

Dear Alek,

First of all, we would like to thank you for your positive and constructive review. We address your comments point by point below. We note that the second reviewer has also provided a substantial list of major comments that we will need to address in the revision. Some of these comments overlap with the points raised in your review, and we have therefore taken both sets of comments into account when planning the revisions to the manuscript. Our responses to your comments are shown in blue.

This paper presents seven years (2012 to 2019) of spatially referenced sea-ice draft observations from upward-looking sonars, combined with ADCP-derived ice-drift measurements at four moorings in Fram Strait. Using these data, the authors reconstruct meter-scale spatial profiles of sea-ice draft and quantify ridge frequency, keel-depth distributions, and the areal and volumetric contribution of ridged ice to Arctic sea-ice export.

Overall, the long-term mooring record combined with spatial reconstruction is particularly valuable, and the focus on shallow ridges is timely given ongoing Arctic sea-ice thinning. The main results are convincing and relevant for sea-ice mass-balance studies, ice-ocean drag parameterizations, and satellite-retrieval insights.

Main Comments

The title and main stated objective doesn't quite feel right to me. The real strength of the study to me lies in quantifying the contribution of sea-ice ridges to regional sea-ice mass balance in the region, which is articulated well at the beginning of the conclusion. Given this, I would encourage the authors to make this contribution/advance more clear in the title and framing.

We appreciate the reviewer's suggestion to orient the title more explicitly towards sea-ice mass balance. However, we prefer to retain the current title, "*Sea-ice ridges are a major component of Arctic sea-ice export through the Fram Strait.*" The rationale behind it is that the role of ridges in the Arctic sea-ice mass balance can be inferred from our results anyways. At the same time, the authors of this manuscript have already directly contributed to several studies addressing the role of Fram Strait as the principal pathway for Arctic sea-ice export, which constitutes an important component of the freshwater flux to the North Atlantic. Quantifying the contribution of ridged ice to this export is therefore relevant not only to the Arctic sea-ice budget, but also to the freshwater forcing of the North Atlantic. For these reasons, we feel that the current title more accurately reflects the main result and broader significance of the study.

In addition, this dataset represents an important modern complement to historical Fram Strait observations (largely from submarine sonar). Clearer and more quantitative comparisons with these prior estimates would significantly strengthen the manuscript, the introduction of proper campaigns and estimates is introduced quite sporadically and vaguely.

We thank the Reviewer for this suggestion. A similar, though somewhat more specific, comment was also raised by Reviewer 2. We would like to note that a more comprehensive compilation and synthesis of historical Fram Strait observations and estimates is already included in a broader review study on sea ice ridges led by Granskog, which was submitted approximately one year ago to *Earth-Science Reviews* and is currently under revision.

As this review is not yet accepted and therefore not yet available to the scientific community, we consider it useful to retain a concise comparison with the most relevant previous estimates in the present manuscript. At the same time, given that the manuscript is already substantial in

length, we would prefer to avoid an extensive literature compilation that would substantially duplicate the broader review. We will nevertheless revise the relevant sections to make the comparison with previous Fram Strait observations clearer and more consistent.

The manuscript often refers to “ice thickness,” yet the analysis is based entirely on ice draft I think! This is confusing and should be corrected throughout. If thickness is inferred or discussed conceptually, the assumptions and limitations should be clearly stated.

We thank the Reviewer for pointing this out. We agree that ice draft and ice thickness are not equivalent quantities and that the terminology was used inconsistently in the original manuscript. The analysis presented in this study is based on ice draft, which is the quantity directly measured by the upward-looking sonars. We have therefore carefully checked the manuscript and revised the terminology throughout to use “ice draft” where the measured quantity is being discussed. The term “ice thickness” is retained only where it is appropriate in a conceptual or contextual sense, or where thickness is explicitly inferred from draft under stated assumptions.

While the authors appropriately acknowledge the limited record length and data gaps, discussion of declining ridge numbers and shallower keel depths should be framed more explicitly as suggestive, rather than a robust trend analysis of any kind, given the significant interannual variability.

