Figure S 8: Comparison of active layer thickness (ALT) for 2021 (cold thawing season) and 2023 (warm thawing season) from three boreholes and two CALM grids in Adventdalen (a). The location of the boreholes and CALM grids is shown in (b). The long-term development of ALT measurements at these sites and the mean annual air temperature (MAAT) development at Svalbard airport is shown in (c), with 2023 underlain in red. The colours of the sites align between the subplots. The background in (b) is from TopoSvalbard (Norwegian Polar Institute, 2014a).

L. 269–270: Please confirm the accuracy of these numbers, as they seem inconsistent with related data.

These numbers have been verified, and they align with the expected subsidence contributions in Figure 6 (average across all sites). Please note that the unit of the expected subsidence is mm and not %. Please also note that the melt of excess ice and the subsequent drainage of the resulting meltwater are listed separately (L. 269-270). We have updated L. 268 to say “on average across all coring sites” instead of “on average”:

> “On average *across all coring sites*, pore ice melt contributed 20 mm to the expected subsidence with a standard deviation (SD) of 8 mm.”

L. 357: Clarify whether “stagnating subsidence” refers to stabilization of the ground surface when the thaw front passes through ice-poor layers or ice-rich layers. Is your "excess ice-rich base" top or bottom of the ice-rich layer at the ALT-permafrost boundary?

We have updated this sentence to clarify that “excess ice-rich base of the active layer” refers to the bottom of the active layer, at the transition to the transient layer, which is very ice-rich at most sites. The updated paragraph is:

> “The period of stagnating subsidence rate is most pronounced between the 24 July and 08 August 2023 at several of the studied sites, all of which have an excess ice-rich uppermost permafrost. *The ground surface stabilization could have been caused by the thaw front reaching the ice-rich transient layer or uppermost permafrost (Shur et al., 2005).* Here, sensible and latent heat effects of warming and melting the excess ice, as well as slow drainage of the excess ice meltwater, could have delayed further surface subsidence, *thereby causing a stagnation in the InSAR subsidence pattern.”*

L. 360: Frost heave during mid-summer heavy rain under sustained high air temperatures (>5°C for 1–2 months) seems unrealistic. Could alternative explanations, such as vegetation growth or instrument malfunctions, account for the reported thaw-season heave (1–2 cm)?
Please note that we did not observe any mid-summer heave in the InSAR time series, only a stagnation in the cumulative subsidence. We questioned if this stagnation might have been caused by other heave effects masking the thaw subsidence.
We agree to remove the hypothesis that ice formation at the freezing front in the lower active layer could have caused a heave signal. However, we still believe that ground swelling could have occurred given the significant rain event. On the other hand, the effect of vegetation growth seems unrealistic, since all sites have very low growing vegetation (mosses, grasses or barren). We have updated this section to:

> “Alternatively, a significant rainfall event (40 mm) during August 4–6, 2023 (Fig. 5), might have temporarily masked subsidence signals due to ground swelling. The rain event could on the other hand also have enhanced the late-season subsidence by percolation of rainwater advecting heat into the lower active layer (Douglas et al., 2020; Magnússon et al., 2022).”

L. 389–391: If the primary focus of your paper is excess ground ice and its relationship to seasonal thaw settlement or frost heave, I strongly recommend revisiting foundational studies on frost heave mechanisms and long-term development of ice-rich layer in the boundary zone between AL and permafrost, such as those by Taber (1930) and Shur et al. (2005).
We have incorporated these studies at other locations in the paper (see also answers to comment 4). We decided to keep this sentence as it is, as it serves as introduction statement to the following paragraphs to discuss how drainage, landform history and sediment grain size influence the excess ice presence in the active layer.

L. 395: One of the main factors to determine the amount of segregation ice is soil moisture. Lateral drainage is one component that controls soil moisture through water balance in the active layer.
Agree. We have updated this sentence by replacing “drainage variations” with “soil moisture variations”.

L. 398: Replace "grain size" with "grain size distribution of mineral soil particles." Also, consider using "soil moisture conditions" instead of "drainage conditions," as good drainage place can still maintain high soil moisture levels depending on local hydrological conditions.
We have replaced “grain size” with “sediment grain size” and “drainage conditions” with “soil moisture conditions”. The resulting sentence is now:

> "Future geomorphological studies could leverage InSAR datasets to enhance ground ice mapping within periglacial landforms by integrating knowledge of *sediment* grain size and *soil moisture* conditions."

