We would like to thank the reviewers for the detailed and very useful review. We appreciate the reviewer's time and effort. We believe the manuscript is now in a much better shape. We hope this manuscript help the community in future approaches for SWE retrieval missions. The changes are relatively minor compared to last revision.

Please find our responses below.

RC1, Silvan Leinss

Review, Revision 1 of TC-2023-95, Snow Water Equivalent Retrieval Over Ideaho, Part A: Using Sentinel-1 Repeat-Pass Interferometry

general comment:
The authors improved the manuscript significantly and I am satisfied with almost all comments.

- Thanks for your useful review. It improves the paper a lot.

Below I have a couple of minor issues that need to be fixed before publication. Specifically, I do not agree that the authors claim to provide a "more general" method than what has been suggested in Leinss(2015). Below (comment for line 140) I compare the validity range of the two approximations and show that the suggested equations (4) is valid for a wider incidence angle range compared to Eq. (17) in Leinss (2015). However, the suggested equation (4) is limited to realistic densities of seasonal while the equation (17) in Leinss (2015) is valid for all densities up to solid ice. The suggested equation is therefore slightly better suited for the analysis of S1 data, but I would not call it "more general".

- We changed it as mentioned below.

I also like to point out that the Equation from Gunneriussen, that relates snow depth, permittivity to phase, and that has been approximated to derrive SWE, is only valid when SWE is considered perpendicular to the local slope, not as a vertical water column as commonly used. I guess, the authors approximate the vertical water column with their approximation and neglect the conversion of SWE perpendicular to the local slope to the vertical water column. If this is the case, it needs to be clearly stated.

- We are also estimating the SWE perpendicular to the surface. As mentioned below we clarify it more in the paper now.

specific comments:

Abstract:
l.9: It would be helpful to provide an approximate value of the total SWE (or some order of magnitude) to have a comparison to the given RMSE and max. Error.

- We add this sentence: "The SWE change measurements vary between -5.3cm to 9.4cm over the entire time series and all the in situ stations."

Could you provide a sentence of conclusion or recommendation from your study in the abstract? What would improve the performance? Lower frequency? Higher repeat-pass intervals? something else?

- We added this sentence: "Higher frequency such as L-band improves the temporal coherence and SWE ambiguity. SWE retrieval using C-band Sentinel-1 data is shown to be successful, but faster revisit is required to avoid low temporal coherence. Global SWE retrieval using radar interferometry will have a great opportunity with the upcoming L-band 12-day repeat pass NISAR data and the future 6-day repeat pass ROSE-L data.”

<u>Introduction:</u>

l. 51: Could you add the imaging method used to map snow pack properties? Was a nadir-looking altimeter used? Or side-looking real aperture radar or a SAR system?

- We add this sentence to the manuscript to make it clear: “The system was normally deployed nadir looking and was a real aperture radar system.”

l. 50-54: This paragraph needs to be improved. According to your response letter, the reason that "FMCW systems requires large bandwidths is to avoid ambiguities". "Something that we do with prf or pulsed signal". In the manuscript, line 52, you write "These [FMCW] sensors need a wide bandwidth for large distance of a spaceborne mission with high resolution". This sentence is incomplete. I cannot follow your argumentation here, neither in the response letter, nor in the manuscript. The cited paper (Marshall and Koh, 2008) discuss FMCW radars in the context of use the term "large distance" in the context of the length of snow transects of a few hundreds of meters. They use the term "high resolution" in the context of vertical slant range resolution, i.e. they use FMCW radar as radar sounders with maximum slant-range distances of a few tens of meters.

FMCW systems estimate distances from the frequency difference f_b ("beat frequency") between the echo and the local instantaneous chirp frequency. The echo returns after a time delay \tau = 2R/c (R: slant range, c: speed of light). With that, the frequency difference is f_b = \beta*\tau with the frequency change rate within the chirp of beta = B*PRF (B: bandwidth, PRF: pulse rep. freq.). By decreasing bandwidth B or PRF the frequency differences f_b decreases. The minimum and maximum distance within the swath determines the bandwidth of f_b that needs to be sampled by the ADC. This bandwidth, however, can be significantly smaller than the bandwidth B of the transmitted chirp that determines the slant range resolution. So, frequency allocation in space limits the possible slant-range resolution (for all radar systems, not only FMCW).

