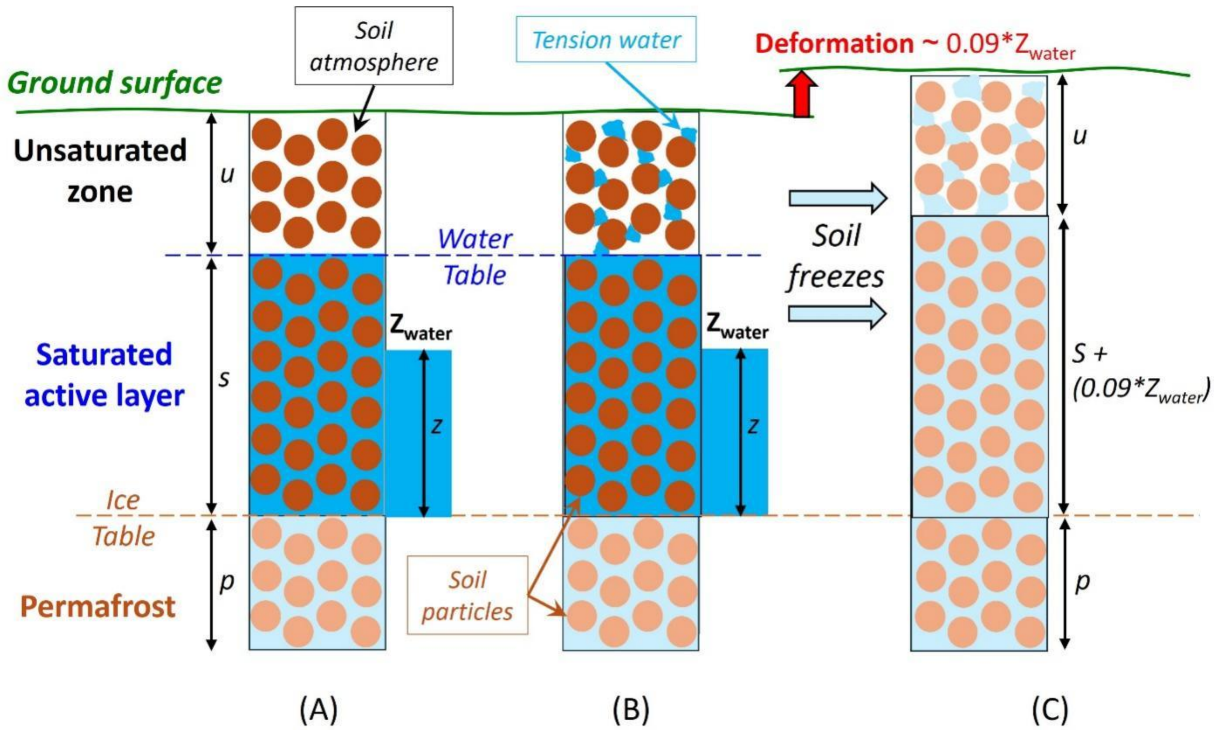
We appreciate the time that the reviewer put into this paper. Below, we provide detailed explanations of how we plan to revise the manuscript. The reviewer's comments are in *blue italics*, our responses are in black, and relevant text from the manuscript, whether newly added or edited, is **bold**. We are grateful for the reviewer's insightful feedback.

This manuscript presents a methodology for estimating the volumetric storage of soil water within the active layer of periglacial landscapes using InSAR observations. The method from a prior proof of concept publication is re-introduced and applied to a larger area of undisturbed tundra in the vicinity of Toolik Field Station on the Alaskan North Slope using L-band ALOS data acquired between 2006 and 2010. The InSAR-estimated equivalent water depth estimates are compared with in situ estimates, showing strong overall agreement. Lastly, there is a discussion of sources of interferometric error due to DEM errors and SAR image to DEM pixel misregistration.
Overall, I think that this manuscript presents several relevant contributions. However, I would first recommend several revisions before the manuscript can be considered for publication. I have a detailed, point-by-point list of specific comments below, but these comments generally fall under a few broader thematic points. These points are:

