

First response to Reviewer 2 to the Review of "Sea-ice ridges are a major component of Arctic sea-ice export through the Fram Strait" (EGUsphere-2025-5511)

We would like to thank the reviewer for the generally positive assessment of our work and for the constructive criticism provided. We appreciate the reviewer's suggestions on how to improve the manuscript and the additional aspects that could potentially be addressed or integrated into the study.

At the same time, we would like to note that implementing all of the suggestions made by the reviewer would not be feasible within the scope and format of a single scientific article. The manuscript is already substantial in length, and addressing all of the proposed topics in one contribution would risk making the paper simply too long and diluting its core content and message. We also acknowledge that the reviewer states in point 8 that addressing only one or two of the suggested aspects would be sufficient for the revision. This is the approach we plan to follow, in part since the initial draft already included, as we find, sufficient novelty based on the two reviews, and the specific changes we propose are outlined in our responses below.

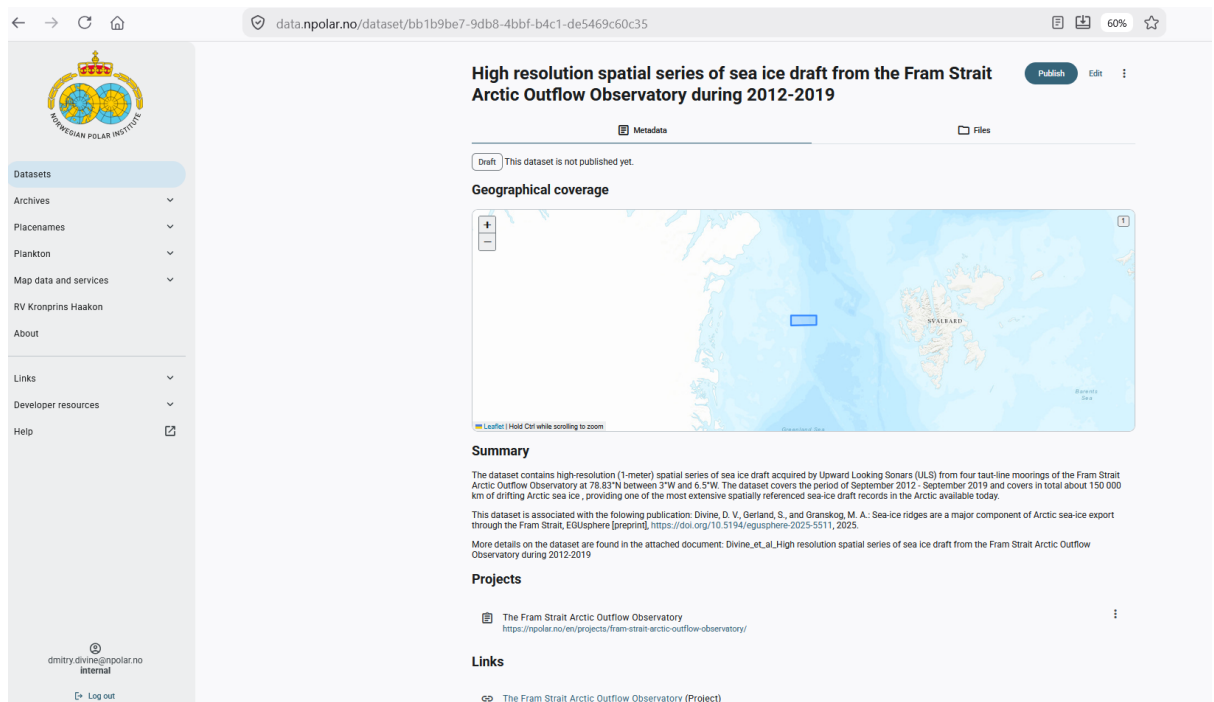
In the response below, we provide our preliminary responses to the reviewer's comments and outline the main revisions we plan to make to the manuscript. Here, we focus primarily on the major comments and provide detailed responses to these, while some of the more specific comments, when of a technical nature and/or related to the major comments, will be addressed in the final response upon submission of the revised manuscript.

Our responses to the reviewer's comments are shown in blue.

Major comments

1. **Data availability.** The data availability statement currently reads only "The data for the study will be made publicly available by the time of its publication." Given how much of the paper's value lies in the dataset itself (a unique, spatially-referenced, decade-scale ridge climatology), I consider public archiving of the processed draft/ridge dataset (and ideally the ridge detection code) a precondition for publication, not an optional add-on. Please provide a DOI/repository link, or at minimum a firm commitment and timeline, before this goes further in review.

The authors can guarantee the timely publication of the complete sea-ice draft dataset used in this study. We consider this commitment sufficient to clarify the data availability, should the "Data availability statement" provided in the original submission have appeared unconvincing for the Reviewer. The dataset is now finalized and ready for publication, and is currently awaiting release through our data repository at data.npolar.no, as illustrated in the screenshot below. The DOI for this data publication is already reserved: [10.21334/NPOLAR.2025.BB1B9BE7](https://doi.org/10.21334/NPOLAR.2025.BB1B9BE7). It will become active/public once the manuscript is accepted to not to jeopardize its publication. We will provide the link to the dataset in the data availability statement in the revised version of the manuscript. The categorized draft series will be presented in a separate dataset publication (in preparation).

