The Cryosphere Discussions

The Cryosphere Discuss., referee comment RC1
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Comment on tc-2021-74

Anonymous Referee #1

Referee comment on "Sentinel-1 snow depth retrieval at sub-kilometer resolution over theEuropean Alps" by Hans Lievens et al., The Cryosphere Discuss., https://doi.org/10.5194/tc-2021-74-RC1, 2021

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.

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.

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.

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.

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.

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.

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.

Specific comments:

P.3L.5: I would disagree with the claim that an increase snow depth automatically causes an increase in volume scattering. If their 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

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.

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.

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 sigma0.

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.

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.

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-looking.

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).

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.

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.

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.

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

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.

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.

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

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.

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

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.