Review

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

General comments

This paper reports results of analysis of UAVSAR data acquired during the 2020 SnowEx campaign to evaluate the capability of L-band InSAR for measuring SWE. Three acquisitions are used to form 3 interferograms, which are then compared to in situ data. The results are important as an early evaluation of the capability of L-band InSAR for measuring SWE in dry or slightly wet snow and are particularly relevant given the upcoming L-band NISAR mission. This paper could be a significant contribution to the literature, but needs some revisions. I think that it will be even more significant if more quantitative analysis is done including an estimation of the uncertainty in the SWE derivation, and from that, recommendations for future measurements. Measurement of SWE is a priority identified in the 2017 Decadal Survey and hence recommendations from the SnowEx campaign results could be are needed to support the next Decadal Survey or NASA Explorer missions.

Specific comments

L3 - Here you say that the measurement of SWE is a challenge in mountain regions. I suggest the problem is more general and is a challenge with remote sensing, period. Also, this first sentence implies that your work is in a mountain region, but you specifically selected a site without much topography to work in. I'd change this from being so specific to something more general.

L10 - I think of 'data fusion' as something more than what you did. You did use both the optical and SAR data, but not in a very sophisticated or novel way and not combining the information together in an algorithm to get more information than available in either data set. As far as I can tell, the optical data was used to make a mask, then the InSAR applied to areas within the mask. Also, there was no analysis to show that the mask was necessary for the InSAR to work. I think of this as akin to using a land mask or other type of mask derived from optical imagery, not real data fusion. I think you are overstating the analysis. I recommend 'novel method' or even 'method' rather than 'novel data fusion method.'

L13-14 - This list of validation data sets corresponds to what you present in the paper. Elsewhere you include fSCA in the list of validation data sets. I'll point those locations out below.

L63 - the phase is related to change in SWE = change in (density*depth), not to change in mass directly. It is a fine point indeed, but worth noting.

L97 - This is the first reference to a figure and it is called Figure 2. I prefer for the figures to be numbered in the order in which they are referred to in the paper. I don't know if Cryosphere requires that, but it is preferred.

L97 - You need to point to the workflow website here if it is open source. Also since this paper touts the workflow and code, it would be helpful to have an appendix or supplement describing the code in more detail. That doesn't go in the main body of the paper though.

Re. workflow website https://zenodo.org/record/7199836#.Y6Japi-B2eQ: I didn't find a README document in there describing what each python script does. Include that there and/or in an appendix to this paper.

L99 - Figure 2c is a lidar DEM not spatial change in SWE. The wording is confusing.

L100 - here you list fSCA as an evaluation product
L100 general - it would be helpful to the reader if you ended this paragraph by pointing out what will be discussed in the subsections.

L103 - "changes to the Earth's surface" - this statement is way too general.

L105-107 - Rosen citation goes after 'repeat pass InSAR' and you need to put individual citations after each topic listed. For example, Mouginot refers to ice sheets but is put after volcanic activity.

L108 - phase change related to CHANGE in dry snow SWE, not VARIATION is SWE.

L109 - The word 'rate' does not belong here. It refers to change over time. 'low attenuation' is sufficient and correct.

L119-120 - There are some issues here. 'Noise' is generally thought of as a random component or input from processing, e.g, sidelobes, but here you are lumping random and systematic errors together, but then ignoring the random noise in your description. Your biggest random noise comes from temporal decorrelation (Zebker et al., 1997). The biggest systematic uncertainty you are lumping in here is roughly as you describe, namely in knowledge of the plane's position. This should be its own term in the equation, but it doesn't appear often because for satellite InSAR the satellite position is much better determined. For UAVSAR that uncertainty is technically not with just the plane's GPS because the plane's position is determined using both GPS and an EGI. Yes, UAVSAR processing accounts for the plane's position as well as is known, but some phase change from uncompensated motion remains. That is the term that most impacts the UAVSAR phase.

