

*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.*

*We have increased the amount of simulations to address the general comment about the limited number of simulations. Every initial ice thickness  $h_i$  features now 5 simulations, which have different initial conditions regarding the inhomogeneity.*

*The review has also led to some additional changes in the manuscript, which we want to summarize here:*

- 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we realized that the description of the initial setup in HiDEM did not fully represent the simulations, but showed some outdated ideas from initial simulations.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 inhomogeneity (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. Additional Figures appear at the end of the answers.*

## **General Comments**

This manuscript aims to use a Discrete Element Model (DEM) to explore how the formation of sea ice ridges impacts the sea ice thickness distribution. The manuscript then compares this to how different continuum sea ice models simulate changes in the ice thickness distribution resulting from ridging. In addition, the results from the DEM are used to motivate a new parameterisation of sea ice ridging to use in continuum models. The research presented here is an important and valuable contribution to both improving understanding of an important sea ice process and sea ice model development. In particular, the improved representation of ridge formation and ice thickness distribution in sea ice models has clear potential impacts on sea ice rheology, momentum exchange between the sea ice-ocean and atmosphere, and the sea ice mass-balance. The use of a DEM to supplement observations of sea ice, develop new physical understanding of sea ice, and motivate new parametrisations for continuum models is something that has been applied successfully before e.g. Wilchinsky et al., (2010), Tsamados et al. (2013). Whilst previous studies have applied DEMs to explore sea ice ridging (as acknowledged in

the manuscript), the novelty here emerges from the use of a three-dimensional DEM and the application of the results to motivate a new ridging parameterisation.

The manuscript is mostly well written with a sensible structure, however at times I find the manuscript difficult to follow with key terms not defined and explanations missing. The figures are generally of a good quality but the axes could be labelled more clearly. The manuscript makes an effective use of references both in the discussion and conclusions to provide a clear context for this research. The manuscript also reaches a clear set of conclusions.

I do have some concerns about the methodology. This relates in particular to the limited number of simulations used in the analysis despite the stochastic method to introduce inhomogeneity into the model and the choice to only consider ridging under uniaxial convergent forcing. Addressing the former point at least requires an additional figure and ideally additional simulations to demonstrate limited model sensitivity to how the inhomogeneities are introduced. Whilst the decision to focus on uniaxial convergence to simplify the analysis and reduce the number of simulations can be justified, there does need to be more discussion about the limitations of this for the conclusions reached and ridging parameterisation proposed. In addition, the manuscript would benefit from a more detailed description and comparison of the different models being used in this study.

Overall, I believe that this paper is within the scope of the journal and makes a valuable contribution to the literature. Whilst I do have significant concerns about the manuscript, I believe the paper should be accepted, provided these concerns can be adequately addressed. I provide a more detailed explanation regarding the points made above in the specific comments below.

## Specific Comments

### *Abstract and Introduction*

P1L1: The abstract would benefit from an introductory sentence explaining what a ridge is in the context of sea ice. *An additional introductory sentence is added.*

P2L29: ‘observed fields (10 km)’. It is not clear what the value in the brackets refers to, and it should also be clarified what the term ‘observed fields’ is referring to here. *10 km refer to the spatial resolution of the observed deformation field data. The sentence is slightly rephrased to reflect this information better.*

P2L46-47: What is meant by the term ‘detailed’ in this section? This term should be replaced by something along the lines of ‘explicitly resolve ridge formation’ or ‘with greater physical fidelity’. *The second detailed in this sentence is replaced by explicit.*

P2L54-P3L69: It is unconventional to summarise the key results in the introduction. This section does an effective job of explaining why this research is both important and novel, but the authors could consider presenting these points in the context of what the manuscript aims to do rather than the results. *We have reformulated this paragraph in the introduction. We now give more emphasis on the objectives of the study as well.*

## *Methods*

P3L78-L86: The methods section would be improved by briefly summarising what the section will cover (e.g. a brief summary of the models that will be used). The two paragraphs currently at the start of the ‘Methods’ section could be a sub-section labelled ‘Simulation Setup’ or equivalent. *The beginning of the method section is restructured as suggested.*