1. *The manuscript would benefit from a more precise description of what the Zwater parameter physically corresponds to. From my reading of prior work, the Zwater parameter is the equivalent water depth of all pore-bound water undergoing seasonal freeze/thaw within the active layer. However, the Zwater parameter is variously referred to in slightly different ways throughout the manuscript, which could raise questions from readers as to what exactly Zwater physically corresponds (i.e., is this the equivalent water depth, or the thickness of the active layer that is fully saturated?) I have a few specific suggestions to address this.*
    1) *First, I would recommend including a physical cartoon or schematic that unambiguously illustrates the Zwater parameter. For example, Figure 2 could be modified by including two additional cartoon cross-sections next to the original soil profiles, with the total porebound water separated from the soil matrix into a single homogenous column of pure water, with the depth of this water column labeled Zwater.*
    2) *I recommend choosing a standard way to describe Zwater and being consistent throughout the manuscript. At various times Zwater is referred to as 'saturated active layer soil water storage', 'saturated soil water thickness', 'soil water equivalent depth', 'soil water depth in the saturated active layer', 'soil water column that experiences the ice-to-water phase change in the saturated active layer (denoted as zwater)', 'active layer water storage' and 'saturated active layer soil water column'.*

<u>Response:</u> In both Chen et al. (2020) and the current paper, we stated that InSAR measures the total soil water storage in the saturated active layer. InSAR thaw subsidence is not sensitive to soil water stored above the water table (tension water or unsaturated-zone pore water, Figure 2). Because the porosity in the organic soil layers is high (~0.78-0.98), water in the unsaturated zone can expand to fill the empty pore space (soil atmosphere) during freezing without contributing to surface deformation. We acknowledge that Chen et al. (2020) did not explicitly discuss the relationship between InSAR thaw subsidence observations and the total soil water stored in the saturated active layer (that was not the main point of that paper). An important contribution of this current paper is to establish the conceptual model that explains what InSAR thaw subsidence over permafrost terrain measures as described in Section 2.1.

Following the reviewer's comment, we updated Figure 2 as:



The figure caption of Figure 2 was updated as:

Figure 2. Conceptual diagram of how the depth of saturated water (Z_{water}) affects soil surface deformation. The soil is divided into three zones: (1) the surface active layer unsaturated zone (*thickness of u)*, which contains soil particles (dark circles), soil atmosphere (white open space), and it may contain tension (capillary) water (shown in light blue in column (B)); (2) the water-saturated active layer (*thickness of s)*, the upper surface of which defines the water table and the lower surface defines the ice table, which separates the thawed active layer from frozen ground (or the permafrost layer at maximum annual thaw depth); and (3) the permafrost (*thickness of p)*, which may or may not be saturated with water as ice. In column (A) the Z_{water} thickness represents the total amount of water in the saturated active layer. In (B), the saturated active layer is the same thickness as in column (A), but there is tension water in the upper unsaturated zone. However, Z_{water} thickness is the same in columns (A) and (B), independent of differences in tension water. The reason is shown in (C), where the entire soil column has now frozen. The saturated water freezes and the expansion heaves the soil column above and the ground surface (*thickness s + 0.09*Z_{water}*), while the tension water freezes but expands into pores containing soil atmosphere and thus does not contribute to deformation of the ground surface (*thickness u does not change)*.

In the revised paper, we improved the clarity of Section 2.1, and we used the term ‘saturated soil water storage in the active layer’ or ‘the soil water stored in the saturated active layer’ consistently throughout the manuscript.

2. *As currently written, the section of the manuscript on Zwater estimation and the section on DEM errors feel rather disjoint from each other. I think that the manuscript would benefit from a tighter coupling of these two elements, as well as a more thorough and explicit*

discussion of the novel advancements made in this manuscript on top of the Chen et al. 2020 proof of concept manuscript.

