

Anonymous Referee #1

I have some problems with the presentation being far too detailed in the result section. Much of the text is repeating what is already shown in figures. This makes the manuscript hard to read.

Thank you for this comment. The result section was revised and shortened not to double the information for figures and tables.

A more serious matter is whether the prediction model for ice thickness is made correctly (data shown in Table 4 with implications for Figure 10). Read details below following Line 448 and Table 4. I suggest the authors carefully examines the integration (cumulation) they have done compared to Su and Wang (2012), and do a revision of the manuscript.

We have revised the ice thickness part of the study following the reviewer's comments following line 448 and table 4 below.

Lines 104-105: wrong unit for salinity. From the numbers I see that salinity is Practical salinity which is defined by having no unit. In 2010 TEOS10 became the new standard with Absolute salinity having unit g/kg, but in this standard, salinity values are higher.

It was verified with Jakacki et al. (2017) and the unit was removed.

Line 162: Larsen et al., 2006 is not in reference list

The reference was added to the bibliography.

Line 168: NPI, 2014 is not in reference list

The reference was added to the bibliography.

Line 246: If you find space, it would be good to indicate position 77N, 15E in the map, Fig.1.

Location of the ERA5 point was added to Figure 1.

Line 260: according to Table 4, freezing point $T_f = -1.8$ deg C. Please indicate here.

It was indicated as suggested.

Line 315: Is ref Fig.4 meant to be Fig. 3b?

Yes, it was meant to be Fig. 3b. We have removed it from here because the reference to Fig. 3b is at the end of the paragraph.

Figure 4: are the figures showing the mean distributions over the years 2002-2025, please specify.

Yes, the wording was changed as suggested.

Figure 5; add seasons to the figure legend or above each column of plots.

We have added the labels above each column.

Lines 390-397: all text here is also shown on Fig.6. Consider not giving the details.

The paragraph was reduced to the first sentence.

Line 409: Fig. 7 does not say anything about how many predictors were included in the regression model, so explain why you show the figure.

Thank you. Figure 7 was removed because it repeats the content from Table 3, but provides less information (e.g. the number of predictors and sign of relationships).

Line 410-425: perhaps use 'correlation' instead of 'relationship', it is less emotional.

We have shortened the paragraph significantly, but also swapped 'relationship' with 'correlation' as suggested.

Table 3: Tell if these regression formulas are the ones used as predictions in Figure 7.

Yes, but Figure 7 was removed now.

Line 448: Temperature should be below -1.8 centigrades (freezing temperature).

Thank you, we have fixed the value to -1.8.

Line 448 and Table 4: Additionally, why are you counting consecutive days? Equation 1 for FDD does not mention that the days must be consecutive. Do you only estimate FDD when cold days follow in a row? And how can the term defined in Equation 3 be larger than FDD when, according to Equation 3, FDD is the largest number it can have..? You must in that case done integration for a different period for Equation 3 than equation 1. It is crucial for Figure 10 that you do the integrations correctly. Your reference Su and Wang (2012) calls eq. 1

and 2 'cumulative FDD' and 'cumulative SDD' They are added during the same periods of time, FDD only when air temperature is below SST freezing temperature (-1.8 centigrades in your case), SDD only when air temperature is above 0 centigrades, and zero value is added to both of them when air temperature is between -1.8 to 0 centigrades.

Thank you for pointing out to the errors with integrating FDD and TDD to calculate ice thickness. We have now revised the whole section (in methods and results).
Specific changes

- We totally removed using the consecutive days
- For the ice thickness calculation we used FDD and TDD over exactly the same period starting from the first day with $T < -1.8$ C and finishing on Aug 31st
- We update the values in Table 4, Figure 9 and the text

Table 4: explain which days are 'on the day'.

It means on the day of the freeze-up and break-up but we removed it completely as the terms are clear without it.

Line 462: I presume Fig. 14b should be Fig. 8b

Yes, we have change it to 7b (figure indexing changed after removing Fig. 7)

Line 676: the publishing year should be 2018.

Thank you, we have fixed the year.

Anonymous Referee #2

The research motivation of this manuscript should be more focused on the climatic, physical and ecological settings of fjords, as some background information concerning the pan-Arctic Ocean is not fully applicable to fjord environments.

