Review of

Sea Ice thickness distribution and ice keel characteristics in the Bothnian Bay based on high resolution ADCP measurements.

By: Björk et al.

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This manuscript examines a unique set of ice draft observations collected by a moored ADCP in the Bothnian Bay during winter 2023-2024. There have been few observations of ice thickness in this area, yet it is an area of winter shipping and proposed development of o shore wind farms, both of which require deeper insight into the ice pack. From the analysis the authors discriminate between the thickness of level ice that is largely the result of thermodynamic ice growth and the presence and thickness of keels which are the submerged portion of ice ridges that form through dynamic processes and are of particular concern to ships or structures that may interact with the ice pack. The authors further try to relate the observed draft with the optical satellite observations, though this last part of the analysis falls short of complete.

The analysis is sound, and I commend the authors for putting this together, but I do have several comments to help improve the structure of the paper and make it easier to interpret the key results. Generally, I'd encourage the authors to give more depth to their interpretation of the results and to discuss details presented in figures and tables, sometimes these are glossed over or not mentioned at all, leaving it to the reader to discern the key points. I encourage them to dive into the means and modes of thickness distributions and the proportion of ice above certain thresholds, support these results with references from this area and other parts of the Arctic where ADCP's have been used to characterize the ice pack and that will make the paper more impactful.

Overall, I think the manuscript is a great start to an in-depth analysis of an ice pack that has been understudied and has real world applications. While my suggested revisions are not overly major, they are more than minor. I would encourage the authors to revise the manuscript and resubmit, at which point I'd be more than happy to provide another review.

Major comments

We thank the reviewer for this thorough and constructive review. Our response to the review comments is written in italic.

1. My biggest comment has to do with the structure of the results, I believe they can be organized in a better way to make the paper clearer. I've outlined a new structure at the end of this comment but will walk through some details here. I would suggest merging the sea ice extent, air temperature, wind velocity and SSH in one panel figure at the start of the section. This would describe the overall sea ice season at the regional scale before diving into the details of the ADCP data. At several points when describing the sea ice season, you jump ahead and to discuss air temperature and winds which influence the local sea ice extent, as a result you refer to figures out of order. Put these together so the relationship is straightforward. Also, can you include wind direction in some way, perhaps monthly wind roses or seasonal wind roses against some type of climatology? The winds are cited as a source of new ice observed by the ADCP and the driver of dynamics against the shore, yet

it's not clear which direction they come from and then whether 2023-2024 was typical of other years.

We agree with this and will move the met data together with ice extent. We will however keep these figures separate since figure 3 includes information about other seasons. We will also move up most of the text from the discussion, regarding If the 2023-2024 season was abnormal or normal or not, to 4.1 (including the figure) since it actually fits in better here. When it comes to wind direction it is indeed included since we show north/south and east/west components in Fig. 4. We will also show wind roses for the years 2004, 2024 and 1995 - 2025 in supplementary material.

I suggest moving section 4.4 Echo Data to the methods. This is proof that the approach works during a specific period and not so much a result.

We agree and will move the echo data figure to methods.

See another comment below but I'd suggest moving Section 4.6 Comparison with Satellite Data to the Discussion as the results are quite tenuous.

We agree – and will move that part to the discussion.

In Section 4.1 I'd encourage you to add a panel showing the sea ice concentration near the ADCP, this would provide insight into the ice conditions and specifically the presence of open water near the ice pack which is influenced by winds and air temperature.

We think that this comment is based on the misunderstanding that the SEAICE_BAL_PHY_L4_MY_011_019 data set can be used to show detailed ice concentration features near the mooring. This is not the case, and we will add more text in Section 2.3 about why this is not the case. See also answer to comment on line 155.

New Results structure.

4.1 The 2023/2024 ice season and meteorological conditions.

4.2 Ice Velocity

4.3 Ice Draft Distribution

4.4 Level Ice

4.5 Ice Keel statistics

2. I'd suggest changing your approach to interpreting Figures 4 and 5 from looking for a relationship between surface roughness and the pixel intensity to just providing context on the ice pack that supports the observed seasonal change from the time series of sea ice extent, air temperature, winds and SSH given before. The two images give an example of a compact ice pack and an "open" ice pack later in the season. This can support the discussion in section 4.1 and provide context on the ice pack near the ADCP.

