

The Cryosphere Discuss., author comment AC3
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Reply on RC1

Hans Lievens et al.

Author comment on "Sentinel-1 snow depth retrieval at sub-kilometer resolution over the European Alps" by Hans Lievens et al., The Cryosphere Discuss.,
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Author responses below are in italic.

This paper builds on the work of Lievens et al., 2019 to extract snow depth from S-1 data in the Alps. As mentioned by the editor, this work is of high relevance to the snow community but also to many other research areas such as water management, tourism, climate change and biodiversity. I appreciate the work that is done here but in its current state, I cannot recommend this paper for publication since I feel there are too many unknowns and too much processing done on the S-1 imagery to be able to retrieve some sort of good quality snow information and give a proper assessment of the results shown here. This is reflected in my comments below.

Contrary to what has been stated by the authors in their response to the editor's comments, I am not skeptical of the relationship between the C-band signal and thick alpine snowpacks. I do question the physics of the approach used in this study and am concerned about the multiple layer of data smoothing in order to get good correlations with modelled data.

If the authors are willing to provide more information on the imagery processing and modify it to make it more physically accurate, I strongly believe this work has great value to the scientific community.

We would like to thank the reviewer for the detailed assessment of our work. We have responded earlier to a selection of the reviewer's comments. In addition to that, a point-by-point reply is given below.

To address the reviewer's main concern about the processing of the S-1, we have carried out a full re-processing of the S-1 data across the Alps with a revised methodology (see details below), without any significant change in the results or conclusions.

We however strongly disagree with the statement that the good correlations with modelled data are due to the multiple layer of data smoothing. The smoothing applied here is limited, as discussed below.

We are hopeful that the revised processing of the S-1 data and a more detailed discussion of the processing and algorithm steps (including some modifications, e.g. regarding the wet snow detection) adequately address the main concerns of the reviewer.

General Comments:

As mentioned above, I do agree with the authors that the cross-pol channel of S-1 can be sensitive to a thick snowpack but I disagree with the physical explanation of the authors. The physical interaction of the microwave signal with the snowpack is very complex and is not solely related to surface/volume scattering and single/double bounce. With snow layer thicknesses close or smaller than the wavelength, you have many interference and coherence effects in the signal. Recent work has shown that volume scattering and depolarization of the SAR signal comes mostly for the snow anisotropy (Leins et al., 2016) and the vertical/horizontal structuring of the snowpack at C-band. This can be achieved by a stratified snowpack horizontally or with snow grains that are structure vertically/horizontally through metamorphic processes. I would agree that with a thicker snowpack, chances are you will get more anisotropy but this is not shown with in situ measurements, temporal analysis or snowpack stratigraphic information.

We appreciate the reviewer's comments on the physics of the signal. Although the mentioned work by Leins et al. (2016) investigates only higher (X- and Ku-band) frequencies, we agree that the anisotropy of snow crystals and/or of clusters of crystals, as well as the snow stratigraphy, can play an important role. We will discuss this upon revision.

With all the processing done to the SAR imagery, it is impossible to assess the physical interactions of the SAR signal with the snowpack since the data has been smoothed multiple times and transformed radiometrically and geometrically. You have multi-looking (averaging 10x10 pixels), border noise removal, thermal noise removal, terrain correction and reprojection to the WGS84 projection. The multi-looking is especially concerning given the topographic complexity of the Alps. It is smoothing all the topographic information (which is crucial for snow retrievals) and emphasizing only the areas of significant snow (snow drifts) which is not representative of a 100m grid cell in the Alps. Then you add incidence angle correction using a DEM (30m) that is of lower resolution than the pixel spacing (10m) of the original image. A DEM with similar resolution should be used but also, the topographic information has already been altered from the multi-looking which is not representative of the local topography. Then there's temporal averaging (Eq.2) which alters the signal even further. Finally, outliers are replaced by a 12-day average to smooth the data once more.

