March 20, 2023

Estimating snow accumulation and ablation with L-band InSAR
By: J. Tarricone et al.

Response to Reviewer #2: Silvan Leinss

Reviewer comments are shown in black. Responses are in blue.

> We added numbers to comments and letters for those with multiple parts. Comments in this response document are referenced using a #. All new text added to the manuscript is italicized.

General comments:

The authors describe the evaluation and interpretation of L-Band repeat-pass radar interferometry. Their aim is to observe changes of the snow water equivalent during partially wet snow conditions.

SWE estimation by radar interferometry is a very promising technique that has been developed within the past 20 years with increasing success. The main problems of this technique are a quick loss of coherence, correction for atmospheric phase delays, existence of phase reference points, and - especially for wet snow - the uncertainty and variability of the permittivity and signal penetration through the snow pack.

The authors tackle some of these problems and show that, even for melting conditions, coherence is maintained at L-band for the 7 and 14 day repeat time of their three acquisitions. They correct large-scale atmospheric phase delays by referencing the InSAR phase to snow free areas in the scene which is useful if no topography-dependent atmospheric phase exists. The authors reference the local InSAR phase to 8-9 pixels around a local snow pit and assume the remaining phase originates from changes in SWE (accumulation or melt) rather than local km-scale atmospheric delays. It is not clear where the km-scale phase patterns originate from (SWE change, permittivity changes or atmosphere).

Unfortunately, during the 14 days of the InSAR observation period, the snow pit data showed hardly any change in SWE which makes it very difficult for the authors to find a statistically convincing relation between the InSAR phase and SWE changes. Therefore, the authors rely on interpretation of local patterns which they assume to be caused by snow melt. Sentinel-2 imagery, as suggested by Simon Gascoin https://doi.org/10.5194/tc-2022-224-CC1, supports the authors assumption and should be considered. See provided images in the supplements (Channel 8 provides some information about snow wetness).

In addition to the unfortunate meteorological conditions I have some concern because the authors use the dry-snow-SWE-to-InSAR equation (Guneriussen 2001, Leinss 2015) even though wet snow (melting condidions) are considered. This equation depends on the permittivity of snow which changes significantly (from 1.5 to at least 2.2) when the snow becomes wet at constant SWE. Therefore, (some of) the observed phase change might be due to increasing wetness rather than a change in SWE.

The paper is very well structured and good to read, but need to be improved by better focusing on the main results; in addition to addressing the above mentioned concerns which could require a major revision of the paper.

> We would like to thank Dr. Leinss for his thoughtful comments, especially surrounding the topic of snow wetness and permittivity.
>
> While we've clarified the manuscript through the specific comments below, we wanted to make clear that our permittivity measurements were taken directly, not derived from snow density.
>
> We also note that Reviewer #1 recommended significant changes to the manuscript. Please refer to our replies there to see the full scope of the updates we have made.

Specific comments:

1. line 50: "where shorter wavelengths (..) have been used to estimate SWE (references)": The authors detail some technical challenges faced when estimating SWE from backscatter. It would be interesting, to provide a rough estimate or precision based on the conclusions from the cited authors, that apply these techniques, to indicate how well they were able to actually estimate SWE from backscatter. For example, accoording to the scattering physics that happen in snow, radar backscatter does not necessarily show a monotonically increasing relation to backscatter. I would claim, that determining the required in-situ parameters to derrive SWE might be even more difficult that determining SWE directly.

   We agree that determining the in situ parameters for the volume scattering approach is exceedingly difficult, especially over heterogeneous terrain. The various implementations of this approach (theoretical, tower-based, airborne), and the different modeling frameworks implemented, make providing even a rough estimate of precision for the technique in general unfeasible.

2. L58: If the authors think it would be beneficial, they could cite Stefko/Leinss(2022) which provides a completely new approach to analyze the radar backscatter from snow (even though it does not claim to derrive SWE or snow height).

Stefko and Leinss et al. "Coherent backscatter enhancement in bistatic Ku- and X-band radar observations of dry snow", TC 2022, https://tc.copernicus.org/articles/16/2859/2022/

While Stefko & Leinss (2022) is a fantastic paper, we don't think it fits into the scope of Section 1.2, which is focused on snow depth and SWE estimation techniques.

3. L91: "accumulation and ablation": It is not totally clear what the difference is of this study compared to the study discussed in the paragraph before (Marshall 2021). In the previous paragraph "a wide range of (..) snow conditions" is specified. This study/paragraphs seems to adress "both snow accumulation and ablation". Do you mean ablation of dry snow by wind drift or evaporation, or do you mean ablation my melt an runoff? In line 92 you mention that the "UAVSAR-based approach" has been only applied to dry snow "but not melt". Does that indicate that you adress melt or, at least, wet snow periods? Try to better describe the differences betwen the two studies (you could also add the study side here, instead of in line 96.).

   In this context, the word 'ablation' was used to describe melt, evaporation, or sublimation. Grand Mesa is a high elevation (~3,400 m) cold location. During the study period of Marshall et al. (2021), there was no measurable precipitation, and the snow depth variations were driven by wind redistribution.

