

# Response to Reviewer Comments (Reviewer #1)

We sincerely thank the Editor and the Reviewers for their careful evaluation of our manuscript and for the constructive and insightful comments. We have revised the manuscript accordingly.

**Comment:** Sharpening the terminology. The term “drift” appears in many meanings throughout the text. “Drift” is problematic in a sea-ice and snow setting. It could refer to drifting sea ice. It also can mean a snow dune. In some context I think you also use it for the phenomenon of wind-induced drifting snow. With the latter there is also a difference between how high the snow reaches, at least in a meteorological sense: if only low, below eye level, it’s called drifting snow; if higher than that, then it’s blowing snow. Please review the terminology carefully throughout the manuscript and keep to selected expressions consistently to avoid confusing the reader. Suggestion: sea-ice drift, snow dune, and drifting/blowing snow.

**Response:** We thank the reviewer for pointing out. In the revision, we use “snow dune” to refer to the bedform and “drifting snow event” to describe the atmospheric transport process. Drifting MYI/FYI is now called drift ice. These terminologies have been revised throughout the manuscript to ensure consistency.

**Comment:** Limited data. Why were exactly these data chosen? Having only one Antarctic field campaign seems odd. Would there be more data available, e.g. Beaufort Sea, Chuckhi Sea, Antarctic? Data repositories such as Arctic Data Center and PANGAEA provide most likely many more campaigns where magnaprobe snow depths and ground-based EM measurements of total thickness follow the same transect. Combining measurements from those two instruments is a rather standard procedure in recent times.

If the authors want to focus on specific test sites, as they call them in the text, then it should be reflected more in the title and abstract that this is a collection of case studies. If snow depth distributions are to be generalized, more structured and comprehensive investigation should be conducted across hemispheres, regions, seasons, various meteorological conditions, and ice types (age and degree of deformation).

**Response:**

Two criteria were applied in selecting the datasets. First, since this study aims to investigate the influence of ice age and SIT on SND distributions, it requires coincident in-situ measurements of SND and SIT from diverse ice types. We therefore selected field campaigns spanning a range of ice ages, surface roughness conditions, flooding states, and seasons, while maintaining a balance among sampling sites. Campaigns with well-established contextual knowledge of meteorological events and local ice conditions were prioritized to support the interpretation of the observations. Second, this study aims to analyse the temporal evolution of the SND distribution, which requires time-series observations of snow and ice properties at approximately weekly intervals. Characterizing snow and ice on this timescale is essential to capture snow accumulation and redistribution processes associated with ice growth and meteorological events (Liston et al., 2020).

Based on the above criteria, the Multidisciplinary drifting Observatory for the Study of Arctic Climate (MOSAiC) 2019-2020 dataset was selected as the primary dataset because it spans a wide range of ice types and provides time-series in-situ measurements along repeated transects. The MOSAiC dataset includes four typical ice types: SYI and FYI from winter to spring, newly-formed smooth FYI in winter, and SYI in summer. Beyond MOSAiC, additional datasets were selected to capture two critical snow and ice conditions that are not represented in MOSAiC. First, to better account for the effects of thick snow on sea ice prone to flooding, we included data from the Norwegian Young Sea ICE Expedition 2015 (N-ICE2015) and the Weddell Sea (2013) campaigns. Second, the Lincoln Sea (2017) campaign was included to represent a distinct rough MYI type, enabling a more comprehensive assessment of SND distributions across different ice ages. We note that only three transects were acquired over newly-formed smooth FYI from the MOSAiC (i.e. Runway), as this site was disrupted by ice motion (Itkin and Liston, 2025). To improve sampling for this ice type, we included the Resolute Bay (2025) dataset, which represents smooth FYI covered by a thin snow layer comparable to that on the MOSAiC Runway. Although co-located SIT measurements were not available for Resolute Bay, field drilling indicated similar ice conditions (see Table 1).

In the revised manuscript, we have added the above text to a new section (Section 2.1, Selection of in-situ field campaigns) to clarify the criteria used to select the field datasets analysed in this study.

We have revised the title to better reflect the regional scope of the study: Snow Depth Distributions on Sea Ice of Different Ages and Thicknesses from Regional Field Campaigns.

Furthermore, we have added discussion of regional limitations in a new Section 5.4 (Limitations, Line 581-589):

A limitation of this study is the regional datasets. The analysis in this article is based on the selected test sites rather than on unified pan-Arctic and -Antarctic datasets. Nevertheless, the sites were carefully chosen to span various snow and ice types and each contains thousands of samples, ensuring statistically robust analysis. However, SND distributions can be sensitive to many local factors such as ice topography, snowfall, wind redistribution, flooding, and melt processes. Consequently, extending these findings to the hemispheric scale requires thorough evaluation using large and comprehensive datasets. Despite this limitation, the regional cases analysed here, supported by high-quality in-situ measurements, provide new insights into how SND distributions depend on ice types and thicknesses at the sub-kilometre scale.

**Comment:** Figures. There are a lot of them, and they need to be revised for clarity. See technical comments for further details.

**Response:** We thank the reviewer for detailed comments on the figures. In the revised manuscript, the figures have been updated to improve clarity and readability. We have refined labels, adjusted layouts, and simplified the figures to improve clarity. Some figures have been merged to reduce redundancy and improve the overall flow of the manuscript.

**Comment:** English language. To my understanding, The Cryosphere no longer provides professional copy-editing as part of their publishing process, i.e. no English language changes will be made. In its current form, the manuscript needs thorough editing, which is evident from the sheer number of technical corrections that caught my eye (see below). There are e.g. unnecessary plurals, some incomplete sentences, and inconsistencies with the journal's guidelines regarding e.g. date and table formats, some of which I will highlight below in the technical comments. The manuscript text should be carefully reviewed by

a native speaker, and there are many of those among the author list, or a professional copyeditor, and special care should be taken to follow the journal guidelines.

**Response:** In the revised manuscript, the text has been carefully and thoroughly reviewed to address grammatical errors, unnecessary plurals, incomplete sentences, and inconsistencies. The manuscript has been checked to ensure consistency with the journal's formatting guidelines, including date formats, table presentation, and overall style requirements.

**Comment:** Discussion. There is no specific dedicated section for discussing the results. I assume that discussion has been combined with the results section. If so, it should be reflected in the section heading. One thing that I find lacking from the discussion is related to the performance of different distribution functions based on RMSE values and different sea-ice thickness classes (Fig. 14). What is the significance of say 0.001 m difference in RMSE between probability density functions and how might measurement accuracy affect that? Furthermore, big global climate models usually apply various sea-ice thickness classes in their simulations, so I would welcome additional discussion on reflecting the sea-ice thickness classes of this study to those used in global climate models and how would different snow depth distributions in different sea-ice thickness classes potentially affect the simulations.

**Response:** In the revised manuscript, we have added a new Discussion Section 5.

In Section 5.1 Comparison with previous studies, we further discuss how our results are consistent with previous findings while also providing new insights into snow-depth variability over sea ice. Furthermore, we have expanded the discussion on the small differences in RMSE between the log-normal and skew distributions for FYI with thickness below 1.5 m and for the MOSAiC Summer sites. In the revision, we have added text in Line 506-513:

In these two cases, the improvement in the log-normal over skew distribution is modest ( $\approx 20\text{--}25\%$ ), calculated from Table 2 using  $(\text{RMSE}_{\text{skew}} - \text{RMSE}_{\text{log-normal}}) / \text{RMSE}_{\text{skew}}$ . However, the skew significantly outperforms the log-normal by  $\approx 60\text{--}80\%$  in other SYI and MYI cases. In particular, large RMSE values of log-normal are observed for MOSAiC Nloop after 30 January 2020 and the Lincoln Sea and N-ICE2015. Hence, although the log-normal distribution performs best in these two exceptions, the skew distribution exhibits greater stability and is less sensitive to extreme deviations from the observed SND distributions. For practical applications, the skew distribution therefore provides a more robust representation of SND distribution over regions containing mixed ice types or lacking a priori knowledge of ice type.