P3L88-P4L92: I find the description of the DEM insufficient, given the purpose of the manuscript is to use this DEM to produce insights into ridge formation to apply in continuum models. In particular, an overview should be provided of the equations governing beam failure and ridge formation. *We modified the first paragraph of Section 2.2 to include additional information on the model. We now shortly describe beam failure and failure criterion and explain how ridge formation occurs. We also provide a reference to the description of detailed equations used to calculate beam strain and stresses. The equations are quite lengthy, including a  $12 \times 12$  stiffness matrix of a standard Bernoulli beam. However, if the reviewer wishes, we can add an additional appendix, yet this would just repeat the supplementary material of our recent paper (Polojärvi et al., 2025). Discrete element models do not include equations for ridge formation: Ridges are formed, when the discrete elements move (based on Newton’s laws) and pile up and down. We also added two references, Cundall and Strack (1979) and Potyondy and Cundall (2004) that describe standard DEM and bonded particle models, which HiDEM in essence is.*

P4L96-L98: What is the significance of each 10 m x 10 m grid cell? Is it that each grid cell is assigned a different single value between 0.4 and 1.0 to determine how many beams were removed? If so, this should be made clearer. Is there any physical justification for this approach to introducing inhomogeneity over other potential approaches (e.g. modifications to beam properties such as the failure threshold)? Were any sensitivity studies conducted to explore the impact of the different choices made here (e.g. changing the size of the grid cells, or the value of the beam probability outside the observation area)? *Yes, every single grid cell is assigned a different, random single value between 0.4 and 0.6 to determine how many beams are present at the beginning of the simulation. We have added more information to this section. We chose this approach because beam probability is already implemented in HiDEM, while alternatives such as varying beam strength or thickness are not. Since all approaches introduce local ice-strength inhomogeneity, no major differences in the results would be expected. Different beam probabilities were tested, and the selected value produced realistic, randomized failure patterns rather than straight fractures or particle-packing-driven behavior.*

P4L99-L103: What was the computational cost of these simulations? Given the stochastic method used to introduce inhomogeneity, the authors should ideally produce a small ensemble (of around 10 members) for each value of  $h_i$  tested for subsequent analysis. *We have now increased the number of simulations to five for each  $h_i$ ; the overall results remain similar for all five simulations. Depending on  $h_i$  the simulations varied from around 7500 GPU hours to 1500 GPU hours.*

P4L102-L104: ‘the resulting ITDs were practically identical.’. There should be a plot in-

cluded in the manuscript to compare the results for simulations with the same  $h_i$  (for at least one value of  $h_i$ ) to demonstrate this point. *Thank you for the suggestion. As mentioned above, the new manuscript will feature a comparison of the variability of the simulations in the appendix. We will also add the Figure showcasing the variability of the ITD from the individual simulations at the end of these answers (Fig. 1).*

P4L105: What was the motivation for choosing  $4h_i$  (rather than a fixed resolution for all values of  $h_i$ )? Could a value of 50 m not have been used, to ensure consistency with neXtSIM output? *We choose  $4h_i$  as the resolution for HiDEM to utilize on the higher resolution of the HiDEM simulations and to not lose any detail of the ridges. Based on this comment, we have also investigated results for HiDEM with a spatial resolution  $dx$  of 50 m, which is the spatial resolution of neXtSIM. At first glance, the comparison of the deformed ice field looks very similar, but if we compare a zoomed in area (Fig. ), here  $3 \times 3\text{km}^2$  between both resolutions, it becomes clear that HiDEM with  $dx = 50\text{ m}$  loses some of the details regarding the ridges. This loss of deeper ridges also influences the shape of the ITD. This result will be included in the new manuscript, as it also supports some of the hypothesis of the differences observed between the ITD from HiDEM and from field observations.*

