Widhalm and co-authors introduce a new soil moisture index tailored for Arctic lowland regions by using the ratio of seasonal thaw subsidence and thawing degree days (they called it as 'subsidence rate'). Utilizing InSAR subsidence observations from three Arctic sites, the study demonstrates a stronger correlation between this new index and in-situ soil moisture compared to traditional indices such as NDWI and TWI. The authors also explore the impact of methodological choices, such as InSAR tropospheric delay corrections and using 2 m air temperature vs. ground surface temperature in thaw index calculation, on the InSAR time series and its relationship with thawing degree days.

This research marks a new advancement in the application of InSAR for Arctic studies. However, there are several areas where the manuscript could be further refined. And I hope my comments can help improve the rigor, clarity, and focus of this very nice piece of work.

0. Terminology: "Seasonal Subsidence Rates"

This is a minor comment. The term "seasonal subsidence rates" used throughout the manuscript, including in the title, could be misleading because it implies changing rates over time. Instead, the proposed index represents a ratio of subsidence to thaw degree days, or subsidence normalized by degree days. If the authors choose to retain this 'rate' term, clarification should be provided at its first mention. Alternatively, a more precise term could be coined for this new index.

1. Soil Moisture Estimates

1a. The key results of this work should be about soil moisture and the new index (alpha). Fig. 8 does present maps of alpha over three regions. But how could readers interpret them? There's also a counter-intuitive representation of negative alpha values (see my comment #5).

1b. Volumetric soil moisture like those presented in Section 5.2 would be more useful output. But what is missing are maps of (categorized) soil moisture derived from the InSAR-based alpha. Is it possible for the authors to include them, which would greatly enhance this work's utility?

1c. It is worth adding further elaboration and discussion on the depth of soil moisture this new index reflects. Terms like 'near-surface' (line 9 and numerous places), 'general' (lines 464, 610,), 'top' (line 614) all imply a shallow depth. However, considering that thaw subsidence measured from InSAR essentially integrates responses from the entire thawed soil column (Liu et al., 2012; Chen et al., 2023), it seems likely that alpha reflects a weighted average of soil moisture within the thawed active layer. Because soil moisture and ice content in Arctic lowlands have strong vertical variations, it would be necessary to make clarification on the depth sensitivity. This also helps when comparing alpha with other soil moisture products and indices such as ESA CCI (passive and combined) and NDMI.

1d. If possible, please specify the depth of in situ soil moisture measurements in Table 2, as this information is crucial for interpreting the results.

2. Normalizing with Thawing Degree Days (DDT)

This work proposes to scale seasonal thaw subsidence with DDT. Below, I lay out a theoretic framework based on Stefan's equation to give an alternative scaling scheme with the square root of DDT.

One form of Stefan equation for time-varying thaw depth $D(t)$ is (e.g., Kurylyk and Hayashi, 2015)

$$D(t) = \sqrt{\frac{2k \cdot DDT(t)}{\phi \rho_w L}}$$

where k is the bulk thermal conductivity of the upper thawed soil, L is the latent heat, ϕ is the volumetric moisture content, and ρ_w is the water density.

To the first order, the magnitude of seasonal thaw subsidence is proportional to thaw depth times volumetric soil moisture ($D * \phi$), therefore

$$\text{Thaw Subsidence} \propto \sqrt{DDT\ \phi}$$

This $\sqrt{DDT}$ dependency serves as the basis for several previous studies (e.g., Liu et al. 2012; Hu et al., 2018) and can capture faster subsidence at the beginning of thaw season (line 335).

It is up to the authors, but it should be very straightforward if they decide to test this alternative scaling scheme. And if it turns out that square-root-of DDT works better, the theoretic framework can be easily refined to build a strong physics base for soil moisture retrieval.

4. Tropospheric Delay Correction

I agree with the authors that it is important to correct atmospheric (tropospheric plus ionospheric) phase delay in interferograms. The manuscript presents a valuable comparison of uncorrected, spatially filtered, and GACOS-corrected InSAR results, and points that GACOS is helpful in some cases but not in all cases. Such a comparison is informative and insightful. However, given the complexity and importance of tropospheric delay correction in InSAR studies on Arctic permafrost, my concern is evaluating the effectiveness and accuracy of the tropospheric delay correction methods deserves a separate study by itself and may not suit the interest of TC readership.

For instance, the assessment presented in this manuscript is largely based on visual inspection (e.g., Fig 4, Fig 6, Fig 9) but lacks quantitative analysis. The spatial filtering is a simplified version of spatial-temporal filtering that is commonly used in InSAR time series analysis. Ideally, spatial-temporal filtering should be included in the comparison. And there are exemplary studies comparing various correction methods (e.g., Bekaert et al., 2015; Murray et al., 2019), none has been done for Arctic permafrost studies.

A more comprehensive and thorough evaluation is outside the scope of the current study and is better suited for a separate publication.

One way to sharpen the focus of this manuscript on the new soil moisture index is to emphasize the importance of atmospheric correction and to put visual comparisons into supplementary materials. This also helps to shorten the lengthy manuscript in its current form.

5. The manuscript does not explicitly state whether InSAR line-of-sight deformation has been converted to vertical displacement (or not). Clarification on this point is needed. Additionally, the manuscript adopts a convention to use negative values for subsidence (which is fine), but leaves the new index (alpha) to be negative. It is confusing as a more negative alpha means higher soil moisture. It should be more intuitive to reverse the sign in the definition of alpha (eq. 1) so that a higher positive alpha means higher soil moisture. Reversing the sign in the definition could enhance its interpretability and align conceptually with other soil moisture indices.

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Wig's preprint has been published as this journal paper:

Wig, E., Michaelides, R., and Zebker, H. (2024). Fine-Resolution Measurement of Soil Moisture from Cumulative InSAR Closure Phase. *IEEE Transactions on Geoscience and Remote Sensing*, 62, 5212315.