In Section 2.2 Instruments (Line 108-116), we have added descriptions of measurement accuracy:

In all campaigns, SND measurements were carried out using automated snow-depth probes (magnaprobes) equipped with a data logger and GPS (Sturm and Holmgren, 2018). The device has a maximum measurement depth of 1.2 m, with an estimated accuracy of  $\sim 3$  mm (Sturm et al., 2006). In the Lincoln Sea, MOSAiC, and Weddell Sea campaigns, total thickness (snow + ice thickness) was sampled using a ground-based electromagnetic (EM) induction system (GEM-2, Geophex Ltd.) operating at multiple frequencies (Hunkeler et al., 2015, 2016). During the N-ICE2015 campaign, total thickness was measured with portable EM instruments (EM31 and EM31SH) mounted on a sledge (Itkin et al., 2023). The precision of the EM measurements is approximately 0.1 m on level ice up to 4 m thick. The accuracy decreases on rough and deformed ice, and the EM can underestimate the ridge thickness by up to 50% (Haas et al., 2009).

In Section 5.4 Limitations (Line 604-614), we have added a discussion that considers magnaprobe precision when interpreting the RMSE values:

Another limitation concerns the measurement accuracy of SND and SIT obtained from the magnaprobe and EM instruments. The magnaprobe has a precision of 3 mm. Given that the typical mean SND within each transect is several to tens of centimetres, 3 mm is small relative to the SND measurements. In the analyses, we use  $\mu_{\text{SND}}$  computed from all samples along each transect rather than an individual sample, which further reduces the influence of measurement uncertainty. Each transect contains hundreds of samples, ensuring that the inferred PDFs are statistically robust; therefore measurement error from a single sample has a negligible effect on the overall distribution shape. When evaluating the fitting performance, the differences in RMSE between two PDFs less than 3 mm are considered comparable (Table 2), consistent with the instrumental precision of the magnaprobe. The precision of the EM measurements is approximately 0.1 m on level ice. Similarly, we use  $\mu_{\text{SIT}}$  calculated by averaging all samples along each transect, rather than individual measurements, thus reducing the influence of measurement uncertainty on the reported SIT-dependent relationship. The  $\mu_{\text{SIT}}$  is used primarily for mean-SIT binning, so the influence of measurement uncertainty on the results is expected to be limited.

In Table 2, when evaluating the fitting performance, the differences in RMSE between two PDFs less than 3 mm are considered comparable, consistent with the instrumental precision of the magnaprobe.

In Section 5.2 Links to sea ice models (Line 542-560), we have added a discussion of reflecting the SIT classes of this study to those used in global climate models:

Castro-Morales et al. (2014) therefore emphasized the need for large-scale field observations to establish representative and robust statistical relationships between snow and sea ice. Our results provide new quantitative insights into this perspective. Specifically, Fig. 14 (now Fig. 11 in the revision) demonstrates that SND distributions exhibit a clear dependence on SIT for FYI, which can be used to evaluate and refine category-specific snow parameterizations in models. Moreover, our findings on SND distributions across ice ages and SIT values can be incorporated into ITD frameworks to better represent the statistical coupling between snow and ice, thereby improving simulations of thermodynamic ice growth during the ice-formation period. As a simplified alternative, a skew distribution can serve as a robust candidate when assuming a single universal distribution for the snow field across the ITD categories.

**Comment:** Conclusions. In its current form, the conclusions section is a summary about the different parts of the study, which is the task of the abstract, but in extended form. The conclusions should be completely rewritten with a special focus on summarizing how this study has advanced the scientific knowledge of the topic and how it ties to the overall objective of the study. What is the main take-home message from this study? At the moment, this is not clear.

**Response:** The Section 6 Conclusions has been rewritten. In particular, we have emphasized how this work advances the understanding of SND variability across sea-ice ages and thicknesses. The revised Conclusions highlight the broader scientific contributions of the work, including its implications for representing sub-kilometre snow variability in climate models and for improving uncertainty estimation in remote-sensing-based SIT retrievals. In addition, we have introduced a new Section 5.5

(Recommendations for practical applications) prior to the Conclusions. This section provides practical guidance and clearly articulates the main take-home messages.

**Comment:** Title: According to journal guidelines, only first words and proper nouns are capitalized. Furthermore, why is “ages” plural and “thickness” not? I’m not a native speaker, but singular “age” sounds better to me.

**Response:** New title: [Snow Depth Distributions on Sea Ice of Different Ages and Thicknesses from Regional Field Campaigns](#).

**Comment:** Line 2ff: there’s only one Antarctic campaign included in your data. Therefore “multiple Arctic and Antarctic campaigns” is probably not warranted. Suggestion: “multiple polar in-situ sea-ice campaigns”.

L8: “the flooded site” is not defined yet here.

L9: the names of the distributions are not usually proper nouns (excluding the ones based on names e.g. Gaussian, Rayleigh, ...); thus they should not be written with a capital first letter unless in the beginning of a sentence: normal, log-normal, skew-normal, gamma. Please correct throughout the manuscript.

L10: snowfall and drifting snow? I will not note future occurrences of “drift\*”, e.g. the very next line, see specific comment (1).

L18: ice age.

**Response:** [Done](#).

**Comment:** L33: Veyssière et al. (2022) seems a bit odd reference here, or is it just an example of such case where uniform SND distribution is assumed?

**Response:** [Yes, it is just an example in which a uniform SND distribution was assumed. We have revised the sentence \(Line 35-37 in the revision\): Due to various meteorological and geophysical processes, snow cover is naturally uneven. Consequently, the assumption of a uniform distribution \(Veyssière et al., 2022\) cannot accurately represent the SND distribution within a sub-grid cell.](#)

**Comment:** L47: coefficient of variation (CV). All abbreviations need to be defined again in the main text.

L49: depend on the ice type

L50: ice age

**Response:** [Done](#)

**Comment:** L53: how does SND and ice dynamics equal snow water equivalent?

**Response:** [Liston et al. \(2020\) demonstrated a linear relationship between the ice age \(days\) and the ice dynamics contribution to snow-water-equivalent \(SWE\) depth \(cm\), see Figure 7 in <https://agupubs.onlinelibrary.wiley.com/doi/10.1029/2019JC015913>. However, we have removed the SWE part and revised this sentence to strengthen its connection with this study \(see Line 55-57](#)

in the revision): Liston et al. (2020) revealed a linear relationship between parcel age and SND, suggesting that an accurate representation of the evolution of SND on sea ice requires a detailed parcel age on weekly timescales.

**Comment:** L66: sea ice age and deformation are key factors affecting

L67: NP not defined

L69: how can it be “between” three things? Suggestion: among

L73ff: Specifically mention also Section 3, like the other sections.

L82: N-ICE and MOSAiC undefined

L85: automated SND probes (magnaprobes)

**Response:** Done

**Comment:** L86ff: what are the measurement accuracies for both magnaprobe SND and ground-based EM total thickness? How are they taken into account in this study?

**Response:** In all campaigns, SND measurements were carried out using automated snow-depth probes (magnaprobes) equipped with a data logger and GPS (Sturm and Holmgren, 2018). The device has a maximum measurement depth of 1.2 m, with an estimated accuracy of  $\sim 3$  mm (Sturm et al., 2006). The precision of the EM measurements is approximately 0.1 m on level ice up to 4 m thick. The accuracy decreases on rough and deformed ice, and the EM can underestimate the ridge thickness by up to 50% (Haas et al., 2009).