P5L107 – L112: As with the DEM, more details about the model are required here. In particular it would be helpful to describe the equations that govern the mean thickness in the model. In addition, there needs to be a summary of the parameter choices used for both neXtSIM and the DEM that are relevant to this study such as parameters that determine ice strength and failure thresholds. This should also include some discussion about whether the parameter choices used across the two models are consistent. *We revised the first paragraph of Section 2.2 to provide additional details on the DEM model parameterization. Beam failure was defined using a critical strain criterion, such that beams break when the strain reaches a critical value,  $\epsilon_c$ . We selected  $\epsilon_c = 8 \cdot 10^{-5}$ . Together with the chosen Young’s modulus,  $E = 4\text{ GPa}$ , this gives a tensile strength of  $\sigma_c = 320\text{ kPa}$  according to Hooke’s law ( $\sigma_c = E * \epsilon_c$ ). Both  $E$  and  $\sigma_c$  are within the range of engineering-scale sea ice properties reported in the literature by Timco and Weeks (2010). Additionally, we commented on the parameter choices of both models at the end of the newly introduced Section 2.1 Simulation setup. In neXtSIM, the sea-ice rheology is described by the Brittle Bingham-Maxwell (BBM) rheology. The input parameters for this rheology were the standard neXtSIM parameters, as given in Ólason et al. (2022). More information about neXtSIM has also been added to the manuscript. Initial thickness of the ice cover is chosen by the user and final mean ice thickness is the result of the failure process. Ice behavior and failure in HiDEM and neXtSIM are governed by different sets of parameters and they cannot be directly compared.*

P5L114-L116: The variability introduced to neXtSIM to account for non-uniform ice strength was between 50 – 100% of the maximum value, whereas in the DEM it is between 40% - 100%. What is the reason for this difference? *The inhomogeneity in neXtSIM was introduced via variation of the cohesion following Dansereau et al (2016) and, thus, following a standard approach for this model. In HiDEM, the beam probability was varied, which is also a standard approach used for this model. Nevertheless, introducing the beam variation associated with a*

*gridded pattern is new. There is no specific reason for the difference between the variability introduced as both use different methods.*

P6L134: Whilst this is covered in the Appendix, it would be helpful to explain how  $\Delta$  is calculated here. *The calculation method from the Appendix is repeated here.*

P6L143-150: As per previous comments, it would be helpful to discuss whether the parameter choices made for each method of simulating ridge formation are equivalent. I note that the two ridging functions have been compared in this way, but not against the DEM or neXtSIM. *Ice behavior and failure in HiDEM and neXtSIM—one being a discrete element and other a continuum model—are governed by different sets of parameters and the parameters cannot be directly compared. For neXtSIM, we chose standard parameters known to reproduce sea ice behavior in Arctic. While different neXtSIM parameters might result in different amounts of ridges, we do not assume that this would significantly change the shape of the ITD. The parameters used for HiDEM, namely  $E$  and  $\epsilon_c$ , were chosen after measured values according to Timco & Weeks (2010). We will add a paragraph into the discussion about these choices and their potential influence on the results.*

## **Results**

Figure 2: Given that ridging will generally occur at locations of joint failure, it would be interesting to compare the ridging pattern produced here to other studies that have used a DEM to simulate sea ice failure under uniaxial convergence e.g. Wilchinsky et al. (2010). *The simulations presented in Wilchinsky et al. (2010) have significantly larger ice floes with an average floe size of 4 km and a Mohr-Coloumb failure criteria, which makes direct comparison difficult. Nevertheless, their investigation of the ratio of tensile strength to compressive strength is interesting for a comparison (see their Figure 10). They observe if the tensile strength increases relative to the compressive strength the orientation of the failure between the floes becomes more perpendicular to the direction of the forcing. These simulations look more similar to our simulations. On the other hand, Åström et al. (2024) demonstrate HiDEMs ability to reproduce failure patterns observed on a local scale, for example across the Kvarken area in the Baltic Sea. We will add some of this into a new subsection in the discussion addressing the simulation setup.*

P7L157: ‘a task for future studies’. Given the motivation of this study is to motivate a parameterisation of ridging for use in continuum models of sea ice, there should be some discussion of how more complex forcing scenarios may impact the results and the proposed parameterisation. *We will discuss this in a paragraph in the discussion. Overall, it would be interesting to investigate whether different forcing scenarios have a stronger influence on the ridging pattern than on the resulting ridge shapes, which may instead be governed more by the amount of deformed ice volume, assuming the number of ridges remains similar.*

P7L162-P8L163: What method was used to classify the ridges into different shapes? *The ridge shapes were manually classified by the first author. For that, each shape was individually looked at and then sorted into one of the three categories.*