1) *The manuscript would benefit from a more explicit framing of the novel contributions that this manuscript introduces to the field over the prior Chen et al. 2020 study. The discrete dem error due to pixel misregistration analysis is one such novel contribution. However, I would also highlight any advancements made in the Zwater estimation process introduced.*
2) *Currently, the Zwater and DEM error sections read as pretty disjoint from each other; these sections could potentially be submitted separately as two stand-alone papers; or, perhaps more discussion linking them could be included in the revised manuscript. For example, to what degree to observed DEM errors propagate into an effective uncertainty for Zwater estimates? Bridging these two sections together through an uncertainty propagation analysis would be one such way to strengthen the cohesiveness of the manuscript.*

Response: Following the reviewer's recommendation, we rewrote the last paragraph of the introduction to better illustrate the new contribution made in this paper on top of the Chen et al., (2020) proof of concept paper:
"Our recent study found that the signal amplitude of the seasonal thaw subsidence is proportional to the amount of water stored in the saturated active layer at the end of a thaw season (Chen et al., 2020). In that paper, we further established a conceptual model that relates InSAR seasonal thaw subsidence observations to the amount of water in the saturated active layer. In the current paper we advance InSAR techniques for the high-resolution mapping of water storage above-permafrost. To demonstrate this, we mapped soil water stored in the saturated active layer using ALOS PALSAR data over a much larger area in the Arctic Foothills than in Chen et al. (2020). We validated the InSAR results using in-situ soil measurements collected at more than 200 remote sites within ~ 100 km of the Toolik Field Station as well as optical imagery and land cover maps. Our results show that InSAR soil water storage estimates derived from two separate satellite frames are consistent with in-situ observations under different vegetation covers. An important contribution of this work is on uncertainty quantification. We determine the primary error sources in Toolik ALOS PALSAR Line-Of-Sight (LOS) measurements, and we discuss how errors in InSAR LOW measurements can be linearly related to errors in soil water storage estimates.".
We note that the uncertainty propagation analysis as suggested by the reviewer is covered in Section 2.2 following Equation (4), and InSAR error sources are discussed in Section 2.3. The results on the uncertainty analysis are presented in Section 3.2 and Section 3.3.

Individual comments are organized by section below:

Introduction:
Page 1, line 22: "Whether the carbon held by the active layer soils will be transformed to carbon dioxide or methane (a more powerful greenhouse gas), or whether it will flow towards rivers and lakes as dissolved carbon in groundwater, depends largely on the wetness or dryness of the active layer (i.e., how much water is stored)." I would recommend including a citation or reference to a few relevant papers that support this statement, as it is a central point that underlines much of the scientific justification for this manuscript.

Response: We included two new citations in the sentence on Line 22-24:

"Whether the carbon held by the active layer soils will be transformed to carbon dioxide or methane (a more powerful greenhouse gas), or whether it will flow towards rivers and lakes as dissolved carbon in groundwater, depends largely on the wetness or dryness (i.e., how much water is stored) of the active layer (Bond-Lamberty et al., 2016; Taylor et al., 2021)."

References:

Bond-Lamberty, B., Smith, A. P., & Bailey, V. (2016). Temperature and moisture effects on greenhouse gas emissions from deep active-layer boreal soils. Biogeosciences, 13(24), 6669–6681. https://doi.org/10.5194/bg-13-6669-2016

Taylor, M. A., Celis, G., Ledman, J. D., Mauritz, M., Natali, S. M., Pegoraro, E. -F., Schädel, C., & Schuur, E. A. (2021). Experimental soil warming and permafrost thaw increase CH_4 emissions in an upland tundra ecosystem. Journal of Geophysical Research: Biogeosciences, 126(11). https://doi.org/10.1029/2021jg006376

Page 2 line 37: "Because ice density is less than water density (and thus ice volume is greater than water volume), the land surface subsides as the active layer thaws from winter to summer (Liu et al., 2010)" I would recommend being explicit here and stating that the amount of surface subsidence depends upon the overall volumetric water content of the thawing permafrost, as this further motivates the proposed methodology. One second thought, this may not be necessary to state here, as you later state it on line 44.

Response: Yes, we did state this on line 44, and as noted earlier, we rewrote the last paragraph of the introduction to better illustrate the new contribution made in this paper on top of the Chen et al., (2020) proof of concept paper.

