

Review of egosphere-2026-785

Sea ice thickness distribution and 1 ice keel characteristics in the Bothnian Bay based on high resolution ADCP measurements

by Bjørk, G., et al.

Summary:

The manuscript presents and analyzes a new continuous sea ice draft data series from the Bothnian Bay for the ice season of 2023/2024. The series was acquired by a Nortek Sig500 ADCP set to a mode that enables a parallel measurement of sea ice drift with a 10-minute interval and sea ice draft with a minute resolution. The acquired data span over 700 km of sea ice surface and demonstrates the evolution of sea ice thickness pdf during the winter season of 2023/2024. From the analysis of changes of the sea ice draft pdf the authors demonstrate a crucial role of sea ice dynamics in the area pointing to the dynamic ice production to outweigh the thermodynamic growth especially in the months of March and April. Details on ridge keels statistics are presented. The authors further compare their results with remote sensing data (S-2 surface reflectance) to show a qualitative agreement between the spatial variability in surface reflectance and the measured ice draft gradient. Analysis of available weather data is also conducted to put the 2023/2024 ice conditions in the area in the context of other winter seasons.

In general, the study presents a valuable relatively high-resolution data series useful for scientific and management applications. Despite (and very unfortunate) poor performance of the internal AST and LE -based algorithms for draft determination, the authors successfully used an alternative 1-minute draft data series. With relatively low ice drift speeds in the area, it enabled the authors to achieve a relatively high spatial resolution in the draft series including identification of individual ridge keels. Some more details/discussion/assessment of limitations of the data, especially with respect to detection of ridge keels for periods with higher drift speed and its effect on the derived statistics should however be presented.

There are a few comments that should be addressed; these are related to ice draft calculations, spatial resolution of the derived series and the resulting ridge keels statistics.

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

- 1) Distance from the instrument to the lower ice surface this, I assume, is calculated from the echo returns internally by Sig500 based on water temperature and salinity, right? Or are water temperature and water salinity set as defaults in the firmware of the instrument? Please clarify this question.

The sound speed is calculated internally in the instrument based on the temperature (from the temperature sensor) and pressure (from the pressure sensor). The salinity is set to a fixed value of 3 g/kg. We will add this missing information to the text. Thank you for pointing this out.

- 2) Temporal variability in S and T in water column above the ADCP will introduce some time varying bias in the retrieved ranges (D) through variations in the column-average speed of sound. Did the authors assess the significance of possible effects of these variations locally?

We don't expect any significant bias of the retrieved range due to variations in S and T at this location. The surface salinity in the open bay (away from river mouths) is close to 3 psu over the entire year and well mixed down to ca 50 m during winter. The temperature is close to freezing during winter months (Feb-April) and well mixed down to 50 m. (Skjevik, Wesslander, and Viktorsson, SMHI REPORT OCEANOGRAPHY No. 78, 2024, The Swedish National Marine Monitoring Programme, 2023). The ADCP temperature was close to 0 °C over most of the record except for some short events in January when the temperature increased to 1-2 °C. This was probably a result of upwelling of warmer water from below 20 m (winter convection had not reached very deep in January). If this temperature increase occurred over the entire 20 m column it will not give any bias since the sound speed will be corrected accordingly. However, as a worst-case scenario, it can be assumed that only the lowest meter say (and not the entire 20 m column) increased in temperature by 2 °C. This will introduce a range bias of about 13 cm (20.13 m instead of 20 m) because the calculated sound speed will be too large compared to the actual sound speed in the major part of the column. The actual bias is likely smaller (< 10 cm) since it can be expected that the warm water occupied a substantial part of the column instead of just a thin bottom layer. Note also that this bias only occurred for short time periods.

- 3) A follow up of comment (2). In Eq. 3 the authors introduced the correction factor which the authors postulate to be constant. This correction factor can actually accommodate the bias due to changing speed of sound. How many open water events have you used to prove this statement? Are the open water events evenly distributed throughout the winter period? Good to show the plot of throughout the winter season in, e.g. Supplementary materials. It well indeed can be constant if the water column is well mixed and S and T measurements made by ADCP (if this is the case) are representative. Please comment on this.