L121 - 'phase influence from atmosphere' better described as 'phase contribution from change in path delay through the atmosphere'

L138 - total annual precipitation

Figure 1 and all figures - be consistent in use of (a), a, A, left, and so on in labeling the subpanels.

Fig. 1 Left - Take out the box. It is in the text and frankly the font is way too small to read. I don't like this figure at all and think it should be removed. It doesn't add anything and the placement of phi_noise is wrong, per point made above.

Fig. 1 Right - The drawing is wrong. Go back to Guneriussen to see why. Del_Ra is not correct. You need to check whether that error propagated into your code. (Just in case you need explanation to understand why the Guneriussen drawing is done as it is: The drawing depicts incoming rays from an infinitely distant source that impinge on the same point on the ground. That is why the two lines depicting incoming rays are parallel to each other.)

L150 - Table 2 does not list which UAVSAR products were used in this study. Somewhere you need to specify exactly which products you used. Did you start from the SLCs, InSAR MLCs, InSAR GRDs, standard product = HH only, or quad-pol = special request? Did you use the UAVSAR InSAR products in one case (to get their phase unwrapping) and UAVSAR SLCs in another, e.g., when you did your own processing?

L150-156 general - I have an issue with processing one pair one way and the others a different way. Just process them all exactly the same way so that a one-to-one comparison can be made. In my opinion, this has to be done. It is not optional.

Fig. 2-
1) (a) should be to the left of (b)
2) I don't see the value of (d)
3) Make (c) exactly the same extent as in Fig. 8. I don't think that it is and that makes it hard to correlate the two.
4) Add slope map showing N- vs. S-facing slopes. With the cut-off on (c)'s colorbar they are not all identifiable.
5) Show where the trees are. This is an important point later in the paper but I can't tell where there are trees.
6) BA pit is not indicated by name in (c)

Fig. 3 -
1) Font is too small.
2) put colorbar outside the plot so that it can be better seen
3) Add map of delta fSCA to make change obvious.
4) you mention fSCA in VG meadow but I can't really see that. Add a zoom image.

Table 1 -
1) bandwidth is 80 MHz

2) This table mixes technical specifications of the UAVSAR instrument with specs from the specific processed products used. I think that the last 6 refer to the products. Also, did you crop the near range to get a 16 km wide swath? UAVSAR scenes are generally 20-22 km wide. If you cropped it, why? Also, you mix specs for MLC products (az & slant rng spacing) with GRD products (ground range spacing). Did you georeference the MLCs yourself to that ground range spacing, in which case it isn't a UAVSAR spec. I can't tell exactly which products you used and what processing you did yourself.

L164 - Re 'stratigraphy' what exactly was measured? Are you saying that all the ones that follow were measured vs. depth?

Section 2.3.2 - provide uncertainties on the measured quantities. I think you mention some later in the text but that information belongs here.

L174 - You need to make it clear that this survey was done only near HQ. What days? times?

L176 - 'changes in material properties' is more correctly described as ' interfaces between material with different dielectric properties'

L181 - Describe what eight times stacking means for the non-expert

L183 - What is meant by 'first break'?

L184 - What is 'dewow'?

L185 - Is ' first break prior to the first peak of the reflection' the 'zero time'? Explain each better.

Eq. 3 - Discuss the assumptions, like uniformity of epsilon. What is the uncertainty in epsilon? What does that translate into as uncertainty in snow depth (eq. 4)? Is that uncertainty propagated into the comparison with SAR-derived SWE?

L191 - observed -> measured

L200 - you use the word 'tether' and I'm not familiar with it in this context. But when I think about it, I realize 'tie' isn't really any better, just more familiar to me. Your choice!