P8L172: ‘bell curved-shaped’. I do not agree with this description of the ITD for HiDEM

shown in Fig. 4. There is a clear asymmetry in the peak being described here. In addition, there are very sharp peaks in the distribution for a  $h_i$  of 2.0 m, which seem worthy of note and discussion. *We replaced this mention of "bell curve-shaped" with description of a bump, which is more commonly used during the manuscript. The initial sharp peaks for  $h_i = 2.0$  m likely stem from rafting as the highest peak is around  $h \approx 4$  m and the second, smaller peak is around  $h \approx 5$  m. Both of these peaks are very shallow and outside of our criteria for ridges ( $h \geq 3h_i$ ).*

Figure 4: The truncation of the plot on the y-axis means it is not possible to see the full shape of the ITD for neXtSIM. Could these plots be arranged over two rows to allow more vertical space for each plot? *Albeit the truncation of the y-axis isn't helpful, the main problem regarding visualization is that the ITD of neXtSIM decreases very fast. We have now decided to pull out the ITD from neXtSIM out of this plot and add another column to the figure, see Fig. 2.*

P12L209-L221: I find the explanation here tricky to follow. Formal definitions should be provided for  $a_{tri}$  and  $a_{tra}$  (and it would help to label both axes in Fig. 7). In addition, it would be helpful to provide a more detailed explanation of how the boundaries between the different parts have been determined (particularly since they are calculated from other parameters and not uniquely determined). The manuscript should also refer to previous sections to explain the decisions that have been made to determine the parameterisation. *We will add labels to the axis in Fig. 7. Additionally, we will add more explanation to the mentioned paragraph and the derivation in the appendix.*

P13L224:  $A_{tri}$  and  $A_{tra}$  are not defined. *Description of  $A_{tri}$  and  $A_{tra}$  is added.*

P13L222-L233: The overall derivation here is difficult to follow as there are several steps implicit in the text. It would be helpful to provide a full derivation in the Appendix. *We will add the full derivation to the appendix.*

P14L240: Why were different values of  $\alpha$  selected for each  $h_i$ ? *The tuning parameter  $\alpha$  has different values for each  $h_i$  as we have chosen it ad hoc. Nevertheless,  $\alpha$  sets the influence of the triangular and trapezoidal ridges on  $n(h)$ , which seems to be different across the different ice thicknesses. Potentially,  $h_i = 1.0$  m and  $h_i = 2.0$  m could use the same  $\alpha$  as their values are close to each other.*

P14L243: 'closer fit than the HI80 approach'. The HI80 approach seems to perform better for  $h_i = 2.0$  m. *That is true. The results for  $h_i = 0.5$  m and  $1.0$  m fit better, which is why the sentence was written that way. We will rephrase the sentence.*

### ***Discussion, Conclusions, and Appendix***

P16L324-P17L339: Whilst there are clear benefits to splitting  $g(h)$  into two separate distributions, there are also disadvantages to this e.g. increase in model complexity, potential conflict with other model parameterisations such as the floe size-thickness distribution used in ICEPACK (Hunke et al., 2024). Could the new ridging parameterisation be implemented within the existing ice-thickness distribution, as per previous ridging parameterisations? *We will include these disadvantages into the discussion, especially regarding the model complexity. Other*

*model parametrizations, which rely on  $g(h)$  could potentially just calculate  $g(h) = u(h) + d(h)$  and operate as usually.*

*Adding the analytical description for ridged ice into the existing ice-thickness distribution probably depends on how one would calculate the area of deformed ice (as mentioned in the discussion) and then potentially the split would not be necessary as the analytical description could replace the calculation of the ridged ice theoretically.*

P17L366-P18L368: Here and/or in the discussion, there should be some acknowledgement and discussion of the limitations of the methodology used in this study (e.g. from only considering uniaxial convergence) and the potential implications of this for the proposed parameterisation. *We will add a paragraph into the discussion, which talks about the simulation setup and it's potential consequences for the results.*

Appendix A: The information presented in Appendix A is not particularly technical or long, so it is not clear why it cannot be included in Sect. 2.1. *We included this information into the appendix as it is more established knowledge.*

### ***Technical Comments***

**P1L13:** ‘We formulate open questions in need of answers to allow implementation of’. I find this phrasing awkward. Consider something along the lines of, ‘We discuss remaining challenges in implementing’. *Thanks for the suggestion. The text is changed.*