We have revised the introduction and removed a section of the introduction that was not relevant to the topic ("*With the decline of sea ice, previously inaccessible routes, such as the Northern Sea Route and the Northwest Passage, are becoming increasingly navigable during summer months (Smith and Stephenson, 2013). This change has led to increased shipping activity in the Arctic (Pizzolato et al., 2016) and opens new opportunities for commercial shipping and resource exploration.*")

When compared with previous studies, the discussion ought to center more on the phenomena and physical mechanisms uncovered by the long-term time series and high-resolution (daily, 50-m) dataset.

We thank the reviewer for this helpful suggestion. We have revised the Discussion to place greater emphasis on the phenomena and physical mechanisms revealed by the long-term, high-resolution dataset, while keeping the structure of the manuscript unchanged.

Specifically, we added text clarifying how the daily, 50 m record resolves:

- the sequence of ice events during freeze-up and break-up;
- the contrasting process-related patterns of the exposed main basin and sheltered bays;
- the different roles of drift ice, landfast ice, thermal forcing, wave exposure, glacier influence and local fjord geometry.

These additions are intended to make clearer what new process-level insight is enabled by the dataset beyond comparison with previous studies.

For the linkage between sea ice and local meteorological as well as oceanic conditions, temporal resolution can be refined to pinpoint the time windows within which each sea ice parameter correlates most closely with specific atmospheric or oceanic drivers. Such refinement helps quantify the sensitivity of individual sea ice metrics to climatic and oceanic variability over distinct periods.

Thank you for this suggestion. We refer to it when answering the comment to line 16.

Additional comparison against existing phenological studies of sea ice in other Arctic fjords is recommended.

We have now added an outline of the situation in other fjords to the discussion "Similar trends were observed in other fjords of Svalbard. For instance, Pavlova et al. (2019) documented an abrupt change in Kongsfjorden, where 2002/2003-2004/2005 were characterised with high sea ice coverage and the subsequent 2005/2006-2015/2016 had a reduced sea ice cover, with the minimum in 2011/2012. In Isfjorden, Muckenhuber et al. (2016) documented the abrupt change starting from the 2005/2006 season and the minimum sea ice coverage in 2011/2012 and 2013/2014. Finally, Johansson et al. (2020) observed a low sea ice coverage in Kongsfjorden in 2005/2006-2007/2008 and 2011/2012-2018/2019, and in Rijpfjorden in 2011/2012, 2015/2016 and 2017/2018." In addition, we have connected our results to similar findings along the Alaskan coastline (Mahoney et al., 2014, Mahoney and

Einhorn, 2026), Laptev Sea (Selyuzhenok et al, 2017) and the Canadian Archipelago (Gupta et al., 2022).

It is recommended to add comparisons with published findings on variations in other climatic, oceanic and environmental factors within Hornsund Fjord over a comparable study period.

We have added an explicit note on that in the discussion: "Combination of observed over the similar timescale as ours 8% increase of AW volumetric fraction per year (Strzelewicz et al., 2021) and faster glacier retreat in Hornsund compared to other parts of Svalbard (Błaszczuk et al., 2013) further highlights the increasing local contrasts between the main basin and the bays, and between drift ice and landfast ice."

Line 16 "The length of the sea ice season was shortened by 19.5 days for every 1°C increase in mean autumn air temperature": You may identify the meteorological factors through a specific period (not just seasonal) most closely correlated with ice season duration (as well as other parameters) based on moving average methods (see Lei et al., 2012), enabling more accurate characterization of the physical linkage between climate change and sea ice variables. Lei et al., Changes in ice-season characteristics of a European Arctic lake from 1964 to 2008, Climatic Change, 2012.

We agree that using seasons or three-month periods has its drawbacks, but it is the approach we consistently used in the previous (Swirad et al., 2024; 2026) and this study. Here, we have also used the period leading the landfast ice freeze up and break up, acknowledging that specific periods rather than fixed days of the year may help identify patterns better. However, we have added a suggestion to the discussion about the potential to explore other options: "An approach for environmental condition description alternative to the 3-month seasonal averaging could be explored for a better fit (Lei et al, 2012).".

Line 19 "with an average coverage exceeding 20% 40 days and 26% one day": Please provide the complete expression.

We have modified it to "exceeding 20% 40 days before the landfast ice onset and 26% one day before the onset"

Line 149 -153: This paragraph describing wind climate appears somewhat disjointed; it is recommended to shorten or remove it.

We have removed the paragraph as suggested.

The constraints on variable T in Equations 1 and 2 are unnecessary. Certain periods with temperatures above the freezing point should be included when calculating freezing degree-days; otherwise, their interfering effects on sea ice

growth would be overlooked. The same logic applies to the calculation of Thawingdegree-days.