L. 400–401: This statement could be reconsidered to align with established observations. Thaw settlement typically stagnates when the thaw front passes through zones with minimal ice lenses (segregation ice) and intensifies when progressing through ice-rich layers.
Yes, we agree and discuss the theory you mention as first explanation for stagnation in thaw settlement in L. 350-355. However, as we describe in that paragraph, the stagnation seems to first occur when the thaw front reaches excess ice rich layers in our data. We therefore discuss that there might be an initial stagnation as the thaw front reaches ice layers, due to latent heat effects and slow meltwater drainage.
This interpretation from L. 350-355 is the basis for the statement in L. 400-401 and we therefore have left this sentence unchanged.

L. 407-409: This sentence could benefit from clarification. Could you elaborate on what is meant by "increase the influence on"? If you are referring to processes at the interface between the active layer and uppermost permafrost, I suggest adopting the concept of the "Transient Layer" as described by Shur et al. (2005).
By "increase the influence of melting excess ice," we meant that in exceptionally warm years, a deeper active layer thickness can cause thaw penetration into the transient layer or uppermost permafrost. Since both the transient layer and uppermost permafrost are often rich in excess ice (Shur et al., 2005), warmer summers can result in larger subsidence signals due to excess ice melt.
However, we have restructured and updated Section 4.2 based on your and the other reviewers' comments. As a result, this specific sentence (L. 407-409) was removed, since we already discuss the late-season subsidence signal from excess ice melt in Section 4.1 (L. 370-371; L. 398-400).
To acknowledge the transient layer, we have now included it in L. 357:

> "The ground surface stabilization could have been caused by the thaw front *reaching the ice-rich transient layer or uppermost permafrost (Shur et al., 2005).*"

L. 412–413: "Negative" should be replaced with "positive"? It seems to be a contradiction with the subsequent sentence. Please revise for consistency.
Thank you. Liu et al. (2012) do indeed assume a positive relationship, whilst our data indicates a negative relationship between pore ice melt and ALT. We have also realized that this section (4.2) was overall difficult to follow and have therefore updated the section by restructuring and adapting it based on your and the other reviewers' comments. The updated section 4.2 does not alter any of the main conclusions. The updated section 4.2 is now:

> "*Previously, InSAR subsidence has been used for ALT inversion by assuming a positive relationship between InSAR subsidence observations of pore ice melt and ALT, with no consideration of excess ice contributions (Liu et al., 2012; Schaefer et al., 2015; Jia et al., 2017; Wang et al., 2018; Peng et al., 2023; Scheer et al., 2023). Some of these studies show good alignment between the InSAR-ALT and ALT from in-situ field measurements (Schaefer et al., 2015). However, recent findings show that the positive correlation between InSAR subsidence*

observations of pore ice melt and ALT used as basis for the inversion is not valid in drier regions, where the correlation is negative (Chang et al., 2024).

We also find a negative correlation between ALT and expected pore ice subsidence (Fig. 4A), indicating that larger ALT can occur at sites with less pore ice. In addition, our results suggest that traditional inversion models are not universal and might not apply in areas with complex ground stratigraphy. Adventdalen is a periglacial valley which has a very diverse geomorphology with a variety of landforms, some of which have abundant excess ice in the active layer, as for example observed in outer alluvial fans (Fig. 6).

Our results indicate that the surface subsidence signal can be dominated by excess ice melt and drainage instead of pore ice melt, which complicates inversions from thaw subsidence to ALT. Further, the correlation between ALT and the InSAR subsidence was close to random in our results. Considering only pore ice melt to explain the observed subsidence in our study area would cause large errors, since the main subsidence contribution is from excess ice (Fig. 6).

In alignment with our observations (Fig. 7a), Antonova et al. (2018) also observed a poor match between ALT and subsidence. They compared *in-situ* ALT measurements to in-situ surface subsidence from reference rods anchored in Yedoma permafrost in Siberia and found a weak positive correlation, with a low coefficient of determination. Further, when comparing the measured in-situ subsidence to the expected subsidence predicted by a pore ice melt model based on soil moisture measurements, they found a moderate alignment but noted significant outliers where the model underestimated the measured subsidence. Very large in-situ subsidence was for example measured in drained lake basins, yet not consistently.