**P3L73:** ‘with a notion on’. I do not understand the phrasing used here. Would ‘accounting for’ be better here? *Thanks for the suggestion. The text is changed.*

**P4L103:** ‘below’. It would be better to reference the section or figure here. *Changed.*

**P6L131-L132:** Sentence beginning ‘Overall’. There is a missing ‘is’ in this sentence. *Added.*

**P8L167:** ‘Large’. Should be ‘larger’. *Changed.*

**P9L180:** ‘triangular, that’. There is a missing ‘and’ here. *Sentence has slightly changed.*

**P12L210:** Here and elsewhere, Figure 7 should be Fig. 7 unless at the start of a sentence (see journal submission guidelines). The same applies to any uses of Equation (Eq.) and Section (Sect.). In addition, the number after Equation should be in brackets e.g. Equation 1 on L218 should be Eq. (1). *All uses of Figure, Equation, Section, Table and Appendix have been adapted.*

**P14L261:** Missing ‘The’ at start of sentence. *Added.*

**P14L262:** Sentence from ‘This observation’ to ‘elastic foundation’. I find this sentence difficult to understand. Consider rephrasing. *Rephrased.*

**P17L366:** ‘an interesting next steps’. There is a typo here. *Fixed.*

**Figure 5 caption:** ‘Development of the 99th percentile is P99 of the ice thickness’. There is a typo/error here. *The caption of Figure 5 is rewritten.*

**Figure 6:** The units are missing on the y-axis. ‘limi’ should be ‘limit’ in the caption. In addition, here and in other figures, make sure axes are clearly defined either within the figure or in the caption. *The typo is corrected. All Figure captions have also been checked and several have been slightly adapted.*

### *References of the review*

Hunke, E., Allard, R., Bailey, D. A., Blain, P., Craig, A., Dupont, F., DuVivier, A., Grumbine, R., Hebert, D., Holland, M., Jeffery, N., Lemieux, J.-F., Osinski, R., Rasmussen, T., Ribergaard, M., Roach, L., Roberts, A., Stekete, A., Turner, M., and Winton, M.: CICE-consortium/Icepack: Icepack 1.5.0, Zenodo, <https://doi.org/10.5281/zenodo.14188409>, 2024.

Tsamados, M., Feltham, D. L., and Wilchinsky, A. V.: Impact of a new anisotropic rheology on simulations of Arctic sea ice, *Journal of Geophysical Research: Oceans*, 118, 91–107, <https://doi.org/https://doi.org/10.1029/2012JC007990>, 2013.

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### *References of the answer*

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.

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Dansereau, V., Weiss, J., Saramito, P., & Lattes, P. (2016). A Maxwell elasto-brittle rheology for sea ice modelling. *The Cryosphere*, 10(3), 1339-1359.

Olason, E., Boutin, G., Korosov, A., Rampal, P., Williams, T., Kimmritz, M., ... & Samaké, A. (2022). A new brittle rheology and numerical framework for large-scale sea-ice models. *Journal of Advances in Modeling Earth Systems*, 14(8), e2021MS002685.

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Potyondy, D. O., & Cundall, P. A. (2004). A bonded-particle model for rock. *International journal of rock mechanics and mining sciences*, 41(8), 1329-1364.

Timco, G. W., & Weeks, W. F. (2010). A review of the engineering properties of sea ice. *Cold regions science and technology*, 60(2), 107-129.

