

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. \(2019\); 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:

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. 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 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).

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 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.

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

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., Tables 1–2 and the overview violin plot) 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 Tables 1 to 2 and in the overview violin plot 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 analyzes (Section 4), the floes/transects are treated separately. In the revised manuscript, Figure 10 and the new Section 4.2.2 (Regional comparison) analyze Floes 503 and 506 individually, including their PDF fitting performance, μ_{SIT} and μ_{SND} values, thereby addressing differences in the underlying SIT distributions identified by Wever et al. (2021). All statistical fitting and variability analyzes are conducted consistently at the individual transect level (Figs. 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 the interpretation. We have added the following explanations in the revision.

The reduced SND variability observed at flooded sites may arise from two physical mechanisms. First, flooding wets the basal snow layer, promoting wet-snow settling and densification that reduce SND (Techel et al., 2008; Wever et al., 2021). 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. In addition, 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), snow warmed by basal flooding is expected to sinter faster, increasing cohesion and surface hardness. Such a process could potentially suppress the development of SND variability over time by comparison to snow that remains unflooded and is therefore colder.

The difference in CV between flooded and non-flooded sites suggests that flooding can imprint a measurable signature in the SND distribution. The proposed mechanisms remain tentative and require validation through targeted field measurements, controlled laboratory experiments, and modelling.

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 initially attempted to remove the connecting lines; however, 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 by stating: “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: The sentence has been rewritten.

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 as below: This observation can be attributed to the fact that Nloop was sampled over 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. 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. Furthermore, we have highlighted the stability and practical applicability of the skew distribution in the new Section 5.5 (Recommendations for practical applications).

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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