I appreciate the authors' response to the review comments from both reviewers 1 and 2. In general, they have clarified several outstanding issues with the prior manuscript version. Additional motivation on the use of a degree day normalization in the thaw season deformation rate is appreciated, and a more standardized terminology of the annual and seasonal deformation terms, as well as a comparison with those presented in the Michaelides and Cao papers, has made the proposed methodology clearer. This revised version is a stronger overall manuscript.

However, there are still several issues that I have identified with the revised manuscript that I think must be addressed before publication is possible. These issues mostly relate to the estimation of annual deformation rates, comparison of said rates to the LiDAR-derived ΔDTMs, and model fitting of these annual deformation rates. I have summarized these points below:

-My primary remaining concerns regard the estimation of the annual deformation rate (Section 4.2.2) and subsequent model regression to estimate the recovery (most notably Figure 4). I will use the YKD case as an illustrative example because I think the potential complication is likely most pronounced here, although this comment applies to the method more broadly and hence all the study regions.

1. While the confidence interval of the fitted model provides an error envelope for the recovery time (the intersection of the model regression with the x-axis, plus or minus the 2.5th and 97.5th bounds), there is the further issue of how estimate uncertainty in the annual deformation rate propagates into the model regression. For example, in Figure 4 no error bars are included on any of the individual annual deformation rate estimates (nor thaw season deformation rate estimates for that matter); I think it is rather important to include these. Since annual deformation rates were estimated via linear regression in Section 4.2.2, there should be an associated estimate such as the RMSE or standard error that can be used.

2. Looking at the list of interferometric pairs used in the PALSAR-2 annual deformation rate analysis, the YKD interferometric network has two sub-seasonal interferometric pairs of 42 days each, while all other interferograms used for the annual deformation rate analysis are truly close to interannual. I imagine that the authors included these two interferograms to ensure that the network remains full rank during SBAS inversion. However, there inclusion also complicates the interpretation of this time series as solely reflecting annual deformation rate, as there is implicitly a seasonal component in the solution. Furthermore, their inclusion in the interferometric network might magnify any bias present in these interferograms into the final annual deformation rate estimate, since the sensitivity to error during SBAS inversion is scaled by the inverse of the temporal baseline. For example, a 1 cm bias in a 42-day interferogram would yield a bias of ~8.7 cm/yr, whereas a 1 cm bias in a 360 day interferogram would yield only a ~1 cm/yr bias. I suspect that this in part explains the much larger dynamic range and higher spatial variability of the annual

deformation rate estimate for YKD compared to the other study regions (Figure 10). The observed dynamic range of annual deformation rate is much larger than the other sites, with areas of the unburned tundra exhibiting a variability in annual deformation rate of -3 cm/yr vs +3 cm/yr over spatial scales considerably smaller than other study sites reported here (for example immediately southeast and northeast of the 2015 Kuka Creek fire, or north of the 2002 Aropuk fire). These large variations over small spatial scales to me suggest an uncompensated bias term such as a varying soil moisture effect or residual phase ambiguity due to the highly discontinuous spatial phase field -- perhaps propagated through from the sub-seasonal interferograms – and not merely a geodetic deformation rate. This is important as this variability will impact the statistics both within the burned and unburned reference areas, consequently impacting the 'burned-unburned' rate and the model fits and associated recovery times that are central to the manuscript. A few suggestions on how the authors might be able to address this:

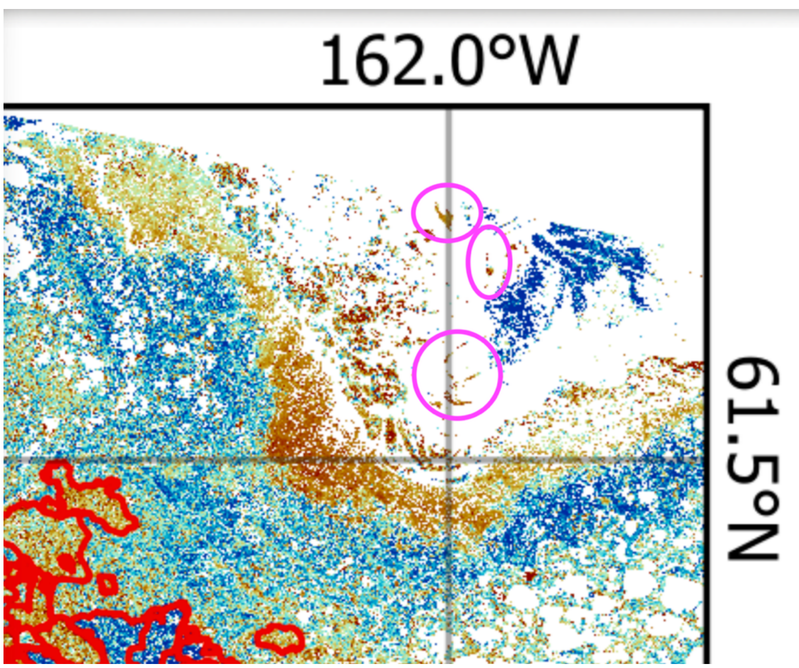
   a. Are there alternative interferograms that can be incorporated into the network such that the sub-seasonal interferograms can be removed?

   b. If the answer to the above is no, can the annual deformation rate be estimated using an alternative approach, such as the stacking method (as in Chen et al. 2020: https://doi.org/10.1016/j.rse.2020.112007), or by performing a linear regression to the individual interferometric pair deformation rates (as in Liu et al. 2010: https://doi.org/10.1029/2009JF001547) rather than the output time series?