We would argue for the contrary: a careful processing is a pre-requisite in order to assess the correspondence between the S-1 signal and snow depth. The processing steps included in our analysis (border noise removal, thermal noise removal, multi-looking, terrain correction and reprojection to a consistent grid) are all standard and necessary procedures, recommended by any manual or handbook on SAR processing. The multi-looking is arguably the only processing step that could be considered optional. However, this was included in order to (i) reduce the impact of radar speckle, (ii) reduce the processing time (note that more than 4000 S-1 images were processed), and (iii) reduce the data storage requirements. In this context, the multi-looking is an important step to keep the processing computationally feasible also for larger areas, not limited to the Alps. However, to address the reviewer's comment about the correction with the DEM, we have re-processed the S-1 data over the Alps, by performing the range-Doppler terrain correction and terrain flattening at the 20 m S-1 resolution instead of at the multi-looked 100 m pixel spacing. We kept the original 30 m DEM (SRTM 1Sec HGT) because this is the standard suggested DEM for processing in the ESA SNAP toolbox and can also be applied in other regions (e.g., outside Europe, or where more detailed DEM information is lacking). However, the pixel sizes of the DEM and the S-1 data were now much more similar with the re-processing. Equation 2 is not performing temporal averaging as stated by the reviewer. It applies a bias correction (of the first two order moments, i.e., the mean and variance) to every individual backscatter observation, without averaging

observations over time. The bias correction reduces the differences between observations from different orbits (e.g., caused by different incidence or azimuth angles) and we strongly recommend this step for any application that aims at combining information from different S-1 orbits. We have deactivated the outlier correction in the retrieval because we observed it was slightly interfering with the wet snow detection algorithm.

Further on the processing, I would avoid talking about sigma-nought when Eq. 1 converts the sigma-nought into a pseudo-gamma-nought multiplied by $\cos(40)$. I say pseudo here because the incidence angle used to convert sigma-nought is the 100m reprojected angle and not the gamma-nought values from the SAR imagery calibration.

At the time we processed the S-1 data, the calculation of gamma0 was not operational in the SNAP software version 7. In the revised processing, we appropriately calculated gamma0 using SNAP version 8, by first calibrating the backscatter observations to beta0 and subsequently applying terrain flattening. Hence, the analysis in the revised manuscript will be carried out using gamma0 and is thus more conform the state-of-the-art.

If we accept the processing chain of the SAR imagery, it is still unclear that what the correlations are showing is linked to the snow depth. The errors obtained from the SAR retrievals (Figure 11) are most of the time larger than the precision of the reference data which is the model simulations. It is very difficult to determine that the correlations are statistically significant in this case and also looking at Figure 10, most of the comparison points are grouped around 0 which tends to falsely boost the correlation.

We do not see any reason that supports not accepting the processing chain, especially considering the re-processing discussed above, which is fully compliant with the state-of-the-art. Figure 11 shows a comparison against in situ measurements as reference data (not model simulations). We are surprised that the reviewer questions the significance of the time series correlations in Figure 11, which are most of the time above 0.8 (for sites that feature snow depths thicker than a meter). We can provide an assessment of the significance along with the reported correlations in the revision. With respect to Figure 10, we agree that the abundance of low snow depths can impact the correlations. Therefore, Figure 5 shows time series correlations (against model simulations) both with and without the exclusion of zero snow depths. Even though more data are clustered around low snow depths in Figure 10, the density plots in our opinion still clearly demonstrate the overall agreement between the S-1 retrievals and the in situ measurements also for the high snow depths, especially for the coarser 300 m and 1 km retrievals.

Given that modelled data is often smoothed and often have difficulty capturing extreme snow conditions and that the SAR data has been smoothed many times and outliers replaced by temporal means, I can't say I am surprised to see a good empirical relationship.

We disagree with this comment, as the S-1 processing does not include multiple smoothing steps as the reviewer states (see above). In our opinion, the strong relationship between the S-1 retrievals and the model simulations is encouraging, and is furthermore corroborated by the strong correspondence between the S-1 retrievals and in situ measurements.

Also, asking scientists to identify themselves in order to get access to the data used in this study does not comply with the open data policy.

We understand this comment. To share the snow data over the Alps, we have used the existing platform via which we also share the corresponding retrievals across the Northern Hemisphere mountains. Upon revision, we can provide the login details directly to access

the ftp site anonymously.