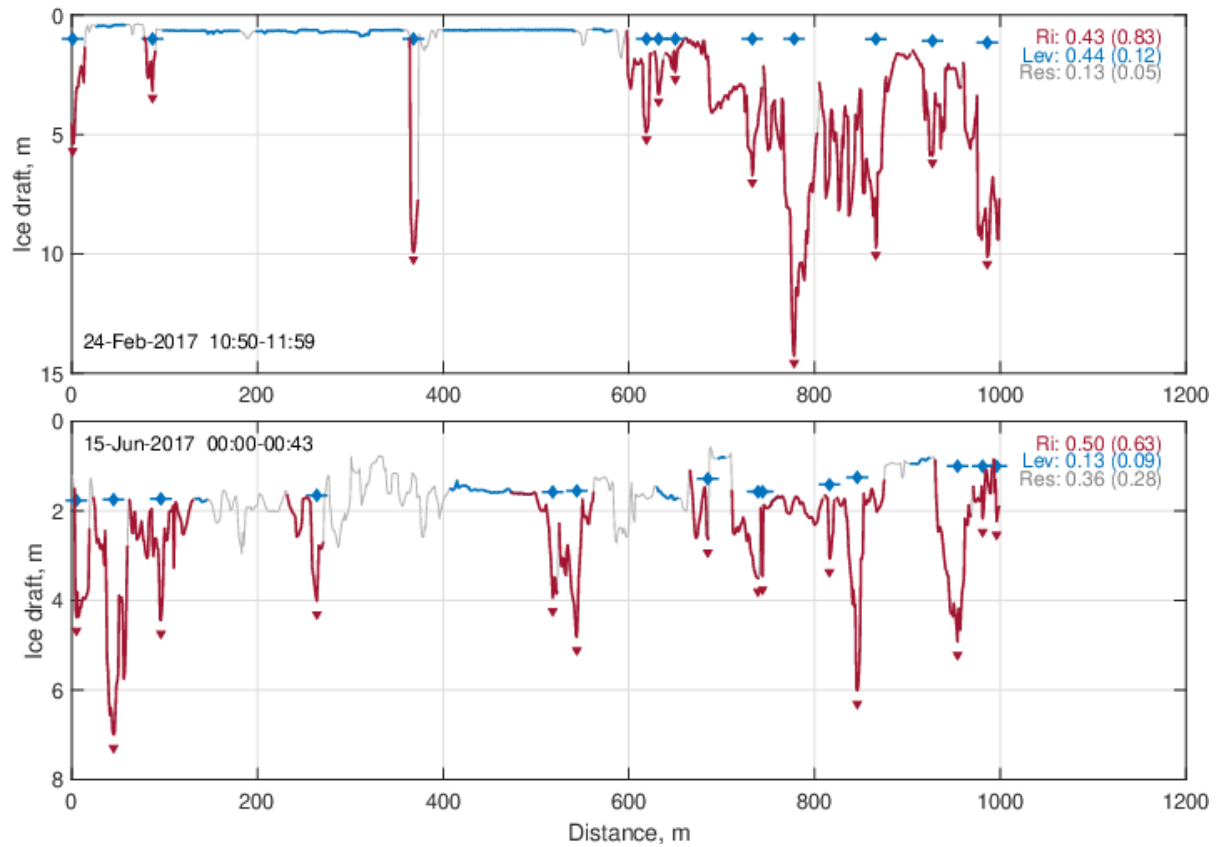


2. **Definition and treatment of the "mixed/other" ice category.** The three-way classification (level ice / ridges / mixed) leaves a residual "mixed" class that is explicitly acknowledged to include shallow rubble fields and possibly dynamically-formed ice that "could not be associated with the other two categories" (P6L174-176), and the authors themselves note in the Conclusions that "the contribution from deformed ice might be even larger" because of this (P13L389-391). Given that this residual class appears to make up a substantial share of the record (see Fig. 3, "Other" category), and that the paper's headline volumetric numbers are all conditional on how this class is treated, I think this deserves much more than a passing acknowledgement. I would like to see: (a) a quantitative breakdown of what fraction of the "mixed" class is level-ice-like vs. clearly deformed but undetected as a distinct ridge; (b) a sensitivity analysis showing how the reported ridge density/areal/volume fractions would change under different treatments of this class (e.g., a bounding scenario where all "mixed" ice is deformed); and (c) example transects (see point 3) showing what "mixed" ice actually looks like in the draft profiles, so a reader can judge for themselves whether the classification is conservative or generous.

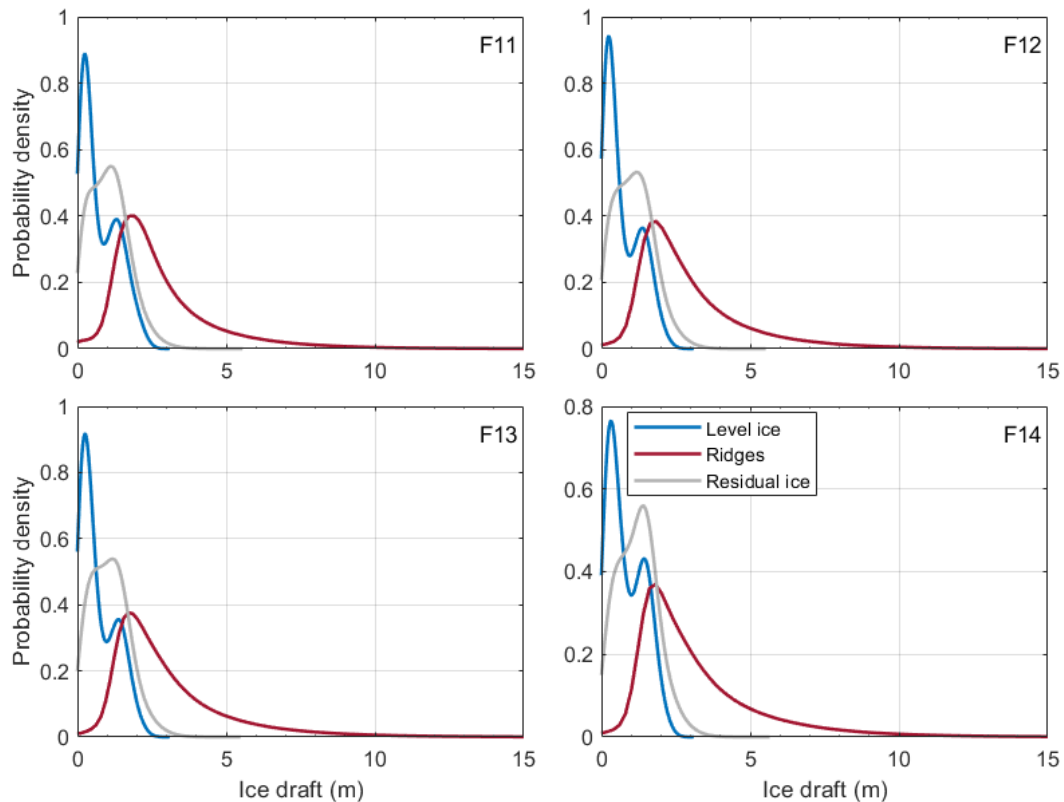
Indeed, we fully agree with the Reviewer that the mixed category was not sufficiently analyzed in the original submission. Although we conducted some checks and numerical experiments during the initial data analysis, these were not sufficiently presented in the original manuscript. Our primary motivation for limiting the presentation was to keep the manuscript concise. Since Reviewer 1 also raised concerns regarding this issue, we will extend the Methods and Results sections with additional analyses and discussion dedicated to this residual category. This will also include a suite of sensitivity experiments.

First of all, we would like to replace Figure S1, which currently shows examples of ridge profiles, with a more informative figure showing two examples of a 1-km-long draft series from the F13 mooring with our classification applied (see below). Note that the residual (mixed) category, highlighted in grey, is defined exactly as described in the text.