Such outliers *in drained lake basins* have also been reported in InSAR-ALT estimates. Schaefer et al. (2015) reported outliers in *InSAR-ALT* estimates over drained lake basins at Barrow, Alaska, where high subsidence led to a large *InSAR-ALT* under the assumption of only pore ice melt contributing to the subsidence. Similar patterns of high subsidence in such landforms were also reported by Liu et al. (2014) on the Alaskan North Slope near Prudhoe Bay, by Strozzi et al. (2018) at *Teshekpuk* lake, Alaska, and by Bartsch et al. (2019) in central Yamal, Russia. Our results *suggest that the mismatch between InSAR-ALT and in-situ ALT is likely due to the omission of excess ice*, which has been previously detected in these landforms (Jorgenson and Shur, 2007; Bockheim and Hinkel, 2012; Kanevskiy et al., 2013).

Overall, our study indicates that the contribution from excess ice should not be neglected in models utilizing InSAR time series for active layer characterization, and that simple active layer models relying on constant pore ice contents are oversimplistic in periglacial environments like Adventdalen, Svalbard. *Future work should investigate integrating InSAR time series with numerical models that simulate ice segregation processes and excess ice content (e.g., Aga et al., 2023). This could help constrain model parameters or improve process representation by leveraging InSAR-derived surface deformation patterns, potentially through data assimilation techniques (e.g., Aalstad et al., 2018).*”

L. 416–418, 425–428: The ALT inversion described in these references is based on an incorrect frost heave theory that attributes ground movement to water volume expansion upon freezing rather than to water redistribution caused by ice segregation. Consider revising these interpretations to align with established frost heave mechanisms.
We acknowledge that these studies omit classic frost heave mechanisms, particularly the role of excess ice. The results of our study support your point. However, we believe it remains important to discuss these other studies, as they, in some cases, show a reasonable fit between InSAR ALT inversion and in-situ ALT measurements, despite notable outliers. As visible in our answer to your above comment regarding L. 412-413, we have strongly adapted this section, including L. 416-418 and L. 425-428. We point out our findings and present other recent observations, which indicate that an ALT inversion from a simple pore ice model is oversimplistic.

L. 415: The alignment between in-situ ALT probing and temperature measurements is not entirely clear. Could you elaborate on what this alignment reveals and how it connects to the following part of the sentence?
In this sentence we are discussing that some previous studies showed a good alignment between the InSAR-ALT based on Liu et al. (2012) and ALT from in-situ field probing and temperature measurements. We believe this is important to mention and have therefore kept this information also in the updated section 4.2 (see above answer regarding L. 412-413), but improved the clarity:

> *“Some of these studies show good alignment between the InSAR-ALT and ALT from in-situ field measurements (Schaefer et al., 2015).”*

L. 428: Correct "Teshepuk" to "Teshekpuk."
Corrected.

L. 432: I am very pleased with the conclusion drawn from your InSAR and in-situ investigations. Your work goes beyond merely addressing the oversimplification of AL-subsidence models; it highlights a critical oversight—the neglect of classic frost heave studies and the role of excess ice in such models. This omission in the previous works was surprising for me.
Thank you! We really appreciate your thorough review, which has improved our manuscript. We hope our answers could clarify your comments.

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Answers to Reviewer 2

In the following, we answer (shown in blue) the copy-pasted comments from the reviewer (shown in black). Changes to the manuscript are shown in italics and the line numbers refer to the original preprint.

The submitted article by Wendt et al. primarily compares seasonal ground surface displacement using InSAR remote sensing for 2023 to expected subsidence derived from ice contents from core drilling at 12 sites in Adventdalen, Svalbard. They establish a reasonable correlation between the InSAR-derived subsidence and that expected from the ground ice content in the cores, which includes determinations for pore ice, excess ice, and water drained upon the melt of excess ice. The authors determine that excess ice melt is the key contributor to observed subsidence at many of the sites, with pore ice typically being of secondary importance. The authors further demonstrate that without detailed knowledge of excess ice conditions, active layer thickness cannot be reliably estimated from InSAR in ice-rich terrain.

The strength of this paper lies in the fact that the InSAR subsidence trends can be partly, and fairly strongly, supported by the in situ ground ice determinations from immediately before the remote sensing record, which are commonly not available in similar remote sensing studies. The sampling scheme was well thought out and captured a significant range in ground ice conditions due to the selection of sites from different landforms and substrate conditions. Overall, I recommend this paper for publication however I have many minor comments and a couple more substantive ones that, if addressed, I think will strengthen the manuscript.

We would like to thank the reviewer for the detailed comments! These have been very useful and are addressed point by point below.