- The FMCW systems have high range resolution at least for SWE on the order of few cm to detect different layers in the snow. The required bandwidth for that kind of resolution is on the order of GHz. The spaceborne allocation for NISAR is 80MHz and not more than that. Therefore, it is not feasible to have spaceborne FMCW for snow. We tried to explain this better now in the paper: “The resolution of FMCW system for SWE application is in cm scale. In order to achieve such high resolution, the bandwidth should be in GHz scale. Due to limitation on frequency bandwidth allocation of a spaceborne active sensor (Tao et al., 2019), FMCW systems cannot be used in spaceborne missions for global coverage due to their wide bandwidth.”

l.62: "achieving high resolution is another challenge of this method": do you mean the SWE retrival method or the data from SoOp? You have not explained what kind of imaging method SoOp is, but I guess, that the limited resolution of this method is the local data (like GPS) or not focused SAR. - but not the SWE retrival method.

- Yes, the challenge is for SoOp data that is not focused. However, the data will be used to estimate SWE. So, the SWE will have low resolution too. We changed the sentence to this:” Achieving high resolution for SoOp data is another challenge.”

l. 68, 71, 106, 428: "for the first time": For sure, you did that for the first time, that's the reason why its worth to publish your results in a scientific journal for "original research". IMHO, you can write "for the first time" in a newspaper, not scientific journal. Usually, when I read such phrases it does not speak for the quality of the paper. The results itself should highlight the quality and impact, not the emphasis that this is the very very first time.

- Thanks for mentioning that. I didn't realize I have used it 3 times in the paper. I guess I was excited about the method working. However, the reason we are saying this is to make it easier for the researchers to follow this kind of work in future. I think mentioning that makes it easier for the scientists to know what in this work is different from other works. To me Leinss et al. 2015 was the beginning of this method in small region and it was easier to follow what has been done afterward. I was lucky to be introduced to this work soon enough and I didn't need to do lots of literature search to realize that. Beside I have seen using this phrase in other people's work. Having said that I removed three instances and just kept this phrase for the conclusion section.

<u>Section 2:</u>

You use a very large range of incidence angles to estimate SWE. Do you estimate the SWE perpendicular to the local slope or the SWE as a vertical column even in terrain with a local slope? The equation of Gunneriussen, and therefore all derrived approximations, are made for horizontal terrain or, equally, the SWE perpendicular to the local slope - which is different from the vertical water column of a snow pack. I assume, you make the assumption that the terrain is not very steep so that both are approximately the same. Please specify.

- The terrain is assumed to be parallel to snow layer. That is the assumption we made which I think is a reasonable assumption. The SWE change that we measure is also perpendicular to local terrain as you mentioned similar to all the other approximations. It is not vertical water column of snowpack. In that sense we need to find the incidence angle to the local terrain with any sort of slope. That is why we looked at the local incidence angle (with large range depending on the local slop) not the general incidence angle for a flat train which changes from 20 to 50 or so.