It primarily appears to consist of deformed ice that does not meet the criteria for a distinct ridge (keel).



Further, we will add a new figure in the main text showing the probability distribution of sea-ice draft for each mooring and each ice category. The proposed figure is presented below. We find this figure to be informative and, based on already conducted sensitivity experiments, supportive of the statements made earlier in the manuscript; an appropriate discussion of this figure will follow in the revision.



Another important aspect of this discussion is the definition of “level ice”. We acknowledge that we should have provided a clearer justification and explanation of how this definition was applied in our work. In the revised manuscript, we will state explicitly that there is no universally agreed definition of level ice for this type of record. Moreover, the appropriate definition may depend on the specific application and data type. In our analysis, we use definition “D2” from Wadhams and Horne (1980, <https://doi.org/10.3189/S0022143000015264>), which was subsequently adopted by Melling and Riedel (1995, <https://doi.org/10.1029/95JC00309>) for similar type of ULS data.

This is how we will present it in the revised text: “We classify a segment as level ice if its draft varies by no more than ± 0.25 m over a distance of 10 m or greater, following the relatively restrictive D2 criterion of Melling and Riedel (1995) and Wadhams and Horne (1980). We additionally impose an upper limit of 2.5 m on the local level-ice draft, which approximately corresponds to the limit of thermodynamic sea-ice growth Maykut, (1971). Only segments meeting these criteria were retained for further analysis. We note, however, that the definition of level ice is to some extent arbitrary and that the minimum segment length is application-dependent. For example, Rabenstein et al., (2010) adopt a substantially more restrictive minimum segment length of 100 m, partly motivated by the larger, approximately 50 m spatial footprint of the airborne EM-bird instrument used in their airborne sea-ice thickness surveys.”

Finally, in response to the comments from both reviewers we conducted several sensitivity experiments, mainly by varying the minimum segment length required to satisfy the D2 criterion for classification as level ice. We found that decreasing the minimum segment length naturally leads to a small increase in the fraction of level-ice coverage (and vice versa if one increases the minimum segment length), accompanied

by decreases in the fractions of the residual and ridge categories. However, we did not find any major deviations from the conclusions presented above, which increases our confidence in the robustness of our results. An additional though indirect support of our statement of residual (mixed) ice class being associated mainly with various deformed ice forms that could not be classified as ridges comes from the recent study of Eilers and Bradley (2026, <https://doi.org/10.1029/2025GL119003>). Their Figure 4 also shows a high (compared with ice ridges) count of minor objects associated with deformed ice, so called “bummocks”. However, the format and scope of their presentation do not allow us to make a quantitative assessment of the corresponding fractions in terms of either feature counts or areal coverage. Therefore, we will also update the figures to include the total deformed-ice volume. We consider including the sensitivity results as a supplementary figure to demonstrate the robustness of our results to the level-ice definition applied.

3. **Show representative transects in the main text.** Figure S1 (referenced at P4L144) apparently shows example ridge/level-ice classifications, but this is relegated to the supplement. Given how central the classification scheme is to every downstream result, I think at least one figure of real draft transects — annotated with level ice, ridge, and “mixed/other” segments as actually assigned by the algorithm — belongs in the main text. This would also let readers visually assess artefacts such as the running-average smoothing (P4L135-138), the local threshold interpolation, and the fixed-vs-variable threshold comparison (currently Fig. S2).

Please see our response above under point #2. The example figure showing the output of the classification procedure will be added to the revised version of the manuscript.

4. **More technical detail on processing artefacts (bias, aliasing).** The paper states ADCP velocities were sampled at 20-minute intervals and used in a “double-weighted double-quadratic interpolation scheme” to produce a spatially uniform 1 m series (P4L104-106), but given that individual ridges can be only a few metres wide, I would like more discussion of how velocity variability within a 20-minute window (particularly during high-drift-speed or highly variable-drift events) could alias into the reconstructed spatial series — e.g., smearing or mis-locating ridge features, or biasing the derived ridge width/spacing statistics. Similarly, the draft accuracy discussion (P4L94-100) gives an overall accuracy of ~0.1 m but does not discuss whether errors could be systematically different for ridge keels vs. level ice (e.g., due to beam-footprint or tilt effects at steep slopes), which would matter for the reported keel-depth PDFs.

This comment addresses a broad range of technical and important aspects associated with acoustic profiling of ice-underside topography. It comprises two separate questions that are closely related to broader discussions of these issues in earlier literature, particularly during the period when the methodology and instrumentation for sea-ice draft profiling were developed.

Firstly, we note that the sampling resolution and modes of operation of ADCP instruments have been developed iteratively over several decades, taking into account both the capabilities of the available instrumentation and the specific requirements of different measurement sites. We also consider that the data-processing procedures applied by ASL Environmental Sciences Inc. to the dataset used in this study follow

established processing practices developed through decades of experience with this type of data.

The two major questions raised by the reviewer are:

Q1: Effects of converting the time series to a spatial series using ADCP-derived ice velocity as well as effects of variability in ice velocity at shorter (sub-hour) timescales.

Q2: Potential differences in sonar bias between keels and level ice.

Q1. The conversion of an irregularly sampled spatial series to a regular distance grid using double-quadratic interpolation is documented in the ice-profiling sonar literature by Melling and Riedel (2004, https://nsidc.org/sites/default/files/documents/technical-reference/draft_movement_beaufort_2004.pdf), with the subsequent double-weighted double-quadratic formulation documented by ASL (2011, IPS processing toolbox) and further evaluated by Ross et al. (2012). Ross et al. (2012, <https://doi.org/10.4043/24601-MS>) compared three different interpolation schemes and investigated their effects on the resulting ridge-detection statistics. Their results indicate that, at a spatial resolution of approximately 1 m, the different interpolation approaches produce similar results, while the double-weighted double-quadratic approach performs particularly well at higher drift speeds. Melling and Riedel (2004) similarly states that “a double quadratic interpolation method provides the best results for the very rough topography of pack ice.”

In the present study, the 20-min ADCP observations are used to derive the cumulative ice displacement, which is subsequently combined with the high-frequency ice-draft observations to reconstruct the spatial series. We agree that rapid changes in ice velocity between successive ADCP observations could introduce uncertainty into the spatial coordinate assigned to individual draft observations. This uncertainty would primarily affect the inferred position of individual features and, potentially, the estimated width and spacing of narrow ridge features, rather than the temporal resolution of the original ice-draft observations.