Main comments/suggestions:

Ice wedges

The ground resolution is stated as 18.2 x 28.2 m. Some of the sites include ice wedge polygons. In years of very deep thaw, presumably thaw would extent into the tops of ice wedges, and this could materially contribute to subsidence (e.g., https://doi.org/10.1002/ppp.2113). Based on the size of the polygons on Svalbard, I assume some pixels that included a core sample may have also included an ice wedge trough (or more than one). If this is the case, it should be discussed. Could this help explain why the 2023 InSAR derived subsidence is commonly higher than expected subsidence (Figure 6)?
This is a good point. Core E8 is located in an ice wedge polygon, and was extracted from its centre, while the InSAR pixel also covers the ice wedge troughs. Core E2 is also located in a polygon. We do not see a clear pattern of higher 2023 InSAR-derived subsidence compared to the expected subsidence and have therefore not discussed this further for Figure 6. Instead, we are now discussing this as part of the limitations of our study (section 4.3, line 461):

> *"Further, the site E8 is located within a low-centre ice-wedge polygon. Whilst the core was extracted at the polygon centre, the InSAR pixel is large enough to include thaw subsidence effects from the ice wedge troughs (Short and Fraser, 2023). Since the ice wedge tops in adjacent polygons have been observed to be located just below the active layer (O'Neill et al., 2025), the late-season subsidence observed at this site might partly reflect this (Burn et al., 2021). Another site, E2, also displays polygonal features indicative of ice wedges. Yet, these sites do not present as outliers in our analysis (Fig. 6)."*

Date of snowmelt

The InSAR record includes scenes following the melt of snowpack at the ADV met station. Did you examine whether snow had melted by this date in ice wedge troughs (or more generally in different topographic settings at different sites)? I presume snow may have persisted later, particularly in deeper troughs, as the snow depths are greater there. I observed this when I was on Svalbard. If this is likely to have occurred also in 2023, you may wish to consider what effect this may have had on the InSAR results at sites with ice wedges, or other settings where deeper snow could have accumulated, and include it in the discussion of limitations.

We agree that there is spatial variability in snow melt-out dates across the study area. We have reviewed the available Sentinel-2 imagery during this period and can confirm that snow cover was still present in topographic depressions, but all except one coring site were snow-free or had mixed pixels at the start of the InSAR time series. We have included this point as a limitation in section 4.3, line 462:

> *"The InSAR time series starts two days after the snow melt-out date at the Adventdalen meteorological station. Sentinel-2 imagery confirms that all sites except coring site S5 were snow-free or had mixed pixels at the start of the InSAR time series, suggesting minimal impact of snow cover. However, initial subsidence just after snowmelt may not be fully captured at some sites, which could affect comparability. At coring site S5, InSAR subsidence remained negligible until after local snowmelt, which occurred around 7 June 2023."*

Thaw penetration and subsidence rates

The role of pore ice in the nature of the subsidence curves over the summer could be better presented and discussed in relation to established theory and observations. During the thawing season, some of the sites follow a characteristic exponential decay curve in subsidence in layers where excess ice is not present. This generally follows the Stefan equation that described expected progression of active layer thawing. The pattern has been examined in relation to subsidence previously, for example in this paper that you cite in your discussion: https://tc.copernicus.org/articles/14/1437/2020/, and in other applications involving permafrost thaw. So, when the authors indicate that pore ice contributes to subsidence in a more "continuous manner" l. 346, I don't think this is the best way to describe it. Though it is continuous, the rate is not. Furthermore, indicating that Schuh et al. 2017 confirmed the inverse relationship with ice content, while not inaccurate, is perhaps not the best option to support the relation observed. An inverse relation exists in the absence of excess ice, and governing equations that relate the thaw rate to the square root of time significantly predate the cited study. So, I suggest familiarization with the

Stefan equation and the expected exponential decline of thaw progression with time and edits to associated text, and reference to pertinent literature.

Thank you for pointing this out. Our intent with saying "pore ice contributes in a more continuous secondary manner" was based on the fact that pore ice is less likely to appear as heterogenous layers with highly variable ice content compared to excess ice, and thereby does not cause step-wise subsidence signals. Also considering the feedback from reviewer 1, we have updated the discussion in L. 346 to:

> "We observed seasonal variations in the InSAR subsidence patterns, which especially align with the distribution of excess ice in the active layer. *Pore ice also contributes to the subsidence, but in a secondary manner, which more closely aligns with the thaw progression predicted by the Stefan equation (e.g. Fig. 5b: site E3).*"

We have also updated the reference for the inverse relationship between ice content and thaw progression to French (2007a).