We will add some more text regarding the general difference (compact and open) between the two images. We think however that it is important to keep the text about horizontal structures of various scales as seen in the images, since the variability of ice draft from the ADCP is likely strongly connected to these structures.

3. Section 4.6 comparison with satellite data is quite tenuous and not given adequate analysis. At one point you say the “the variability in reflectance appears to correlate reasonably well with the measured ice draft” (Line 483-484) but then later you say that this qualitative comparison should be considered as a “feasibility assessment” and that the “sensitivity appears limited”. I completely understand the desired outcome, but the current level of analysis is inadequate. There is no obvious relationship from Figure 18 and some sort of statistical analysis should be done to support any conclusions presented. Perhaps, this section can be moved to the discussion section where you have more latitude to discuss this potential application and support it with citations from relevant works. I’ll also note that Figures 16 and 17 are very similar, I would suggest removing Figure 17.

Yes we agree and will move this to discussion and combine fig 16 and 17 to a figure with two panels.

4. In the introduction I think it is worth noting that ADCPs have been used elsewhere in the Arctic and Antarctic to characterize ice packs. For example, Humphrey Melling has several papers where moored acoustics were used to monitor the ice pack in the Beaufort Sea (Melling et 1995). Additionally, there are the JOIS Moorings in the Canada Basin (Krishfield et al., 2014), the Fram Strait moorings (Sumata et al., 2022 and several others), and others that have been deployed in Hudson Bay and Strait (Kirillov et al., 2020 and Babb et al., 2021), plus several other locations across the Arctic. I think noting this work has been done elsewhere would help build the context and importance of the manuscript. You could also then build in comparison to the results, for example highlighting just how thin the ice cover is in the Bothnian Bay.

We will refer to relevant references from other locations in the Arctic including the ones below plus some additional ones from the Beaufort Sea and Barents Sea.

• Melling et al., (1995) Measurements of the Underside Topography of sea ice by moored subsea sonar, Journal of atmospheric and oceanic technology, https://doi.org/10.1175/1520-0426(1995)012%3C0589:MOTUTO%3E2.0.CO;2

• Krishfield et al. (2014), Deterioration of perennial sea ice in the Beaufort Gyre from 2003 to 2012 and its impact on the oceanic freshwater cycle, J. Geophys. Res. Oceans, 119, doi:10.1002/2013JC008999.

• Sumata et al., (2022) Unprecedented decline of Arctic sea ice outflow in 2018, Nature Communications, https://doi.org/10.1038/s41467-022-29470-7

• Kirillov et al. (2020). Atmospheric forcing drives the winter sea ice thickness asymmetry of Hudson Bay. Journal of Geophysical Research: Oceans, 125, e2019JC015756. https://doi.org/10.1029/2019JC015756

• Babb et al. (2021). Sea ice dynamics in Hudson Strait and its impact on winter shipping operations. Journal of Geophysical Research: Oceans, 126, e2021JC018024. https://doi.org/10.1029/2021JC018024

Minor comments

Line 24: Replace “here” with “there”.

OK we fix that.

Line 42: “give” is not correct here. Revise.

We will change to “generate”.

Line 59: I think the influence of OWF’s will be very localized. Perhaps some ice directly in contact with the structures may become immobilized, but with winds and currents it seems like the ice will largely remain mobile around the structures with no great influence.

We don’t find any relevant literature about this - it appears to be work in progress. We will however adjust the text accordingly.

Line 83-88: This section needs to be expanded upon! What do these EM and old ADCP data show of the ice pack? I think some information needs to be given here then the data can be compared in the discussion section. Right now, all the details are saved for the discussion section which is too late.

We agree and will add more information about the EM data:

“The EM data show, in addition to thermodynamically formed ice, also a high percentage (50-70 %) of thicker deformed ice (typically as pressure ridges) which is formed by dynamical processes.”

…(Björk et al., 2008). “This data show also a considerably amount of deformed ice between 30-60 % depending on the month with the largest value in March.”