We checked around 20 open water events spread over the entire period and adjusted the correction factor manually until a good fit was obtained within 2-3 cm. There were no obvious differences between different parts of the period and the same factor could be used for the entire series. We also checked the warm events specifically and found no extra bias connected to these events which shows that the instrument corrects the sound speed accordingly. We will add some more information about the correction factor in the text. In the supplementary material we will show the distance to ice bottom for the entire series as well as details of short open water periods used for adjusting the correction factor. The January figure is shown as an example below.

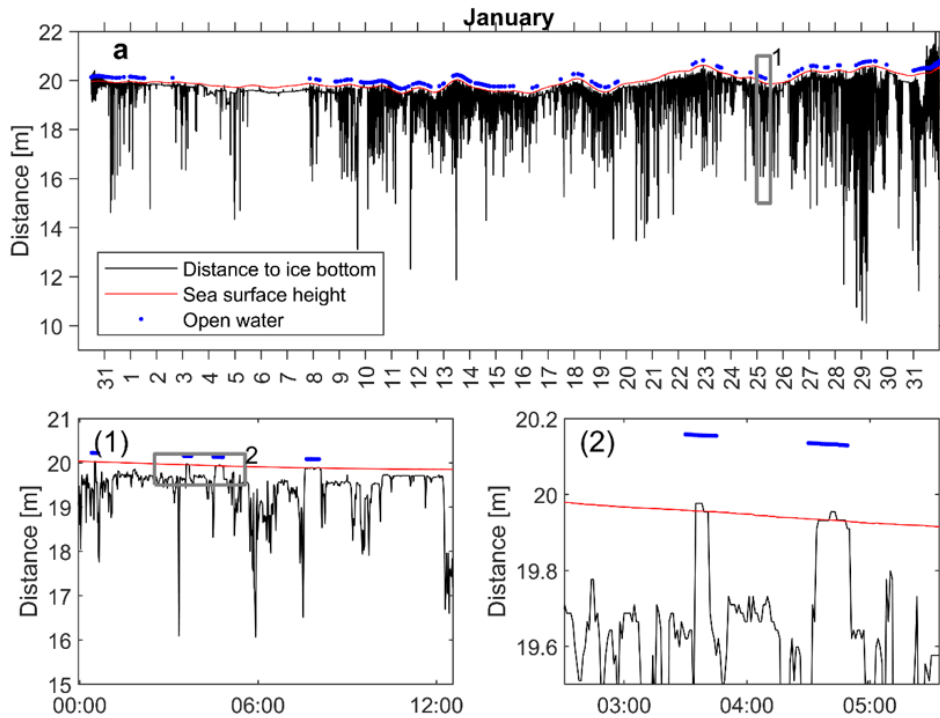


Figure S2. Distance (D) from the ADCP to ice bottom (or sea surface if open water) based on acoustic echo intensity (black line), sea surface height (SSH) based on ADCP pressure and air pressure (red line) and open water recordings (blue dots) plotted 0.2 m above SSH , for the months January (a), February (b), March (c), and April (d). Upper panels show data from the entire month, and the lower panels show details inside boxes 1 and 2. The boxes are chosen to show examples of short periods of open water which was used to match the SSH to D .

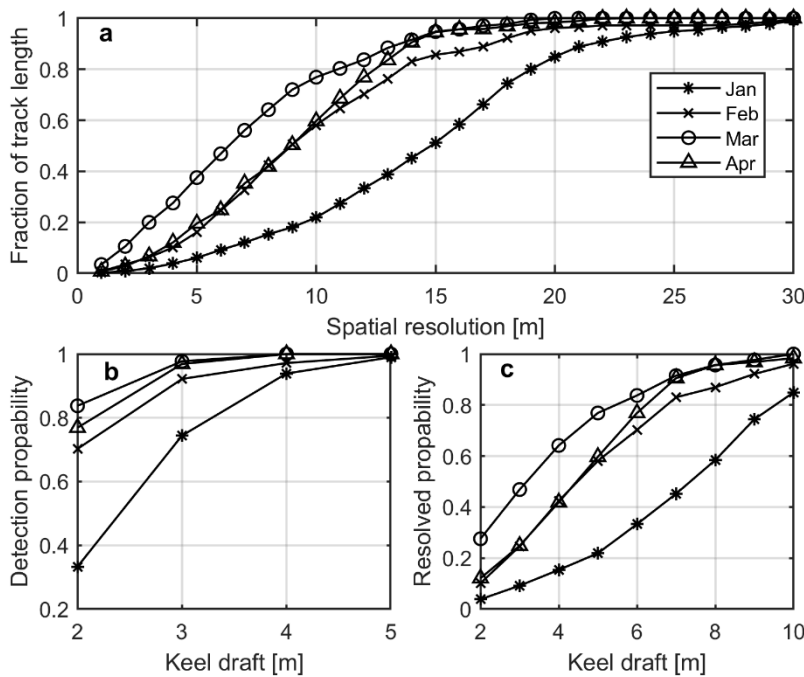
- 4) The authors postulate that a 1-minute draft series is sufficient to reconstruct a bottom topography with a resolution sufficient for isolation of individual ridge keels. This is indeed correct for a certain fraction of ice drift conditions (Fig.21) when drift speed was low enough. Apparently in case of high drift speeds detection of keels may not be possible. For the intermediate drift speeds like 0.1 m/sec (6 m sampling increment), keels below 18 m wide could not be resolved. In such conditions any means will always be biased high unless some speed thresholds are applied for making statistics. Was it considered in Table 2 where ice keel statistics is presented? Was any resolution cutoff applied for ridge keel detection? Good also if Table 2 can be supplemented with a fraction of time/month and eventually ice distance when ridge detection was possible/not possible. This can give a hint on the true number of ridges that could have travelled over the mooring.