   We updated the text to: *"The overall goal of this study is to assess the performance of L-band InSAR for monitoring SWE changes in an environment where there is both snow accumulation and ablation (melt, evaporation, or sublimation). Currently, this UAVSAR-based approach has only been applied to cold dry snow conditions on Grand Mesa (Marshall et al. 2021), where the snow depth variations were mainly driven by wind redistribution, but not melt or evaporation. Towards this end, the specific objectives of the work presented here are to (1) analyze InSAR SWE retrievals over a complex mountain region, and (2) validate the retrievals using satellite and in situ data.."*

4. L25 - 130: The equation from Guneriussen requires knowledge about snow density rho_s and the snow permittivity epsilon_s. Due to a lack of spatially (and vertically) distributed information, these two variables seem to be determined from measurements in two snow pit data as written in line 255-257 which could potentially introduce a significant bias on the derrived SWE values. However, as shown in Leinss et al. (1015) [Figure 8left, Figure 9], for dry snow (and only for dry snow), there is an almost linear relationship between SWE and the InSAR phase which does hardly depend on snow density rho_s or epsilon_s. I think it is worth considering or mentioning this as it simplifies SWE determination.

Reference: Leinss et al. "Snow water equivalent of dry snow measured by differential interferometry", IEEE JSTARS (2015), https://doi.org/10.1109/JSTARS.2015.2432031

We determined epsilon_s in each snowpit vertically (10 cm segments) using the A2 Photonics WISe sensor.

This text has been clarified in Section 2.3.3: *"Measurements of snow depth, snow layer stratigraphy (grain size, grain shape, hand hardness, and manual wetness), ρ_s, ϵ_s, and temperature were recorded for each pit. ρ_s, ϵ_s, and temperature were measured in 10 cm increments starting at the top of the pit. Stratigraphic layer size is variable and defined by the observer. In situ ρ_s measurements have been shown to have an uncertainty of ~10 \% (Conger and McClung, 2009; Proksch et al., 2016). ϵ_s was measured using an A2 Photonics WISe instrument (A2P, 2021), which Webb et al. (2021b) showed to have a mean absolute error (MAE) of 0.106 when compared to other in situ observations… Ultrasonic snow depth sensors have a known uncertainty of $\pm$ 1 cm (Ryan et al., 2008)."*

We also added: *"For dry snow, there is a direct relationship between ϵ_s and ρ_s, whereas for wet snow, the relationship becomes more complex, with even small amounts of liquid water vastly increasing ϵ_s values. Recent studies from Eppler et al. (2022) and Leinss et al. (2015) found that error in density estimates only biases total SWE change by < ~ 5 \% for dry snow in a wide range of θ (< 50°) and ρ_s (< 500 kg m $^{-3}$). Leinss et al. (2015) also showed a nearly linear relationship between ΔSWE and interferometric phase for dry snow, which simplifies the SWE estimation. That said, we used Equation 2 because our study considers melting snow and ϵ_s is a direct input."*

5. L25 - 130, continued: In contrast to dry snow, for melting conditions, the linear relation between the InSAR phase change and SWE does not hold anymore. Please provide some information about the permittivity of wet snow compared to dry snow. From the references [Sihvola 1986, Webb 2021, Hallikainen 1986, fig. 9] I obtain that the real part of the permittivity increases from 1.5 to about 2.2...2.6 for the same parameters of density (300 kg/m^3), LWC (5%) and frequency (1 GHz). This should cause a significant change in the observed phase change even for a constant SWE.

   For references, see comment about Table 2 further below.

   Furthermore, the penetration depth into wet snow could vary considerably from the penetration into dry snow, at least for higher frequencies. For L-band and wet snow, the expected penetration should be checked to be larger than the snow depth.

This is an excellent point. The reason that this method still works is that we are taking observations of permittivity in the pits rather than calculating permittivity from density and liquid water. These pits are also dug a few hours after the UAVSAR acquisition, and likely slightly overestimating epilson_s. This will certainly be difficult in situations where direct observations of permittivity are not available, but that is outside the scope of this study as it could certainly be a future research paper on its own.

See #4 for updated text.

Snow penetration depth was confirmed in L393 - 394 (of original submission).

6. L129/130: Please mention how strongly the liquid water content can affect the real and imaginary part of the permittivity of wet snow. The real part has a stronger effect and changes are considerable. See further references and hints in the comment below for Table 2.

   See #5 above for updated text.

7. L132-142: To better understand the study area, I highly suggest showing Sentinel-2 images as suggested by Simon Gascoin to illustrate the land-cover of the studied area. There are S2 images from 2020-02-06, 2020-02-16, 2020-02-21, 2020-02-26.

   We added Landsat true color images from 18 Feb. and March 5th in Figure 4d and 4e, respectively.

8. Figure 1: The length Delta R_a does not correspond to the illustrations in (Guneriussen 2001, Leins 2015) and to the underlying physics of equation (2).

   We removed the left side of Figure 1 per the recommendation of Reviewer #1 and inserted an adapted version of Figure 7 from Leinss et al. (2015).

9. Note that it might be better to reference the peer reviewed paper of Guneriussen rather than the IGARSS proceeding (here and other places, possibly cite both).