140: "we try to make a more generalized approximation": Could you specify, here, what is more generalized? Later, you wrote that you have a single equation (4), that depends on \theta, for the assumption of snow density between 0.15 and 0.45, and an incidence angle between 0 and 80° (note: line 158, you specify the error only up to 70°). According to Fig. 1c, the maximum errors relative to, i guess, the equation from Gunneriussen are below 15% for densities between 0.15 and 0.45 and incidence angles between 0 and 70°. Limiting yourself to 10..60° reduces the maximum error to 5% (you won't analyze data at 0° incidence angle due to layover). Comparing that to Equation (17) in Leinss(2015): With the simplification of alpha = 1 the maximum error for the same density and incidence angle range is 12.5% (at incidence angles of 60°). The error increases for larger incidence angles (gray contours in Fig. 9, right). However, equation (17) in Leinss (2015) holds for all densities up to solid ice, but is limited to incidence angles of below, let's say, 60°. Your equation (4) is limited to common snow densities between 0.15 and 0.45 g/cm^3 but provides a lower error at large incidence angles (let's say, up to 70 or 80°). So both cover the same two dimensions (density, incidence angle) to slightly different extent. Both equations depend on incidence angle (which is usually known) and both do not depend on density (which makes both equations very valuable for SWE retrival). The "Leinss-Equation" is more accurate (I would not say more general) for high densities of snow up to solid ice, while the

"Oveisgharan-Equation" is more accurate (again, I would not say more general) for incidence angles above 50 or 60 degree and therefore probably better suited for an analysis of Sentinel-1 where such large incidence angles are likely to occur on slopes facing off the radar. So please clearly describe the differences in limitation for density and incidence angles as detailed above. I do not agree with the claim your equation is more general. I would agree that it is more general, if the equation would covers another variable, e.g. if it would be independent of the liquid water content, or if it would be at least valid for the same density range but valid for an significantly increase incidence angle range.

- I only want to mention the reason we didn't use the snow densities more than 0.5 is that the terrestrial snow density normally ranges between these values. As you can see in figure 1.c the error gets large for small density. However, I agree that Leinss's approximation is valid for wider range of snow density. We changed the text in paper and we hope this captures your concern: "The approximation is limited to a smaller range of incidence angle than Sentinel-1 incidence angle. However, Leinss et al. approximation applies to a wide range of snow density up to solid ice density. Due to the wide range of Sentinel-1 incidence angle in a frame, we tried to make a more accurate approximation for a wider range of incidence angles and snow densities limited to terrestrial snow.

In you response letter, for line 139, you write "I think our equation is applicable to a wide range of snow densities". That is not true, see above. It is only applicable to a wide range of incidence angles which makes it convenient for S1.

- We change that as mentioned above

l.145 vs. l.149: C(theta, rho) is specified with two different equations. Use a different symbol, e.g. C', for the approximation in line 149 and subsequent references to it.

- We changed it to C^ for L149.

149: "as seen in fig. 1(a), the line intercept is very close to zero": It is not clear what Fig. 1a shows (see comment above), but as the shown lines have a small curvature, these are likely C(theta, rho) from line 145. As this is not a linear function, you can't talk about line intercepts. Why not adding two dashed lines, defined by the linear function C(theta, rho) from line 149, and concluding from that that C(theta, rho) from 145 is very close to the linear function (C in l.149) without y-axis intecept.

- We added the fitted line to figure 1.a. eventhough the error of this fitting is also shown in figure 1.c. We also added to text:" The blue and red dashed lines show $\hat{C}(\theta,\rho)$ at incidence angles equal to zero and 70, respectively. As seen in figure 1(a), the fitted line with zero intercept is a good approximation. The Zero intercept approximation is essential to retrieve ΔSWE independent of snow density."

Fig. 1b: Please show the fitted values of the line slope A(theta), together with the polynominal approximation A from eq. (3).

- Done!

Fig. 1c: Please specify the caption how the error was calculated. Line 157 provides some hint.

- We add the formula to the caption too.

l.198: You specify the height of your stations between 3200m and 9520m. Do you mean feet? Maybe, you should convert feet to meters?

- Thanks for catching that, we changed it to: "975m to 2902m."

l.231: "Any SNOWTEL data with ... is unreliable in our study": Do you mean "Any SWE retrived from S1 for locations where SNOWTEL data indicates positive in-situ data is considered unreliable"?

- Correct! We changed it to: “Any retrieved SWE with SNOTEL near surface air temperature more than 0 is unreliable in our study.”

Conclusion:

l. 411: "We showed that retrieved Delta SWE..." Could you specifify if you mean the SWE between two acquisitions or the total DeltaSWE (Eq. 5) during the dry snow period?