We admit that the analysis of ridge keel detection with respect to 1-minute draft data was not fully developed in the original text and we thank the reviewer for commenting on this. We have now made a more thorough analysis.

When starting the new analysis, it was noted that we had used a criterion that a keel should include at least 2 data points with draft > 1m. There is actually no reason to have this 2-point criteria, so we changed this to 1-point. This increased the total number of identified keels from 5565 (2-point criterium) to 6968 (1-point criterium).

In the new analysis the statistics is based on the **fraction of track length** sampled within a spatial resolution interval instead of **frequency of observations** within a spatial resolution interval (old Fig 21). The fraction of track length is a better measure than the frequency since it takes into account the actual distance between samples. For example, it is shown that close to 40 % of the measured track length had a resolution better than 5 m in March but only about 7 % in January (new Fig. 21a).

The analysis is further extended with the probability of detecting keels of various drafts based on the aspect ratio (Fig. 21b). The detection probability is quite low for 2 m keels in January (about 35 %) meaning that 65 % of all keels with draft 2 m might have been undetected. However, the detection probability increases rapidly with draft such that more than 90 % of the 4 m keels could be detected. The analysis also includes a statistical measure of the probability to detect keels with basic resolution (sampling distance < 1/3 of the keel width, Fig. 21 c). Most of the text in this section will be rewritten according to the new analysis. We will move this part to right after Table 2 (section 4) to provide information about the sampling restrictions.



New Figure 21. a) Fraction of track length with sampling distance (spatial resolution) less than a specific value. b) Detection probability of keels as a function of keel draft assuming an aspect ratio of 6. c) Resolved probability as a function of keel draft. The condition for the resolved probability is that the sampling distance is less than 1/3 of the keel width, assuming an aspect ratio of 6.

- 5) The paper can be shortened without a loss of context. Now it includes 21 figures: some can be combined, others moved to Supplementary materials. I made some suggestions in the other comments listed below but I do recommend the authors to think through the concept of the paper and retain the most important details in the main text leaving the rest for Supplementary.

We agree and will move figure 2 to supplementary material and combine others (fig 16, 17 and 19, 20).

Minor to moderate comments.

Page 3-4. Can one consider combining Figures 1 and 2?

We will move figure 2 to supplementary material where interested readers can see the more detailed bathymetry. The local bathymetry is actually quite important since Falkensgrund is a shoal reaching up to about 10 m depth and can therefore be an obstacle for deep reaching keels and affect the ice motion..

Page 4: ...the Nortek software sought a maximum in echo intensity elsewhere in the water column. There are also unrealistic data when the LE distance changes with the same increment between data points for a series of points, which appears as lines with constant upward slope and located well below the expected ice surface.

I assume here the authors present two types of anomalous data. The first is caused by reflections from various objects other than the sought bottom ice surface, like bubbles, troughs of surface waves (in case of open water conditions), or different objects of biological origin etc. The second sounds like a description of multiple reflections (or double bounces) when the instrument logs the echo of a second (or even third) return.