   T. Guneriussen, K. A. Høgda, H. Johnsen, and I. Lauknes, "InSAR for estimation of changes in snow water equivalent of dry snow," IEEE Trans. Geosci. Remote Sens., vol. 39, Art. no. 10, 2001, doi: http://dx.doi.org/10.1109/36.957273.

   We added the correct citation. Thank you for catching this.

10. L118-121 (145-155): How was the interferometric data processed?

We standardized the interferometric processing per the request of Reviewer #1 (all done by the UAVSAR team at JPL) and added more information on how the InSAR data was processed in Section 2.3.1.

“UAVSAR is a fully polarimetric L-band radar deployed on a NASA Gulf Stream III aircraft, traditionally flown at ~13,700 m with a 22 km nominal swath width (Hensley et al., 2008; Rosen et al., 2006). Detailed technical specifications of the radar are provided at the top of Table 1. UAVSAR data were accessed using the Python package uavsar_pytools (Keskinen & Tarricone, 2022). It uses the asf_search API (https://github.com/asfadmin/Discovery-asf_search) for easier downloading, formatting, and analysis of UAVSAR data. The flights used in this study occurred in the mornings of 12, 19, and 26 February 2020. The UAVSAR team at the NASA Jet Propulsion Laboratory (JPL) processed two 7-day (12--19 and 19--26 February) and one 14-day (12--26 February) ground projected (GRD) InSAR pairs. They were unwrapped using the Integrated Correlation and Unwrapping (ICU) algorithm (Goldstein et al., 1988). Processing parameters are outlined at the bottom of Table 1, and information about the specific products used is provided in the Supplementary Material. For the three flights used in this study, the flight track baseline was maintained within < $\pm$ 3 m, which is within the < $\pm$ 5 m requirement (Hensley et al., 2008).”

Table 1. Technical Specifications of the UAVSAR L-band radar (top). InSAR processing and data parameters (bottom).

Parameter	Value
Wavelength	23.84 cm
Frequency	1.26 GHz
Polarization	Quad Pol
Bandwidth	80 MHz
Pulse Length	40 μs
Radar Look Direction	Left
Range Swath Width	22 km
Average Near Range Look Angle	28.01°
Average Far Range Look Angle	68.9°
Ground Range Pixel Spacing	6 m
Number of Looks in Range	3
Number of Looks in Azimuth	12
Phase Unwrapping Method	ICU
Phase Unwrapping Filtering Method	Low Pass
Phase Unwrapping Filter Window Size	3 $\times$ 3 pixels

Was there a perpendicular (across-track) baseline between the different flight tracks that caused a topographic phase that had to be removed?

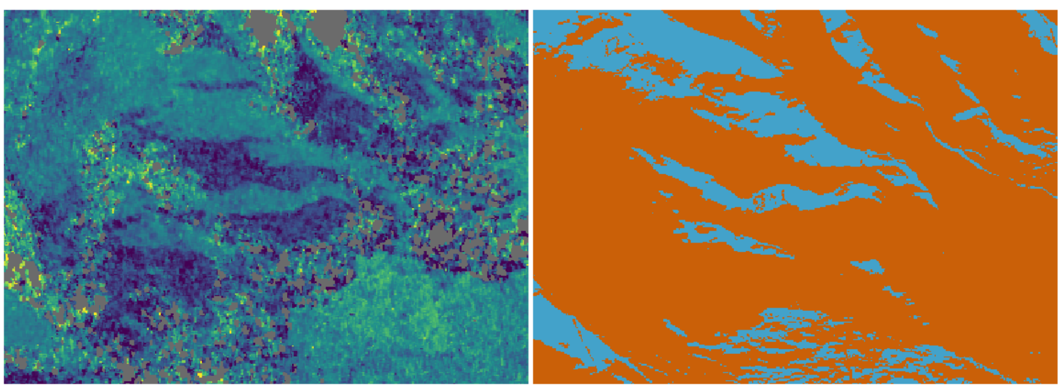
There is no note of an across-track baseline for any of the three InSAR pairs used in our study. The UAVSAR team will leave a note on the data when there is an issue with the flight tracks.

Why was the SRTM used for processing and not a better DEM with lower height noise (see e.g. Fig. 6)?

SRTM is the standard DEM used in processing by the UAVSAR team. We're not exactly the reason for this. NISAR will utilize the Copernicus 30 m for InSAR processing.

