Answers to Reviewer 2

In the following, we answer (shown in blue) the copy-pasted comments from the reviewers (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.

Based on feedback from both reviewers, we have added additional supplementary figures, which are currently numbered sequentially after the previously existing ones. In the revised manuscript and supplement, we will update the numbering and order of the supplementary figures to maintain a logical sequence when referring to them in the revised manuscript.

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 three days after the snow melt-out date at the Adventdalen meteorological station. Sentinel-2 imagery confirms that all sites except 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 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. S9), which shows photos from each site.

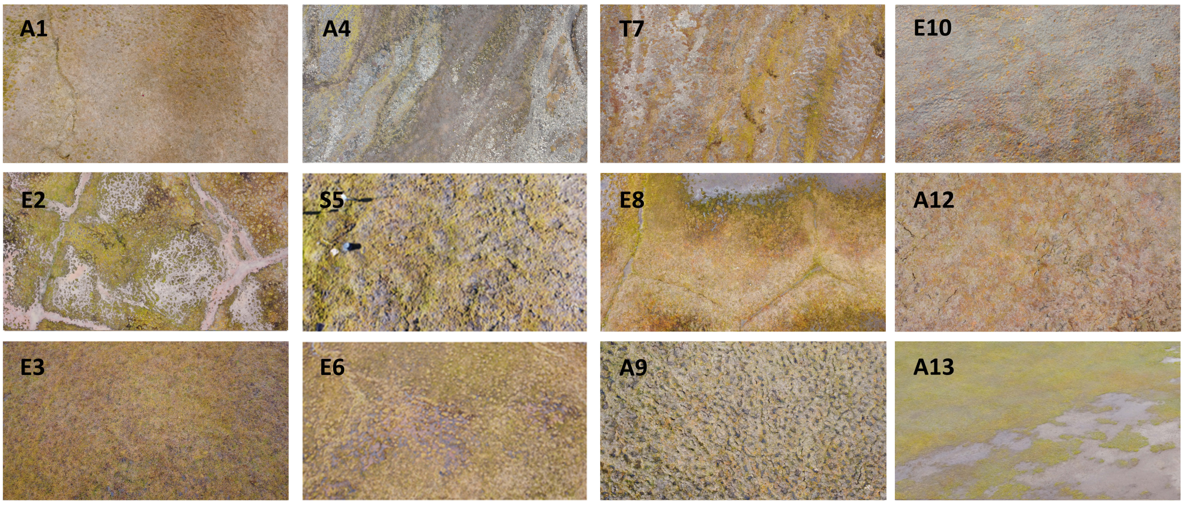

Figure S9: Pictures of all coring sites from September 2023, taken with a drone approx. 20 m above ground (width of picture = approx. 30-40 m). The respective coring site is in the centre of each image.

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:

“Excess ice melt can cause a *larger thaw subsidence magnitude*, since the resulting meltwater exceeds the soil pore space and may drain away (Morgenstern and Nixon, 1971).”

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.

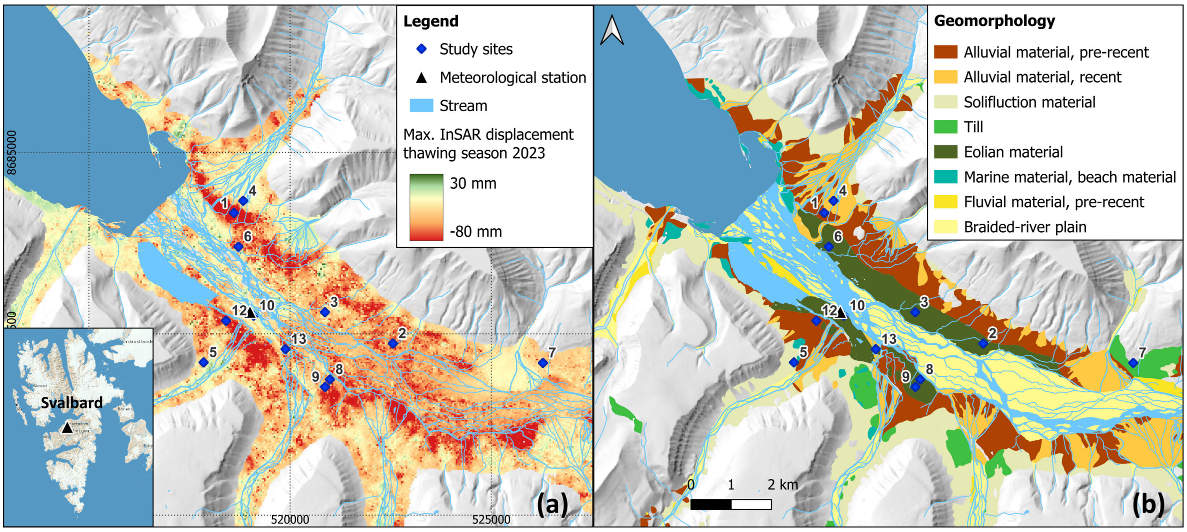

Figure 1: (a) The Adventdalen study area with the location of the coring sites and their label names. The background is the maximum InSAR seasonal displacement of 2023. Subsidence is shown with negative values (red) and heave with positive values (blue). Note that the color scale is saturated for visualization. (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."*

References:

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O'Neill, H. B., Gilbert, G. L., and Christiansen, H. H.: Site-scale variation in ground-ice content and physical properties of loess in permafrost, Svalbard, High Arctic, https://doi.org/10.4095/pfmq507fg6, 2025.

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