Figures

Line 242 indicates that "Due to the dominant contribution of excess ice melt and drainage to the total subsidence…". However, this isn't presented or established until Figure 6, so the statement is confusing to the reader because this result hasn't yet been shown. Figure 6 showing this partition should come earlier. I understand this would entail showing the InSAR max earlier than the InSAR section, which is not ideal, but I think it is perhaps better overall because at least then the readers will be familiar with the expected subsidence in section 3.1.

We agree that the statement in its current form is confusing, as it presents information which first makes full sense when Figure 6 is shown. However, instead of moving Figure 6, we reformulated the statement in line 242 to:

> "The comparison between the measured in-situ ALT and the expected subsidence from pore ice melt revealed a strong negative correlation ($R^2 = 0.71$, Pearson's $r = -0.84$, Fig. 4A). *Conversely, there* was no correlation between ALT and the expected subsidence from only excess ice melt and drainage (Fig. 4B). *Overall, there was a poor correlation between the ALT and the total expected subsidence ($R^2 = 0.03$, Fig. 4C). This result suggests that the excess ice (not correlated with ALT, Fig. 4B) has a more significant contribution to the total expected subsidence than the pore ice (correlated with ALT, Fig. 4A). Results from Section 3.2 confirm this hypothesis."*

It would be useful to have a figure showing photos of some of the site types: e.g., an Eolian one in ice wedge polygon fields, and alluvial example, slope/solifluction example, etc.

We have created an additional figure for the supplement (see below, Fig. S1), which shows photos from each site, and we are now referring to it in L. 113:

> *"Field pictures of the 12 coring sites are shown in Fig. S1."*

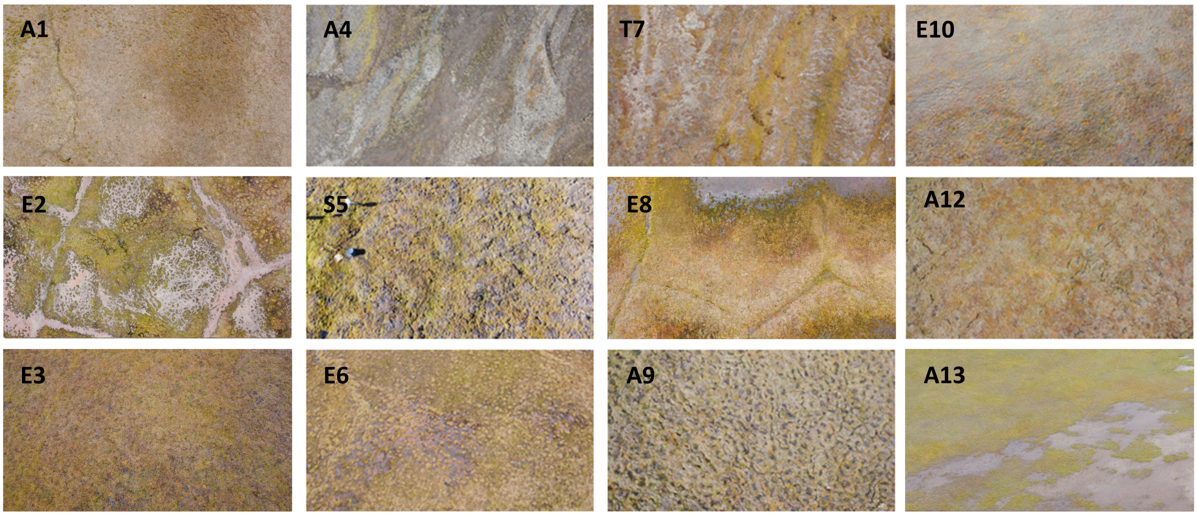

Figure S1: Pictures of all coring sites from September 2023, taken with a drone approx. 20 m above ground (width of picture = approx. 30-40 m). The respective coring site is in the centre of each image.

Minor comments/suggestions:

Line 16: change “allowing to estimate” to “allowing estimation of”
Updated.

Line 18: delete “thickness” after “active layer”.
Updated.

Line 31. I presume here you mean increases in ALT are “largely influenced by the presence of ground ice” but the link is not explicit nor explained. Suggest restructuring this sentence.
We have removed the second half of the sentence, since the influence of ground ice on the ground thermal regime is explained in the following sentences. Additionally, we have added a reference to the GCOS ECV parameters, of which one is ALT:

> “An increase in the active layer thickness (ALT) serves as a key indicator of permafrost degradation *(GCOS, 2022).”*

Line 34-35 “Long-term ground ice loss is associated….” This sentence should be supported by appropriate reference(s). This recent one covers the topics described: https://doi.org/10.1002/ppp.2261
Thanks, reference added.