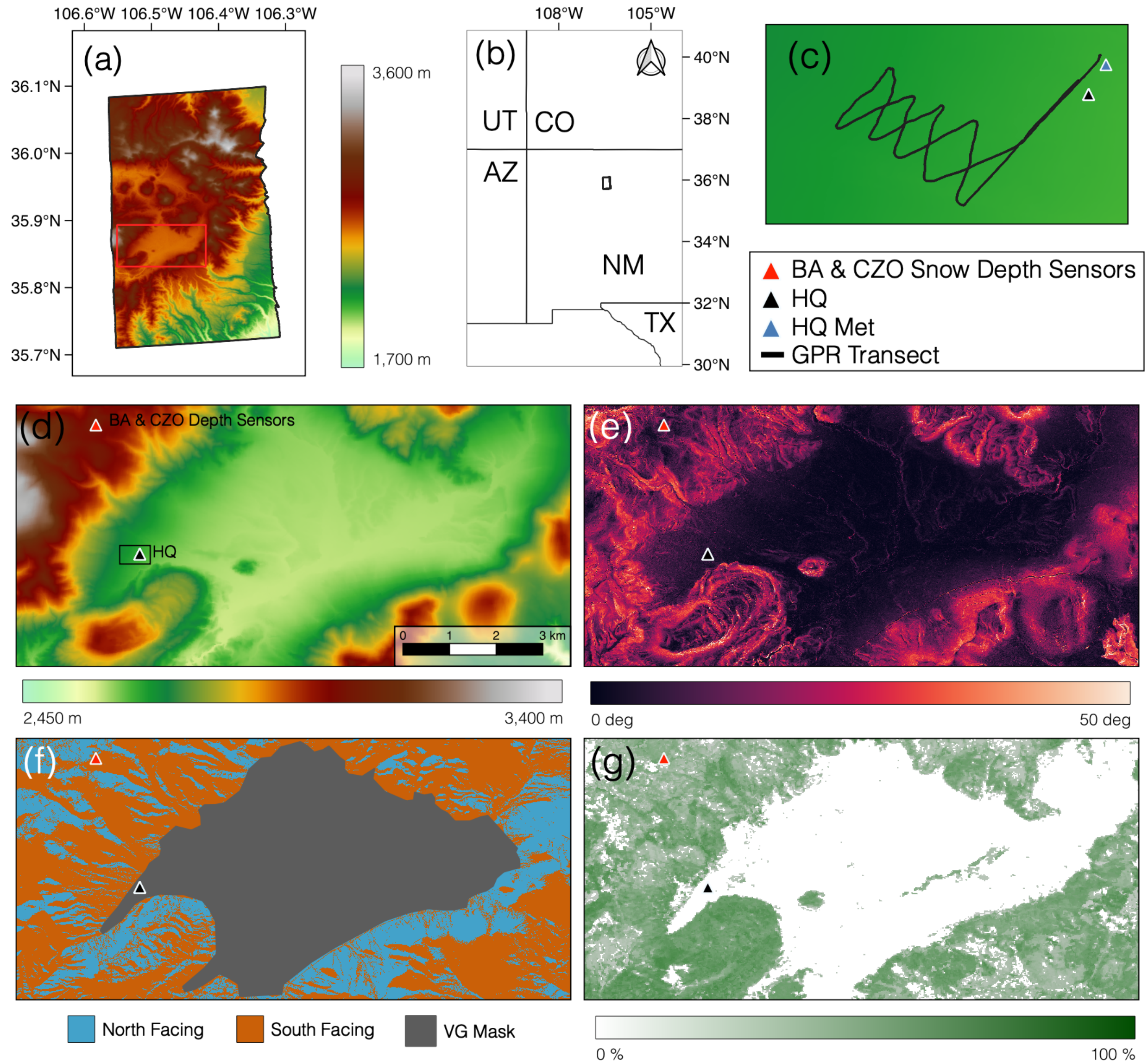
Could the phase differences along the south-west exposed slopes be an artifact resulting from a DEM not perfectly coregistered with the radar data? Nevertheless, Sentinel-2 data suggest, that indeed, snow melt is observed here.

While this is a great thought, we don't believe the uncertainty with the SRTM DEM propagated into the geolocation of phase data. Below is a zoom in the unwrapped phase (left) from 12-19 Feb. compared to the lidar-derived north (blue)/south (orange) aspect classification right. The phase data aligns well with the aspect classification, with clear differences between the north/south facing slopes. Per the request of Reviewer #1, we added a quantitative comparison. See #62 in response to Reviewer #1.



11. Figure 2, caption: Are the six CZO snow depth sensors all at the same location? I see only one black diamond.

    Captain has been clarified. The sensors are all relatively close together (~20 m), so they overlap each other at the scale of the map.



*"**Figure 2.** (a) DEM of the UAVSAR acquisition provided by NASA, with a red rectangle outlining the study area. (b) Map showing the area of the UAVSAR acquisition (black outline) in the Jemez Mountains, NM. (c) A close-up of the GPR transect outlined by the black rectangle in (d), with the HQ Met (blue triangle) and HQ snow pit (black triangle) displayed. Due to their close proximity, a single red triangle represents the BA pit and CZO snow depth sensors. Within the study area extent: (d) lidar DEM, (e) lidar-derived slope, (f) lidar aspect binned to north (270-90°) (blue) and south (90-270°) (orange) facing slopes, with the grey area representing the flat VG meadow where aspect values are not valid, and (g) NLCD canopy cover percentage of VG."*

12. L160: Section 2.3.2 (Snow Pit) contains no information about SWE estimation even though table 2 lists SWE values. How were the listed SWE values determined?

    We added clarification about how SWE measurements were determined in Section 2.3.3 Snow Pit and Meteorologic Station Data and in the caption for Table 3.