We fully agree with the Reviewer that the observed decrease in ridge numbers and shallower keel depths should not be presented as robust long-term trends, given the relatively short and spatially incomplete records and the substantial interannual variability. Note this is already stated in the original submission.

This recommendation partly contrasts though with the suggestion of Reviewer 2, who requested that more details on the trend analysis be provided in the manuscript. We therefore consider it more pragmatic and balanced to retain the trend analysis, but to make its statistical basis and limitations more explicit. We will slightly extend Table 2 to include the actual trend statistics, or provide these in an additional table if this proves more appropriate. We will also revise the accompanying text to emphasize that the derived trends are indicative and should be interpreted with caution, more as suggestive evidence of changes in ridge occurrence and keel depth.

Adaptive approaches for estimating level-ice surfaces are not really new, despite the framing of this paper. Relevant examples include methods based on low elevation change over a given along-track distance (e.g., Wadhams and Horne, 1980) and modal elevation detection (e.g., Williams et al., 2015; sail analysis in Petty et al., 2016). I think it's better to make that much clearer.

We agree that adaptive approaches for identifying the level-ice surface are not new, and we have not intended to claim priority for the use of an adaptive threshold. Indeed, we already refer explicitly to the previous study by Brenner et al. (2021), which applied a similar approach to isolate ridges from ice-draft records. We will further strengthen this context by citing the recent study of Eilers and Bradley (2026, doi 10.1029/2025GL119003), which uses a related approach, with some methodological modifications, for ridge identification. We also acknowledge the relevance of the earlier approaches based on local draft variability and modal elevation discussed by the Reviewer, including Wadhams and Horne (1980), Williams et al. (2015), and Petty et al. (2016).

Our contribution is therefore not the introduction of the adaptive-threshold concept itself, but its application and evaluation in the context of the presented ULS data series. In particular, our analysis demonstrates that using a fixed threshold without accounting for variations in the local level-ice draft can lead to a substantial undercounting of ridges. Simply lowering the threshold is not necessarily an adequate solution, since a threshold that is biased low can interfere with the identification of ridge flanks, whereas a threshold that is biased high can result in shallow ridges being missed. We will revise the relevant text to make this distinction between the established methodological concept and the specific contribution of our study more explicit.

The distinction between ice rubble and ridges (L143) is introduced rather briefly, despite being a potentially important conceptual and methodological point. A clearer explanation is needed I think!

We agree with the Reviewer on this point. A similar recommendation was actually provided by the second reviewer. We will address it through a better description of the mixed (residual) ice category. We also address the Reviewer to our detailed response to Reviewer 2 on this subject.

The discussion in Section 3.4 is overly qualitative and vague. This section would benefit from clearer, more quantitative comparisons with existing studies of ridge fraction and ridge volume, ideally incorporated directly into figures rather than solely discussed in the text, to better contextualize these data.

Please see our response to your similar comment above.

The manuscript reports both ridge count and ridge density. The rationale for including both metrics and how they differ in interpretation should be explained explicitly or dropped, I didn't quite understand why ridge count was included.

The main rationale for reporting ridge count is to place our observations in the context of a previous study on the topic and area. More specifically, Ekeberg et al. (2014) cited in our manuscript, report the number of ridges registered by one of the moorings during 2006–2011 (a “composite” series from Hansen et al., 2013, doi:10.1002/jgrc.20393) providing a useful basis for comparison with our observations. Therefore, in addition to providing continuity with previous observations, the ridge counts provide useful climatological context given the relatively short duration of the four spatial series analyzed here and provide qualitative support for the possible long-term reduction in ridge occurrence in the region.

Ridge density, the number of ridges per km of ice, in turn provides a spatially normalized measure that is more directly comparable between periods or locations with different amounts of ice transport. Ridge density is also a commonly reported quantity in many previous studies and has been used as an indicator of the intensity of sea-ice deformation/ridging.

We therefore consider both metrics useful. We will clarify the distinct roles and interpretation of ridge count and ridge density in the revised manuscript.

The macroporosity discussion is confusing to me. Macroporosity affects ridge density rather than geometric volume I think. While excluding air pockets may be justified in terms of actual ice volume, I don't really understand if this was needed here.

