identified as a ridge once $k > ch_i$ with c being 1.5, 2.0, 2.5 and 3.0. For all these values of c, the number of ridges decreases with increasing h_i. We will rephrase the the text in the manuscript.*

- Line 162: "y = 1.0, 2.0, ...": Do you mean x? Same for "along the y-direction" in the caption of Fig. 3. *Yes, the transects are cut at $x = 1.0, 2.0, 3.0, 4.0$ and 5.0km. We adjusted the text and Fig. 3.*

- Line 170: "Overall, the frequency of trapezoidal ridges increases with h_i.": In Figure 3 it looks like the total frequency of trapezoidal ridges decreases with increasing h_i. Do you mean that the fraction of ridges with trapezoidal shape increases? *Yes, this sentence means the fraction of ridges with trapezoidal shapes. The sentence was removed as it repeats the information stated before.*

- Figure 4: Can you please clarify whether this shows the ITD based on area or volume? *This shows the ITD based on area. The Figure 4 caption is adapted.*

- Line 179-181 "HI80 is, ...": I don't understand this sentence. Please rephrase, maybe split it up. *The sentence is split up and reformulated.*

- Line 203-204: And this constant value is the value k_{max} has at t=2h? Please mention explicitly. *Added a sentence to mention it explicitly.*

- Figure 7: Even though this is just a conceptual figure, please label the axes to give the reader an easy access to the understanding of their physical meaning. *The figure will be adapted to include the axes labels.*

- Line 215-217: "Thus, this boundary..." Please consider rephrasing this sentence to make it better understandable. *The sentence is split up and reformulated.*

- Line 225: "a higher α results in a more pronounced "bump", signaling more trapezoidal ridges": Shouldn't it be the other way around? *Yes, this is correct. The text is adapted.*

- Line 282: "ice deformation that has occurred in multiple stages over longer periods": Obviously it is much beyond the scope of this manuscript to implement anything alike, but can you comment on the feasibility of modeling such multiple ridging events over longer periods? *The feasibility of modelling multiple ridging events is mainly limited by the simulation time. If one would allocate a lot of simulation time, this could be possible. The simulation time for the here presented simulations varied from around 7500 GPU hours to 1500 GPU hours depending on h_i (which influenced the number of particles). Further, the simulations need to account for* *some form of consolidation within the ridges or restart simulations with consolidated ridges. Otherwise it would be difficult to initiate "new" deformation events, based on the observations made during the simulations presented here.*

- Line 321: "the model developed by Salganik et al. (2020)": Please remind us what this model does and why it is relevant here. I am also not sure if "therefore" is the right word here, this depends on what exactly you would like to say. *The model describes ridge consolidation. The sentence was adapted.*

Technical corrections

- Line 50: "code" → model *Changed.*
- Line 71: insert "the" before "sea ice redistribution model" *Added.*
- Line 90: "being due" → just "due"? *Removed.*
- Caption Fig 3: "... ranged of the axis" → range of the axes *Changed.*
- Line 186: "more smaller ridges" → a higher density of smaller ridges *Text adapted.*
- Line 191: insert "the" before HiDEM *Text adapted.*
- Caption Fig. 6: "Therefore" is the wrong word here. Maybe you mean something like "For this purpose, ..."? *Changed.*
- Caption Fig. 6, last sentence: "limi" → limit *Corrected.*
- Caption Fig. 7, first sentence: "the the" → the *Corrected.*
- Line 240: Remove "thus" *Removed.*
- Line 249: "highlighting" → suggesting, or similar *Changed.*
- Line 256: "a triangular shapes" → decide for "a triangular shape" or "triangular shapes". Same for trapezoidal shapes later in the sentence. *Changed.*
- Line 258: Insert "a" before "variety" *Added.*
- Line 259: "DEM" → DEMs *Text adapted.*
- Line 260: Insert "The" before "number" *Added.*
- Line 305: Insert "a"/"the" before "distribution" *Added.*
- Line 305: Remove "Thus" *Removed.*
- Line 334: "land fast" → landfast *Changed.*
- Line 337: "are" → being. Or split sentences and remove "with" in line 336. *Sentence is split.*
- Line 362: "... captures the triangular and trapezoidal ridge shapes" → captures the effect of triangular ... *Changed.*
- Line 366: remove "of" *Removed.*
- Line 384 "compared to" → consider "as opposed to" or similar *Changed.*
- Line 455: Please include DOI https://doi.org/10.1175/1520-0493(1980)108¡1943:MAVTSI¿2.0.CO;2 *Added the DOI.*
- Please review the use of commas throughout the manuscript. I am not good with commas either, but I suspect there are mistakes in lines 256, 260, 375, 382, 404, 405 (possibly more). *We*

reviewed the whole manuscript for commas and hope, that the amount of mistakes has decreased.

References from the reviewer:

Rabenstein, L., Hendricks, S., Martin, T., Pfaffhuber, A., and Haas, C.: Thickness and surface-properties of different sea-ice regimes within the Arctic Trans Polar Drift: Data from summers 2001, 2004 and 2007, Journal of Geophysical Research: Oceans, 115, 2009JC005846, https://doi.org/10.1029/2009JC005846, 2010.

Sumata, H., de Steur, L., Divine, D.V. et al. Regime shift in Arctic Ocean sea ice thickness. Nature 615, 443–449 (2023). https://doi.org/10.1038/s41586-022-05686-x

Wadhams, P.: A Comparison of Sonar and Laser Profiles along Corresponding Tracks in the Arctic Ocean, Sea Ice Processes and Models, University of Washington Press, Seattle, Washington, pp. 283–299, 1980.

References from the answers:

Amundrud, T. L., Melling, H., & Ingram, R. G. (2004). Geometrical constraints on the evolution of ridged sea ice. *Journal of Geophysical Research: Oceans*, *109*(C6).

Åström, J., Robertsen, F., Haapala, J., Polojärvi, A., Uiboupin, R., & Maljutenko, I. (2024). A large-scale high-resolution numerical model for sea-ice fragmentation dynamics. *The Cryosphere*, *18*(5), 2429-2442.

Eilers, C., & Bradley, A. (2026). Characteristic geometry of keels in Arctic sea ice ridges. *Geophysical Research Letters*, *53*(3), e2025GL119003.

Ringeisen, D., Hutter, N., & von Albedyll, L. (2023). Deformation lines in Arctic sea ice: intersection angle distribution and mechanical properties. *The Cryosphere*, *17*(9), 4047-4061.