    Text updated to: *"Measurements of snow depth, snow layer stratigraphy (grain size, grain shape, hand hardness, and manual wetness), ρ_s, ϵ_s, and temperature were recorded for each pit. ρ_s, ϵ_s, and temperature were measured in 10 cm increments starting at the top of the pit. Stratigraphic layer size is variable and defined by the observer. In situ ρ_s measurements have been shown to have an uncertainty of ~10 \% (Conger and McClung, 2009; Proksch et al., 2016). ϵ_s was measured using an A2 Photonics WISe instrument (A2P, 2021), which Webb et al. (2021b) showed to have a mean absolute error (MAE) of 0.106 when compared to other in situ observations… Ultrasonic snow depth sensors have a known uncertainty of $\pm$ 1 cm (Ryan et al., 2008)."*

13. L164/165 and table 2: Does the change of epsilon_s result from an increase in density or a change in liquid water content?

    That is the observed epsilon_s values so it is from both increases in density and changes in liquid water. Given what we know about permittivity and snow properties, we could probably assume that much of those changes are probably from liquid water.

14. Table 2: The listed mean epsilon_s values appear to be only the real-value of the permittivity. However, as the snow condition is melting, I would at least mention the order of magnitude of the imaginary part of epsilon (permittivity of wet snow). For an imaginary part of epsilon on the same order (or larger) as the real part, equation (3) is not valid any more because the refractive index depends on both, the real and imaginary part. Fortunately, from references about the permittivity of wet snow around 1 GHz [Sihvola 1986, Webb 2021, Hallikainen 1986, fig. 9] I obtain eps''_wetsnow = 0.05..0.1 at 1 GHz, density 300 kg/m^3, liquid water volume content LWC=5%, which is by a factor of 5-10 smaller than the change of the real part of the permittivity: epsilon'_wetsnow increases from 1.5 to 2.2...2.6 for the same parameters of density, LWC and frequency.

    References:

    M. Hallikainen, F. Ulaby, and M. Abdelrazik, "Dielectric properties of snow in the 3 to 37 GHz range," IEEE Trans. Antennas Propag., vol. 34, Art. no. 11, 1986, doi: 10.1109/TAP.1986.1143757.

A. Sihvola and M. Tiuri, “Snow fork for field determination of the density and wetness profiles of a snow pack,” IEEE Trans. Geosci. Remote Sens., Art. no. 5, 1986.

Webb, R.W.; Marziliano, A.; McGrath, D.; Bonnell, R.; Meehan, T.G.; Vuyovich, C.; Marshall, H.-P. In Situ Determination of Dry and Wet Snow Permittivity: Improving Equations for Low Frequency Radar Applications. Remote Sens. 2021, 13, 4617. https://doi.org/10.3390/rs13224617

This is an excellent point. However, Table 2 is a summary of direct observations which were taken on average a few hours after the UAVSAR acquisitions. Therefore, the impact on permittivity (in particular the imaginary part) will be added to the discussion for future research. You are correct that the relatively low wetness for our site will have low impact, but future research will definitely need to account for wetness in a more accurate manner.

New text: *“... (2) consider the effect of snow wetness on Equation 2 due to the potential changes in signal refraction with increased wetness…”*

Section 2.4:

15. First, you mention a high-pass and low-pass filtering sequence, then you mention that the wet delay is caused by spatial variations in water vapor. But finally, if I understood right, there is simply a linear phase ramp removed. Reading line 202-211 I would expect a more advanced atmospheric filtering method. So maybe, you could better justify that removing a linear phase ramp is sufficient.

    You’re understanding of what we did is correct. As seen in Figure 7, there is a significant relationship between slant range (LKV) and unwrapped phase for 12-19 Feb. The uncorrected, unwrapped phase IQR was 2.25, compared to 19-26 Feb. which was .86. After subtracting the linear phase ramp, the IQR reduced to .66 from 2.25.

    As we discuss later in Sections 4.2 and 4.3, correcting for the atmospheric delay over large heterogeneous topography is a significant challenge facing this technique. We believe our technique provides good results in this situation, but preface that the homogenous near-to-far ramp seems unique within the UAVSAR dataset.

16. L215-225: Is there any difference between the PLC and slant-range? If both is equivalent, then I guess, you are only removing a linear (or possibly quadratic) ramp from the interferogram in the slant-range radar geometry.

    PLV is the geolocated slant range. We have updated the text to just use the look vector (LKV) because that is what the file extension is called in the UAVSAR data.