The second type of anomalous data appears as LE points along lines with constant slopes which is not realistic. The slope of the points is always upward (from small to large LE-distance with time) and can include many data points (40-60). The sloping points seem to appear when the LE algorithm somehow misses the main echo at the ice bottom. The next point then shows up much deeper down in the column (often close to 6 m above the instrument). The following points then line up with constant slope towards the surface. This is unrealistic and might be due to some flaw in the LE algorithm. We will adjust the text around this to better clarify how the data looks like.

Page 4:” However, the AST data has a minimum distance of about 13.5 m which makes this method not useful for drafts larger than about 7.5 m.” what was the rationale behind this threshold?

There was no rationale behind this threshold. The 13.5 m threshold was pre-set in the AST algorithm by the manufacturer, and we were not aware of this before the deployment.

Page 5: “An estimate of the total ice thickness can easily be found by multiplying the draft with some isostatic factor of around 1.14 obtained from literature (e.g. Vinje et al., 1998).” Note that this factor was based on measurements made on summer sea ice in the 1980s in Fram Strait (i.e. on preferentially older ice types), with reduced snow load or even without snow at all. So, good to mention that the actual ice thickness would vary depending on ice density variations and snow load.

Yes – we agree and will add some additional information regarding the isostatic factor, including a new reference dealing with the uncertainties (Geiger et al., 2015).

Page 10: The ice was mobile during the first month of the period such that the ice speed followed the wind speed rather closely which indicates that the ice cover was mostly in free drift with low internal stress. Later, in February and March there were numerous occasions when the ice was standing still despite significant wind speed.» What was the effect of storm Ingunn in late January the authors have mentioned on sea ice drift?

The effect was to move the ice northward and leave behind open water at the mooring site. We will add this in the text.

Page 12: The detail (Fig. 8c) shows that the ridge structures appear to be well resolved by the 60 s sampling interval. Please see my major comment (4). Note also that the example demonstrated in 8c is for the period when the drift speed was below 0.1 m/sec, i.e. leading to ca. 6 m spatial resolution and implying ridge structures less than 18 m wide could not be resolved with such sampling rate. Should be mentioned. Page 16: Most of the keels had a width of 6-40 m resulting in typical aspect ratios of 5.5-6.5. The same comment as before. Keels of the width as low as 6 m could be detected only in conditions of almost no drift (with a 2 m spatial resolution required by a drift speed of 0.05 m/sec). Makes sense at least to emphasize, that most of the detected (or isolated) keels...

Yes, we agree and will flag in the text that this good resolution is for a low speed event. The other aspects of this comment are treated in the main comment 4 above.

Page 16: The number of observed rubble-like structures (width more than 100 m) as well as the maximum width is presented in Table 2. The spatial resolution, as such. This statement needs to be considered in the context of rubble fields when detected during fast drift periods can also be unresolved keels. It is known that ridges are not evenly distributed along the ice surface but rather tend to cluster in space.

We will also include uncertainties about rubble fields in the text.

Page 19: Figure 16. I wonder if there were any tidal effects registered in ice drift pattern?

The tides are very weak in the Bothnian Bay, and we don't see any tidal effects in the drift pattern.

Page 17: Figure 17 cross is very difficult to discern. Page 21: Figure 18 can be placed into Supplementary.

We will do a two panel figure instead including the 3d view (and fix the cross).

Page 21: "...assessment, exploring whether high-resolution optical imagery can detect 496 ridge sails as manifestations of subsurface keels. Based on the present dataset, the sensitivity appears limited. Radar-based approaches, such as SAR, which are more responsive to surface roughness and independent of illumination conditions, may provide a more suitable framework for future work." This can be moved to Discussion.

We will move the entire text about comparison with satellite images to the discussion since it is meant to be more like a feasibility study and not a full analysis..

Pages 21 and 22. Figures 19 and 20 can be moved to Supplementary (or at least combined into a single two-panel figure)

We will make a 2-panel figure instead.

Page 22: "Is not seen at all in the EM data having near zero frequency already at 4 m ice thickness and where the thickness bins end at 5 m." Heli EM is known to undercount deeper ridges due to a large footprint of the EM-bird (about 50 m as far as I remember). Detailed comparison is therefore not straightforward if not impossible at all.

We agree that detailed comparisons with helicopter EM data is not possible due to the methodological differences. However, since a lot of EM data is published it appears important to mention the basic differences between EM and ADCP data.