We have added the above text in Section 2.2 Instruments Line 108-116.

The magnaprobe has a precision of 3 mm. Given that the typical mean SND within each transect is several to tens of centimetres, 3 mm is small relative to the SND measurements. In the analyses, we use  $\mu_{\text{SND}}$  computed from all samples along each transect rather than an individual sample, which further reduces the influence of measurement uncertainty. Each transect contains hundreds of samples, ensuring that the inferred PDFs are statistically robust; therefore measurement error from a single sample has a negligible effect on the overall distribution shape. When evaluating the fitting performance, the differences in RMSE between two PDFs less than 3 mm are considered comparable (Table 2), consistent with the instrumental precision of the magnaprobe. The precision of the EM measurements is approximately 0.1 m on level ice. Similarly, we use  $\mu_{\text{SIT}}$  calculated by averaging all samples along each transect, rather than individual measurements, thus reducing the influence of measurement uncertainty on the reported SIT-dependent relationship. The  $\mu_{\text{SIT}}$  is used primarily for mean-SIT binning, so the influence of measurement uncertainty on the results is expected to be limited.

We have added the above text in Section 5.4 Limitations Line 604-614.

**Comment:** L89: total thickness

**Response:** Done.

**Comment:** L91ff: More information about methods is needed! How was the drift correction done? Was it included in the data products or did you carry out the correction? What data were used to do it? How was co-location done, closest neighboring value or some interpolation? If the correction and co-location methods differ between datasets, how do they affect the results?

**Response:**

For the MOSAiC and Lincoln Sea campaigns, the datasets were already drift-corrected and co-located, as described in (Itkin et al., 2023; Haas et al., 2017). For the Weddell Sea and N-ICE2015 campaigns, the datasets were drift-corrected (Wever et al., 2021; Merkouriadi et al., 2017). Co-location between magnaprobe and GEM measurements was performed using nearest neighbor interpolation in this study. The drift-correction and co-location procedures vary slightly between campaigns, but follow consistent principles to ensure spatial consistency between measurements.

Itkin et al. (2023) described the drift correction and co-location approach for the MOSAiC dataset. The GPS coordinates were transformed into a local metric coordinate system using FloeNavi (Hendricks, 2022) to account for sea ice drift. Although individual transects may appear to deviate within the FloeNavi reference frame, the actual transect paths remained consistent in physical space. For the co-location between the magnaprobe and GEM measurements, nearest-neighbor interpolation was applied onto a 1 m grid. This approach is justified because the original sampling spacing (1–3 m) is comparable to the grid resolution.

Wever et al. (2021) described the drift correction for the Weddell Sea dataset. On each ice floe, two GPS base stations were temporarily installed and continuously recorded throughout the ice-station period, while a third GPS unit was used for survey measurements. This configuration enabled post-processing kinematic GPS, providing coordinates relative to one of the base stations in a reference frame invariant to floe drift and rotation. The start time of the magnaprobe survey was defined as the synchronization reference, and all floe drift and rotation before and after this time were corrected accordingly. In this study, nearest-neighbor interpolation was applied to co-locate magnaprobe and GEM measurements. The resulting typical positional mismatch is approximately 2–5 m, corresponding to the distance between the magnaprobe and GEM-2 instruments during the survey (Wever et al., 2021).

Haas et al. (2017) described the drift-correction and co-location procedures for the Lincoln Sea dataset. EM measurements were georeferenced using GPS positioning. The GPS data were used to resample the EM measurements to a spacing of 5 m, correcting for variations in walking speed and pauses during acquisition. These GPS records were subsequently used to perform ice-drift correction and to resample the EM data to the exact locations of the snow-thickness measurements. magnaprobe measurements were collected at a constant interval of two to four steps, corresponding to a spacing of approximately 1–3 m, depending on the operator.

Merkouriadi et al. (2017) described the drift-correction procedure for the N-ICE2015 dataset. Snow transects were conducted together with total ice measurements. Each transect started and ended at the same location, allowing GPS-based correction for ice drift during the survey. In this study, nearest-neighbor interpolation was applied to co-locate MP and GEM measurements.

Accurately quantifying the residual uncertainty after drift correction and co-location is challenging, as it depends on GPS accuracy, ice dynamics, and the spatial spacing of the measurements. Overall, the resulting positional uncertainty is on the order of metres.

Given that our analysis is based on values averaged over the transect ( $\mu_{\text{SND}}$  and  $\mu_{\text{SIT}}$ ) rather than individual measurements, positional uncertainties at the me scale are unlikely to influence the results. For the PDFs fitting, each transect contains hundreds of samples, ensuring that the inferred PDFs are statistically robust; therefore, the errors from drifting correction and co-location do not affect the overall distribution shape. In addition,  $\mu_{\text{SIT}}$  derived from EM are used mainly for SIT binning and ice regime classification. Therefore, the findings of dependence of SIT on the SND distribution will not be driven by measurement uncertainty.

We have added text and references into the revised manuscript in Section 2.2 (Instruments, Line 117-123) and Section 5.4 (Limitations, Line 604-614).

**Comment:** Fig. 1: Undefined jargon/abbreviations: Nloop, Sloop, PS81/503 etc. In caption, the date format should follow the journal guidelines 31 July 2013.

Fig. 2 & 3: The figure panels should be equal aspect to avoid distortion, so that a meter on both axes is equal length. Why are x labels tilted for panel f)? The total length and number of measurements for each transect would be a nice addition to the figure panels.

Fig. 4: N-ICE floes were SYI, so they should be red? Is the blue for SIT lighter than for SND, or is it just my printer?

**Response:** We have corrected Fig. 2-4 as suggested. Fig. 2 & 3 have been merged into Fig. 2 in the revision.

**Comment:** Table 1: According to journal guidelines, horizontal lines should be placed only above and below the table and separate the header from the rest of the table. Vertical lines are not used. Moreover, earlier on L96, standard deviation was abbreviated with capital letters (STD). Or, as you later in the manuscript use symbols  $\mu$  and  $\sigma$  for the mean and standard deviation, why not use these? With the corresponding subscripts SIT and SND.

**Response:** We have updated the format of Table 1 as suggested.  $\mu_{\text{SIT}}$ ,  $\mu_{\text{SND}}$ ,  $\sigma_{\text{SIT}}$ , and  $\sigma_{\text{SND}}$  denote the mean and standard deviation of each transect, respectively. The statistics in Table 1 were calculated for all transects within each test site. We have added explanations in the caption.

**Comment:** L94ff: Please make sure that the same information is provided for each test location, maybe even in the same order, e.g. location, time period, covered area, number of measurements, flooding (if present), etc., so that this information is easy to find for the reader.

L100ff: How many measurements were there in total/per transect? Should flooding be mentioned here, too?

**Response:** We have revised the manuscript to ensure that consistent information is provided for all test locations, including location, time period, covered area, number of measurements, and flooding (where applicable). For the Weddell Sea 2013 campaign, we have added the total number of measurements and indicated the floes where flooding was observed.

**Comment:** L113: no commas separating year in dates

L124: examined

L125ff: 12 April (2017), 17 April. I suggest always adding the year, too, since your datasets originate from different campaigns.

L129: wrong citation format, (Haas et al., 2017).

L130: the expression “areas ranging from x-y m by x-y m” does not make sense. Replace with a range of actual areas  $m^2$  and the expression “ranging from ... to ...”.

L139: space missing before the unit: 18 kHz

L140ff: date format should be DD Month YYYY

L145: an area of  $400 m^2$  is simply false, that would mean an area of 20 m by 20 m!