17. L215-222: Partially redundant information (PLV). Please check and remove redundancies.

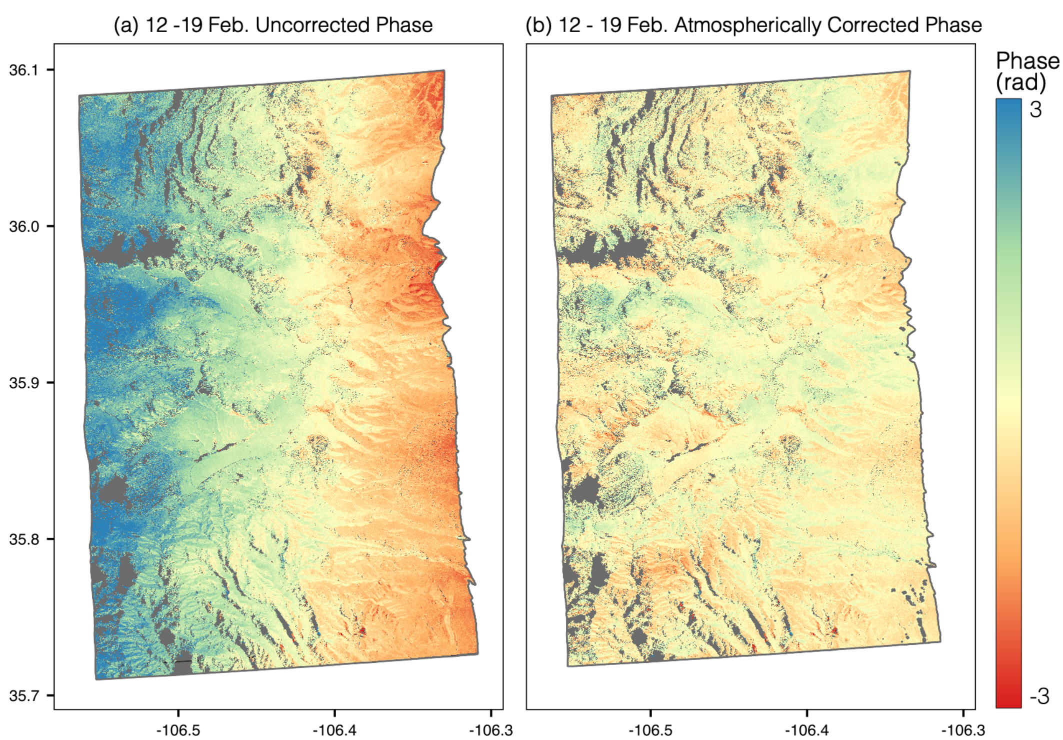
Corrected.

18. L228: raw data: do you mean the unfiltered interferogram? In the context of radar, raw data usually refers to unfocused radar raw data.

We removed the word 'raw' and replaced it with 'uncorrected'.

Text updated to: *"...we subtract the estimated atmospheric component from the uncorrected data to achieve…."*

We also updated Figure 6, which also used the word raw. Updated figure and caption below.



*"**Figure 6.** (a) The uncorrected phase from the 12--19 February pair and (b) the atmospherically corrected phase data. There is a linearly increasing phase ramp from near-to-far range (east to west), which is a distance of ~22 km."*

19. L237: "significant errors within the original SRTM DEM": Are these "errors" larger than the specified accuracy of the SRTM? I don't think so. I think these undulations are due to

phase noise in the SRTM interferograms. I would expect, that the same noise level is observed for snow free areas at lower altitude.

After closely reviewing all areas of the SRTM data as well as this DEM in other locations, it appears this pattern is consistent throughout. Thank you for catching this. We removed the paragraph about snow cover causing the error.

Text updated to: *"We found errors within the original SRTM DEM used in the UAVSAR data processing (Figure 8c). This error is likely due to phase noise in the SRTM interferograms as it is consistent throughout the dataset and falls within the known SRTM vertical uncertainty of $\pm$ 16 m (Rodríguez et al., 2006; Sun et al., 2003)."*

20. L262: "the Delta SWE values for the eight surrounding pixels" - do you mean phase values?

    No, we converted to SWE and then averaged surrounding pixels.

21. L275/276: Would averaging the HH and VV polarization (or the complex interferograms) improve the phase noise?

    Most likely, yes. We did not find phase noise was a significant issue with UAVSAR data.

22. L280: "likely caused by snowpack LWC attenuating the radar signal": Is there any independent indication that LWC in this area is larger than in the surrounding area of same altitude? When looking at the backscatter image, I interpret the very low coherence and the significantly lower backscatter (compared to the surrounding separated by a sharp transition from high to low backscatter) rather as liquid surface water or wetland, possibly below ice covered by a thin layer of snow. Could you check? You mention "riparian area" indicating wetland.

    This is a very good observation. Surface water below the snow (under a thin layer of ice) could contribute to the decrease in amplitude and coherence.

    Text updated to: *"This backscatter decrease is likely caused by snowpack LWC or subnivean surface water attenuating the radar signal."*

23. L292-298: You write about SWE changes and SWE mapping, even though Figure 8 shows results of the InSAR-derrived Delta SWE results. Try to make clearer in this Section (3.2) that you describe InSAR-derrived results, and not field-measured SWE changes.

    Section 3.1 changed to *"InSAR ΔSWE"*

24. L331: Looking at Figure 11, I think VG shows changes in snow height, not in SWE.

    In this context, VG was referred to the InSAR-derived retrievals. Per the recommendation of Reviewer #1, this section was rewritten, and this sentence was removed.