We thank the reviewer for this important comment. We agree that excluding periods with temperatures above the freezing point from the freezing-degree-day calculation, or excluding cold periods from the thawing-degree-day calculation, would overlook their opposing effects on thermodynamic ice growth and melt.

We have revised the thermodynamic ice-thickness calculation section. Specifically:

- FDD and TDD are now calculated over the same seasonal period, from the first day with $T < -1.8^{\circ}\text{C}$ to 31 August;
- FDD and TDD are combined following Su and Wang (2012) as $\vartheta = \text{FDD} - 3 \times \text{TDD}$;
- this ensures that alternating freezing and thawing conditions are accounted for in the thermodynamic ice-thickness estimate;
- the equations, explanatory text, Table 4, sea-ice-thickness results, and related discussion have been updated accordingly.

Equations 3 and 4 describe the analytical model for thermodynamic sea ice growth, whose key assumptions and limitations need to be highlighted:1) Snow cover effects are neglected; 2) Oceanic heat flux is not incorporated (this flux is generally relatively large in fjord regions); 3) Vertical ice temperature profile is assumed to linearly, and the air temperature equals the ice surface temperature, etc. Potential impacts of these assumptions on estimation results also need discussion.

We thank the reviewer for pointing this out. We have revised the discussion of the thermodynamic ice-thickness estimate to state the main assumptions and limitations more explicitly.

The revised text now notes that Eqs. 3 and 4:

- do not account for oceanic heat flux;
- assume a linear vertical ice-temperature profile;
- ignore snow-cover effects.

We also clarify that the calculated thermodynamic thickness should be interpreted as an estimate, and that the persistence of positive calculated ice thickness at the observed break-up indicates that additional processes, including wave-driven mechanical break-up, are likely important.

Though the relationships are weak and statistically insignificant (Fig. 3a and other text or figures: Statistically insignificant trends or correlations are not recommended for presentation.

We have opted for keeping the insignificant relationships in the figures but remove them from the text.

Figure 6: For ice seasons with large deviations from linear regression, their anomalous features and potential causes deserve further discussion.

Thank you. We have added some information on that to the discussion "Deviation of points from the trendline in Fig. 6 suggests that T is not the only determinant on ice conditions. For instance, the 2002/2003 had the lowest Twinter, but with did not have the highest ice coverage. Similarly, 2011/2012, 2013/2014 and 2015/2016 all had the highest Twinter, but 2015/2016 had considerable shorter landfast ice season (Fig. 6). Additionally, the meteorological station at PPT does not well represent thermal conditions in the whole fjord (Araźny et al., 2018)."

Wind exerts complicated effects on ice formation. Increased wind speed can accelerate ocean heat loss and favor ice freeze-up; meanwhile, stronger winds frequently fracture thin ice and hinder the development of stable ice cover or landfast ice. Intensified ice deformation by the enhanced wind is commonly observed in sea ice across the central Arctic Ocean. It is therefore necessary to further distinguish between onshore-offshore wind effects and wind overall effects, as onshore winds facilitate stable sea ice formation.

Thank you. We have expanded the discussion about the wind effect: "Interestingly, there is a statistically-significant decline in winter wind speed. Wind has a complex effect on ice formation and persistence. Wind at the beginning of the sea ice season may accelerate ice production through accelerated ocean heat loss and ice deformation (Kwok, 2006). This phenomenon may become limited with decreasing wind speed, further contributing to slower in situ ice production. In the nearshore or fjord environment, cold katabatic winds from glaciers can decrease near surface water temperature in the bays enhancing the ice production. However, wind from the open sea will break up the forming landfast ice, particularly thin one at the beginning of the season."

Line 447 "but the difference between FDD over consecutive days with $T < 1.8^{\circ}\text{C}$ (79°C days) and the θ over the entire period from the first day with $T < 1.8^{\circ}\text{C}$ ": The value here should be -1.8 .

Thank you, we have fixed the value to -1.8 .

Some cited references are unnecessary. For instance, De Steur et al. (2023) reported a correlation between sea ice extent in the Fram Strait and water temperature observed at depths ranging from 55 m to 2440 m, yet their

physical mechanisms differ fundamentally from those addressed in your study. Many other such examples can be found.

We have removed some unnecessary references: De Steur et al., 2023; Smith and Stephenson, 2013; Pizzolato et al., 2016; Barzycka et al., 2020; Shulman et al., 2024