A quantitative assessment of this effect would require higher-frequency, coincident measurements of sea-ice drift velocity, on timescales of seconds to minutes, covering a sufficiently broad range of drift conditions. Such measurements are not available for the present study. Lower-frequency independent observations of sea ice drift, such as from buoy or remote-sensing measurements, would likewise provide limited information on the short-timescale velocity variability relevant to this particular question. An alternative approach would be to investigate the effect through dedicated numerical or synthetic experiments using prescribed ice-draft profiles and assumptions about short-scale variability in ice velocity. However, such an assessment should also take into account that the nominal geometric footprint of the IPS instrument does not correspond to a uniformly weighted sampling 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.

We therefore acknowledge that the reconstructed spatial coordinate cannot be considered accurate at the centimeter scale, particularly under rapidly varying drift conditions. At the same time, we consider that the relevant question for the present

study is whether such positional uncertainty is large enough to materially affect the derived ridge statistics at the approximately 1-m spatial resolution used in the analysis. Based on the established interpolation methodology and the available literature, we consider the approach appropriate for the objectives of the present study. We will nevertheless clarify this limitation in the revised manuscript and discuss its potential implications for ridge-width and spacing statistics also referring to the studies discussed above.

Q2. We also agree that the uncertainty in acoustic draft measurements may not be uniform between level ice and ridge keels. The nature of the sounding acoustic beam with its finite acoustic footprint and off-axis returns from steep or strongly deformed ice surfaces can result in a positive bias in the draft attributed to the beam axis, particularly when strong returns from an off-axis keel exceed the detection threshold. A good discussion on this subject also referring to/summarizing previous studies is found in e.g. Vinje (1998, <https://doi.org/10.1029/97JC03360>, see Appendix: footprint error) and later in Behrendt et al., (2013), “10. Systematic error” there (doi:10.5194/essd-5-209-2013). A discussion on elaboration of the proper design /configuration of the sonars for ice profiling goes back to the studies of e.g. Melling et al., 1998 ([https://doi.org/10.1175/1520-0426\(1998\)015<1023:SSFSIA>2.0.CO;2](https://doi.org/10.1175/1520-0426(1998)015<1023:SSFSIA>2.0.CO;2)) who points that narrowing an acoustic beam and reducing sidelobes tends to reduce such bias. Vinje et al., (1998) acknowledge a positive bias of up to 10-25 cm due to the “footprint error” which is expected to be more pronounced for deformed ice. We note that the IPS instrument used here was specifically designed to minimize this effect through a narrow acoustic beam and small footprint, and instrument tilt is measured and accounted for in the standard processing. Nevertheless, we agree that a residual topography-dependent bias cannot be excluded completely but its magnitude appears insignificant for us on the scale of an individual ridge. We will therefore add a brief discussion of this issue to the revised manuscript, refer to previous studies as listed above, and clarify that the reported keel-depth distributions should be interpreted in light of these instrumental uncertainties.

5. **Deeper interpretation of the observed spatial/temporal patterns.** The east–west gradient in ridge density and the seasonal cycle in keel depth are both attributed qualitatively to differences in sea-ice source region / deformation history and to winter atmospheric forcing (e.g., P8L241-247, P10L297-307), citing Sumata et al. (2023) and Krumpen et al. (2025) rather than demonstrating the link with the authors' own data. Since ice motion products (e.g., Tschudi et al., already used for Fig. 1) and reanalysis wind fields are readily available, some quantitative back-trajectory or correlative analysis (e.g., relating monthly ridge density/keel depth to upstream drift speed, residence time, or wind stress) would move this from a plausible narrative to a demonstrated mechanism, and would substantially strengthen the paper's contribution beyond a descriptive climatology.

We agree with the Reviewer that these interpretations should be supported by a more in-depth analysis of the available data. We will therefore conduct the necessary analyses using, in particular, the recently released sea-ice age dataset by Korosov et al. (2026, doi: 10.21343/sqvr-rq75), and will revise the corresponding statements in the manuscript as appropriate based on the emerging results.

6. **Closer connection to ice–ocean/ice–atmosphere drag parameterization needs.** The introduction motivates the study partly by its relevance to drag parameterizations (P2L49-51, citing Tsamados et al., 2014), but the results section does not return to this explicitly. Tsamados et al. (2014, and its supplementary material) identify specific unresolved parameters that limit current parameterizations — keel/sail spacing, keel-face angle or slope distribution, and macroporosity, among others. This paper already derives ridge density (i.e., spacing), keel-depth PDFs, and makes explicit macroporosity assumptions (P11L337-346); it would strengthen the paper considerably to explicitly report these in the form needed by drag parameterization schemes, and to discuss what is still missing (e.g., keel angle/shape, which cannot be derived from 1-D profiles) rather than leaving this only implicit in the ridge-orientation caveat (P4L145-147, P11L324-327).

We appreciate the Reviewer's suggestion to make the connection to ice–ocean and ice–atmosphere drag parameterizations more explicit. As noted in our response to the earlier comments, we would, however, like to maintain a balance between the scientific scope of the manuscript and the size of the presentation. A comprehensive assessment of the implications of the derived ridge statistics for drag parameterizations would constitute a study on its own, particularly if the effects of the different ridge parameters were to be evaluated quantitatively within an actual drag formulation. And we feel that even if we only presents a climatology of relevant parameters, this data set carries such novelty that it stands on its own.

The ridge-detection procedure used in this study in fact provides a range of ridge metrics, besides ridge density/spacing or keel-depth distributions already presented in the original submission, but also other relevant geometrical characteristics. These quantities were derived for the full dataset, but only a subset is presented in the manuscript in order to keep the presentation focused and reasonably concise. We therefore do not consider that adding further ridge metrics alone would substantially strengthen the present study without also undertaking the corresponding drag calculations and assessing their sensitivity to the different parameters.

The use of the derived ridge statistics for explicit ice–ocean drag calculations is currently being investigated in a separate study, led by an early-career researcher. We consider that a dedicated treatment of this question, including the quantitative impact of the different ridge parameters on the calculated drag, is more appropriate in that context, where it can form the central focus of the analysis. We agree that this limitation deserves more explicit discussion, particularly because apparent ridge width and other individual geometrical properties depend on the angle at which a ridge is intersected by the sonar transect.

The following discussion refers to Reviewers comment that such metrics as keel angle/shape cannot be derived from 1-D profiles. Associated specific comments are found below (P4L145-147, P11L324-327).