3. Figure 4: the 95% confidence intervals are now significantly tighter than in the previous version of the manuscript. The authors state in their response that "Upon revisiting the analysis, we found that the interval shown in the Figure 4 corresponded to a prediction interval, which is inherently wider than the confidence interval of the fitted model." However, the previous manuscript version reports these intervals as confidence intervals and not prediction intervals. Some clarification is needed on this point. To avoid ambiguity and make these results as directly comparable to the prior referenced literature, I would recommend that the 95% confidence intervals that the authors report be estimated for each fitted model by reporting the predicted value +- 1.96 times the standard error, which is the method used in Cao and Furuya 2025 (note that Cao uses a parametric bootstrap to estimate the confidence interval of the integral of the annual deformation rate fitted function, but not for the confidence interval of the annual deformation rate fitted function itself).

-I have a clarifying question on the method used for the unburned reference using a 5-km buffer: When you say "Areas with multiple fire events were excluded", does this mean that all locations affected by repeat burn were excluded? If yes (which I agree is important to remove), do the authors also account for any and all burn scars that fall within the 5-km buffer of nearby fire scars? Looking at the density of fire scars particularly in the YKD and the Noatak, there are several fire scars for which adjacent single-burn fire scars will fall within the 5-km buffer, but the text is not clear whether these pixels are excluded from the unburned buffer calculation. To be clear, I think that these pixels should be removed from the mean unburned reference estimate so as not to bias the 'burned-unburned' values reported in the manuscript.

-On the choice of reference pixels: The authors' selection criteria for reference pixels is sensible and consistent with prior studies. However, I wonder what effect there is by choosing the airstrip in Bethel as a reference pixel for the YKD, when this is approximately 80 km from the study region, significantly farther than the 20 km radius used for the atmospheric high pass filter. It seems plausible that this choice could result in a residual low frequency phase term that is not accounted for in the high pass filter. Investigation of areas of barren ground within the study region, which one would expect to exhibit zero or near-zero annual deformation rates based on the authors' rationale for reference points in other study regions, instead exhibit large uplift rates of almost 3 cm/yr (see my very hastily constructed zoomed in screenshot with a few barren areas as identified from the Alaska NLCD 2016 circled in magenta):



The pervasive uplift signal to the southwest of the Yukon river, which corresponds to an area of boreal and mixed forest transitioning into low shrubland, also raises some concerns, and does not seem physically plausible. I suspect that if the authors instead used this barren ground closer to the study region for phase referencing, this would result in a systematic reduction in the annual deformation rate across the scene of about 2-3 cm/yr. I don't think that this would necessarily affect the 'burned-unburned' calculation that the authors use, although it would be worthwhile to confirm this. Nonetheless, this would certainly affect the annual deformation rate estimate. To the broader point of reference point selection – which is a notoriously difficult aspect of InSAR analysis in periglacial landscapes – it might be valuable to include some sort of assessment of the chosen reference pixels. One possibility would be to verify that the annual deformation rate over other pixels of barren ground also exhibit no significant deformation rate.

-On the comparison with InSAR interannual deformation rates with Lidar ΔDTMs, line 422: "Moreover, the spatial distribution of pronounced annual subsidence rates observed in the DTMs in parts aligns with annual subsidence rate patterns derived from PALSAR-2, as illustrated in Figure 9. This similarity in spatial patterns is also reflected in a linear trend visible in the comparison of the value distributions 425 (Figure 10b)." I'm sorry, but I remain unconvinced that there is a robust, quantitative agreement between these two datasets. While the statistical distribution of values are broadly similar (Figure 10a), the violin plot (10b) demonstrates that there is both an additive bias (evident from the offset between the 1-1 line and the line that would pass through the means of the violin plots) and a conditional bias (evident from the shallower slope of the line passing through the means of the violin plots compared to the 1-1 line) between the two estimates of annual deformation rate. The authors note that the two datasets broadly qualitatively agree 'in parts', but there are conversely many other places where the two datasets deviate rather significantly 'in parts'. For example, the authors note the pronounced annual subsidence rates observed in the DTMs align with subsidence observed from PALSAR-2, but there are also clear areas for which the DTMS record uplift while PALSAR-2 observes subsidence. If the authors intend to use the lidar ΔDTMs as a form of validation for the InSAR results, then this section would benefit from a more rigorous quantitative treatment of the biases between these two datasets and a discussion of whether a thorough quantitative validation is possible amidst such biases.