L152: MOSAiC summer transect includes parts of Nloop, see e.g. Fig.2 in Webster et al. (2022) <https://doi.org/10.1525/elementa.2021.00>

L157: on 4–6 April 2025

L160: either 300 m x 500 m or  $300 \times 500 m^2$

**Response:** Done.

**Comment:** L168ff: what does the subscript l stand for, and why does it disappear from Eq. (2)?

**Response:** We have corrected the subscripts for consistency.

**Comment:** L181: 14 November 2019. I’ll stop marking these now. Please make sure all dates adhere to journal guidelines.

Fig. 5: In panel a), shouldn’t the vertical axis have a unit of  $m^{-1}$ , because the area under the PDF equals 1? In panels b)-e), quantiles shouldn’t have units of meter, right? Furthermore, I would suggest using the same colors for different distributions as in panel a), i.e. points in panel b) in green, red in d), etc. In fact, it would be very useful to use the same colors for the distributions in all figures throughout the manuscript! Here, the 1:1 line could be then e.g. gray or black. In caption, the space before the unit is missing: 5 cm.

**Response:** Done. Yes, the vertical axis has a unit of  $m^{-1}$ . The figures (Fig. 4 and 8-11 in the revision) have been updated with the suggested color format.

**Comment:** L201: remove full stop before citation

L206: a reference would be good here. What is a Matheron estimator?

L212: Beyond the effective range

L224: This can be explained by younger and thinner ice forming and evolving...

L225: and their snow cover

**Response:** Done.

**Comment:** L226: what do you mean by minimal wind redistribution? With little surface roughness, even a little wind can redistribute snow.

**Response:** We have rewritten the sentence, see Line 275-276: A clear linear relationship is observed between  $\mu_{\text{SND}}$  and  $\mu_{\text{SIT}}$  for FYI. This can be explained by the concurrent increase in SND and SIT during the ice thermodynamic growth season.

**Comment:** L231: CV already defined earlier.

**Response:** Done.

**Comment:** Fig. 8: In panel a), is the color scale capped at the min/max values presented there, or should the color bar be extended by triangle-like markers at one/both ends? In panel b), is the y axis intercept point set at 0? What is the confidence interval of the linear fit?

**Response:** This is now Fig. 7 in the revision. Panel (a), the colorbar is from the minimum value to the maximum value of  $\mu_{\text{SIT}}$ , which is 0.6 m to 4.1 m. Panel (b), y axis intercept is at 0. We have revised the fitting line so that the intercept is visible. We have added the 95% CI (confidence interval) and 95% PI of the linear fit in the revision.

**Comment:** L243: remove “likely”. How else can flooding happen?

L244: remove unnecessary commas between the values and the units

L245: replace “eaten away” with “turned into slush and, if refrozen, into snow ice”

L246: something is missing in this sentence

L250: wrong citation format

L260ff: split infinitive. “. . . to interpret the fitting performance further.”

L267: remove plural: (green line). There is only one green line in Fig. 9a.

**Response:** Done.

**Comment:** L273ff: an indent is missing at the beginning of the paragraph. Remove plurals from “lines”. Please rewrite this sentence, because “performance of Gamma and Skew . . . become comparable RMSE values” does not make sense.

**Response:** We prefer to leave the indent. The indent was removed because this does not start a new paragraph, as is the case with ‘After 30 January.’ The discussions of the MOSAiC Nloop are organized into three parts: before 19 December, from 19 December to 30 January, and after 30 January. This is done by starting a new line without an indent. The sentence has been rewritten.

**Comment:** L274ff: Remove sentence “To better understand. . .”, it’s unnecessary repetition from three paragraphs ago.

**Response:** Done.

**Comment:** Fig. 9: This and many subsequent figures need quite some work to make them clearer. Due to multiple vertical axes, the background grid is very messy. Please choose the tick spacing so that the grids overlap. E.g. RMSE should range from 0 to 0.1 m,  $\mu_{\text{SIT}}$  from 0 to 5 m, and  $\Delta_{\text{SIT}}$  from 0 to 10 m all in five steps. In figures, where the horizontal axis is date, it must be a proper time axis. Now here the measurements are equally spaced even though the time period between them varies.

For example, the April snowfall events have different width in Fig. 9a) and b). I further suggest that the time axis of Fig. 9a) and b), and others if applicable, is shared, i.e. the meteorological events line up with each other. It is of course great that the exact transect measurement days are noted, but this could be done e.g. in a table in the appendix. Later on, you often refer to the respective kurtosis and skewness plots together with the distributions, which is why I suggest that you rearrange the figures by campaign/location and add Fig. 11a) in a panel above/below Fig. 9a) in the same figure. To keep the number of figures in control, perhaps combine MOSAiC plots into one figure. In addition, in the caption of Fig. 9, the citation and date formats are wrong. Regarding the changes in Sloop geolocation, the shape of the transect loops in Fig. B1 look very similar, just shifted laterally. Is this due to ice deformation i.e. lead formation, and the sampled ice and snow is still mostly the same?

**Response:** We appreciate the reviewer's detailed comments on the figures. In the revised manuscript, we have adjusted the tick spacing and aligned the time axes to ensure consistency across subplots. We have reorganised and combined the original Figs. 9, 10, and 11a–d for the MOSAiC sites to improve clarity. The updated figures are now Fig. 8 and 9 in the revision. The exact dates of each transect measurement are now explicitly shown on the time axis. In addition, we have reorganised the original Figs. 11e–h and 13 into now Fig. 10 in the revision to improve overall readability.

Yes, the sampled ice and snow were mostly the same, but the coordinates of the transect lines changed due to minor errors in the FloeNavi positioning and more significantly due to sea ice dynamics (Itkin et al., 2023). For the MOSAiC dataset, GPS coordinates were transformed into a local metric coordinate system using FloeNavi to account for sea-ice drift. Note that the errors from FloeNavi positioning were minor and typically on the order of 0–10 m (Itkin et al., 2023). Therefore individual transects may appear slightly offset within the FloeNavi coordinate system, while the actual transect paths were designed to follow the same spatial track (Wagner et al., 2022). Sea-ice deformation and relative motion between ice floes can induce substantial geolocation shifts, on the order of several hundred metres. A strong ice-dynamics event on 16 November 2019 affected the Sloop transect; in this case, partial corrections were applied to account for this shift (Wagner et al., 2022). Ice dynamics also influenced the Sloop transects from 11–12 March 2020 onward, when leads and cracks opened along the measurement paths (Wagner et al., 2022; Itkin et al., 2023). Positional corrections and manually co-locations were implemented to minimize the impact of these deformation events on the transect measurements, see Fig. 4 in Itkin et al. (2023). Nevertheless, some influence on time series cannot be completely excluded (Wagner et al., 2022).

**Comment:** L279: indent missing?

**Response:** The indent was removed because this does not start a new paragraph, as is the case with "After 30 January." The discussions of the MOSAiC Nloop are organized into three parts: before 19 December, from 19 December to 30 January, and after 30 January. This is done by starting a new line without an indent.

**Comment:** L281: remove "from"

**Response:** Done.

**Comment:** L285ff: was any of the authors on the MOSAiC expedition to confirm this?

**Response:** We have rewritten this part.

**Comment:** Fig. 10: weather events are missing (at least panel a). Fix background grid by adjusting vertical axes ticks. Use a proper time axis.

L290: split infinitive, “To capture the deformation levels better”

L292: 6.79 m before 30 January to 4.13 m after 30 January

L295: SIT  $\rightarrow \mu_{\text{SIT}}$  ? I suggest adding “seasonal thermodynamic growth”

Fig. 11: Wrong citation format in the caption. Also here, where applicable, use a proper time axis. Then also the green bar indicating log-normal distribution superiority should be continuous, e.g. panel d).