The authors are aware that this is not a unique limitation only specific to the present dataset, but a general issue associated with deriving three (or “two” for area-average metrics)-dimensional properties of elongated structures from one-dimensional profiles. See for example [Key&Peckham,\(1991,doi:10.1029/91JC01843\)](#) where a similar discussion emerges for leads in ice pack. The effect of ridge orientation on apparent

ridge width has previously been discussed, for example, by Davis and Wadhams (1995, 10.1029/95JC00007). In that study, ULS transects in the region of Fram Strait and northward (see their Fig.1) were combined with side-scan sonar observations and satellite imagery to obtain information on ridge orientation and to account for its effect on the inferred ridge widths. The authors also demonstrate that ridge orientation may exhibit preferred directions in individual regions which were about 50 km size or less, while noting substantial spatial variability. Thus, the assumption of a particular orientation distribution cannot necessarily be generalized from a limited number of observations to an extended region or to a multi-year dataset.

Amundrud et al. (2006, 10.1029/2005JC002895) provide another relevant example in which a population-level correction is derived under an assumption of randomly oriented ridges. Under such an assumption and defining θ as the angle between the transect and the direction perpendicular to the ridge axis, the observed cross-sectional width is related to the true width through $W_{\text{true}} = W_{\text{obs}} \cos(\theta)$. For a uniform orientation distribution over a half-circle, the corresponding population-average projection factor is $2/\pi \approx 0.64$. Such a correction can therefore be applied statistically to a population under the stated orientation assumption but cannot be applied to individual ridges without knowledge of their orientation. Other general metrics derived from ridge cross-sections can be corrected too in a similar fashion, yet again in the average sense. In the absence of independent information on ridge orientation, we therefore do not consider it justified to apply such a correction to the individual ridge metrics derived from our ULS profiles, and we will explicitly state this in the manuscript.

More broadly, the problem is related to stereology and stochastic geometry, where lower-dimensional observations are used to infer properties of higher-dimensional structures. A classical stereology (Delesse Principle) demonstrates that under appropriate sampling conditions a phase fraction measured on a two-dimensional section can provide an unbiased estimate of its three-dimensional volume or two-dimensional areal fractions. However, the theoretical variance of the components would be strongly associated with anisotropic properties of ice cover. We note that a variability in ice drift trajectories may partly remedy the possible directional anisotropy in sea ice cover enabling sampling in different directions from the main axis (of axes) of sea ice properties.

We therefore consider the orientation issue primarily as a limitation on the interpretation of individual ridge geometrical properties, rather than as evidence for a systematic bias in the relative areal or volumetric fractions of the ice categories derived from the profile lengths/cross sections. The same should apply to such metrics as ridge density (number of identified ridge intersections per unit length) and thus spacing. We will clarify this point in the revised manuscript and discuss the implications for the use of ridge density as a parameter relevant to drag parameterizations.

To conclude, a more complete assessment of the effects of ridge orientation and anisotropy would require additional observations or dedicated numerical experiments. Possible approaches include

- (1) block bootstrap experiments using monthly transects while explicitly accounting for spatial anisotropy;

(2) exploiting periods with substantially different ice-drift directions to obtain quasi-independent sampling directions;

(3) generating synthetic three-dimensional ridge fields with prescribed geometrical and orientation properties and sampling these fields with one-dimensional transects.

These approaches could provide a quantitative assessment of the uncertainty introduced by the reduction from three-dimensional ridge structures to one-dimensional observations but would constitute a methodological study beyond the scope of the present manuscript.

7. **Sail-keel comparison.** The manuscript excludes ridge sails from the volumetric calculations and instead relies on a literature-derived assumption that sails contribute "not more than 10% of the total ice ridge volume or mass" (P11L354-357). I appreciate that co-located sail data are not available at these moorings and that this is a genuinely hard problem, but I would still like to see this discussed more critically — e.g., how sensitive would the total ridged-ice volume/mass budget be if the true sail fraction were, say, 15–20% instead of 10%? Even a short sensitivity discussion, or explicit flagging of this as a priority for future co-located (e.g., altimetry) validation, would be useful.

The Reviewer is correct that sails were excluded from the analysis. This was largely based on several assumptions concerning other factors that we considered to have a more significant influence on the overall ice-volume budget, which led us to consider ridge sails as relatively minor contributors. We acknowledge that this assumption warrants further consideration and will address it in the revised manuscript. In particular, we will extend the discussion using estimates of sail-to-keel area from the following very relevant recent publication by Guzenko et al., (2026): <https://doi.org/10.1016/j.joes.2026.03.004> , which was published after the original submission of our manuscript.

Guzenko et al., (2026) presents a detailed analysis of the morphometric characteristics of 155 ice ridges and stamukhas (grounded pressure ridges) measured in situ (i.e. using direct methods) during 2013–2017 in the Kara, Laptev, and East Siberian seas. **Table 2** in Guzenko et al., (2026) provides along other ridge statistics average estimates of sail and keel areas, together with the average sea-ice density of ice ridges. These values can be used to estimate the relative contributions of sail and keel ice to the total ridge-ice volume, and thereby assess the contribution of sails to the overall sea-ice ridge volume budget. In general such data are very sparse, as surely the reviewer knows, and a discussion beyond this would be very speculative and out of scope for this paper.

Region-year	Sail area (m ²)	Keel area (m ²)	A_s / A_k	A_k / A_s
I–2013	31	248	0.125	8.00
I–2014	24	221	0.109	9.21
I–2015	22	199	0.111	9.05
II–2014	34	258	0.132	7.59

Region-year	Sail area (m ²)	Keel area (m ²)	A_s/A_k	A_k/A_s
II–2015	63	536	0.118	8.51
III–2014	20	197	0.102	9.85
III–2015	22	204	0.108	9.27
IV–2014	29	252	0.115	8.69
IV–2015	37	310	0.119	8.38
IV–2015 MYR	128	1031	0.124	8.05
V–2017 FYR	19	159	0.119	8.37
V–2017 FYS	129	293	0.440	2.27

The table above is based on Guzenko et al., (2026) and indicates a typical keel-to-sail cross-sectional area ratio (A_k/A_s) of about 8, which would imply a somewhat higher estimate of the ice volume fraction contained in the sail than the 10% assumed in our original submission. Notably, the ratio is much lower (2.27) for the final entry, which represents stamukhas. For grounded ridges, the sail height-to-keel depth ratio may be strongly influenced by the water depth at the grounding location, while the role of hydrostatic balance in determining the sail-to-keel geometry is reduced. In the revised paper, the contribution of the sail to the volume budget will be estimated in a similar manner to that used to account for ridge porosity.

8. **Overall, the results read as largely descriptive.** Much of Section 3 presents time series and comparisons to prior literature without much synthesis beyond "our values are broadly consistent with study X." I don't think every one of my other major comments needs to be addressed to move the paper forward, but I would encourage the authors to select 1–2 of the above (my suggestions: the mixed-ice sensitivity analysis, and a more quantitative treatment of drivers) to elevate the paper from a valuable descriptive climatology to one that also demonstrates mechanism.