Methods:

An important point to raise is that, in addition to assuming stationary thaw conditions from year to year, the interannual stacking method also implicitly assumes no variations in excess ground ice content from year to year. While I think that this is a justifiable assumption, it might be worth explicitly mentioning this, and discussing recent work that has demonstrated that InSAR is sensitive to interannual variations in excess ground ice formation and melting: https://doi.org/10.5194/tc-15-2041-2021, https://doi.org/10.1029/2023WR035331

Response: We agree with the reviewer that interannual variation in excess ground ice formation and melting are key processes that can lead to variations in observed seasonal thaw subsidence from year to year. The conceptual model described in Section 2.1 can be used to study inter-annual variation in excess ground ice formation and melting, when high quality InSAR thaw subsidence observations of a single thaw subsidence are available. We clarified at the end of Section 3.1:

"Due to the limited ALOS PALSAR data availability, the investigation of inter-annual variability of InSAR thaw subsidence patterns is outside the scope of this work. Future work can focus on studying how the signal magnitude of seasonal thaw subsidence changes over multiple years using Sentinel-1 data collected with 6-12 day revisit cycles (Zwieback and Meyer, 2021; Zwieback et al., 2024)".

References:

Zwieback, S., & Meyer, F. J. (2021). Top-of-permafrost ground ice indicated by remotely sensed late-season subsidence. The Cryosphere, 15(4), 2041–2055. https://doi.org/10.5194/tc-15-2041-2021

Zwieback, S., Iwahana, G., Sakhalkar, S., Biessel, R., Taylor, S., & Meyer, F. J. (2024). Excess ground ice profiles in continuous permafrost mapped from InSAR subsidence. Water Resources Research, 60(2). https://doi.org/10.1029/2023wr035331

Section 2.3:
-What are typical pixel misregistration values for ALOS? They are surely processor dependent, and the InSAR processor used is not explicitly mentioned. However, I imagine they are still relatively small, no more than 1 or 2 pixels in any direction, and usually sub-pixel.

Response: We clarified in Section 2.3:
"In this study, we employed the same image co-registration routine as the standard InSAR processing software such as the InSAR Scientific Computing Environment (ISCE) (Rosen et al., 2012) or GMTSAR (Sandwell et al., 2011). The 2-D cross-correlation method for image alignment can achieve sub-pixel accuracy in most cases. However, the alignment can be worse than 1 pixel, because SAR images and DEM data were acquired from sensors with different spatial resolutions and imaging geometries".

References:
Rosen, P. A., Gurrola, E., Sacco, G. F., & Zebker, H. (2012). The InSAR Scientific Computing Environment. In *EUSAR 2012; 9th European Conference on Synthetic Aperture Radar*, pp. 730–733.
Sandwell, D. R.Mellors X.Tong M.Wei and P.Wessel (2011), Open Radar Interferometry Software for Mapping Surface Deformation, *Eos Trans. AGU*, 92(28), 234.

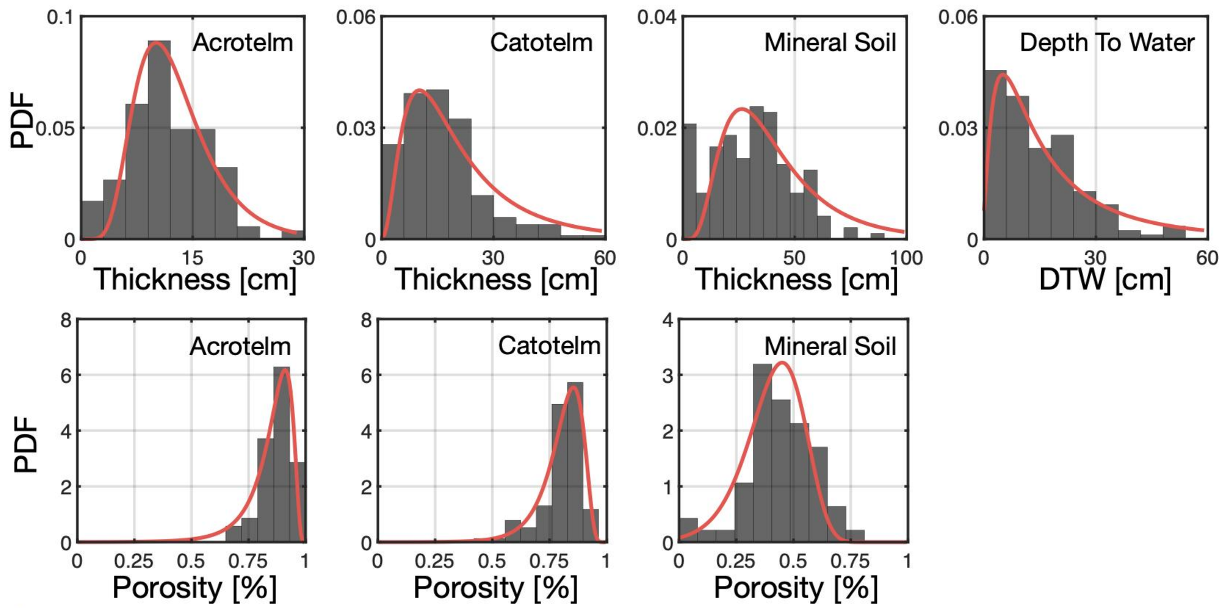
Page 10 line 252: This is a minor point, but the comparison between the InSAR pixel (10-100 m) and field measurement (30x30 cm^2 area plot) is a 'linear to area' comparison.
Response: We updated the text: "*A pixel in an InSAR-derived deformation map is ~ 100-by-100 meter, while field measurements were collected at sites with size ~1000 cm^2 (30-by-30 cm plots)*". Note that we are comparing a sampling area to a pixel area.