Thanks for pointing this out. We acknowledge that the term “density” may be confusing in this context. In the manuscript, “ridge density” refers specifically to the number of ridges per km of ice, and not to the density of the ice a ridge consists of. This terminology is used in a number of

previous studies of sea-ice ridging, but we agree that the distinction should be made more explicit.

Regarding macroporosity, its role in our analysis is separate from ridge density. The geometric volume derived from the ridge profiles represents the volume enclosed by the ridge geometry, including sea water-filled voids between ice blocks. Because our objective is to estimate the actual volume of sea ice contained within the ridges, we apply a macroporosity correction to account for these void spaces. Thus, macroporosity does not modify the ridge density in the context used in this study, or the geometrically derived ridge volume itself; rather, it affects the conversion from geometric ridge volume to the corresponding volume of solid ice.

We will clarify this distinction in the revised manuscript and revise the terminology where necessary to avoid confusion between ridge density and material density.

Minor Comments

Figure 2: Since longitude is fixed for each mooring, a table might be more efficient. “2019-2019” should read “2012-2019.”

We respectfully disagree on this point. We consider the present visualization to be more informative than a table, as it allows us to show both the periods with available IPS data and the periods with high-resolution resampled data used in this study (i.e., the data derived for periods when sea-ice drift velocities were available). Presenting all this information in a table would result in a relatively crowded format and would make it more difficult to visually identify the temporal coverage. We therefore consider the current figure format to be more effective and informative. However in response to the comment of the R2, we will also add a total number of days for each mooring when the ADCP option was not available (and hence the spatial series could not be constructed). “2019-2019” will be changed to “2012-2019”, thanks!

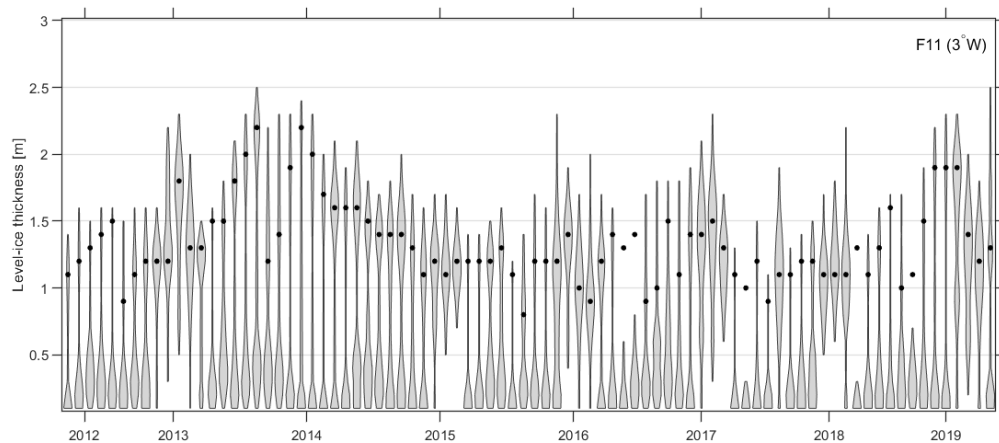
Figure 3: Clarify in the caption that fractions are calculated relative to ice-covered periods only (open water excluded). Clarify the “mixed” category in the legend.

Thank you for the comment, we modified the caption accordingly. It now reads as:

“Monthly areal fractions of ice ridges, level ice, and residual ice (mixed ice not attributed to ice ridges or level ice) for the four moorings, calculated relative to the total ice area. Here, “area” refers to the fraction of the along-track length occupied by the respective ice categories, with open-water sections excluded from the calculations. Multiannual means for each category and mooring are shown as numbers, with colors matching those used for each specific category.”

Figure 4 is difficult to interpret. Violin plots may provide a clearer representation of the thickness/draft PDFs. The meaning of the black line is unclear, and the phrase “maxima in modal” is confusing.

We have also explored presenting these distributions as violin plots, and an example of one of the panels is provided below.