Line 89-102: Please add some references to datasets or other papers that discuss the limitations of these various satellites. Also, you mention radar altimeters (CryoSat-2) and its limited spatial footprint, but ICESat-2, a laser altimeter, offers much higher resolution that may be suitable for the Bothnian Bay.

• Macdonald et al (2024) compare RCM and surface roughness in the Canadian Arctic https://doi.org/10.1029/2023GL107261

• Duncan, K., & Farrell, S. L. (2022). Determining variability in Arctic sea ice pressure ridge topography with ICESat-2. Geophysical Research Letters, 49, e2022GL100272. https://doi.org/10.1029/2022GL100272

We thank the reviewer for pointing out the capabilities of ICESat-2 and for providing the suggested references. We agree that ICESat-2 offers substantially higher spatial resolution than conventional radar altimeters and can resolve sea-ice ridge topography at scales relevant to the present study. We will revise the manuscript accordingly and add the suggested reference. In the revised text, we emphasize that the principal limitation of satellite altimetry in the context of seasonal ice monitoring is not only spatial resolution but also the sparse temporal sampling associated with narrow-track observations. We also note that satellite altimeters measure ice freeboard rather than draft directly, whereas the ADCP provides continuous observations of the subsurface keel geometry throughout the ice season.

Line 100 and throughout: It’s obvious that satellites don’t observe the ice draft, but they do observe ridge sails which correspond to keels. I think this relationship can be outlined here and supported by some references on the relative size of keels and sails and the aspect ratio of ridges.

Yes, we agree that basic information about ridges was missing and will add text about ridge structures in the introduction based on field data from the Baltic (Leppäranta and Hakala, 1992).

We agree also that satellite sensors may observe pressure-ridge sails and other surface manifestations of ice deformation, which are physically related to the underlying keel structure. We will revise the text to clarify that satellite observations do not measure ice draft directly, but may provide indirect information on deformed ice through the observation of ridge sails, freeboard, surface roughness, and deformation features. We will also add discussion of the statistical relationship between sail height and keel depth, citing Timco and Burden (1997). At the same time, we note that considerable variability in ridge morphology and sail-to-keel ratios makes the quantitative estimation of keel draft from satellite observations challenging. This challenge provides part of the motivation for the present study, which explores the extent to which high-resolution satellite observations can be related to in-situ measurements of ice draft.

Figure 1 and 2: Both figures show bathymetry yet bathymetry is rarely referred to in the paper and has limited influence on ice thickness. I'd suggest removing Figure 2 and perhaps instead of bathymetry in Figure 1b you can put in a Satellite image from either Figure 4 or 5 to give context on the regional ice cover. This would save space and tighten up the paper.

We will move fig 2 to supplementary material but will keep the bathymetry since Falkensgrund is rather shallow (minimum 10 m) and deep keels can actually reach to bottom and affect the ice motion above the shoal. We will mention this in 4.3 about the "ice velocity observations".

Line 129: I'd suggest saying "ice bottom" rather than "ice keel" as not all interfaces that the ADCP observes are keels.

Yes - we will change to "bottom" instead.

Line 155: Please expand. What is the source, is this Passive microwave, what is the temporal resolution? What is the uncertainty and how does that a ect the observed seasonal cycle? Also, is there a way to look at sea ice concentration rather than just sea ice extent in the broader Bothnian Bay? And back to a previous comment but the specific sea ice concentration at the ADCP location.

We have added additional text to better describe the SEAICE_BAL_PHY_L4_MY_011_019 dataset and its limitations. Our focus is on thick ice keels and not ice concentration so we don't think it is appropriate with additional analysis!

Line 291: Replace "fever" with "fewer".

OK!

Line 292 and throughout: Check that units are separated by a space.

OK!

Section 4.3: I'd suggest that instead of scaling the winds by 0.03 you use two y-axes, one with ice drift speed and one with Wind Speed. From that you can calculate an average

speed reduction factor that you can then compare to the typical values of 2-3%. As it is now it's tough to interpret wind speeds, and once again there is no direction consideration.