Fig. 12: Shouldn't the vertical axes unit be  $\text{m}^{-1}$ ? The red histograms are not dashed as the legend suggests. Also the date format is wrong.

L313: be consistent with subscripts, why is it  $\sigma_{\text{snow}}$  and not  $\sigma_{\text{SND}}$ ? Check throughout the manuscript.

**Response:** Done.

**Comment:** Table 2: According to journal guidelines, horizontal lines should be placed only above and below the table and separate the header from the rest of the table. Vertical lines are not used. Later in the manuscript you use symbols  $\mu$  and  $\Delta$  for the mean and range, why not use these? With the corresponding subscripts SIT and SND. In addition, the date format is wrong.

**Response:** We have updated the format of Table 2 as suggested.  $\mu_{\text{SIT}}$ ,  $\mu_{\text{SND}}$ ,  $\sigma_{\text{SIT}}$ , and  $\sigma_{\text{SND}}$  denote the mean and STD for each transect, respectively. The statistics in Table 2 were calculated for all transects within the site. We have added explanations in the caption.

**Comment:** L320: or does it work better just after 30 Jan when most deformed/thickest ice was sampled less?

**Response:** This section has been rewritten in Section 4.2.3 (Dependence on ice age and thickness). We interpret the shift in the best-fitting PDF from log-normal to skewed distributions to be related to cumulative wind redistribution processes. Snow over older and thicker ice has undergone a longer period of wind-driven redistribution, which is associated with reduced kurtosis and a distribution shape better captured by skew distribution.

**Comment:** L323: perhaps replace “level” with “values” to avoid confusion with level ice thickness

L329: remove “heavy”, thickness alone does not imply heaviness

L333: replace “heavy” with “a thick”

L341: I suggest moving the reference to the supplement earlier, when you first mention Churchill

L347: contradicts Table 2, Gamma has the lowest RMSE, not skew (albeit by very little)

Fig. 13: Fix background grid by adjusting vertical axes ticks. Use a proper time axis. Why is there no markers for  $\Delta_{\text{SIT}}$  anymore?

L350: split infinitive, “To generalize the dependence of fitting performance on SIT further”

**Response:** Done.

**Comment:** L355: is 0.001 m difference in RMSE significant enough to draw the result “most accurate fit”? Gamma and skew are not far behind. How does measurement accuracy affect the RMSE values?

**Response:** When evaluating the fitting performance, the differences in RMSE between two PDFs less than 3 mm are considered comparable (Table 2) in the revision, consistent with the instrumental precision of the magnaprobe.

We have expanded the discussion on the small differences in RMSE between the log-normal and skew distributions for FYI with thickness below 1.5 m and for the MOSAiC summer sites. In these two cases, the improvement in the log-normal over skew distribution is modest ( $\approx 20\text{--}25\%$ ), calculated from Table 2 using  $(\text{RMSE}_{\text{skew}} - \text{RMSE}_{\text{log-normal}})/\text{RMSE}_{\text{skew}}$ . However, the skew significantly outperforms the log-normal by  $\approx 60\text{--}80\%$  in other SYI and MYI cases. In particular, large RMSE values of log-normal are observed for MOSAiC Nloop after 30 January 2020 in Fig. 8a and the Lincoln Sea and N-ICE2015 in Fig. 10. Hence, although the log-normal distribution performs best in these two exceptions, the skew distribution exhibits greater stability and is less sensitive to extreme deviations from the observed SND distributions. For practical applications, the skew distribution therefore provides a more robust representation of SND distribution over regions containing mixed ice types or lacking a priori knowledge of ice type.

The above explanations have been incorporated into Discussion Sections 5.1 Line 506-514. We have also added a discussion in Section 5.4 (Limitations, Line 604–614) on how measurement accuracy may affect the analyses.

**Comment:** L368: the term skew-normal hasn't been used since the methods section. If you refer to skew-normal distributions with just skew, it should be clearly mentioned in the very beginning and used consistently throughout the manuscript. Please double-check.

L371ff: This sentence needs rewriting, e.g. “we considered the impacts of ice age, SIT, SIT range, and meteorological conditions on the best PDF. . .”

L373: SIT  $\rightarrow \mu_{\text{SIT}}$ , like on the next line

L374: be consistent with subscripts, why is it  $\Delta_{\text{ice}}$  and not  $\Delta_{\text{SIT}}$ ? Check throughout the manuscript. In addition, “as well as FYI that grows thicker. . .”

L379: wrong date format

Fig. 14: What do the colors mean? Could this information be condensed into one figure panel, where boxplots are grouped by SIT class and the box colors follow the same color scale of different distributions, e.g. all skew boxplots green, log-normal blue, etc. Then the vertical axes could be extended so the smallest boxplots could be better readable. In the caption, the definition of whiskers is wrong, it should be  $Q3 + 1.5 * (Q3 - Q1)$  and  $Q1 - 1.5 * (Q3 - Q1)$ , the whiskers are not centered at Q3. Is the line in the box the median value?

**Response:** All done.

**Comment:** L386ff: why did they fail to reach constant semi-variance value?

**Response:** Semi-variogram analysis can fail to detect slope-change points at large length scales due to undulations in the variogram introduced by local non-stationarities (Moon et al., 2019). In such cases, the effective range cannot be robustly estimated from the semi-variogram, and more advanced approaches such as multifractal temporally weighted detrended fluctuation analysis are required (Moon et al., 2019).

We have added the above text in the revision Line 448-452.

**Comment:** L392ff: are the values average +/- STD?

**Response:** Yes, we have added it (mean  $\pm$  standard deviation) in the revision. Note that the abbreviation Std is only used for Table 1.

**Comment:** Fig. 15: Panels a) and b) are not explicitly introduced. It also seems that they don't include the same points?

**Response:** The figure has been removed to increase readability.

**Comment:** L407: wrong citation format

Fig. 16: Fix background grid by adjusting vertical axes ticks. What are the red and blue (hidden in the legend) dots? Is the horizontal line in the box the median? Definition for the whiskers is wrong in the caption, see earlier comment on Fig. 14.

L421: split infinitive, "To investigate the temporal changes of the spatial heterogeneity of snow cover further"

L424: Fig. 17a

L427: an example where you use "drift" for "dune"

**Response:** Done.

For former Fig. 16, The red and blue colors are outliers, sample  $> Q3 + 1.5 \times (Q3 - Q1)$  and sample  $< Q1 - 1.5 \times (Q3 - Q1)$ . In the updated figure (Fig. 12 in the revision), we have removed the outliers to maintain consistency with the all other box-plot.

**Comment:** Fig. 17: Fix background grid by adjusting vertical axes ticks. Use proper and common/shared time axis for panels a) and b). Could this information, essentially just one curve more, be added to revised Fig. 9? There are a lot of figures in this manuscript.

**Response:** We have merged the two panel into one (Fig. 13 in the revision) to save space. Since the former Fig. 9 (now Fig. 8 in the revision) is already busy, we prefer to have a separate figure for this section.

**Comment:** L437: SIT  $\rightarrow \mu_{SIT}$

L443ff: (ii) Coefficient of variation remains independent of SIT and ice type with an approximately constant value of 0.50,

L449: ice age

L470ff: why do all the subsequent section contents start with a full stop?

L487ff: wrong citation formats, both of them

L491: spell out names instead of initials

References: nearly all dois are missing

L520: fix author name

L525: journal name, volume, page, doi missing

L533: provide date

L597: pages/article number missing

**Response:** All done. Note that the Conclusions section has been rewritten.

**Comment:** L607: pages/article number missing

**Response:** This reference has no page. The string (i.e, e2019JC015764) is an article identifier used instead of page ranges.