Specific comments:

P.3L.5: I would disagree with the claim that an increase snow depth automatically causes an increase in volume scattering. If there is not sufficient anisotropy in the snowpack, there will not be any volume scattering in C-band. The theory will show that even if you increase the snow depth and keep all other snowpack parameters constant, you will not have a significant increase in volume scattering

We will better address the impact of snow microstructure in the revised manuscript. However, recent radiative transfer model simulations using Bic-DMRT have shown that cross-polarized backscatter at C-band can increase with an increase in SWE (or depth) while keeping other parameters (snow grain size, snow clustering) constant (personal communication with Prof. L. Tsang, University of Michigan).

P.3L.6: Again, this comment is highly dependent on the stratigraphy and anisotropy of the snowpack. This section needs to be supported by snowpit measurements of the studied area or referred to past work done in the area analyzing the snowpack properties.

The statements on P.3L.6 are general assumptions based on which the empirical change detection retrieval approach is built. We have not yet analyzed these assumptions using snowpit measurements as suggested by the reviewer, but this is foreseen in future research. However, Figures 3 and 4 (based on model simulations) support the statements that (i) an increase in snow depth generally increases (especially cross-pol) backscatter, that (ii) the snow scattering (in cross-pol) is not negligible compared to the ground scattering, and (iii) that ground surface properties remain relatively constant in time due to the insulating properties of snow, thus the main changes in backscatter over time relate to changes in the snowpack.

P.3L.7: This comment is most likely true for the studied area but again, no reference or field measurement is provided to support this claim.

Please refer to the response above.

P.3L.9: Again here, I strongly disagree with this claim. The microstructure, anisotropy changes and stratigraphy, especially in the bottom layers of the snowpack will most likely drive the changes in σ_0 .

We will further investigate the impact of microstructure and stratigraphy in future research, based on tower-mounted radar measurements currently being collected in the Rocky Mountains, US. We will generalize the statement to "the main changes in σ_0 over time can be related to changes of the snowpack".

P.3L.30: Even though this is common processing of SAR imagery, this is considerably altering the SAR signal, considerably smoothing it and making it very difficult to link to any ground snow properties.

The alternative (i.e., not performing thermal noise removal, border noise removal, radiometric calibration, and terrain correction) would lead to inferior processing results, which we believe would be far less suitable to investigate the relationship between backscatter and snow depth. The multi-looking has been adjusted and is not impacting the terrain correction and terrain flattening in the re-processing. Please also refer to our responses above.

P.3L.32: Multi-looking (or block averaging here) is a good way to reduce speckle noise in

flat terrain. Here though, the topography is very complex (as mentioned by the authors) and it is emphasizing on the geometric distortions and the areas of significant snow (snow drifts) which is often not representative of a 100m grid cell in alpine areas.

Please refer to our responses above.

P.4L.10: Using "local" incidence angle correction on a multi-looked image is not an accurate method. A DEM with similar resolution as the raw image should be used to correct for local incidence angle before multi-looked.

This has been addressed by the reprocessing to gamma0 with terrain correction and flattening being applied at the 20 m S-1 resolution.

P.4L.15: This relationship was developed for areas of flat terrain and is not representative of the studied area. Proper analysis of the backscattered signal as a function of local incidence angle needs to be conducted in alpine areas in order to find the proper normalization relationship. A before and after image should show that this is not normalizing the image properly. Also, this is exactly taking sigma-nought and converting it to gamma-nought and then multiplying it by $\cos(40)$.

This comment has been accounted for by processing to gamma0.

P.4Eq.2: Here again, temporal smoothing of the data. There's no way of linking the spatio-temporal snow properties of the original SAR imagery.

We disagree. Equation 2 is not performing temporal smoothing, but bias correction, which results in an improved S-1 processing quality and therefore benefits the analysis with respect to snow depth.

P.4L.27: Excluding March to July is very subjective here. First, it is removing a lot of snow properties variability which can occur in March. Anisotropy and stratigraphy is stronger in the later winter season. Second, with climate change, we know that wet snow is detected outside of this period.

In the revised version, we will limit our entire analysis (processing and retrieval evaluation) to the winter season until end of March, to avoid the strongest impacts of wet snow (from April onwards). We fully agree that wet snow can also impact the observations earlier than March. We will revise the wet snow detection in the retrieval algorithm, to not limit the detection only to the period from February onwards (as is currently the case).