We appreciate the Reviewer's suggestion to strengthen the synthesis and interpretation of the results. At the same time, we do not fully agree with the characterization of the results as to be largely descriptive. The objectives formulated in the title and Introduction are, in our view, addressed through the analysis and interpretation of the results. This is actually supported by Reviewer 1 who identifies below in specific comments several of the results and statements in the manuscript as important, and even if it is considered only a climatology, such data and analysis make it an important contribution. Nevertheless, we agree that the synthesis of these results can be made more explicit. We will therefore incorporate additional analysis and discussion, in particular in line with major comments 2,4 and 7, as suggested by the Reviewer. These additions will strengthen the interpretation while keeping the study focused on its primary observational and climatological objectives.

Specific comments

P1L20 — I find that the claim ridges are "arguably the least studied component of Arctic sea ice" is a strong statement to open on; a supporting citation (or softening to something like "a relatively understudied component") would be useful, especially since the following sentences already cite several ridge-focused studies.

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 Riedel1996, 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>). “

P2L28-29 — I find that the oft-cited "~90% of sea-ice export" figure would benefit from a one-sentence caveat on the timescale/period it applies to (interannual variability in export fraction is itself substantial per Sumata et al. 2022, cited later).

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.”

P3L73-76 — I find that a compact site map inset with bathymetry contours (in addition to Figure 1) would help the reader relate the strong westward gradients reported throughout the results (e.g., water depth 2470 m at F11 vs 270 m at F14, Table 1) to the ridge statistics, since shelf proximity/water depth is a plausible additional driver of the west–east ridge gradient beyond source-region history.

We agree that bathymetry and proximity to the shelf or open water may provide additional relevant context for interpreting the observed west–east gradient in ridge characteristics. However, it is not entirely clear to us to what extent water depth itself could act as a direct driver of the observed ridge statistics (water depth >200m). In particular, the large difference in water depth between the eastern and western sites does not necessarily imply a corresponding difference in ridge formation processes, as the ridges observed at the moorings are primarily related to the regional sea-ice drift and deformation history.

Proximity to open water could potentially provide an additional mechanism, for example through differences in the seasonal exposure of the ice cover to warm water intrusions. However, in this case one might expect a stronger seasonal signal at the eastern sites. Such a difference is not evident in our time series. We therefore consider the role of bathymetry or proximity to open water as a possible contributing factor rather than an established direct driver of the observed west–east gradient.

P3L77-79 — I find that "minimal possible temporal gaps" is somewhat optimistic given Figure 2 shows quite substantial multi-month-to-year gaps at several sites (e.g., F13 2013–2015); I'd suggest rewording, or explicitly quantifying total data coverage as a percentage of the 7-year period per site.

This is explicitly provided in Table 1, see length of data records in days. The statement of "minimal possible temporal gaps" is now deleted from the text.

P4L94-100 — I find that the accuracy discussion would benefit from explicitly stating whether the ~0.1 m accuracy figure is expected to hold at ridge keels (steep slopes, beam-footprint/tilt effects) as well as over level ice, since this bears directly on the keel-depth PDF fits in Section 2.3.

Please see our response to major comment 4

P4L104-106 — I find that more detail is needed on the "double-weighted double-quadratic interpolation scheme": what is being weighted, and over what window? Given the 20-minute ADCP sampling vs. 2 s draft sampling, some discussion of potential aliasing of ridge features into the reconstructed 1 m spatial series (see Major Comment 4) would be valuable here.

We will extend the ADCP ice velocity data acquisition description for clarity, as well as the DWDQ procedure used.

"Ice velocity measurements were acquired using an ADCP (Teledyne RDI Acoustic Doppler Current Profiler operated in "Ice-Tracking Mode"), deployed on the moorings some 8 m below the ULSs. The ADCPs recorded ice velocities at 20-minute sample intervals. Each measurement was derived using the observed Doppler shifts measured by four individual acoustic beams, with each beam oriented at an angle of 20 degree from the vertical. The 20-minute ice-velocity estimate was obtained as an ensemble average of 42 acoustic pings. These measurements were used in a double-weighted double-quadratic interpolation scheme to generate a series of sea-ice draft with a spatially uniform increment of 1 m (see Mudge et al., 2015 for more details on the procedure, 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). This scheme was adopted to minimize positive bias in interpolated ice drafts in segments of the series characterized by high draft gradients. To better represent the ice draft at low ice speeds, the draft data were first interpolated to a spatial increment of 0.10 m and subsequently block averaged to 1.0 m.

A graphical depiction of the procedure and the accompanying equations are provided in the publicly available report by ASL Environmental Sciences (Mudge et al., 2015; Figure 3-6, which we will cite in the text for clarity. We do not consider it necessary to reproduce this figure in the manuscript, as the procedure is already well documented in the cited reference and the underlying interpolation equations are standard mathematical formulations.

P4L107-111 — I find that Table 1's "km of ice" and "days of ice" columns partially address this, but it would help to also show, per site, what fraction of days had ULS-only vs. combined ULS+ADCP data, since this determines representativeness of the "effective" statistics reported later.

[This will be added to Table 1.](#)

P4L118-122 — I find the description of the Rayleigh criterion clear, but a schematic (even reproducing a simplified version of the Ekeberg et al. 2015 figure referenced here) directly in the main text, alongside a real example transect (see Major Comment 3), would make the method much more accessible without requiring the reader to consult another paper.

[We thank the Reviewer for this suggestion. We agree that a graphical illustration can be useful for explaining the Rayleigh criterion. However, we do not consider it necessary to reproduce an additional schematic in the main text. The Rayleigh criterion is a well-established concept in physics, originating from optical/diffraction resolution applications, whereas the version used for sea-ice ridge detection is an adapted morphological criterion. General graphical depictions of the Rayleigh criterion are readily available in standard physics textbooks. In the present manuscript, as the Reviewer noted, the criterion is described explicitly and clearly, and we consider a representative real transect to be more informative for illustrating its application to our data. We will therefore retain the description of the criterion in the text and illustrate its application through the example transect discussed in Major Comment 3.](#)

P4L133-138 — I find that the specific choice of a 0.025 m^{-1} gradient threshold and a 3 m running-average smoothing window are adopted from Wadhams and Horne (1980) without discussion of whether they are appropriate for this dataset's 1 m sampling and modern (generally thinner, more variable) ice conditions. A brief sensitivity check (e.g., how ridge counts change under $\pm 50\%$ variation in the smoothing window) would strengthen confidence in the method.