Page 11 line 260: I suggest removing, or restating 'waviness of the ice-table', as it is not precise.
Response: we removed 'waviness of the ice-table'.

Page 11 line 260: "we found that the fitted PDFs stayed mostly the same" This statement is also imprecise. Can you quantify what 'mostly the same' means?
Response: Below we include the PDF fitting results as presented in Chen et al., (2020). We clarified that:
"To reduce estimation bias, we targeted specific vegetation cover types and soil layers needing larger sample sizes over time to improve statistical robustness. The PDF fitting results changed very little after a second year of sampling, indicating that the sample size in this study is sufficiently large to capture the statistical characteristics of soil properties".
We inspected the histogram fitting based on visual inspection. Additionally, we calculated the Bhattacharyya distance between the fitted PDFs. The calculated Bhattacharyya distance (Bhattacharyya, 1946) between the 2018 and 2019 fitted PDFs ranged from 0.07 to 0.12, suggesting that the sample size in this study was large enough to reliably represent the soil properties at the regional scale."

Reference:

A. Bhattacharyya, "On a measure of divergence between two statistical populations defined by probability distributions" Bull. Calcutta Math. Soc. , 35 (1943) pp. 99–109



Results and Discussion:

Section 3.3:

Page 18 line 387: Why do these three scenes likely exhibit more severe pixel misregistration errors compared to the other scenes?

<u>Response:</u> These scenes tend to have a relatively large spatial baseline with respect to the reference orbit. In this case, this leads to more noticeable image distortion, which makes it more difficult to track the pixel offset.

Can an alternative (or compounding) interpretation for the observed phase difference between east and west facing slopes be due to viewing geometry rather than DEM misregistration? Comparing to a descending path frame track over the same area would shed light on this, and allow the authors to rule out a difference in the projection of downslope deformation (e.g., solifluction) onto the LOS vector vs. a pixel misregistration issue.

Response: Unfortunately, only ascending ALOS data are available in this area. Nonetheless, we predicted the phase errors due to DEM mis-alignment (Figure 12) based on Equations (6)-(7), which look very similar to actual phase observations from multiple interferograms (Figure 11). We evaluated the estimated InSAR phase errors due to DEM-misalignment in magnitude and spatial distribution (Figure 13). At the same location, this error increases with InSAR perpendicular baselines (Figure 14; blue dots), a key feature of DEM-related InSAR phase errors. By contrast, deformation signals related to solifluction processes are not controlled by InSAR perpendicular baselines, and they should not resemble the simulated DEM mis-alignment patterns (Figure 12) so closely.

Conclusions:

Page 22, line 431: "InSAR-estimated seasonal surface thaw subsidence measures the amount of water stored in the saturated soil active layer above permafrost, which can be used to constrain hydrologic models and water mass budgets." Rather than saying InSAR thaw subsidence measures *water storage, I might suggest instead something like 'is sensitive to' or 'is related to', as this is not a direct measurement of soil water storage, but rather a model-based estimation.*

<u>Response:</u> We changed "*measures*" to "*can be related to*" as suggested by the reviewer.