Line 37. Consider indicating specifically which traditional methods you mean (e.g., probing, thaw tubes, dGPS surveys, etc).
We have updated the sentence to:

> “Traditional methods for monitoring ALT and mapping ground ice *(e.g. thaw depth probing, temperature monitoring in boreholes, drilling and geomorphological surveys, thaw tubes)* typically rely on labour-intensive, time-consuming in-situ surveys.”

Line 39. Consider examining use of “utilized” throughout the text and replace with “used”, which is more concise and generally has the same meaning.

Updated to “used” throughout the text.

Line 43. “stronger consolidation” suggest changing this to “larger magnitude thaw subsidence”.

We have updated this sentence to:

> “*The melting of excess ice has an even more pronounced effect, as the loss of ice that exceeds the soil’s pore space can cause significant subsidence when the resulting meltwater drains away* (Morgenstern and Nixon, 1971).”

Note: In the previous author comment (AC2), this comment was mistakenly not updated to reflect the changes made in response to AC1. We have now updated this here in the author’s response.

Line 44. “likely drains” suggest changing to “may drain away.”

Updated.

Figure 1. The stream and lake (reservoir) colour is the same as InSAR heave; suggest changing all waterbodies to a colour not in the InSAR legend. The inset map of Svalbard is very hard to discern, and the contrast between land and water is poor. Consider enlarging and colour changes.

We modified Figure 1 (see below) to account for your comments.

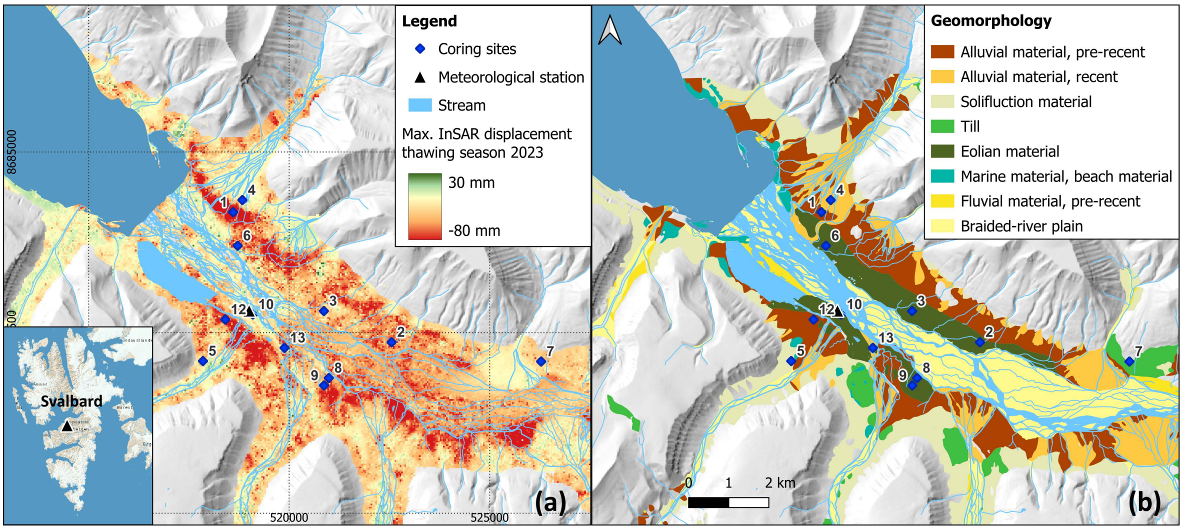

Figure 1: (a) The Adventdalen study area with the location of the coring sites and their label names. The background is the maximum InSAR seasonal displacement of 2023. Subsidence is shown with negative values (red) and heave with positive values (green). Note that the colour scale is saturated for visualization. (b) Simplified geomorphological map of the study area with the main sediment deposits (Rouyet et al., 2019; modified from Härtel and Christiansen, 2014). Background: hillshade of a digital elevation model (Norwegian Polar Institute, 2014b). Coordinate System: WGS 1984 UTM 33N.

Line 105. Add “anticipated” before “InSAR subsidence magnitudes” since 2023 magnitudes were not known when sites were selected.

Added.

Line 111. Clarification on sampling – "soil moisture conditions were considered to include dry and wet locations". Explain specifically how soil moisture conditions were considered. I presume there were not soil moisture instruments at each site, and that this was done based on some visual or field interpretation?

Indeed, the soil moisture conditions were assessed based on the NDWI remote sensing index applied to Sentinel-2 imagery from summer 2022 (Gao, 1996). We have extended the sentence to:

> "Further, soil moisture conditions were considered to include dry and wet locations *based on the NDWI (Gao, 1996) remote sensing index of Sentinel-2 imagery from summer 2022*."