L206-207 - Are you listing the Michaelides paper because you followed that method? If I understand correctly, you only applied a high pass filter in what you did. (If that isn't the case, then a better explanation of the method you used is needed when you present it in later section.) If you are just presenting papers that corrected for atmosphere from airborne SAR, then Bekaert et al. can be included and their method is different than Michaelides'. There might be others that I am not familiar with.

Bekaert, D. S. P., C. E. Jones, K. An, M.-H. Huang (2018). Exploiting UAVSAR for a comprehensive analysis of subsidence in the Sacramento Delta, Remote Sensing of Environment, 220, 124-134, doi:10.1016/j.rse.2018.10.023.

Paragraph around L220 - Why are you calculating this (PLV)? The UAVSAR SLC product contains the .lkv file, which gives the slant range in ENU components including accounting for the DEM = local topography. You can sum them in quadrature to get the slant range.
From UAVSAR product spec: *LKV file (.lkv): look vector at the target pointing from the aircraft to the ground, in ENU (east, north, up) components.*

L224 - Your equation 1 has snow as a separate term from atmosphere, not 'embedded' in it.

L224 - 226 - Re. ' By only calculating the atmospheric delay of snow free pixels from the Landsat fSCA product on 18 February, we were able to confirm...' - This isn't what you did. You compare the snow free pixels to the snow-on pixels, so you calculated it for both. Maybe you mean to say ' By calculating the atmospheric delay of only snow-free pixels from the Landsat fSCA product on 18 February and comparing to the atmospheric delay of only snow-on pixels...'

L229 - Was the same correction applied to all scenes or was the same method applied to get the correction? I'd think the latter but this says the former.

L235 - Only n_hat is previously undefined. Also 'site' -> 'sight'

Fig. 6
1) You need some commas
2) 'undulating' is the wrong word, it implies motion.
3) Did you use the incidence angle provided by JPL? I think you said previously that you calculated it. Please check for consistency and be clear throughout about where you used one and where you used the other.

L241 - 'mounds and undulations' are better described as 'artifacts' in this case.

General comments on section 2.6 -
1) So far fSCA is used only for generating a mask of snow-on/off and all of the 'fusion' relates to this product so it needs to be clear what its value is. What does this mask look like? Exactly how is it used?
2) Later in the paper 'masked' pixels are attributed to phase unwrapping. So what is the impact of the fSCA mask? Does the later mask not include it at all? Is it needed at all?
3) What was SWE in the fSCA-masked areas? This is a measure of the uncertainty/error in the SWE extraction. It is a good parameter to calculate and report.

L258 - change in SWE, not SWE

L258-259 - Be more precise in your description, what does 'tether' mean in this context? Calibrating? Validating?

L282 - Your in situ measurement uncertainties on all the data need to be reported in the earlier sections where you describe the measurements. Reporting them in the results section for the first time happens a lot, so I'm not going to mention it any more, just check throughout.

L263-264 - 'eight surrounding pixels' - did you exclude the snow pit pixel itself? I can understand why you might, but it should be made clear.

L263 - Geocoding of UAVSAR is not a problem - see Fore et al. You average to reduce the RANDOM errors, i.e., temporal decorrelation.

> Fore, A. G., Chapman, B. D., Hawkins, B. P., Hensley, S., Jones, C. E., Michel, T. R., & Muellerschoen, R. J. (2015). UAVSAR Polarimetric Calibration. IEEE Transactions on Geoscience and Remote Sensing, 53(6), 3481-3491.

Section 3.1 - This entire section should be placed earlier in the paper, before or after discussing in situ data.

L275 - what does 'preserved in phase unwrapping process' mean precisely?

L276 - You need more description of the ISCE processing. Did you multilook? Filter? Coherence depends strongly on multilooking so that needs to be specified. Things like this are why I stated that the processing has to be done the same for all interferograms, otherwise comparisons don't mean much.

L281-282 - Re. 'the spatial variability in LWC...causes...' - this is assumed, not known via measurement. If you want to state this then justify it, maybe with a reference, and discuss other possible sources.