- We mean delta SWE between two acquisitions. Equation 5 is called total SWE in this study. We make it clearer in the text too: “We showed that retrieved Delta SWE between two consecutive Sentinel-1 observations”

technical corrections:

l. 12: I do not think that the phase-unwrapping algorithms themselves degrade, rather their performance or results. I would end the previous sentence with something like "... because of unsuccessful phase-unwrapping."

- We changed it to:” We also show that the performance of the phase unwrapping algorithm degrades in regions with low temporal coherence.”

43: "restricted to passive retrival errors" - do you mean "restricted/limited by passive retrival errors?"

- Changed the “restricted to” to “limited by”.

62: check grammar of this sentence. I don't get the meaning.

- We removed this sentence. The relationship between phase and snow depth in wet snow is not relevant to this work. As you mentioned it needs more explanation.

90: to the ground and the scattering.. -> so that the scattering

- Done!

RC2, Jorge Jorge Ruiz

After the revision my concerns have been cleared. I think the new content added is valuable and has improved the article. Congratulations once again to the authors.

- Thanks so much for reviewing our work. Your feedbacks really improved the work and paper.

I have some minor comments to add:

Regarding my request about Figure 9. What I was asking for was adding some information about the spatial variability in the window you used for comparing the retrieval with the SNOTEL stations. I think that error bars containing the std within the window could add some useful information. After the revision it is clear how you addressed the calculation of the values of DeltaSWE, so I see no need of including this.

- I see, I am glad the revision makes it clear.

Regarding my comment about Line 75, it seems the equation I wrote in the comments disappeared as I copied into the portal… I was asking for an explicit expansion of DeltaDepth to DeltaDepth = DeltaSWE*rho_water/rho_snow, but I see it has been addressed in the revision.

- Yes, it is now added correctly in the paper, thanks for clarifying.

Figure 2. I think figure has improved quite a bit! Another idea that may help improve visualization is assigning different colours to the boxes of Banner Summit and More Creek. Now is not easily identifiable. I see this is explained in line 364 but perhaps it could be clearer.

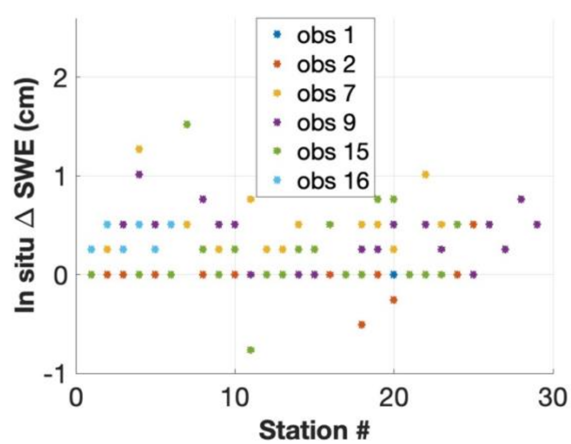
- I changed the color of Banner summit and explained it in the text.

Figure 6.c: in the text you refer to observations but in this figure, you have dates. Is a bit confusing.

- It was difficult to mention the date each time. However, we added the date of observations to the text. For instance, "observations 1, 2, 7, 9, 15, and 16 (first date of 12/01, 12/07, 01/06, 01/18, 03/07, and 03/13)".

Line 304: I don't completely agree with this claim. An accumulation of 0.5 cm of SWE should induce approximately pi/3 radians of phase. I think this is a sensible amount of accumulation at C-band. Can you specify if the value of the average dSWE is in each interferogram or along all the interferograms?