P.4 L.30: This is not rigorous. Removing outliers is another method to smooth out the data and get better correlation with modelled data. But here they are not only removed, they are replaced by a smoothed average.

We have deactivated the outlier removal, because we observed it was slightly interfering with the wet snow detection. More specifically, the outlier removal caused some wet snow events to be undetected, because the backscatter had been modified by the outlier correction.

P.5 Eq.5: Is A applied to the ratio or only the cross-pol channel?

A is applied only to the cross-pol channel, enhancing the sensitivity to snow depth which is primarily driven by the cross-pol observations.

P.6L.17: I appreciate this approach where the index varies in time but I feel like the threshold is still limiting. I would see a temporal analysis of the SAR signal through

multiple years to try and identify the proper threshold.

We have tested a range of threshold values. The lower the threshold, the better wet snow impacts are reduced, however, at the expense of reducing the coverage. We identified a threshold of 1.25 (which may be revised to 1.5) to strike a balance between wet snow filtering and data coverage.

P.6L.25: Again, the February start is very subjective as wet snow conditions can be detected earlier and the September-November period is most likely to be the period where you have the highest backscatter and all the values that are 3dB below might be because of small surface moisture or percolating water which is not uncommon in Alpine snow.

In the revised version, we will activate the wet snow detection earlier (in January), and include a second wet snow detection mechanism. The latter will consist of (i) excluding backscatter observations (for any time step during the full snow season) that are a threshold (e.g., 2 dB) below the 10-percentile of backscatter observations during snow-free conditions (this approach is more similar than that proposed by Nagler et al., 2016), and (ii) excluding negative snow index values from January onwards. More specifically, approach (i) improves the detection of early wet snow, often in autumn, whereas (ii) mainly improves the wet snow detection in the valleys, where a sharp decrease in backscatter during snowmelt is often lacking. Furthermore, the wet snow will be provided along with the unmasked snow depth retrievals, allowing the user to choose whether or not to mask out wet snow, or to use another mask (e.g., derived from modeling or an alternative wet snow detection approach). In the 300-m and 1-km datasets, the wet snow will be provided as a fraction (0-1) of wet snow pixels, which allows a user to define the level of wet snow allowed. A first evaluation of this approach at the 1 km resolution shows for instance that a spatio-temporal correlation of 0.84 is obtained when no masking is applied. This correlation improves to 0.90 when eliminating pixels with a wet snow fraction larger than 0. Intermediate performances are found when applying fractions in-between.

Conversely as the reviewer hypothesizes, the September-November period is typically the period in time with the lowest S-1 backscatter values, especially in cross-pol (if not including wet snow conditions in spring). Earlier (in summer), vegetation often contributes to higher backscatter, whereas in mid-winter, a higher backscatter is caused by snow accumulation. Part of the lower backscatter values in September-November can also be explained by the potential freezing of the soil surface, and/or by early wet snow.

P.11L.7: There is no mention of layering and anisotropy which is most likely the main reason of signal backscattering of dry snowpacks.

We will include this in the revised version.

P.11L.11-13: These comparisons do not really apply to the current studies. As was mentioned by the authors in the response to the editor these studies were conducted in shallow snow conditions in tundra/taiga landscapes.

The Alps include areas with shallow snow for which the references to literature are relevant. The literature comparison also helps to indicate that (to our best knowledge) studies with cross-pol observations in deep snow are lacking.

P.11L.20: This is a strong assumption since in alpine regions you can have strong surface roughness that will depolarize your signal.

The ratio of cross- over co-polarized backscatter is considerably lower in areas with limited vegetation. Hence, this statement is supported by S-1 observations.

P.11L.33: This is normal since most of the volume scattering and depolarization will come from the forest cover. For this study, I would have masked out the forested areas because this adds unnecessary complexity to a study that is already complex. Masking the forested areas would allow to focus on the snow retrieval without getting confused in multiple empirical relationships and heavy data processing.

One could either mask out the forested regions, or stratify the performance based on forest cover. We here opted for the stratified performance assessment (see Figure 6), which is more complete. We do not consent with the assessment of 'heavy' data processing.