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 offshore 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

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.

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.

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.

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.

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.

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.

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.
    - 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".

Line 42: "give" is not correct here. Revise.

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.

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.

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

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.

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.

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

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 affect 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.

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

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

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.

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

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.

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.

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.

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

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

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

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

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.

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

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.

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.

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

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.

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

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.

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.

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

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

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