**Comment:** Fig. A1 & A2: Use proper time axis. What do the whiskers indicate? Some violin plots seem to have two horizontal lines, what do they represent? The caption mentions black line, but I cannot see any. Fixed vertical axes makes some panels unreadable. The whiskers/two horizontal lines mark the maximum and minimum values. We have enlarged the MOSAiC Nloop/Sloop/Summer and moved these figures into supplementary to save some space.

Fig. B1: Date format is wrong in the figure annotations and in the caption. Panel b) has a random “-500” floating around at approximately (-800,100). Why is the transect route partially outside the background image? What is the source of the airborne laser scanner data in the background? Add the citation. What is the meaning of the color scale?

Fig. C1: The unit for the probability density axes should be  $m^{-1}$ , right? The date format is wrong. One legend is enough for the figure, no need to repeat it in each panel. I recommend using the same colors for the distributions as in the main manuscript figures. What is the bin size for the histogram?

Supplement, L2: date format

Supplement, Fig. 1: The unit for the probability density axes should be  $m^{-1}$ , right? The date format is wrong. One legend is enough for the figure, no need to repeat it in each panel. I recommend using the same colors for the distributions as in the main manuscript figures. What is the bin size for the histogram? Why is 6 Dec so much thicker than all the others? Are these not data from a repeated transect, but a different route each time?

**Response:** We have moved the former Figs. A1, A2, and C1 to the supplementary material. Fig. S1-S4 in the revised supplementary material have been updated as suggested. Missing information has been added in the captions.

The former Fig. 1 (Supplementary) has been removed because they were not directly relevant to the main focus of the study and could potentially distract readers. The former Fig. B1 has been removed, as it can be referred to Wagner et al. (2022) (Fig. 3).

We again thank the reviewer for constructive comments, which have greatly improved the manuscript.

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## Response to Reviewer Comments (Reviewer #2)

We sincerely thank the Editor and the Reviewers for their careful evaluation of our manuscript and for the constructive and insightful comments. We have revised the manuscript accordingly.

**Comment:** This paper analyzes snow depth and sea ice thickness relationships from in situ observations obtained during MOSAiC, the Lincoln Sea, N-ICE2015, Resolute Bay, and the Weddell Sea field campaigns. This collection of analyses span various ice types, conditions, and seasons. Snow depth distributions are fit to Normal, Log-Normal, Gamma, and Skew distributions and compared accordingly across ice thickness bins. Lastly, the authors examine snow depth correlation lengths against sea ice thickness to test these relationships at various distances. Key findings show snow depth varies considerably across ice types/thicknesses. The Log-Normal distribution tends to perform best for snow atop thinner ice conditions (newly formed ice, first-year ice and thinner multi-year ice) and under thicker ice conditions (>1.5 m), the Skew distribution is optimal. They also note that thicker ice is associated with longer correlation lengths (between depth and thickness) as a result of the common presence surface features such as hummocks and ridges.

The paper is very detailed, well-written, and illustrated. The authors' concluding remarks calling for SIT-dependent model parameterizations is supported by their findings. Just a couple of minor comments are offered below by line number of the submitted manuscript.

**Response:** [We thank the review for reading the manuscript and offering comments.](#)

**Comment:** Line (L) 6-7: Leave this definition of the coefficient of variation (CV) for the methods section, though I think it is fine to report the CV as needed here in the context of key findings.

**Response:** [Corrected.](#)

**Comment:** L85-90: Magnaprobe accuracy could be noted somewhere in this section.

**Response:** [We have added clarification in Section 2.2 \(Instruments, Line 109-110\) that the Magnaprobe has a maximum measurement depth of 1.2 m, with an estimated accuracy of 3 mm \(Sturm et al., 2006\).](#)

[We have also specified the performance characteristics of the EM/GEM system. Line 113-116 in the revision: The precision of the EM measurements is approximately 0.1 m on level ice up to 4 m thick. The accuracy decreases on rough and deformed ice, and the EM can underestimate the ridge thickness by up to 50% \(Haas et al., 2009\).](#)

[Furthermore, we have added a new discussion \(Section 5.4 Limitations, Line 604-614\) that addresses how instrument uncertainties may influence the SND distributions.](#)

**Comment:** L112: Since “drifting” mentioned earlier in the sentence, “drifting with the ice” can be omitted here.

**Response:** We removed the "drifting platform".

**Comment:** Figure 9: Some brief description of each of the meteorological “events” could be offered in the methods or folded into results. It is unclear, for example, what how “storm” is characterized.

**Response:**

We have added a description of the meteorological events by summarising the information reported in Wagner et al. (2022), see Line 323-331 in the revision:

Storms were closely related to ice deformation events. The first major change in ice conditions occurred around 16 November 2019, when a storm triggered strong ice deformation near the observation site. Another significant ice deformation event occurred around 11–12 March 2020 and periodically until 7 May 2020 (Wagner et al., 2022). Drifting snow events were identified by calculating the horizontal mass flux from snow particle counter (SPC) measurements. During periods of instrument downtime, a critical friction velocity threshold was applied to infer potential snow transport. The most significant drifting snow event occurred on 24–25 February 2020, during which the lower SPC recorded a cumulative mass flux of  $1.014 \times 10^6 \text{ kg m}^{-2}$ . During this event, 1-hour average wind speeds at 2 m above the ice reached approximately 11 m/s, with instantaneous wind speeds at shorter temporal scales likely exceeding this value (Wagner et al., 2022).

**Comments:**

Figure 17: Blue bars in panels a) and b) indicating storm presence appear to have different coloring.

L467: Change to “Future work should involve...”

**Response:** Corrected.

The authors again thank the reviewer for constructive comments, which have greatly improved the manuscript.

## References

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## Response to Reviewer Comments (Reviewer #3)

We sincerely thank the Editor and the Reviewers for their careful evaluation of our manuscript and for the constructive and insightful comments. We have revised the manuscript accordingly.

**Comment:** The authors use datasets of snow and ice thickness distributions from both Arctic as well as Antarctica sea ice to describe and investigate the relationships between snow and ice thickness distributions, to investigate the temporal evolution, and describe the spatial distributions, on small scales (under a km). The manuscript definitely fits within the scope of the Cryosphere, and provides several novel results, which is of interest to the sea ice community, in my opinion. My major concerns are the relatively poor discussion with existing literature, as well as the seemingly somewhat ad-hoc data selection and data treatment, as I will point out below.

**Response:** [We thank the review for reading the manuscript and offering comments.](#)

**Comment:** My biggest concern with the current manuscript is that it is not strongly connected to existing literature. For example, Mallett et al. (2022) [10.1017/jog.2022.18] has published a manuscript with the title: "Sub-kilometre scale distribution of snow depth on Arctic sea ice from Soviet drifting stations". Note the similarity between the title of this manuscript: "Sub-kilometer Scale Snow Depth Distribution on Sea Ice of Different Ages and Thickness". However, Mallett et al. (2022) is only referenced twice in the Introduction. Similarly, violin plots from the Weddell Sea data were published in Wever et al. (2021) [10.1017/jog.2021.54]. Violin plots are reproduced here, without relating them to the already published work from 2021. In its current form, the manuscript does not have a Discussion section. However, I think that such a section is really needed, to make connections with the existing literature: to what extent are previous results confirmed, or improved upon? It also needs to be discussed how the results relate to existing snow depth and ice thickness retrievals using satellites.