Line 117. "0.5 m core" is ambiguous, indicate this is the core length.

Updated the sentence to:

> "The drilling was conducted using a STIHLTM BT 121 Earth Auger equipped with 0.5 m *long* core barrels.".

Line 119. "freezing container" is unclear. Do you mean a cooler? Or something that actively freezes contents?

Yes, we do mean a container with an active cooling system. We have updated the sentence to:

> "Cores were retrieved in 5–30 cm sections and were immediately packed, air-sealed, and stored at the end of each field day in a container *with an active freezing system."*

Line 127. Add "area" after "surrounding".

Added.

Table 1. Header for column 5 does not indicate that the information at the top of the cell is the ALT measurement date. Row E8 – "Drill location in center" of what (ice wedge polygon)?

Thank you! We have updated the header for column 5. We also added in row E8 and E2 "*Drill location in centre of ice wedge polygon.*".

Line 139. What classification was used to classify cryostratigraphy? A reference should be provided.

We used the classification from French and Shur (2010) and have updated this sentence to:

> "The intact subsections were scraped, the cryostratigraphy classified *based on French and Shur (2010),* and visual ice content and sediment type described."

Line 151. "the factor 1.09 represents the density of ice relative to water". The equation deals with volumes, so it is better to say that the 1.09 is to "estimate the equivalent volume of ice" from the water volume as Kokelj and Burn did.

We have updated this sentence part accordingly to:

> "and the factor 1.09 *is to estimate the equivalent volume of ice from the water volume."*

Line 156. This should be 9% shouldn't it? This is why the factor in Eq. 2 is 1.09. This would also affect your derivation of Eq. 4. You have 0.92 in Eq. 4 but this should be 0.912 I think, so rounded to 0.91. So, you will likely have to redo your calculations though they won't differ much. I think the confusion/error lies in the fact that the percent difference is 9.2% (e.g., see percent difference equation at https://www.calculatorsoup.com/calculators/algebra/percent-difference-calculator.php). This depends on if you are converting from a water volume to an ice volume, or vice versa. From water to ice, the volume change is indeed +9.1 % relative to the initial water volume. From ice to water, the volume change is -8.3 % relative to the initial ice volume. These calculations are based on a water density of 1000 kg/m^3 and an ice density of 917 kg/m^3. Since Eq. 4 considers the transition from an initial ice volume to a water volume, the volume reduction is ~8 %. We therefore did not adjust our calculations.

Line 164. Change "which" to "that".
Updated.

Line 176. Can you clarify to the reader whether "temporal baseline" is synonymous with the return frequency of the satellite?
We have updated this section to:

> "For summer 2023, only Sentinel-1A imagery was available, *which has a revisit period of 12 days. Therefore, the minimum temporal baseline for constructing interferograms was 12 days.* To mitigate temporal decorrelation and phase ambiguities from strong subsidence in the exceptionally warm summer 2023, a maximum temporal baseline of 24 days was used."

Line 227. Indicate it's expected for two-sided freezing specifically.
Thanks, the sentence is updated to:

> "This pattern is consistent with the expected distribution of ground ice from *two-sided freezing* (French, 2007).".

Line 295. Delete "coring" and "located", these words are not required. Also, suggest changing "rather" to "mostly" in second sentence.
We changed "rather" to "mostly".
We did not remove "coring" in "A1 coring site", since we use the term coring site throughout the manuscript when we refer to our sites.

Line 316. Add "sand" after "dry".
Added.

Line 317. Add "early in the thawing season" after "quickly"
Added.

Figure 8. "Grain type" figure title should perhaps be changed to "Material type" because organic is not a grain type, and neither is ice lens, or disturbed.
We have updated this in Fig. 8, S5, S6, and S7.

Line 341. Add "from the active layer and upper permafrost" after "in situ-ice contents".
Thanks, added.

Line 357. Remove "excess" because ice-rich permafrost, by definition, includes excess ice. Check this throughout.

We have updated "excess ice-rich" to "ice-rich" throughout the manuscript. We only kept "excess ice-rich" when referring to results of the study of Zwieback and Meyer (2021), who used this term in their work.

Line 365. I think it would be good to give a few examples of sites from Figure 6 where it dominates (e.g., the A sites).

We are now referring in this line to Figure 6:

> "The expected subsidence from pore ice melt lies within a plausible range, yet our results indicate that excess ice melt and meltwater drainage can significantly dominate the expected subsidence signal *(Fig. 6: A1, A9, A12).*".