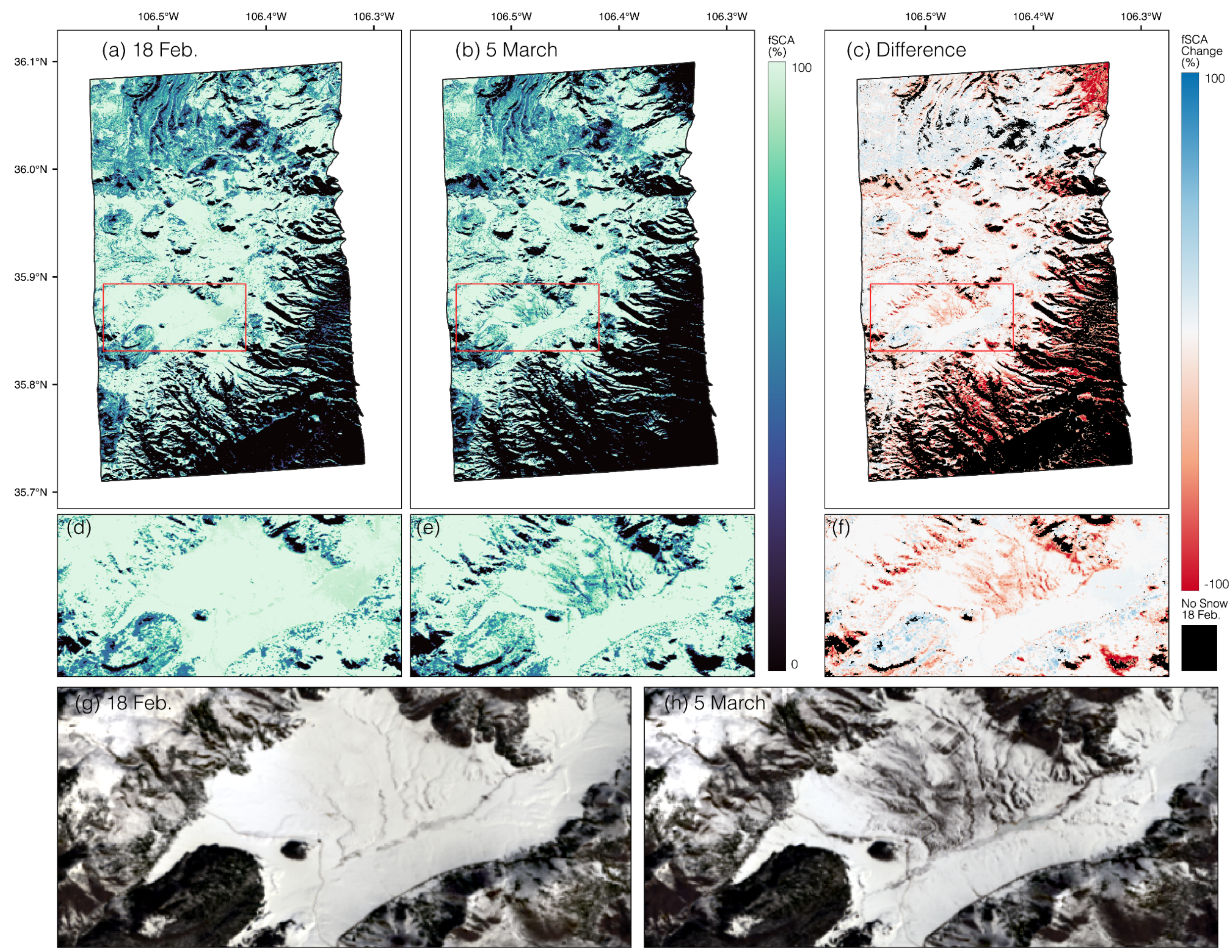
25. L334: "we used new rho_s": Do you mean, "we used a density of rho_s = 240 kg/m^3" for new snow?

    Yes, thank you for clarifying.

    Text updated to: *"We used a ρ_s of 240 kg m^{-3} for new snow measured…"*

26. L338, Section 3.5: Comparing Figure 13 a and b (Delta SWE vs. fSCA), some patterns agree. However, in the south-eastern corner, there is a 1km large area that shows -100% change of snow while the surrounding area shows no change. In contrast, the SWE change map shows no spatial variation at all of SWE for this area. Can you explain that?

    We added true color imagery of the two Landsat acquisitions in Figure 4d and 4e. The area in the SE corner you're referencing is forested and has a much higher uncertainty in fSCA estimation than open areas. There appears to be a reduction in fSCA by the darker color in the 5 March image, yet it's impossible to say if that was a complete melt-out due to the forest cover. We focus our analysis on the open VG meadow because of the higher confidence in both the fSCA and InSAR retrievals. Remote sensing of SWE in forests is a significant challenge in snow hydrology, but we don't specifically address that in this research.



*"**Figure 4.** Landsat fSCA clipped to the UAVSAR swath extent (black outline) for (a) 18 February 2020 and (b) 5 March 2020. (c) The pixel-wise percent fSCA change between the two dates, with the black area representing 0 % fSCA from 18 February 2020. The study area (red box) (a) 18 February 2020, (b) 5 March 2020, (c) and the difference between the two dates. Landsat true color image in the study area for (d) 18 February 2020 and (e) 5 March 2020."*

27. Furthermore, in the south-western corner, south of the GPRI measurements, there is a few hundreds meter high mountain where Delta SWE is negative while fSCA is positive. Could you explain also this?

    Refer to the true color imagery in #26. This SW area you reference is a denser canopy than the SE section discussed in #26. Any fSCA increases are likely due to uncertainty within the algorithm.

