Review of: Evaluating Snow Depth Retrievals from Sentinel-1 Volume Scattering over NASA SnowEx Sites

General Comments:

This paper analyzes the performance of the Sentinel-1 snow depth retrieval developed by Lievens et al. on an independent dataset acquired during the SnowEx campaigns of 2019-20 and 2020-21.

This paper is well structured and well written. The fact that the authors have put in the work to make this algorithm open source is a big accomplishment and is of major relevance to the snow community. I really appreciate the amount of work this takes to publish such an open-source framework. This algorithm is highly debated in the snow community and was previously difficult to reproduce. This in itself warrants publication.

That said, given the empirical nature of this algorithm and the diverse set of geophysical properties of the SnowEx dataset, there are some major comments that I think should be addressed before publication in order to iron out the applicability of this algorithm.

Major Comments:

The first major comment relates to the interpretation of the cross-polarization signal with different snow properties. Unlike many previous publications, I appreciate that the authors try to define the scattering mechanisms that can depolarize the signal. The authors state themselves that the poor understanding of the radiative transfer mechanisms at C-Band is a weakness of this algorithm. This is very complex and needs deep understanding of the snow physical parameters but also of microwave radiative transfer physics of snow. I feel like some textbook information on signal depolarization within snow is needed in the introduction section (section 1.1). I also added comments which can help to improve the description (see minor comments).

I also think that the impact of snow on the S1 signal ratio is a bit simplified and could be improved by looking at the evolution of the polarizations individually. There seems to be more in Figure 8 than what is discussed, and looking at the polarizations individually could improve the interpretation of Figure 8.

This might be out of scope for this publication but one criticism that the Lievens et al. algorithm has is its empirical nature, and how a single set of parameters and one equation is used for a wide range of landscapes analyzed in this study. I would add a section to the Results section where different retrieved parameters for the different landscapes (forest cover, altitude, snow depth) would be analyzed. A simple comparison between the parameters presented here and the ones from Lievens, with regards to the landscape properties, would be very relevant. Some of this information is already included in the appendix and could be included in the results. The fact that there is imbalance in the data points for the different landscape properties indicates that some landscapes will show better results and others not.

Last major comment relates to some part of the results/discussion needs to the quantified rather than simply described qualitatively (see minor comments).

Minor Comments:

L.6: I would remove "we develop the first open-source software package" here and add the "first open-source software" part in the sentence of line 16. This sentence should focus on the algorithm implementation and its application. Moving this part will make the abstract more concise and avoid repeating information.

L.10 I would include the nRMSE here. 0.92m of RMSE when we don't know the mean snow depth is difficult to interpret. Alpine snowpacks can have several meters of snow.

L.12 Just a personal preference but I would call it the "cross- to co-pol backscatter ratio" (CCPR). The cross-pol ratio sounds more like VH/HV. But it is defined in the text so there is no major issue with it.

L.13 remove "cross".

L.14-15: I would reword the last part of the sentence. It is not clear to me is there is or not a correlation/relationship.

L.16: correct "frame work" to "framework".

L.24: I would change "the defining hydrologic variable of the seasonal snowpack" to something like: "an important hydrological variable of the seasonal snowpack". It's not the only important hydrological variable for water management.

L.33-34: I would break this sentence into two parts. I would put the correlation length part in the second sentence. In terms of correlation length, I imagine it relates to snow surface spatial auto-correlation. Correlation lengths is used in many ways in snow remote sensing, a bit more detail would be useful here.

L.36: I would specify here that it's for the NASA SWE product. The algorithm uses 19 and 37GHz where the 37 GHz saturates but the lower frequencies of AMSR-E and AMSR2 are sensitive to deeper snow than this.

L.52: I feel like the paragraph with the overview of SAR methods should site the review paper of Tsang et al. (2022) which is cited further in the paper.

L.53: change "a type of" to "an".

L.57: add "and polarization" to "SAR signal's frequency".

L.57: replace "for retrievals of" to "to retrieve".

L.67: delete "a".

L.72: no need to specify the frequencies here but it can span further than 40 GHz

L.74: Needs a textbook reference here.

L.75: The citations are in a sentence alone.

L.79: delete substantial.

L.79: replace “the depth of microwave penetration” to “microwave penetration depth”.