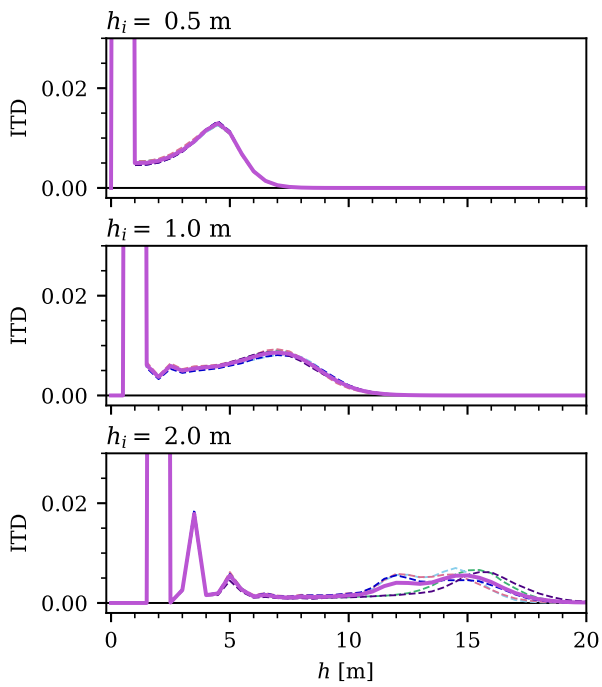


Figure 1: The ITD from the final deformed ice cover ( $t = 2$  h) from HiDEM for each initial ice thickness  $h_i$  (0.5 m, 1 m and 2 m). Each individual simulation is shown as a dotted line, while the mean is given as a solid line.

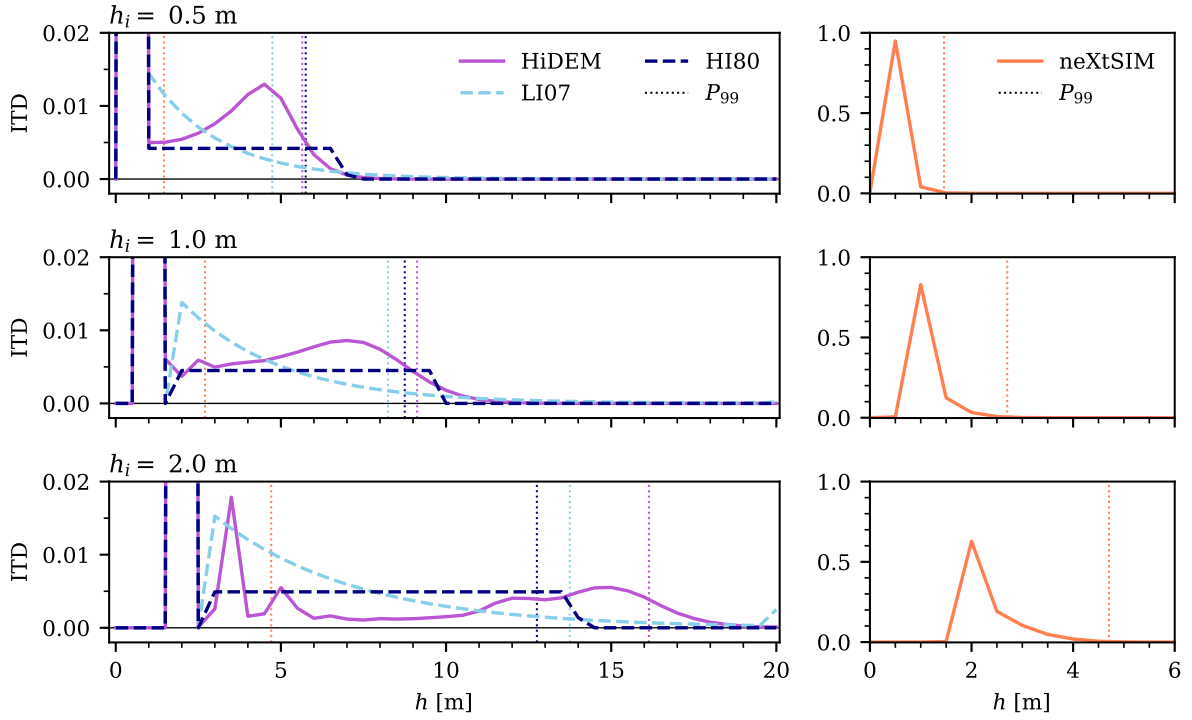


Figure 2: The ice thickness distribution (ITD) based on sea-ice area after  $t = 2$  h. The results for HiDEM and the redistribution function utilizing two different ridging functions, LI07 and HI80, are shown in the left column, while the results for neXtSIM are shown in the right column. The dotted vertical line represents the upper 99<sup>th</sup> percentile  $P_{99}$ .  $P_{99}$  from neXtSIM is given in both columns to visualize that neXtSIMs ITD contains less thick ice. Note that the  $x$ - and  $y$ -axes ranges differ between both columns. The ice thickness  $h$  categories are equally spaced categories form 0 to 20 m with a bin width of 0.5 m. The results of neXtSIM and HiDEM show the mean ITD and mean percentile across all five simulations.