The purpose to present data in this way was to show that there were periods with typically free drift in contrast to periods with significant ice stresses. Calculating an average speed factor appears not relevant since there are many occasions when the ice is stationary in spite of strong winds. It is of course possible to analyse the wind speed factor more in detail and also include the turning angle, but it must be kept in mind that the main focus of this paper is to present data of basic sea ice characteristics (draft, keels…) and not ice dynamics. A more thorough analysis of the ice dynamics (as in Björk 2006) would lengthen the paper considerably and be out of the present scope. An analysis of the dynamics could well be the subject for a separate paper. We will however include an extra scale in the wind/ice speed figure to show the actual wind speed. The wind direction can be found from the wind components in figure 4.

Line 308: Rather than "Standing still" I think "stationary" would be better terminology.

OK - we will change that.

Line 310-311: Not only is it when the ice is pushed against shore, but the ice must also be thick enough to transmit the internal stress and oppose motion, please note this here. Also, I think it's worth noting here that the ADCP was roughly 50 km from the western shore, meaning that for stationary ice to be observed there would be easterly winds compressing the ice pack quite some distance.

We will add some more information that the internal stress becomes transmitted offshore. The location of the ADCP is such that there is no significant difference between the distance to the western shore or to the northern shore/archipelago. This makes it hard to judge (without going into a comprehensive analysis) if easterly or southerly winds are more effective to build up stresses from the coast and stop the motion. Judging from the generally stronger north/south than east/west wind component and the dominant north/south motion of the progressive vector in Fig. 1 one might conclude that it is the northern shore that is mostly responsible for generating internal stresses. But again, such an analysis is out of scope of the present study.

Line 313-314: based on this sentence I'd suggest presenting sea ice concentration at, or within some radius of, the ADCP for insight into the pack ice it is observing. For example, during April it seems like the concentration dropped, exposing greater areas of open water.

This data set (SEAICE_BAL_PHY_L4_MY_011_019) is based on expert interpretation of satellite observations and in situ information. The handdrawn ice charts only represent the large-scale change of the ice fields and are not suitable for detailed analysis at the local scale. We therefore don't think it is appropriate and meaningful to analyse the local sea ice concentration at the ADCP location using this product, and we are not aware of any other suitable sea ice concentration product for that purpose. However, we will add additional text in the manuscript to better describe the limitations of the SEAICE_BAL_PHY_L4_MY_011_019 product.

Line 335: Rather than "shows a significant peak" why not say "mode". Throughout the paper I'd focus on reporting the mean draft, the modal draft, and then the work on the proportion of the ice pack greater than some threshold. Consistency in terminology will make it easier for the reader to interpret.

We agree that the terminology is important and will use "mode" instead of "peak".

Line 339 -343: There is repetition between the sentences. Focus on the last sentence that gives the % of ice observed above certain thresholds.

We will modify the text.

Line 348: A spot where it would be great to contrast the monthly means and modes.

We don't really agree since we focus mostly on the thicker part of the distributions at this point. The main mode of level ice is treated in more detail later in a special section.

Line 379: This peak is in January, note that here.

We will specify "January"

Line 381-382: Suggest revising to read "The mode varies and becomes smaller in February and March before being very thin in April".

OK we will adjust the text.

Line 384-385: This sentence specifically would be supported by some discussion on wind speeds and the formation of polynyas and new ice in the Bothnian Bay. Some added discussion on the broader dynamics back in section 4.1 would help clarify these results.

We will add some introductory text about the broader dynamics in 4.3.

Line 388-389: Suggest citing Rankainen et al., (2018) again here as that is where the 40-60 cm value comes from.

We will add the reference SMHI 2024 where the ice charts can be found.

Line 400: Should be Week 12 and remove "old", that is a specific type of ice and not something that is present in this area. This is also where reference to wind speeds and polynyas would be helpful in interpreting Figure 13.

We will change "old" to "early in the season" We have also introduced (in 4.3) the concept of new ice formed in e.g. polynas and leads and then arriving at the ADCP site at different ages. However, going into more details of possible origins (locations) of the ice observed at the site would be interesting and could be subject for a future special study.

Line 418: Aspect ratios of ridges have not previously been introduced or discussed in the paper. Given the focus on ridges in the results section I think there should be some discussion of ridges in the introduction where you can introduce aspect ratios, the porosity of ridges and the fact that they are often smoothed over by EM data… hence why ADCP data is so useful at identifying them.

