We thank Reviewer #1 for their thoughtful comments. The reviewer's text appears in black with our responses in bolded blue.

Based on both reviewers' comments, we significantly revised and reorganized the introduction of the paper. We also moved the section of governing error equations from the appendix to the main body of the text (now Section 2), provided more detail in the derivations, and removed all references to the unpublished companion paper.

Summary and Recommendation

This paper quantifies and compares non-snow errors for InSAR-based retrievals of change in snow water equivalent (SWE). The analysis is conducted at 13 SNOTEL sites in the western United States. Six error sources are considered, including ionospheric effects, atmospheric humidity and pressure, soil permittivity, vegetation permittivity, and surface deformation. The paper finds that errors due to the ionosphere are large and can easily exceed the median SWE value at many sites with lower snowpack accumulation. If ionosphere effects are removed, then the remaining cumulative errors are on the order of 2 to 7 cm in SWE, with some errors offsetting each other. For 10 out of 13 sites, these errors are within 10% error for April 1 SWE, which is within the target accuracy set forth by the U.S. decadal survey.

I find this to be a straightforward and useful analysis with good potential to support SWE error assessments with InSAR-based retrievals like from NISAR. I think it would be a great contribution to the journal following attention to some comments, as elaborated below.

Thank you for these positive comments; we are glad you found the paper to be straightforward and useful.

1. The specific selection of these 13 SNOTEL sites could use better justification. I understand they were sampled from different regions/zones of the western United States. However, in each of those zones, there are multiple sites which could have alternatively been selected, and the low sample size leads me to wonder about the stability/robustness of the results. Would the results have changed with different sites select
2. Two of the SNOTEL sites (Disaster Peak and Quemazon) are in post-burn areas and likely have altered snow accumulation and melt dynamics relative to their pre-fire condition (see Smoot and Gleason, 2021). One might argue these two sites may not be broadly representative of their respective geographic zones due to the fire. At the same time, there are broad swaths of the western U.S. where fire has impacted snow dynamics. Similar to the previous comment, I would ask the authors to provide more justification for specific site selection and elaborate on what is being represented by the sites.

Thank you for these points. We added more details about the site selection criteria (lines 198-205):

"To illustrate how non-snow errors vary across snow climates, we applied our analysis at locations across the WUS representing 13 major mountain ecoregions denoted by Trujillo and Molotch (2014). We chose a 10-year period between water years 2016–2025 (October 1,

2015 through September 20, 2025), which includes high (2023), average (2022), and low (2016) snow seasons, to examine the temporal variability of the errors. All other datasets we use to calculate the non-snow errors (Table 1) are also available for water years 2016–2025. Within each ecoregion, stations were selected based on data availability (98% complete data records for SWE, air temperature, and 2~inch soil moisture for each of the 10 seasons). When more than one station in an ecoregion met the data availability criteria, we selected so that the final 13 stations span a range of snow classes, vegetation types and heights, and other environmental characteristics (Table 2).”

It is indeed possible that results would change if different sites had been available. However, our intent is not to draw broad conclusions about the method based on the 13 sites selected; rather, we aim to illustrate that the non-snow factors we considered here can have different magnitudes/effects in different snowpacks. We added this language explicitly in the Discussion in lines 457-467, including some new discussion about snow/fire interactions and shallow snowpacks:

“We analyzed errors at only 13 SNOTEL stations across the WUS. Although this selection was partially dictated by data availability (see Section 2.3), it is possible that our results would change if we had selected different sites. For example, two of our three shallow snowpack stations (Disaster Peak and Quemazon) are in post-burn areas and may have snow accumulation and melt dynamics that are not fully representative of their broader geographic regions (Smoot and Gleason, 2021). However, our intent is not to draw broad conclusions about the method based on the 13 sites selected; rather, we aim to illustrate that the non-snow factors we considered here can have different magnitudes and effects in different snowpacks. Especially given that wildfire area has been increasing in snow-dominated regions across the WUS since the 1980s (Kampf et al., 2022) and WUS snowpacks are projected to continue their general decline (Siirila-Woodburn et al., 2021), it is likely that the WUS will become more reliant on shallow and post-burn snowpacks for water resources in the coming years and decades. We encourage future work in snow remote sensing to continue examining shallow and marginal snowpacks (López-Moreno et al., 2024).”

3. To what extent might error/uncertainty in satellite location/position induce a change in InSAR phase and thus an error in retrieved SWE? I assume this might manifest as a bias over the scene and could be detected/corrected. However, if much of the scene included a change in SWE along with other non-snow errors, then I wonder to what extend orbit error might be detected or corrected.

Other than during periods of very high solar activity, the final estimates of satellite orbits tend to be highly accurate, especially in comparison to the airborne UAVSAR platform referenced in several studies in the introduction. Baseline estimation errors appear as consistent linear phase ramps in interferograms that are quite recognizable and easily removed using fast-Fourier-transform techniques. These effects should be not a significant barrier to InSAR-derived SWE measurements.