However, we found that the violin-plot representation was less effective for visualizing the data. Given the large number of monthly distributions to be presented, the violin plots become visually crowded and make it more difficult to identify the temporal evolution of the level-ice thickness distribution. We therefore consider the currently used surface-plot representation to be more appropriate, as it provides a compact visualization of the probability density while allowing the evolution of the level-ice thickness distribution over time to be readily identified through the colour scale. We have consequently retained the surface-plot representation in the manuscript. The “black line” referred to in the comment was intended to improve the visualization of the temporal evolution of the old-ice modal peak, i.e., the typical thickness of the thicker level ice transported from the Arctic (see Hansen et al., 2014, doi:10.1002/2014JC010048, for the definition). The monthly modal peaks were originally highlighted using black dots connected by a line. We have now removed the connecting line to improve the visual appearance and replaced the black dots with black dashed markers. Note also that this figure will be moved to Supplementary, and replaced by another figure that shows composite draft probability densities for the three sea ice categories. This is done to address one of the major comments of R2.

Figure 5: A similar analysis for daily ridge density within each year could be useful. While there are hints of seasonality, it is difficult to assess this from the current presentation. The added value of showing daily data as plotted should be clarified. Note that daily ridge densities are calculated over short drift distances (typically <20 km), contributing to high variability.

We thank the Reviewer for this suggestion. We agree that the daily ridge counts estimates exhibit substantial variability. We therefore decided to remove Figure 5 from the main presentation. The monthly ridge counts presented in Figure 7 and a seasonal cycle in Figure S4 provide a clearer representation of the seasonal and interannual variability and are more informative for this purpose. The distribution of daily (effective) ridge densities is nevertheless retained in Figure 6, where the daily values are summarized by their empirical probability density functions together with the corresponding lognormal fits. This presentation preserves the information on the variability and distribution of daily ridge density without introducing the additional visual noise associated with plotting the individual daily estimates.

Figure 6: The explanation of symbols and intervals is unclear. The caption should clearly define that circles and horizontal lines show median ridge densities and the 67% prediction intervals?

We kindly ask the Reviewer to specify what is not clear in the caption since it already states this explicitly, please see the quotation below:

“Filled circles and horizontal lines show median ridge densities ($e\mu$) and the 68% prediction intervals of $[e\mu/\sigma, e\mu*\sigma]$, respectively, calculated from the log-normal probability distribution fitted to the data.”

Figure 10 caption: Clearly state which macroporosity assumption is used for the reported mean values and trends.

The mean values and trends currently shown in Figure 10 were calculated assuming zero macroporosity. We will revise the figure and its caption to make this assumption explicit.

In addition, following the suggestion of Reviewer 2, we will add a second series showing the corresponding mean values and uncertainties for the 30% macroporosity assumption. We will also incorporate the additional modifications requested by Reviewer 2 to account for the contribution of ridge sails. These additions will allow the sensitivity of the estimated ice volume and its temporal variability to the assumed ridge porosity and sail contribution to be assessed directly from the figure.

L11-12: This statement is speculative (though reasonable) and likely too strong for inclusion in the abstract.

We agree that the statement in the Abstract is somewhat speculative and should be supported by a more quantitative analysis before being presented in this form. A related comment was also raised by Reviewer 2, who requested a more in-depth analysis of ice-drift trajectories and sea-ice age to investigate the possible influence of source regions and deformation history.

We will therefore conduct this analysis for the revised manuscript and reassess the corresponding interpretation. The statement in the Abstract will be revised accordingly, based on the results of this additional analysis, and will be formulated more cautiously if the available evidence does not support the original interpretation.

L20: The claim that ridges are the “least studied component” of Arctic sea ice is too strong given extensive submarine, airborne, and growing satellite-based papers. Consider softening or qualifying.

The second Reviewer has also commented on this statement finding it to be too strong. This is now modified and a relevant recent citation is also added.

“ large fraction of the sea-ice cover resides in ridges (Wadhams, 1990, Melling and Riedel 1996, Hansen et al., 2014), dynamically formed structures that make up the thickest part of the Arctic ice pack, which is one of the least studied components of Arctic sea ice (Granskog, 2026, <https://doi.org/10.1016/j.polar.2026.101421>). “

L24: This sentence appears unnecessary given the breadth and maturity of the existing literature.