[Please see our response to your major comment 2.](#)

P4L142-143 — I find that the choice of $h_{k,\min} = 2 \text{ m}$ "to filter out ice rubble" is a fairly consequential threshold (it directly bounds the population of "shallow ridges" analyzed throughout) and deserves brief justification beyond the single sentence given — e.g., was this value tuned against the data, or taken from prior literature?

[This is indeed a relevant question in the broader context of ridge classification. There is to our knowledge no universally accepted classification or categorization of sea-ice ridges based on specific dimensional thresholds, as, for example, they exist for icebergs or for level ice in the WMO Sea Ice Nomenclature. The choice of \$h_{k,\min} = 2 \text{ m}\$ in the present study was not tuned to the data, but was made as a practical criterion to exclude small-scale ice rubble while retaining the smaller deformed-ice features present in our observations.](#)

[In previous studies based on observations of generally thicker Arctic sea ice, including the studies by Wadhams and Melling cited above, ridge populations have often been characterized using substantially larger minimum keel depths, with 5 m being a commonly used threshold for identifying ridges. Applying such a threshold to present-day sea-ice conditions would, however, exclude a substantial fraction of the smaller deformed-ice features observed in our data. The 2-m threshold was therefore selected to retain these smaller ridges while excluding the shallowest features that are more likely to represent ice rubble or minor deformation.](#)

Interestingly, a similar, though somewhat stricter, threshold was applied in the recent study by Eilers and Bradley (2026; doi 10.1029/2025GL119003), which was not available at the time of preparing our original submission. They also use 2 m as the minimum keel depth for ridge identification, although their threshold is defined relative to the surrounding level-ice surface, rather than as an absolute keel draft as in our study. Features with relative keel depths between 0.5 and 2 m are classified as “bommocks”, which would correspond in our study more generally to deformed ice. Their approach therefore provides useful independent support for the order of magnitude of the threshold adopted here, although some differences in the number of minor ridges would be expected because of the different definitions and because we impose a minimum level-ice draft whereas Eilers and Bradley do not. We will clarify this rationale in the revised manuscript.

P4L145-147 — I find this an important and honest caveat (true keel widths cannot be inferred without ridge orientation), but as noted in Major Comment 6, I think its implications for the derived ridge-density/spacing statistics (which are used later as a proxy for model-relevant spacing parameters) should be discussed more explicitly rather than left as a single caveat sentence.

Please see our response to Major comment 6

P6L174-181 — I find this section is where the "mixed" residual category is defined, and as detailed in Major Comment 2, this deserves substantially more quantitative treatment given its apparent size and its acknowledged potential to include undetected deformed ice.

Please see our response to major comment 2.

P6L182-199 — I find the literature comparison of level-ice fractions useful, but quite long relative to its analytical payoff; a summary table (study, period, region, method, level-ice fraction) would let the reader absorb this comparison more efficiently than the current prose list.

We thank the Reviewer for this suggestion. We agree that the comparison of level-ice fractions can be presented more concisely and that a summary table would provide a more efficient overview of the previous estimates, including the study period, region, methodology, and reported level-ice fraction. We will therefore revise this part of the manuscript accordingly and reducing the length of the corresponding discussion.

P7L217-221 — I find this a strong, clearly supported point (fixed 2.5 m threshold rarely reached, so fixed-threshold ridge detection would undercount) — this is one of the better-substantiated claims in the paper and could be foregrounded more in the abstract/conclusions.

We thank the Reviewer for highlighting this point. The importance of shallow ridges is already reflected in the Abstract, where we state that they constitute a substantial fraction of the total ridge count. We agree, however, that this finding can be emphasized more explicitly. We will therefore modify the final sentence of the Abstract to highlight the implications of the substantial contribution of shallow ridges to the total ridge population. We will also emphasize this result more clearly in the Conclusions.

P8L224-227 — I find that the statement "the effective ridge density can be as high as 30 ridges per km" would benefit from context — is this a single extreme daily value, or a sustained condition? A brief note on the tail behaviour (e.g., what fraction of days exceed 20 ridges/km) would help.

We refer the Reviewer to Figure 6, where the empirical distributions of daily ridge density are presented together with the corresponding fitted lognormal distributions and their parameters. The referred value of up to 30 ridges km^{-1} refers to the upper tail of the observed daily ridge-density distribution rather than to a sustained mean condition, as is clear from Figure 6. The fitted distributions provide an explicit characterization of the tail behavior, and the fraction of days exceeding any given ridge-density threshold can be readily obtained from the corresponding cumulative distribution function.

P8L241-247 — I find this explanation of the westward increase in ridge density (attributed to source-region/deformation-history differences) plausible but not demonstrated with the authors' own data; see Major Comment 5 for a suggested quantitative approach (back-trajectories or correlation with available ice-motion/atmospheric products).

We will consider this point in more detail/conduct necessary analyses in the revised paper (see our response to major comment 5)

P9L257-264 — I find the comparison with Ekeberg et al. (2014)'s >5 m-keel counts convincing and a nice consistency check; I would encourage a similar quantitative cross-check for the shallow-ridge fraction, if any independent estimate exists, to validate that part of the method as well.

We thank the Reviewer for this suggestion. An interesting independent comparison is provided by Figure 4 in Eilers and Bradley (2026) we cited earlier in our responses. The figure shows an increasing number of ridges with decreasing keel draft, consistent with the strong contribution of shallow ridges observed in our results. The relationship shown in their figure is non-linear but the corresponding fit or underlying numerical data are not provided. It is therefore not possible to perform a straightforward quantitative comparison of the shallow-ridge fraction from the graphical information alone. We will nevertheless refer to this result in the revised manuscript as an independent qualitative consistency check of the observed increase in ridge occurrence toward smaller keel drafts.

P9L262-264 — I find that reporting the decline as "statistically significant only at F12" without also reporting the p-values or trend magnitudes (with confidence intervals) for the other three sites makes it hard to judge how close they came to significance; consider adding these to Table 2 or the text.

Thanks, we will extend Table 2 accordingly.

P9L280-282 — I find the speculation that the deepest keels (25–30 m) could be "small icebergs from the Greenland ice sheet" interesting and worth retaining, but since this bears on the tail of the reported keel-depth distribution, I would like to see even a minimal discussion of what criteria (if any) could in principle distinguish an iceberg fragment from a genuine pressure ridge keel in a 1-D draft profile (e.g., width, shape, isolation from surrounding ice), even if the authors ultimately conclude this cannot be done with the current data.