Line Comments

- L. 43: Delete “a”.

Done.

- L. 51: Delete “will”.

Done.

- L. 66-67: Why is it 2*pi for equation 1 but 4*pi for the appendix equations? I assume the 4*pi is due to the two-way distance from satellite to surface, so it is a little odd that equation 1 is developed as 2*pi.

This comment has been addressed by the expanded equation derivations (now Section 2). The 4*pi is indeed due to the two-way travel distance, but the Phase-to-SWE equation has a factor of 2 in the denominator. We made this explicit in Section 2.1 (specifically lines 80-81).

- L. 162: Delete “season”.

Done.

- L. 210: Remove first “early”.

Done.

- L. 220: Add “the” before “largest error”.

Done.

- L. 236-243 and Fig. 6: What about cases when delta SWE = 0? This may not be an uncommon occurrence (e.g., cold regions/periods with no new snow and no ablation), and would be important to document. I understand the challenge with including this case (i.e., cannot divide by zero for Fig 6b) but perhaps there is another way to summarize and report expected errors for the dSWE=0 case?

Thank you for this suggestion. We extended this exceedance analysis by separating the daily observations into days where dSWE = 0 and days where dSWE != 0. As noted by the comment, it is not feasible to add a curve to Figure 6b when dSWE = 0, and the separate zero/nonzero curves were quite similar to the ones already in Figure 6a. We did not change the error curves in the figure but added a new paragraph in lines 363-369:

“We used a similar analysis to investigate whether non-snow errors behave differently on days with vs. without a snow event. We split the October 1 through April 1 dataset into days where ΔSWE = 0 (1784 total measurements) and days where ΔSWE =/= 0 (21,698 total

measurements), representing both accumulation and ablation events. When ∆SWE = 0, the 50% exceedance thresholds were 0.210~m for total non-snow error and 0.031 m for non-ionospheric error. When ∆SWE =/= 0 the 50% exceedance thresholds were 0.230~m for total non-snow error and 0.030 m for non-ionospheric error. The error curves for the two cases (not shown) were quite similar to those in the left panel of Figure 6. This result indicates that non-snow factors affect the phase relatively consistently, regardless if a snow event occurs or not."

During this analysis we also caught a typo in the title of Figure 6a. Previously we had n=23,397 but we revised this to n=23,482 to reflect the true number of observations in the dataset.

- L. 338: Add "of" after "effects".

Done.

- L. 380: add "(i.e., surface deformation)" at the end of this line because that is what is causing the change in distance.

This comment has been addressed by the expanded equation derivations (specifically Section 2.2.5).

- L. 410: This canopy height is from LandFIRE, right? Please clarify.

This comment has been addressed by the expanded equation derivations (specifically line 178 in Section 2.2.4 and Table 1 in Section 2.1).

Figures and Tables

- Table 1 – how is surface deformation mapped to the SNOTEL locations, given that these two stations may be separated by some distance?

We make the assumption that the 12-day surface deformation at a given SNOTEL site is equivalent to that of the nearest GNSS station, even though the SNOTEL and GNSS points are not colocated. We do not attempt to spatially interpolate or otherwise adjust the GNSS data to account for the separation distance. We added these details explicitly in lines 218-221.

- Figure 1 – I think it is better stack the 4 panels vertically to align their common axis (time).

We agree and have made this change (now Figure 2).

- Figure 2 – Similar to previous comment. I think this would display more effectively if it was flipped with 2 rows by 3 columns. That way, the SWE errors can be compared more readily to SWE on the common axis (time).

We agree and have made this change (now Figure 3).

- Figure 2 – remove "6" at the start of the caption.

Done (now Figure 3).

- Figure 3 – I realize these are ordered by site number, but think that this could be more effective if arranged by bias (from most negative to most positive, on a median basis).

During revisions we remade a draft of the figure as suggested. While it is perhaps more visually appealing, sorting this figure in a different order than all others in the paper suggests that we are trying to draw some conclusion about the sites based on the median bias. As the rest of our analysis shows, the errors interact in complex ways in both space and time and we think it would be inappropriate to draw general conclusions based on the median error values at these 13 sites. Ultimately we decided not to implement the change in the revised manuscript. This figure (now Figure 4) is unchanged.

- Figure 7 – the longitude and latitude labels on the horizontal axes of all 3 panels should be swapped (e.g., longitude should be latitude, and vice versa).

Done.

- Figure 7 – would it help to denote the intersection points between the transects on each of the 3 panels?

We attempted to add this to the figures during revision but could not find a clean way to do it, since the intersection points are not common between all subpanels. Ultimately, we decided that the additional text/labeling required to denote the intersection points distracted from the data and ultimately did not improve the figure.

References

- Smoot, E. E. and Gleason, K. E.: Forest Fires Reduce Snow-Water Storage and Advance the Timing of Snowmelt across the Western U.S., Water, 13, 3533, https://doi.org/10.3390/w13243533, 2021.