    We updated the text to read: *"fSCA gains are recorded in the areas south of VG, which are shown in the true color imagery (Figure 4d and 4e) to be densely forested. Uncertainty arises in forested areas from how the fSCA algorithm deals with sub-canopy snow estimation."*

28. L381: "Our GPR data provide further justification in our analysis beyond single point comparisons." Looking at Figure 12, I consider the GPR data as not convincing enough to provide reasonable justification.

We updated our GPR data analysis, as both reviewers did not agree with our original interpretation.

Updated text in Section 3.2 'InSAR vs. GPR, Snow Depth Sensors, and Snow Pits ΔSWE':

"The InSAR-derived SWE retrievals were compared to three types of in situ SWE data: snow depth sensors, snow pits, and GPR. Figure 11a is a plot of ΔSWE values from the six CZO snow depth sensors and BA pit (~3030 m), and HQ Met snow depth sensor and pit (2650 m) against the InSAR ΔSWE values. Due to many of the in situ measurements being on or near the edge of a pixel, the InSAR ΔSWE values are an average of the pixel in which the measurement falls and the four closest pixels. The InSAR retrievals had a root mean square error (RMSE) of 1.46 cm and an MAE of 1.16 cm compared to the in situ measurements (n = 27, r^{2} = 0.34). The small snowfall event noted in Section 2.3.3 is registered in the higher elevation CZO sensors and BA pit and not in the HQ Met location (Figure 5a). We see this same pattern for InSAR-based returns in Figure 9b (19--26 February), which is also shown by the mostly positive values of the pink dots in Figure 11a. The study area shows mostly SWE loss, while the higher elevation area in the northwest corner of the plot shows an increase indicating general agreement in the ablation and accumulation patterns. \par

We compared the InSAR and GPR ΔSWE between 12--26 February (Figure 11b). No significant relationship was found (r^{2} = .042), and the RMSE and MAE increased to 3.03 cm and 2.57 cm, respectively. The error metrics were calculated using the GPR data as validation, yet offsets in acquisition timing between UAVSAR and the GPR likely caused increased uncertainty when comparing the two datasets. On 12 February, the GPR acquisition began ~3 h after the 0946 LT UAVSAR flight, and on 26 February, the GPR data collection started ~2 h after the 1027 LT UAVSAR acquisition. During these acquisition time offsets, both temperature (Figure 5b) and incoming solar radiation (Figure 5d) were increasing. These atmospheric conditions presumably led to increases in snowpack LWC and ϵ_s, which would explain why 44\% of the GPR ΔSWE values showed increases when no measurable snowfall occurred in VG during the study period. We note that the presence of liquid water in the snowpack can cause a GPR signal delay that could be incorrectly interpreted as an increase in SWE. However, it should be stated that many of these points remain within the known uncertainty ($\pm$ 1 cm SWE) of L-band GPR observations for a dry snowpack (Mcgrath et al., 2019), with higher uncertainty expected during wet snow conditions.

Furthermore, the mean GPR derived ΔSWE product is ~0 cm, which matches well with the pit-observed change of ~0 cm (Table 3). The InSAR-derived ΔSWE product has a mean of -2.63 cm in VG; this indicates potential differences arise from using the pit observed ϵ_s measurements occurring later in the day than the InSAR retrievals and at the same time as the GPR survey. The potential change in snowpack properties that can occur during this time, as previously mentioned, could further explain these differences between the GPR and InSAR-derived products. However, it is important to note that these differences of 2--3 cm remain small in the context of other remote sensing techniques, especially when considering complex spring snowmelt conditions."

Updated text in Section 3.4 'Errors and Uncertainty: *"The InSAR ΔSWE retrievals showed a stronger correlation to the snow pit and snow depth sensors ΔSWE compared to GPR. The depth sensors estimated SWE from snow height at a single point location and a bulk ρ_s value from the nearby BA snow pit. GPR is a spatial observation that depends on the snow's dielectric properties, similar to InSAR retrievals. This makes the radar methods for deriving SWE more sensitive to variability in snowpack properties such as density, LWC, and ϵ_s. The GPR survey was conducted during mid-day when LWC can vary significantly as a result of increased solar radiation, which in turn increased the uncertainty in observations (e.g., 44 % of GPR pixels showed increasing SWE). The GPR measured some slight SWE increases, meaning there were increases in ϵ_s; this is a sign that melt had begun during the afternoon acquisitions. Future GPR analyses will benefit from validation data collected over larger areas, synchronous morning timing with remote sensing, and greater SWE variations between acquisitions. We believe GPR is a vital tool for future InSAR SWE validation efforts."*

Technical Comments:

29. Typo in Figure 2: Meteorlogical Staitons

    Corrected.

30. Figure 3: UAVSAR swath extend: add "(black outline)" to clarify that the fSCA map is clipped to the outline from UAVSAR.

    See #26.

31. L161-163: Could you put a label for HQ and BA into Figure 2c?

    Added.

32. L164: for each of the two snow pits?

Text updated to: *"...were located in close proximity to both snow pits."*

33. Table 2: UAVSAR start, Pit start: I guess this a local times. Could you add [hhmm] to the table header line or "start time of snow pit creation" to the table caption?

    Table 2 updated.

34. L235: "line of site" -> line of sight

    Corrected.

35. Figure 11: Could you add to the caption of the figure the elevation of Redondo peak and HQ?

    Added to the Figure 5 caption.