**Response:** [In the revised manuscript, we have added a new section \(Section 5.1 Comparison with previous studies\) to discuss how our results are consistent with previous findings while also providing new insights into snow-depth variability over sea ice. This section includes comparisons with Mallett et al. \(2022\); Wever et al. \(2021\), as suggested by the reviewer, and further discussion in the context of Liston et al. \(2020\); Moon et al. \(2019\), and Iacozza and Barber \(2010\).](#)

[We have added a new section \(Section 5.3 Links to satellite observations\) to discuss the implications of our findings for the remote-sensing community. Specifically, we highlight two key applications: \(1\) incorporating ice type in addition to freeboard could improve empirical snow redistribution functions used in laser-altimetry-based SIT retrieval, and \(2\) quantitative characterization of sub-grid snow variability enables an estimation of uncertainty in altimetry-derived sea-ice thickness \(SIT\) retrieval.](#)

**Comment:** I also did not understand the particular dataset selection. Is there really only one suitable dataset from Antarctic sea ice? Why is the other data from Mallett et al. (2022) not additionally included? Why are none of the remote sensing products included? At first I thought that the criterion was the availability of both concurrent sea ice thickness and snow depth data. But then according to Section 2.3.5, this is also not the case for the Resolute Bay data. I strongly recommend starting Section 2 with listing the criteria that have been used to include datasets in the analysis. I found for example also this dataset: 10.1002/2017GL075434, and there must be quite a few more out there. I think that AWI collects snow and sea ice thickness distributions during most of the Polarstern campaigns, for example.

**Response:**

Two criteria were applied in selecting the datasets. First, since this study aims to investigate the influence of ice age and SIT on SND distributions, it requires coincident in-situ measurements of SND and SIT from diverse ice types. We therefore selected field campaigns spanning a range of ice ages, surface roughness conditions, flooding states, and seasons, while maintaining a balance among sampling sites. Campaigns with well-established contextual knowledge of meteorological events and local ice conditions were prioritized to support the interpretation of the observations. Second, this study aims to analyse the temporal evolution of the SND distribution, which requires time-series observations of snow and ice properties at approximately weekly intervals. Characterizing snow and ice on this timescale is essential to capture snow accumulation and redistribution processes associated with ice growth and meteorological events (Liston et al., 2020).

Based on the above criteria, the Multidisciplinary drifting Observatory for the Study of Arctic Climate (MOSAiC) 2019-2020 dataset was selected as the primary dataset because it spans a wide range of ice types and provides time-series in-situ measurements along repeated transects. The MOSAiC dataset includes four typical ice types: SYI and FYI from winter to spring, newly-formed smooth FYI in winter, and SYI in summer. Beyond MOSAiC, additional datasets were selected to capture two critical snow and ice conditions that are not represented in MOSAiC. First, to better account for the effects of thick snow on sea ice prone to flooding, we included data from the Norwegian Young Sea ICE Expedition 2015 (N-ICE2015) and the Weddell Sea (2013) campaigns. Second, the Lincoln Sea (2017) campaign was included to represent a distinct rough MYI type, enabling a more comprehensive assessment of SND distributions across different ice ages. We note that only three transects were acquired over newly-formed smooth FYI from the MOSAiC (i.e. Runway), as this site was disrupted by ice motion (Itkin and Liston, 2025). To improve sampling for this ice type, we included the Resolute Bay (2025) dataset, which represents smooth FYI covered by a thin snow layer comparable to that on the MOSAiC Runway. Although co-located SIT measurements were not available for Resolute Bay, field drilling indicated similar ice conditions (see Table 1).

In the revised manuscript, we have added the above text to a new section (Section 2.1, Selection of in-situ field campaigns) to clarify the criteria used to select the field datasets analysed in this study. The suggested dataset (DOI: 10.1002/2017GL075434) is the Lincoln Sea dataset, which is already included in this study.

We have revised the title to better reflect the regional scope of the study: Snow Depth Distributions on Sea Ice of Different Ages and Thicknesses from Regional Field Campaigns.

Furthermore, we have added a dedicated discussion on regional representativeness and the extent to which the findings may be transferable to similar sea-ice conditions. This is addressed in the new Section 5.4 (Limitations, Line 581-604).

**Comment:** Also regarding this point, are the data from representative floes for the region? How do average snow depth and ice thickness compare to regional statistics? Given that the selection of data seems to have been quite restrictive, it is important to provide the reader with information about the representativeness of this data compared to overall climatology of sea ice in the region.

**Response:** Thanks for this point. The primary objective of this study is to characterize sub-kilometre-scale SND distributions, and therefore direct comparison with large-scale regional statistics derived from models or satellite products (typically at tens-of-kilometres resolution) is not necessarily comparable due to scale mismatch.

We acknowledge that the regional analysis of this study is a limitation. The analysis in this article is based on the selected test sites rather than on unified pan-Arctic and -Antarctic datasets. Nevertheless, the sites were carefully chosen to span various snow and ice types and each contains thousands of samples, ensuring statistically robust analysis. However, SND distributions can be sensitive to many local factors such as ice topography, snowfall, wind redistribution, flooding, and melt processes. Consequently, extending these findings to the hemispheric scale requires thorough evaluation using large and comprehensive datasets. Despite this limitation, the regional cases analysed here, supported by high-quality in-situ measurements, provide new insights into how SND distributions depend on ice types and thicknesses at the sub-kilometre scale.

In the revision, we have added the above text in the new Section 5.4 (Limitations, Line 581-589). Furthermore, we have summarised the climatic conditions and snow and ice properties for all test sites in Section 5.4 (Limitations, Line 590-604).

**Comment:** For the Antarctic sea ice data, were station 503 and 506 combined? It sounds like this from the sentence (L105/106): "For the FYI floes (503 and 506), the mean SIT and SND were 0.69 m and 0.19 m, respectively." Does it make sense to combine both datasets? In Table 2, there is only one entry for Weddell Sea FYI (even though there are 2 floes, from different locations and time periods), whereas in Fig. 13d, the floes are analyzed separately. In Wever et al. (2021), the distributions are already shown, and it is clear that 503 and 506 have different underlying sea ice thickness distributions (as also shown in Fig. 13d). Similarly for the N-ICE2015 campaign. In Fig. 4, it looks like all 4 transects are combined in a single violin plot, even though they were captured over the course of a few months, from different locations. I don't think it is justified to combine them like this. In contrast, the MOSAiC data seems to not have been combined. Why are the MOSAiC datasets then analyzed separately?

**Response:** We clarify that Floes 503 and 506 were combined only when reporting summary statistics (e.g. Table 1–2 and the violin plot Fig. 3 in the revision) to provide a general characterization of FYI conditions in the Weddell Sea from June to August. The same descriptive aggregation by ice age is applied consistently across all campaigns, to ensure comparability when reporting overview statistics in Table 1-2 and Fig. 3 only for descriptive purposes. For N-ICE2015, the four transects are all from winter segments (January to March) on SYI. For MOSAiC, we group the transects into Nloop (November - May) on SYI, Sloop (November - May) on FYI, Summer (June - August on SYI), and Runway (Jan - Feb) on newly-formed FYI. The purpose of this aggregation is descriptive, allowing readers to compare mean and standard deviation values across ice-age categories in a consistent framework across all regions.

In detailed statistical analyses (Section 4), the floes/transects are treated separately. In the revised manuscript, we have updated Fig. 10 and Section 4.2.2 (Regional comparison, Line 406-411) to analyse Floes 503 and 506 individually, including

their PDF fitting performance,  $\mu_{SIT}$  and  $\mu_{SND}$  values, addressing differences in the underlying SIT distributions identified by Wever et al. (2021). All statistical fitting and variability analyses are conducted consistently at the individual transect level (Fig. 8–9 for MOSAiC and Fig. 10 for the other sites in the revision).

