

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. We now 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:

To minimize the risk of arbitrary conclusions, two criteria are applied in the selection of datasets. First, datasets are required to provide known SND, ice type, and SIT information. For this purpose, coincident SND measurements from the MP and SIT data from the GEM are needed. We include sites spanning a range of ice age, surface roughness, flooding condition, and season, whilst maintaining a balance across sampling sites. In-situ campaigns for which well-established contextual knowledge is available are prioritized, enabling appropriate site specific interpretation of the observation. All Arctic sites used in this study involved participation by co-authors, ensuring detailed knowledge of the datasets. Secondly, this study analyzes the temporal evolution of SND distribution, which requires an accurate representation of snow and ice properties at approximately weekly

resolution. 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 data source because it spans a wide range of ice regimes and, importantly, provides time-series measurements along the same 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. In addition to MOSAiC, further datasets were incorporated to capture two critical snow and ice conditions not represented in the primary dataset. First, to better account for the effects of substantially thick snow on sea ice, we included data from the Norwegian Young Sea ICE Expedition 2015 (N-ICE2015) and the Weddell Sea (2013) campaigns; the Lincoln Sea (2017) dataset was incorporated to represent a distinct MYI regime, 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 during MOSAiC 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 and relatively uniform snow layer comparable to that at 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 Across Sea-Ice 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). We acknowledge that the regional analyses of this study is a limitation. The analysis is based on selected field sites rather than on unified pan-Arctic/Antarctic datasets. Nevertheless, the sites were carefully chosen to span various snow and ice regimes and each contains thousands of samples, ensuring statistically robust analysis. At the same time, each site is characterized by different environmental conditions, and SND distributions may be sensitive to local factors such as ice topography, snowfall, wind redistribution, flooding, and melt processes. Consequently, extending these findings to hemispheric scale will require further evaluation using broader and more uniformly sampled datasets. Despite this limitation, the regional cases analyzed here, supported by high quality in-situ measurements, provide new insights into how SND distributions depend on ice type and SIT at sub-grid 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 these two cases, the improvement of log-normal over skew 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 $\approx 60\text{--}80\%$. Large discrepancies of log-normal are observed from the MOSAiC Nloop after 30 January 2020 and the Lincoln sea and N-ICE2025. 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, it therefore provides a more robust and reliable representation.

In Section 2.2 Instruments, we have added descriptions of measurement accuracy. In all campaigns, SND measurements were carried out using automated SND probes (Magnaprobos) 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 ~ 3 mm (Sturm et al., 2006). However, when using a Magnaprobe, slight tilting of the basket on the snow surface or the probe not being perfectly vertical can result in measurement errors greater than ~ 3 mm. The precision of the EM measurements is approximately 0.1 m on level ice up to 4 m thickness. The accuracy decreases on rough and deformed ice, and can underestimate the ridge thickness by up

to 50% (Haas et al., 2009). In Section 5.4 Limitations, we have added a discussion that considers Magnaprobe precision when interpreting the RMSE values. 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.

In Section 5.2 Links to sea ice models, 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) 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. Our findings on SND distributions across ice age and SIT can be incorporated into ice thickness distribution (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 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 Conclusions section has been rewritten. In particular, we now emphasize 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-grid 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 Across Sea Ice 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: Due to various meteorological and geophysical conditions, the snow cover is naturally highly uneven, thus the assumption of being distributed uniformly (Veyssière et al., 2022) can not accurately represent the snow distributions within the sub-grid. As such, a distribution based on the mean depth has been deployed more recently (Stroeve et al., 2024; Heorton et al., 2025).

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 this study: Liston et al. (2020) revealed a linear relationship between parcel age and SND, suggesting that an accurate representation of SND evolution on sea ice requires realistic matching of 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 SND 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 ~ 3 mm (Sturm et al., 2006). However, when using a Magnaprobe, slight tilting of the basket on the snow surface or the probe not being perfectly vertical can result in measurement errors greater than 3 mm. The precision of the EM measurements is approximately 0.1 m on level ice up to 4 m thickness. The accuracy decreases on rough and deformed ice and can underestimate the thickness of the ridge by up to 50% (Haas et al., 2009).

We have added the above text in Section 2.2 Instruments.

The Magnaprobe has a precision of 3 mm. Given that the typical variability of SND within each transect is several to tens of centimeters, 3 mm is small relative to the observed spatial variability. In the analyses, we use μ_{SND} computed from all samples along each transect rather than individual measurements, 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 SIT values derived from EM are used primarily for mean-SIT binning and regime classification. We again use μ_{SIT} calculated from all samples along each transect rather than individual measurements, which minimizes the influence of measurement uncertainty on the reported SIT-dependent relationship.

We have added the above text in Section 5.4 Limitations.

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. 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 Magnaprobe and GEM measurements, nearest neighbour interpolation onto a 1 m grid was applied. 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 recorded continuously 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 neighbour 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 neighbour 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 meters. Given that our analysis is based on values averaged over the transect (μ_{SND} and μ_{SIT}) rather than individual measurements, positional uncertainties at the meter 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, μ_{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) and Section 5.4 (Limitations).

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 one figure 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 μ and σ 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. μ_{SIT} , μ_{SND} , σ_{SIT} , and σ_{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 conditions (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 $300 \times 500 \text{ m}^2$

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 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. The observed linear relationship between μ_{SND} and μ_{SIT} for FYI can be explained by younger and thinner ice growing and evolving alongside snow cover during the same seasonal cycle.

Comment: L231: CV already defined earlier.

Response: Done.

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: Panel (a), the colorbar is from the minimum value to maximum value of μ_{SIT} , which is 0.6 m to 4.1 m. Panel (b), y axis intercept is at 0. We have replotted 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. Note that the original L244-246 parts have been rewritten.

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, μ_{SIT} from 0 to 5 m, and Δ_{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 exact dates of each transect measurement are now explicitly shown on the time axis. In addition, we have combined and reorganised the former Figs. 11e–h and 13 for the other sites 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 meters. 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 (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 -> μ_{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 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 σ_{snow} and not σ_{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 μ and Δ 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. μ_{SIT} , μ_{SND} , σ_{SIT} , and σ_{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.

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 Δ_{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 of log-normal over skew 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 the case of SYI and MYI. Large discrepancies of log-normal are observed from the MOSAiC Nloop after 30 January and the Lincoln sea and N-ICE2025. 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 and reliable representation.

The above explanations have been incorporated into Sections 5.1 and 5.4 of the revised manuscript.

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 -> μ_{SIT} , like on the next line

L374: be consistent with subscripts, why is it Δ_{ice} and not Δ_{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 because local non-stationarities introduce undulations in the semi-variogram (Moon et al., 2019). In such cases, the effective range cannot be robustly estimated from the semi-variogram, and a more advanced approach, such as multifractal temporally weighted detrended fluctuation analysis, may be required (Moon et al., 2019).

We have added the above text in the revision.

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 $> Q3 + 1.5 * (Q3 - Q1)$ and sample $< Q1 - 1.5 * (Q3 - Q1)$. 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 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 -> μ_{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 C2 to the Supplementary Material. The figures have also been updated as suggested. Missing information has been added in the captions. The former Fig. 1 (Supplementary) has been removed as 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 can be referred to Wagner et al. (2022) (Fig. 3).

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

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