- This is correct. However what we mean is that for instance the average of swe change on day 2 is less than 0.5cm among all 31 stations (the average of swe change between 31 stations on day 1, 2, 7, 9, 15, and 16 is 0, 0, 0.4, 0.3, 0.2, 0.4cm, respectively) . So, even though the mean is less than 0.5cm, most stations have swe change of close to zero. And the correlation between retrieved and in situ is not meaningful. We added this sentence to make it more clear in the text. "Among the observation dates with correlation less than 0.35 (observation 1, 2, 4, 7, 9, 15, 16, and 17), observations 1, 2, 7, 9, 15, and 16 (first date of 12/01, 12/07, 01/06, 01/18, 03/07, and 03/13) have very small snow accumulation (the average ΔSW E is less than 0.5cm with ΔSWE close to zero for most stations)." There are couple of stations with relatively higher swe change but with most stations close to zero swe change, we won't expect a good correlation. Average swe change less than 0.5cm is sort of an indication that there hasn't been snow storm in many stations. We sort of take that as an indication of where/when we expect to a good measurement. However, for stations with large swe change for those dates and even 0.5cm we should be able to see phase change. What we mean here is that the correlation for all stations won't be meaningful. For your reference we include the figure showing the SWE change for these dates below



Figures 10, 11 and 12: Consider adding a point with the location of the SNOTEL stations. Feel free to ignore this idea!

- We added the SNOTEL station to figures 10(a) and 11(a) and explained in the text: "As shown in figure 2(b), Banner Summit covers SNOTEL 2 and Mores Creek covers

SNOTEL 21. These two SNOTEL stations are shown by diamonds in figures 10(a) and 11(a).”

Figure 13 (b): The correlation for observation 1 is quite high although it’s at the beginning of the winter. Can you provide an explanation of why this is observed?

- That is a very good observation. Note station 21 is relatively in the low LIDAR snow depth region. Looking at the retrieved SWE for the first date we see snowstorm in the high topography region. We believe, there has been snow in the first day in high elevation regions and that is what we see in this image. We added this sentence: “Although ΔSWE for station 21 is zero for the first observation, the correlation between LIDAR snow depth on 03/19/21 and total SWE on the first observation is relatively high, as seen in figure 13(b). As shown in figure 11(a), station 2 is in the relatively low snow depth region. We believe there has been a snowstorm in the high-altitude region of Mores Creek. The high correlation is simply the correlation between LIDAR data on 03/19/21 and the first snowstorm in Mores Creek.”

Line 420: I don’t fully understand this sentence. Do you mean that the retrieval degrades for interferograms with small dSWE?

- What we meant is that the correlation between retrieved dswe and in situ dswe is small for days with small dswe. We clarified it more in the text:” Interferograms with small average of in situ ΔSWE show low correlation between retrieved and in situ ΔSWE.”

Section 6: I think you could discuss how the calibration strategy may have help overcome some limitations regarding phase ambiguity. In case you consider this is of relevance.

- You are right! However, we didn’t show that in the paper and it is one of our future work to compare different reference point methods. We added this sentence: “We think using in situ station as the reference point helps reducing the phase ambiguity error, at least locally, compared to other methods for referencing the interferometric images. If the temporal coherence is large enough for the entire image to reduce the phase unwrapping error, using the in situ ΔSWE as the reference point reduces the phase ambiguity error in a larger region. Using a snow-free point or snow-free corner reflector as the reference point, cannot address the phase ambiguity in regions with deep snow.”

Technical Comments:

Figure 2. Caption: “The red diamonds show SNOTEL stations with ΔSW E more less than 2cm in at least one observation in the time series.”

- Corrected: “The red diamonds show SNOTEL stations with ΔSW E error more than 2cm in at least one observation in the time series.”

Line 241: comma? (, using the average…)

- I checked online, it seems either or doesn’t need comma!

Line 251: geometric

- Corrected, thanks!

Line 252: is negligible.

- Done!

Line 355: isn’t it clearer “two of the stations”?

- I changed it to “two stations”.

Line 394: add °C.

- Done!

Line 399: LIDAR

- Fixed!

Line 404: missing blank space?

- Done!

Line 439: double dataset in the sentence(?)

- Corrected: “With the L-band NISAR launch coming next winter, the new dataset would be a great opportunity for global SWE retrieval.”