We still would like to keep this sentence given the i) potential biological importance of ice ridges, ii) knowing that there are so much ice in ridges that has not been explored from this perspective. We also would like to add two very recent publications that highlight the ecological role of ice ridges, namely the recently published studies of Muller et al., (2026,

<https://doi.org/10.1038/s43247-026-03364-8>) and Olsen et al., (2026, doi:10.1038/s43247-026-03486-z).

L29: Please verify this statement.

Please see our response to Reviewer 2 below who had a similar comment:

We thank the Reviewer for pointing this out. We agree that the ~90% estimate refers to particular observational periods and estimates of sea-ice export through the different Arctic gateways, and that the relative contribution of Fram Strait may therefore vary interannually with variations in sea-ice fluxes through the different gateways. Davis Strait is, in fact, the second-largest contributor after Fram Strait. Another point is that the estimates for the individual gateways themselves may also vary between studies, depending on the methods and data used, as illustrated, for example, by the different estimates for Fram Strait reported by Kwok et al. (2004, 10.1029/2003JC001785) and Serreze et al. (2006, 10.1029/2005JC003424). We added a brief caveat to the text and clarify the period and context of the cited estimate:

“Fram Strait is the main gateway that connects the Arctic Ocean and the North Atlantic Ocean, through which almost 90% of the sea-ice export from the Arctic Ocean occurs (Spren2020,Sumata2022NatCom). This estimate should, however, be considered in the context of specific observational periods, as the relative contribution of Fram Strait may vary interannually depending on the magnitude of sea-ice fluxes through Fram Strait and the other Arctic gateways.”

L33: When stating that the dataset is “one of the most extensive,” briefly clarify the basis for comparison (e.g., time span, spatial coverage).

The data set presented in this study is one of the most extensive in terms of both the spatial coverage and the time span. This is to be clarified in the text.

L42: Please reword for clarity. More explicitly explain that ice velocity is required to convert time-series draft measurements into spatial profiles.

Thanks for pointing this out. We have modified the sentence to make it more clear:

“While a wealth of sea-ice draft data have been collected from moored observations, relatively few datasets have been referenced in space. Because moored ice-profiling sonars measure ice draft as a function of time, measurements of ice velocity are required to convert the time series into a spatial (distance) series along the ice-drift trajectory. This can be achieved by combining the draft measurements with in situ ice-velocity observations, for example from an ADCP Melling et al., (1995)”

L57: Too vague, please clarify whether this refers to ridging studies in general or specifically to keel measurements.

We referred to ice draft studies in general. This will be clarified in the revised text.

L74: Clarify whether the quoted ULS footprint diameter refers to the nominal instrument depth or varies with actual mooring depth.

We thank the Reviewer for pointing this out. The quoted footprint diameter refers to the nominal instrument depth stated in the manuscript (50 m), and the actual footprint varies with the instantaneous depth of the instrument. Given the nominal beamwidth of 1.8°, the geometric footprint can readily be calculated for any given instrument depth.

Analysis of the auxiliary mooring data used during processing indicates that, under normal conditions, the instrument depth remains relatively stable and generally within approximately 1 m of the mean deployment depth. The mean deployment depth can vary somewhat between deployments, and occasional periods of mooring pull-down, particularly at F11 and F12, result in substantially greater instrument depths. These events are relatively infrequent, however, and the nominal depth provides a representative value for describing the typical footprint.