Line 371. This part could be strengthened by giving examples of the magnitudes/proportions accounted for by excess ice in the late thawing season.

We cannot provide exact numbers for this, since we do not have thaw front progression data for the different sites. We therefore only refer to the comparison figures per coring site, which display the InSAR subsidence time series and the respective in-situ ground ice content measurements:

> "The findings of Zwieback and Meyer (2021) align with our results, which *indicate* that excess ice can be the major contribution to the InSAR late-season subsidence signal *(Fig. 8, S5-7)."*

Line 393. "drainage variations can control the presence or absence of excess ice". While I don't disagree, because fundamentally moisture is required for ice formation, based on detailed coring I conducted in the eolian sediments (a GSC Open File is now in press), excess ice was mainly controlled by grain size of the eolian materials, regardless of present-day moisture conditions in the polygons (the polygon with standing water and wet active layer had on average half the excess ice content in the top 1 m of permafrost). Siltier layers, which imply slower rates of loess aggradation and different climatic, eolian source conditions, and likely microtopography in different time periods, were associated with higher ice contents. You have not measured "lateral drainage" (l. 395) in this study, though you may have observed it at the surface. Also, it is hard to know whether drainage conditions at the surface today reflect those when the ground ice aggraded in the past, as the syngenetic polygons fields are dynamic. Therefore, you cannot confidently say that the drainage conditions are controlling the ice contents at much depth beyond the current permafrost table; this text should be modified.

Thank you for sharing your observations. In the meantime, your GSC Open File was published, and we have included it. We have updated this section to:

> "Our data *exemplify* that, even within the same type of sediment deposit *excess ice presence can vary in the active layer and uppermost permafrost*. For instance, eolian fine-grained loess terraces show significant variability: some have very low ground ice contents and lack excess ice (e.g., coring site E10), while others have large ground ice contents, particularly in the lower active layer (e.g., coring sites E2, E8) (Fig. 8, S5). *This is likely related to drainage and grain size variations (O'Neill et al. 2025), as well as the site-specific formation history of the sediments (Gilbert et al. 2018)."*

Line 412 last word: Should be "basis".
Updated.

Line 435. This sentence suggests that InSAR time series could be used in conjunction with models incorporating ice segregation processes/excess ice content. The reader is left confused what the objective of such an exercise is. Is it to use the excess ice content from the model to validate an InSAR signal? If so, this would surely not be appropriate given that such models cannot accurately capture conditions that lead to excess ice formation over hundreds or thousands of years, and thus cannot produce accurate estimations of ground ice at the site scale. This is discussed in, e.g.,: https://agupubs.onlinelibrary.wiley.com/doi/full/10.1029/2023JF007262. If this is not the intent, then can you please clarify specifically what you mean in terms of "combining" InSAR with such models?
We updated this sentence to:

> *"Future work should investigate integrating InSAR time series with numerical models that simulate ice segregation processes and excess ice content (e.g., Aga et al., 2023). This could help constrain model parameters or improve process representation by leveraging InSAR-derived surface deformation patterns, potentially through data assimilation techniques (e.g., Aalstad et al., 2018)."*

Line 444. At this time of the year, the whole active layer is frozen, so the meaning of this sentence is unclear. Please clarify what you mean in terms of where/how water is moving at this time in relation to the anticipated ground temperature gradient(s) from the ground surface to the upper permafrost at the time of drilling.
We did not mean that ground ice contents would change at the time of drilling, but rather that at the beginning of the thawing season water could infiltrate into the ground and refreeze, thereby changing the in-situ ground ice contents. Such infiltration of meltwater and refreezing has been observed before and our cores would in such a case underestimate the amount of in-situ ground ice.
We have updated this sentence to:

> "The cores were collected at the end of the freezing period, yet water *infiltration at the thaw season onset* into the frozen part of the sediment column could have caused aggradational ice growth after core collection (e.g. Mackay, 1983, *Scherler et al. 2010*)."

Line 462. Remove "excess".
Removed.

Line 463. Why might probing be less precise than borehole measurements? This is highly dependent on the spacing of thermistors, and the material being probed in. This should either be explained further or amended.
We have updated this sentence to:

> "In this study manual probing was employed, which allows a more widespread coverage of the pixel, *yet is dependent on rather fine-grained ground conditions. Additional borehole temperature measurements could have aided in determining the ALT and provided thaw progression measurements throughout the thawing season, which would have been valuable for comparison against the InSAR subsidence progression."*

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