Icebergs/iceberg fragments can in fact be distinguished from pressure-ridge keels with some confidence from the ULS draft series based on their characteristic geometry and occurrence in the profile. A recent example of such an analysis is provided by Pliss et al. (2026, doi: 10.3390/jmse14171612), where individual iceberg-related features are identified and discussed in ULS draft data. We will refer to this study. However, given the primary focus of the present study and the insignificant contribution of these deepest features to the ridge statistics of

interest, we do not consider a more detailed analysis of potential iceberg fragments necessary here. We will therefore retain the observation as a possible interpretation of the deepest keels while making clear that these features do not materially affect the statistical results presented in the manuscript.

P10L297-307 — I find this discussion of atmosphere-driven seasonal ridge formation plausible but, again, presented as narrative rather than demonstrated; see Major Comment 5.

The seasonality of deformation rates in Arctic sea ice, with late fall (young-ice formation in formerly ice-free areas and increased atmospheric dynamics) and spring identified as periods of enhanced sea-ice deformation and ridge formation, has been addressed in a number of studies. Specifically, one can refer to Kwok (2006, [doi{10.1029/2005JC003246}](https://doi.org/10.1029/2005JC003246)). The authors are aware that the maximum in deformation rates does not necessarily correspond to the maximum in ridge occurrence, as the latter is a cumulative result of the deformation history.

As already mentioned in our response to Major Comment 5, we will conduct an analysis of sea-ice age variability, and hence the length of the deformation history, at the mooring locations in an attempt to assess whether the observed gradients in average ridge density and keel depth can be related to differences in sea-ice age and potentially to different source regions. We consider this analysis to be sufficient to address this aspect within the scope of the present study, while a more comprehensive attribution of ridge characteristics to deformation history and source regions would be better suited to a dedicated future study.

P10L308-314 — I find the link to the well-documented 2018 outflow minimum (Sumata et al., 2022) a nice, specific, testable example of interannual coherence across sites — this is one of the more compelling pieces of physical interpretation in the paper and could be a model for how the source-region/seasonal arguments elsewhere could be similarly substantiated.

We thank the Reviewer for this positive assessment. We agree that the 2018 outflow minimum provides a good example of this kind, and we consider this to be an important aspect of the physical interpretation presented in the manuscript.

At the same time, we would like to emphasize that a full-scale attribution of the observed spatial and temporal variability in ridge characteristics to, e.g., specific source-region, atmospheric, oceanic, or other processes would substantially extend beyond the scope of the present study, constituting a study in its own right. We therefore would like to limit ourselves to retaining the specific and well-supported examples of physical coherence presented in the manuscript, while leaving a more comprehensive attribution for future research.

P11L324-327 — I find the "anisotropy" assumption used to justify treating linear-profile areal/volume fractions as orientation-invariant reasonable as a first-order argument, but it would help to state explicitly what averaging length scale is assumed sufficient for this to hold, and whether this has been tested (e.g., via bootstrap over sub-segments of the record) rather than simply asserted.

Indeed, the authors base this argument on a first-order assumption. To our knowledge, there seems to be no universal length scale at which Arctic sea-ice properties become isotropic or statistically homogeneous. We have also not performed simulations specifically to test this assumption. We therefore prefer not to interpret the full transect length as a length scale implying statistical isotropy. Rather, the reported category fractions represent spatial averages over the complete sampled transect or over its monthly segments.

We will clarify this assumption in the revised manuscript and assess the sensitivity of the inferred fractions to the chosen averaging length by performing spatial block-bootstrap simulations on draft segments of different lengths to investigate the convergence of the estimated fractions. We note, however, that such an analysis cannot establish that the underlying ridge field is isotropic or determine its actual characteristic spatial scales. Rather, it can indicate the linear spatial scale at which the estimated category fractions become relatively insensitive to the choice of averaging length.

P11L337-346 — I find the treatment of macroporosity as an envelope (0–30%) a good, transparent way of expressing this uncertainty, and would suggest the same envelope approach be considered for the sail-fraction assumption (Major Comment 7) and possibly for the "mixed" ice class (Major Comment 2).

Thanks, we will modify Figure 10 in accordance with this suggestion.

P11L347-353 — I find the shallow-ridge volume contribution (~40% for keels <5 m, rising toward F11) one of the paper's most important and useful findings; I would suggest it (along with the P7L217-221 finding) be given more prominence in the abstract, since together they make the strongest case for the paper's central "shallow ridges matter" argument.

This comment is similar to the previous specific comment P7L217-221, it will be addressed in the revised version of the manuscript.

P11L354-357 — I find the 10% sail-volume assumption under-discussed relative to its role in the volume budget; see Major Comment 7.

This point will be addressed in the revised version of the manuscript. Please see our response to Major comment 7.

P12L358-361 — I find this paragraph (fractional coverage/volume decline, significant only at F11) would benefit from the same treatment suggested for P9L262-264 — report trend magnitudes/CIs for all four sites, not just flag which one is significant.

We will report trend magnitudes in Table 2 or directly on the figures. The actual practical format is yet to be decided.

P13L389-391 — I find this sentence ("the contribution from deformed ice might be even larger...") is really the crux of Major Comment 2, and I think it deserves to be a quantified sensitivity analysis in the Results rather than a single caveat sentence in the Conclusions.

This comment is in line with Major Comment 2 and the previous specific comment P11L337–346. We will address this point quantitatively by adding an additional line to Figure 10 showing the estimated ice volume when the contribution from the residual-ice category is also taken into account. This will allow us to quantify the potential additional contribution of deformed ice to the total ice volume and will make the implications of this uncertainty more explicit in the Results/Discussion.

Table 1 — I find the inclusion of both "days" and "effective days of ice" (weighted by concentration) very useful; it might help to also add a column for the fraction of the record with combined ULS+ADCP coverage (see P4L107-111 comment above).

This will be added to Table 1.

Table 2 — I find this table, summarizing per-site ridge count, coverage, volume, density, and keel depth, to be one of the most useful elements of the paper for other researchers (e.g., for model parameterization, Major Comment 6); I would suggest adding the shallow-ridge (<5 m) fraction and the assumed sail-volume contribution explicitly as additional columns here, consolidating scattered results into a single reference table.”

We will try to find the best practical solution to accommodate all of the detailed information requested by the Reviewers. Can be implemented as an additional table if Table 2 becomes overpopulated.

Figures 3, 5, 7, 9, 10 — I find these clear and effective, but note that none of them show an actual example draft transect with classified segments (Major Comment 3); I think one additional figure of this kind, in the main text, would substantially aid interpretability of everything that follows.

See our response to Major comment 2/3.

With regards

Dmitry V. Divine on behalf of the coauthors.