We have revised the manuscript to make this distinction explicit and to clarify that the quoted footprint is a nominal geometric footprint rather than a uniformly sampled area. The revised text also notes that the acoustic return depends on the spatial distribution and scattering strength of the ice underside within the acoustic beam and therefore does not necessarily represent the spatial mean draft over the nominal footprint. Please see below;

“The IPS5 employs a narrow acoustic beam with a nominal full beamwidth of 1.8 (degree). For an IPS5 operating at a nominal depth of 50 m, the footprint of the sonar beam has a diameter of about 1.8 m. However, the nominal geometric footprint should not be interpreted as a uniformly sampled area. The acoustic return depends on the spatial distribution and scattering strength of the ice underside within the acoustic beam and therefore does not necessarily represent the spatial mean draft over the nominal footprint (Melling1995, Melling1998)..... The actual depth of the instruments might vary though, both due to slightly different settings between different deployments, and during particular season in response to the variable ocean drag”

L87: If ASL is a company, this detail may be unnecessary. Focus instead on how the processing is done and how the data are provided.

We note that related, though more specific, request for additional information on the data-processing procedures was also raised by Reviewer 2, particularly regarding the conversion of the time series of ice draft to spatial series. We have therefore expanded the description of this processing step in the revised manuscript.

Regarding the more detailed technical aspects of the raw-data processing, we have retained the information that is most relevant for understanding the data and the subsequent analysis, while avoiding an extensive description of instrument-specific processing procedures that would substantially increase the length of the Methods section. The processing methodology is described in detail in the established methodological literature, including Melling et al. (1995) and Fissel et al. (2008). A particularly detailed and publicly available example of the processing procedures is also provided by Mudge et al. (2015, https://www.nodc.noaa.gov/archive/arc0111/0164964/1.1/data/0-data/Chukchi_IndustryMoorings_reports/reports_public/2013-14/Shell/ASL_Report_PR-862_Chukchi_Sea_Shell.pdf), which we cite for readers seeking further technical information.

We therefore consider the revised description, together with these references, sufficient to document the processing procedures relevant to the present analysis while keeping the manuscript focused on the scientific objectives of the study.

L89: Consider reporting ridge numbers per day or per hour, which may be more informative.

We could not link this comment with the actual line number indicated. The text in Line 89 states

“The sampling rate of the instruments was set to 2 s, corresponding to ca. 1.3×10^6 ice draft observations per month.”

L91: A citation is needed; the stated value seems quite low to me.

Thanks for pointing this out. We will specifically cite Spreen et al., (2020, doi 10.1029/2019JC016039), who shows two years of ADCP-based monthly southward sea ice drift speed at F11 and F12 to vary within 5-30 km/day (up to 0.7 knot on average) with 1-sigma uncertainty varying between -5 to 45 km/day (up to 1 knot). One should note also that F11 and F12 moorings are located in the core of the East Greenland Current where generally higher ice drift velocities are observed.

L95-100: The discussion of draft uncertainty conflates instrument precision and interpretive uncertainty. Clarify that ~10 cm represents an estimated total uncertainty, including both random and systematic components.

We will clarify this referring to a discussion on this subject from Melling et al, (1995 doi:10.1175/1520-0426(1995)012<0589:MOTUTO>2.0.CO;2).

L123-129: Could the same effect be achieved by simply lowering the threshold?

Not exactly, please see our response to a similar comment on the application of adaptive approaches. Actually, for the revised manuscript we have conducted a suite of sensitivity experiments to see the manifestation of the approach we used to changes in major parameters, such as the minimal length of level ice segment and the maximum thickness of level ice. A more detailed response can be found in our letter of response to R2.

L200-205: When comparing level-ice fractions to historical submarine studies, briefly note differences in season or sampling that limit direct comparison.

Thanks, we will note this limitation in the revised text.

L272: “This analysis” is not an effective sentence opening, please rephrase.

Thanks for pointing this out, we will revise it.

With regards

Dmitry V. Divine on behalf of the coauthors.

References:

Petty, A. A., M. C. Tsamados, N. T. Kurtz, S. L. Farrell, T. Newman, J. P. Harbeck, D. L. Feltham, and J. A. Richter-Menge (2016), Characterizing Arctic sea ice topography using high-resolution IceBridge data, *The Cryosphere*, 10 (3), 1161–1179, doi:10.5194/tc-10-1161-2016.

Wadhams, P. and Horne, R. J.: An analysis of ice profiles obtained by submarine sonar in the Beaufort Sea, *J. Glaciol.*, 25, 401–424, 1980.

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