L283 - Re. 'in this riparian area' - Okay, this is why I want a map showing where the trees are and where they aren't. I can't check the images to verify what you are saying. Also, are you saying the LWC is varying only in the riparian area? What is the connection?

L283-284 - These 'artifacts' are definitely not the product of UAVSAR processing. They look to me like RFI (radio frequency interference) from external sources, e.g., FAA radars. I checked the UAVSAR products that I think you used and see that for the 2/12-19 pair those features show up in the coherence and the interferogram, but not in the unwrapped phase product. That is probably because additional spatial filtering is applied during UAVSAR standard phase unwrapping. My guess is that the details of the processing implemented in ISCE vs. used by the UAVSAR group are different, which is why the streaks don't show up in the 12-26 pair's coherence. Certainly, they can't all be due to the Feb. 19 acquisition since all the streaks in the

12-19 pair's coherence aren't in the 19-26 pair's coherence. Yet again I say to use exactly the same processing for all pairs.

Table 3 -
1) Include the 12-26 pair in this table.
2) The law of time reversal invariance tells us that the HV and VH backscatter from the surface must be the same. Differences reported in this table cannot relate to real differences in the surface scattering. Any differences in measured values come from errors in calibration, instrument cross talk, etc. Therefore, when making PolSAR products for UAVSAR, the average of HV and VH is used for a generic 'HV' product, the cross-polarization normalized radar cross section. You should do the same in your analysis. Otherwise, remove the values from the table since you only use the HH polarization in the end. The HV and VH products are provided separately for people working with very dark scenes who want to estimate the noise, and that is not the case here.

L288-289 - I don't agree with this statement. Most of the area around the VG meadow doesn't seem bad. It certainly phase unwrapped. If you are pointing out something important, it is definitely not obvious so add a figure and maybe quantify the difference.

L293 - values shown are not just in VG

L298 - It would be valuable for you to quantify the difference between S and N facing slopes.

3.2 Changes in SWE, general - You don't show the change in SWE for the BA area, but that is where the snowfall occurred. Please show that since you are using HQ as your reference. Being able to measure the SWE at BA is very important to your study, and if it is inconclusive or just very different then that too is important for estimating the uncertainty. Your study is the first of its kind and it is important to report both success and limitations so that future studies can improve on it. This is an opportunity to discuss in the conclusions what improvements need to be made (more snow, more frequent measurements, etc.)

L303 - 'small storm' - all information about this and snowfall need to go in the data section, much earlier. This includes Fig. 14.

L311 - Title needs to be Changes in SWE: InSAR vs. GPR and snow depth sensors, and hence you should combine sections 3.3 and the LAST SENTENCE of 3.4

L320 - I do not understand why this is 'likely' to be the MAXIMUM error. A 5% uncertainty in GPR can be positive or negative. Also, you didn't propagate all errors to get a maximum.

Section 3.4 -
1) All but the last sentence belongs much earlier in the data section.
2) What about discussing the results? The bias is much less than for GPR (Fig. 12). Some discussion of why is needed.

L338 - Why is the comparison only done for 12-26 pair? Shouldn't it be to the 19-26 pair? That is much closer in date to the optical data's dates and the snow fall event happened after 2/19. Compare to the 19-26 pair.

L344 - The loss on S-facing slopes is not easy to see. Like I mentioned above, we need a map showing slope directions and, hopefully, some quantification of differences between S and N. Alternatively, you could show plots with just the values on the S slopes and just the values on the N slope.

L353 - Like I mentioned above, we need a map showing where the forested areas are. You could overlay an outline on figures if you want to highlight something.

Fig. 7 - add points to show location of BA and HQ sites on the LH plots.

Fig. 9 - Add a comparison of values inside VG vs. outside VG. Also make it clear exactly what area is covered by the histogram. I think it is the entire scene extent in Fig. 7 but it isn't clear.