**Comments:**

L248-249: I'm not really sure I follow this proposed mechanism. Regarding the first physical mechanism, there is indeed something like enhanced compaction of wet snow, particularly upon first wetting. I found this conference proceeding which describes this: <https://scispace.com/pdf/the-first-wetting-of-snow-micro-structural-hardness-2islaa1vn8.pdf> But maybe the authors can find some peer-reviewed literature in support of this. Regarding the second mechanism, I'm not sure that it works like that in reality. Why would refreezing of the flooded layer be faster with higher overburden pressure? The only thing I can think of is the higher the overburden pressure, the higher the density at the base, the smaller the capillaries, the higher the capillary suction. Please provide more robust explanation, possibly with a citation, of what is meant here. I think the phrasing: “can turn into snow-ice more easily” is unclear. “More easily” is not a clear expression to describe a process.

**Response:** We thank the reviewer for the reference and interpretation. We have added the following explanations in the revision, Line 290-307:

Flooding wets the basal snow layer, promoting wet-snow settling and densification that reduce the mean SND (Colbeck, 1979; Marshall et al., 1999; Techel et al., 2008; Wever et al., 2021). Although flooding reduces mean SND, it is also associated with a lower-than-expected variability, leading to a reduced CV.

The reduced SND variability at flooded sites may arise from two physical mechanisms. First, because overburden stress increases with SND, thicker snow can undergo greater compaction once wet (Colbeck, 1979; Marshall et al., 1999), leading to a greater reduction in SND than thinner snow and thus reducing spatial SND variability. Mallett et al. (2024) reported the lowest brine wicking in the lowest-density layer, while the denser layers tended to exhibit higher wicking heights. A plausible explanation is that smaller capillaries in denser basal snow increase capillary suction, enhancing upward brine infiltration and increasing the volume of snow that becomes slush and subsequently freezes into snow ice. If snow-ice formation is more extensive in initially thicker snow, this could amplify net snow-thickness loss there and further suppress spatial SND variability. Second, snow may become more cohesive and mechanically stronger after flooding, making it less susceptible to wind-driven redistribution. In winter, upward conductive heat flux from the ocean warms the ice–snow interface, increasing the temperature gradient across the snow (for a given air temperature) and also raising the snow's mean temperature. Since sintering accelerates at higher temperatures (Blackford, 2007; Clemens-Sewall et al., 2022), snow warmed by basal flooding sinters faster, thus increasing cohesion and making the snow less susceptible to wind redistribution and thus reducing SND variability. The difference in CV between flooded and non-flooded sites suggests that flooding can imprint a measurable signature in the SND distribution. The above proposed mechanisms still require validation through sufficient in-situ SND and SIT measurements at flooded sites.

**Comment:** Fig. 11, 13, etc.: when data is from the same location, but only differs in time, it makes sense to draw lines between data points. However, for N-ICE2015 and the Weddell Sea data, they are from different dates and locations, and even from different types of ice. This should then not be shown with a line graph.

**Response:** We have reorganised the figures, and the former Figs. 11e–h and 13 have now been merged into a single Fig. 10. We have attempted to remove the connecting lines; however, then the separated data points appeared visually cluttered. To improve readability, we therefore included very thin connecting lines for visual guidance only. We have clarified this in the caption of Fig. 10 - “Note that the thin lines connect samples within each test site for visual clarity only and do not imply time-series measurements.”

**Comment:**

Caption Fig. 5: “The histogram were generated with a bin width of 5cm for display.” This sentence has a grammatical error, and I don’t understand what “for display” means in this context.

Abstract, L9-10: Instead of (or in addition to) writing what you did: “We investigate ...”, (also) write the conclusions of what you found with this analysis.

**Response:** Corrected

**Comment:** Note that a “snow drift” is a bedform that forms when snow deposits during drifting snow conditions. The event itself is called “drifting snow”. This term is mixed up in the manuscript (see for example L11-12 vs L43). Please make the terminology consistent throughout the manuscript.

**Response:** We have revised the terminology throughout the manuscript to ensure consistency. Specifically, we now use “snow dune” to refer to the bedform and “drifting snow event” to describe the atmospheric transport process.

**Comment:** L245: “eaten away”: to avoid any conclusion, please explain in a more process-based way what happens here. “Eaten away” sounds like something disappears, but is it not rather the conversion into snowice that forms after refreezing of the flooded layer?

**Response:** We have removed the “eaten away”. This part has been rewritten according to the previous comment, see Line 290-307 in the revision.

**Comment:** L281-282, L454-455: This sentence, arguing that drifting snow leads to a more evenly redistributed snow layer, is not logical and stated too generalized. In the absence of drifting snow, a 10cm snowfall would simply deposit an even layer of 10cm of snow. Only if the initial snow depth distribution would not be homogeneous, because of differential melt, or previous drifting snow events, or different ice ages in close proximity, or the presence of ridges, then drifting snow could make the snow depth distribution more homogeneous.

**Response:** We have revised the sentences (Line 353-357 in the revision) as below: It can be attributed to the fact that Nloop was sampled on thick and deformed SYI with pronounced ridges, as supported by the large SIT range reported in Table 2.

Such deformed ice topography promotes a non-homogeneous SND distribution. The subsequent drifting snow event likely redistributed the snow, leading to a more homogeneous SND distribution and resulting in a distribution better described by the skew distribution.

**Comment:** L371-372 / Fig. 15: Even though Log-normal may perform well in some cases, it also exposes one to the risk of having some of the largest RSME when it doesn't match, as displayed in Fig. 9, 13a,b... Something to be aware of. One can argue that other distributions behave more stably, and are less prone to large discrepancies with the actual snow depth distributions. So they might be considered the best option, given this issue.

**Response:** We agree that the skew distribution can provide a more robust representation of SND distributions, particularly over heterogeneous ice regimes, as it offers a balance between fitting accuracy and statistical stability.

We have incorporated this conclusion into the abstract (Line 12-14 in the revision): Notably, although the log-normal distribution performs best under specific conditions (e.g. new or melt-season snow), its performance can deteriorate substantially over older and thicker ice, whereas the skew distribution provides a robust representation across heterogeneous ice types.

In addition, we have added discussions (Section 5.1 Comparison with previous studies), where we have compared the advantages and limitations of log-normal and skew distributions. Line 506-513 in the revision:

Note that in these two cases, the improvement in the log-normal over skew distribution is modest ( $\approx 20\text{--}25\%$ ), calculated from Table 2 using  $(\text{RMSE}_{\text{skew}} - \text{RMSE}_{\text{log-normal}}) / \text{RMSE}_{\text{skew}}$ . However, the skew significantly outperforms the log-normal by  $\approx 60\text{--}80\%$  in other SYI and MYI cases. In particular, large RMSE values of log-normal are observed for MOSAiC Nloop after 30 January 2020 in Fig. 8a and the Lincoln Sea and N-ICE2015 in Fig. 10. Hence, although the log-normal distribution performs best in these two exceptions, the skew distribution exhibits greater stability and is less sensitive to extreme deviations from the observed SND distributions. For practical applications, the skew distribution therefore provides a more robust representation of SND distribution over regions containing mixed ice types or lacking a priori knowledge of ice type.

Furthermore, we have highlighted the stability and practical applicability of the skew distribution in the new Section 5.5 (Recommendations for practical applications, point 4).

**Comment:** L445-447: Given the already provided explanation in L245-249, I'm not understanding here in which direction this additional research is supposed to go. I suggest writing this in a more specific research direction, or to remove it from the manuscript.

**Response:** We have removed the sentences.

We again thank the reviewer for the constructive comments, which have greatly improved the manuscript.

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