L.96: there is also second order scattering, i.e. the impacts of multiple scatterers from multiple interfaces in different orientations (roughness) which increases the cross-polarization backscatter. This effect could be highlighted by a thicker snowpack.

L.97: these anisotropic clusters are rarely at the surface and are created within the snowpack via metamorphism. It requires a strong temperature gradient which can be amplified by a thicker snowpack. A good reference for the impacts of the anisotropic snow grains is the publication of Picard et al. (2022) on “microwave snow grain size”.

Figure 1: Not exactly how a SAR system works, here it seems like the emitter and receiver are on two different platforms. Also, the volume scattering at C-Band is a very small portion of the backscattered signal. The other components should be highlighted.

L.103: Again, the cross- to co-pol ratio is more intuitive to me.

L.122: IW could in theory be HH+HV but VV+VH is the preferred polarizations over land. I would rephrase: […] IW swath mode, dual-polarized vertical transmit, and vertical/horizontal receive (VV+VH).

L.130: multi-looking is used in many different flavors for SAR applications. Do you mean a 3x3 block average, which reduces the 30m resolution to 90 m resolution?

Figure 2: The boxes are small. Subplots of close ups of the three bigger AOIs would be more useful here, i.e. three maps and their bounding boxes could be shown in the broader map at the top right.

L.163-164: These lines seem out of place. Delete?

L.180-181: The coefficient values are results and could be discussed with regards to the Lievens parameters. I would move this to the results section in a new section on the parameterization. Some info from the appendix could also be included in this new section.

L.186-195: This section should be in results/discussion

Table 2: I would include the nRMSE as well.

L.233-234: Given the empirical nature of the retrieval method, it would be useful to determine what is the fraction of pixels that belong to the different "classes". This alone could explain these results if you have an imbalance dataset where most pixels/measurements have deeper snow, moderate FC, and are at higher elevation.

Not surprised with the coarser resolution since you remove the spatial variability of snow and landscape properties.

L.241-243: This needs to be quantified and not just analyzed qualitatively.

L.246-248: Also needs to be quantified.

Figure 8: This figure shows more than what is discussed here… The fact that <0.5 m you have no change in deltaCR indicates that you have no significant change to the signal coming from the snow. Then the drop seems to indicate that there is attenuation of the deltaCR with .5 to 1m of snow. Finally, with 1m of snow and more, there seems to be an added contribution to the deltaCR (volume scattering?). That figure in itself is not sufficient to explain the different scattering mechanism in action. The same figure but for VH and VV separately would help identify which mechanism dominates and its link to snow depth.

L.262: It's the first time SNR is mentioned. It's an important aspect of SAR retrieval algorithm. I imagine you mean an increase in SNR for VH (VV should be fairly stable in dry snow conditions).

L.263-266: No-correlation needs to be quantified.

L.267: I would even reduce it to snow-air and snow-soil interfaces since the dielectric contrast between two snow layers is very low, and vertical polarization is less sensitive to interface reflectivity than horizontal polarization.

L.270-271: This should be included in the introduction with the SAR sensitivity to snow.

L.277-278: This could be due to the low number of sampling points with low FC to retrieve the new empirical parameters of Eq. 1.

L.282: SNR could be a way to mask out some pixels. Wet snow will usually show a very low VH signal which, depending on the surface roughness, would be close to the noise floor.

L.307-308: I was under the assumption that the parameters were fixed using the 90m scale data. This second point seems to indicate that the parameters were changed for different resolutions. Please clarify.

L.322: Not necessarily true, this is true for passive microwave signals. Madore et al. (2023, see chapter 6) have shown that K-Band signal could be sensitive to several meters of snow which is at a higher frequency than Ku-Band.

L.331: Retrieving eq. 1 parameters on dry snow conditions tested? or all data was used.

L.336: but there seems to be a detectable decrease between <.5 and .5-1 m of snow. Any possible explanation?

L.343: That is a major weakness of this algorithm. It could be included in the introduction since it was mentioned by most reviewers publicly in the original Lievens paper in The Cryosphere.

L 350: Including passive microwaves is very difficult in high topography areas with its coarse resolution.

L.350: I would also include polarimetric SAR approaches.

L.353-354: I agree that this is more important than trying to tune an empirical algorithm for the infinite number of landscapes we can find globally.