Fig. 10 - Instead of having V and H axes, have the 1:1 axis at 45deg. Also the colors for the two data sets are too similar.

Fig. 11 - This goes much earlier, in the data section.

Fig. 12 - Your fit includes Feb 12-26 CM. That overcounts the short temporal baseline pairs relative to the independent Feb 12-26 pair. Remove the points for Feb 12-26 CM from the graph and recalculate the fit.

Fig. 13 - As said above, use 19-26 Feb instead. Maybe add a plot of delSWE vs. DelfSCA to show the correlation better.

Fig. 14 - Move to the data section. Extend plot out to the end of the fSCA data. Show fSCA (Landsat) acquisitions as vertical lines also. Add plots of wind speed and maybe direction since you mention that information in section 4.1.

L368-369 - Sentence 'While ...' belongs in the data section.

L373-374 - I think that your argument at change in LWC caused the InSAR decorrelation also supports the statement that water moved downslope.

L378 - Given the lack of snowfall, I think you could do a quantitative comparison, as I've suggested above.

L381 - 'remarkably well' - I would avoid qualitative statements like this. I think this is an overstatement and without more quantitative comparisons I would not draw this conclusion.

L382-383 - I think that all your GPR data did was show a bias of -2 cm wrt InSAR, and that number disagreed with the snow depth measurements. Rather than general, and arguable, statements, use the in situ data to quantify the uncertainty and then use that to recommend different, i.e., more, InSAR measurements and different, ie., possibly more, in situ measurements in the future. Remember that this is one of the first studies and you can use it to justify and lay out a plan for the future. It wouldn't be a bad idea to reference the targeted observables of the Decadal Survey and suggest missions for it, possibly in conjunction with NISAR.

L383 - I suggest adding the need for higher SWE during the measurements.

L392 - The calculation goes in earlier in the paper, but you should add a discussion of the conclusions re. LWC change here.

L401 - I don't agree that this is known to be the maximum bias. I don't think that was shown.

L405 - This is the first mention of 'lightly forested areas'. I don't think that this paper as written has really explored the difference between forested (heavy or light) vs. unforested in a quantitative way. In the event that you do, then discussion is justified.

L408 - could be water content also

L416 - statement about Eppler study goes in data section, then can be referred to here.

L431-432 - Tandem-X is X-band, so likely to measure canopy height. Why do you think that it will be better at getting the land surface than SRTM? I didn't read the entire reference but it would be worth stating quickly why if this statement is correct.

L454 - In fact, you showed that it WASN'T necessary to identify snow-covered pixels to do the atmospheric correction. Or at least that is my take-away from Fig. 5.

L454-455 - You never showed the difference between using a snow-on mask vs. not using a snow-on mask anywhere in this paper. I cannot therefore conclude that it is necessary. In fact, the great value in using that is in determining the uncertainty in your SWE measurement, which was not done. It should be.

L460 - Ditto above - need for this was not demonstrated to this reviewer. If you think that you showed it, then make it more obvious.

L468 - List delfSCA as a validation data set here.

Data and code availability - general - Specific product names for everything you used needs to be provided. It is not enough to point to huge databases where someone searches and guesses what exactly you used.

technical corrections

The entire manuscript needs to be read over to identify and correct errors in grammar, spelling, and language usage/wording. I only point out a few below, but there are lots of places that deserve more attention.

Everywhere - 'data' is plural, so data are, data were
L36 - 1970s
L71 - pairs from the Envisat...
L72 - lack of in situ
L76 - its
L104 - 'can calculate' needs to be 'can be used to calculate' or 'is related linearly to'
L 140 - have two 'the'
L 207 - correct, not corrected
L335-336 - 'pixels they're located' -> deltaSWE
L415 - in situ measured epsilon...

references

incomplete citations for Mouginot , Brucker, Sandmeier, Selkowitz
L580, L681 - why all caps?
L607 - check, something is wrong