We will include text about the structural properties of ridges in the introduction and also introduce the limitations of EM data..

Line 421-422: Please elaborate and interpret the information in Table 2. These are key results that should be presented and interpreted.

We will elaborate on the text.

Line 505: Again, is there any way to present wind direction rather than just speed. Given this is an enclosed Bay the direction is very important.

Wind directions for 2023/2024 can be seen in Fig. 6. We have also included wind roses for 2003/2004, 2023/2024, and the period 1995-2025 in supplementary materials, see below. We will also add text about this in section 5.1.

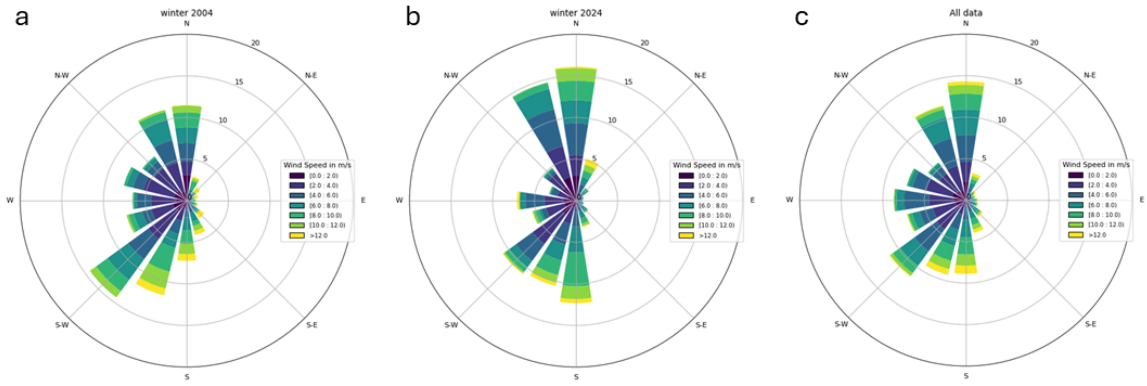

Fig. S3: Wind roses for the months December to March for the years 2004 (a), 2024 (b), and the years 1995 - 2025 (c).

Line 513: This could be moved to the new results in section 4.1 when you merge sea ice extent with air temperature.

We will move most of the text and figures from the discussion 5.1 "The 2023/2024 ice season in perspective" to section 4.1 . See also response to major comment 1. The new discussion 5.1 will then be shorter with a more general focus.

Line 537-539: Suggest citing previous work from Christian Haas on airborne EM data and specifically the fact that EM data smooths over ridges. I think it's also worth noting that the EM footprint is nearly double the observed keel width (Table 2). This specifically shows that the EM data misses ridges.

The cited reference (Ronkainen et al., 2018) gives a comprehensive description of the restrictions regarding airborne EM data and we see no need for additional citations. We will however add that the footprint is about twice the observed ridge width.

Line 546 and on: The discussion of your previous work from 2008 is left way too late. This should be discussed earlier and used as a reference throughout the paper.

We will add more information about the 2008-work in the introduction including mean ice thickness and fraction of deformed ice.

Line 605: this is where highlighting the process that introduces thinner ice into the ice thickness distribution throughout winter would be good to highlight. Oshore winds, drive the formation of a polynya where new ice forms and is subsequently advected over the mooring.

We don't see that this type of process description fits well as a conclusion within the scope of this paper. The text includes just some general description about some possible ways that level ice can form, become deformed, and eventually move toward the mooring.

Line 614: Worth noting that it wasn't 17 days in a row but rather a total of 17 days throughout winter.

Thank's - we will fix this.

Line 617 – 620: It would be great to refer back to the application of shipping and o shore wind farms here. That's part of what motivated the study so it should be outlined here.

Sure - we agree and will add: "The frequent presence of rather thick ice ridges shows that more knowledge about ice deformation in the Gulf of Bothnia is needed to facilitate guidance for design and placement of offshore wind farms and coexistence between wind farms and shipping. This is not becoming less important in a warming climate where we expect more mobile ice and thereby also more ice deformation."