This paper compares remotely sensed seasonal ground surface displacement, measured using the InSAR technique with Sentinel-1 (C-band) data, to soil texture and ground ice profiles obtained from 12 core samples in contrasting landform locations in Adventdalen, Svalbard. While there is a rapid increase in studies dealing with permafrost InSAR (particularly SBAS-type), field validation studies remain limited. This research offers significant progress in explaining InSAR spatial variations using detailed frozen-ground core analysis on a watershed scale. I am glad to see 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. Although I identified several weaknesses and limitations, I support the publication of this paper following necessary revisions outlined below.

<Cause of Frost Heave/Seasonal Thaw Settlement and Ice Content Values>

Classic studies on frost heave mechanisms (e.g., Taber, 1930) indicate that frost heave is primarily caused by water redistribution and ice segregation during soil freezing, while the contribution from water expansion due to phase change is generally negligible in most natural surface ground layers (e.g. Bai et al., 2020). Consequently, seasonal thaw settlement should be explained mainly by the thickness of ice lenses (i.e., excess ice). Discussions on frost heave or thaw settlement based on a model of water phase expansion/shrinkage are therefore inconsistent with these fundamental principles.

While the manuscript acknowledges that pore ice melt is a secondary factor contributing to thaw settlement, its estimated contribution of over 20% to total subsidence (as shown in Fig. 6) may have been overestimated. The formulation in Eq. (4) is not based upon the above-mentioned classical frost heave works and requires further justification. Could you provide references or evidence supporting such a high contribution from pore ice melt?

I suggest revising the Introduction, Discussion, and Conclusion sections to incorporate these considerations and align with established frost heave theories. Additionally, the EIC values in Fig. 8 and S5 require verification. For instance, certain upper horizons of silty soil with high VIC (e.g., A1/E9/E10/A12) show minimal or no excess ice, which seems unusual to me. Similarly, some EIC values in transient layers match VIC levels (e.g., A1/E2/E3/S5/E9/A12), which is strange. If available, could you include cross-sectional photos of the cores to validate these observations?

<Uncertainty Discussion>

As acknowledged in the manuscript, a key limitation of this study is the low representativeness of the in-situ data, with only one core analysis being used to represent a much larger footprint of remote sensing data. Furthermore, the uncertainty discussion focuses solely on InSAR displacement measurements and ice content measurements from the sample cores. The cited 1 cm InSAR uncertainty originates from studies in mid- to low-latitude regions, which differ significantly from Arctic frozen ground conditions. In Arctic environments, issues such as decorrelation and unwrapping errors are exacerbated by surface moisture, snow cover, and vegetation influences. Additionally, the spatial heterogeneity of ground ice content in Arctic regions is far greater than what might be inferred from similar depositional conditions.

To strengthen the discussion, I suggest incorporating spatial variations of InSAR subsidence (categorized by sedimentation-based land classifications), ALT measurements (CALM data and your observations), and ground ice content (potentially derived from literature). This would provide a more comprehensive perspective on the uncertainties and enhance the paper's robustness.

For the InSAR analysis, it would be beneficial to elaborate on how highly decorrelated interferograms were handled. Please clarify the threshold coherence values and justify the acceptance of a maximum temporal baseline of 24 days. Including a summary of coherence statistics for the accepted interferograms would greatly aid reproducibility and facilitate future comparisons.

This additional information would address current gaps in the uncertainty discussion and provide critical context for interpreting the results.

<Method>

L. 125: How many points were measured for ALT? Could you include statistics in Fig. S3?

L. 128: Why is it necessary to adjust the original ground surface height based on a single-year core analysis? Frost heave caused by ice segregation can vary significantly from one winter to the next, depending on climatic and hydrological conditions. For seasonal analyses, it would be more appropriate to base the ground surface level on the moment of maximum thaw, as this approach minimizes the influence of interannual variability in frost heave. However, such an adjustment could be justified if the primary focus is on inter-annual subsidence trends, such as those driven by thermokarst development or long-term permafrost degradation.

(Section 2.3 Analysis of Ground Ice Content)

How was the volumetric ice content (VIC) determined for disturbed samples? Please provide a detailed explanation of the method used to measure the volume of sediments and supernatant water. This additional detail would help clarify the accuracy and reliability of the VIC measurements.

I find it difficult to agree with the interpretations in L. 346–347 and L. 371–373, as they appear to rely on the assumption that water phase-change expansion contributes significantly to ground ice dynamics. This assumption is inconsistent with established studies, which typically suggest that water phase expansion plays a negligible role compared to ice segregation processes. Could you provide further clarification or supporting evidence for this claim?

Additionally, the approximate 8% contribution from Liu et al. (2012) is mentioned but not sufficiently explained. Please elaborate on how this value was derived and its specific relevance to your study.

<Additional Suggestions and Corrections>

L. 256: How exceptional were the 2023 ALT values? Could you compare them with previous years' data, if available?

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

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?

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)?

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).

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.

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.

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.

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).

L. 412–413: "Negative" should be replaced with "positive"? It seems to be a contradiction with the subsequent sentence. Please revise for consistency.

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.

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?

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

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.

References

- Taber, S. (1930). *The mechanics of frost heaving.* J. Geol., 38, 303–317.
- Bai, R. et al. (2020). *Investigation on frost heave of saturated–unsaturated soils.* Acta Geotechnica, 15(11), 3295–3306. https://doi.org/10.1007/s11440-020-00952-6
- Shur, Y., Hinkel, K. M., & Nelson, F. E. (2005). *The Transient Layer: Implications for Geocryology and Climate-Change Science.* Permafrost and Periglacial Processes, 16, 5–17.