June 25th, 2024
Evaluating Snow Depth Retrievals from Sentinel-1 Volume Scattering over NASA SnowEx Sites
By: Z. Hoppinen et al.

Reviewer comments are shown in black. Responses are in blue.

Response to Reviewer #4:

General Comments:

Lievens et al. (2019)'s work showed very good results for estimating snow, and had some debate in the snow community on why/how it works. This works is very valuable to verify the retrieval finding.

Thank you for these valuable comments. Please see our responses below.

General Comments:

The fact that they showed the results don't match well enough the LIDAR data is very important. However, the fact that the Hoppinen's snow depth results also differ from Lievens, is not a good sign and need to find the reason and maybe find A, B, C, they have used. On the other hand, I would like to see the same verification that Lievens did, i.e. comparing with in situ data. I think the authors can compare the estimated snow depth with WUS snow depth and see if they get the same results as Lievens or not. If they did not then it is safe that the results are not verified. If they do, it may be errors in LIDAR data in mountainous regions or something.

The main challenge with comparing the two snow depth datasets is that the published methods in Lievens et al. (2022), which we follow here as closely as possible from their description in the paper, differ from the actual methods used to generate the data and figures in the paper as well as the data available in the C-SNOW repository (https://ees.kuleuven.be/eng/apps/project-c-snow-data/). This discrepancy was confirmed to us by personal communication with Dr. Hans Lievens which we cite in our manuscript (line 216). Additionally there is little to no documentation or metadata available on C-SNOW to clarify which data version relates most closely to the published 2022 methods.

Lievens et al. (2022) report A, B, and C parameter values optimized on a dataset from the European Alps. We initially used the same values in our calculations but unexpectedly large errors prompted us to re-optimize the algorithm for the different snow climate in the western US. We followed the optimization methods described in Lievens et al. (2022) and included relevant details in the main text (lines 203-205) and Appendix B. We found that the algorithm performed better using our WUS-optimized parameters than the original set of Alps-optimized parameters. We also do compare our re-optimized algorithm to in situ SNOTEL data (Figure 7), although the in situ network is less dense across our validation sites than what is available in the Alps. Lidar snow depth uncertainties are on the order of 2-20 centimeters (Table 1) and cannot fully explain the ~meter scale errors we find in this study.

While the differences between our results and the C-SNOW data are concerning, we have recreated the published methods from Lievens et al. (2019, 2022) and have established an open platform for future users to iterate and evaluate this C-band depolarization technique. We hope that going forward the original authors can release the full details necessary to exactly replicate the C-SNOW data. If the missing information and full algorithm were made available we would gladly implement them into our open-source version.

1. Line 187: there should be more investigation of this much low correlation. I suspect even changing A, B, C parameters will change thing much.

   See response above as well as a discussion of parameter optimization in Appendix B.

2. Line 241/Figure 7: I think it is not a correct comparison, you need to use the S1 CR at in situ locations for comparing with measured snow depth

   Agreed. We have changed this analysis to use S1 CR for a 1km buffer around the SNOTEL stations (Figure 7) and made appropriate changes to the caption and text (lines 266).

3. Figure 5: it is very misleading. The histograms should have the same normalized values. For instance, 5a blue and orange has almost the same maximum but blue is very narrow. I assume compareing mean and std of will give a better and more quantitative comparison. So. I suggest to generate the same plot for mean and std for different x-axis parameter. This way you can show the results for all sites in the same figure too.

   We disagree that this figure is misleading. In panel 5a the distributions are generated by grouping the lidar snow depths themselves. So we impose strict bounds on the lidar (blue) snow depths and compare the corresponding S1 (orange) snow depths, which have a much wider range of values and therefore wider/shorter distribution curves. When the selected lidar and S1 distributions have similar ranges (e.g. 75-100% forest cover in panel 5b or 500m spatial resolution in panel 5f) the relative height of the distributions is closer. The 25th, 50th, and 75th percentiles are notated with dashed lines in the distributions for a quantitative comparison; we have added this information explicitly to the figure caption.

Minor comments

4. Line 61: “radar approaches are more directly related to SWE than depth” : the only radar approach that is directly related to SWE is InSAR whereas two frequency amplitude ones are related to snow depth. Need to correct this sentence.

Agreed. We have made significant revisions to this section and believe we have corrected this poor/incorrect wording. Specifically see lines 62-71.

5. Line 123: 2-6 days revisit for Sentinel-1 is too much. I guess it assumes both Sentinel-1 are on and making observation. We know that this is not the case everywhere and all the time. Need to fix this. Also it needs to be clarified if it used both ascending descending observations or just one direction.

Revised this sentence to clarify we are talking about the spatial overlap of S1 swaths from different orbit geometries imaging a point every 2-6 days, while the revisit interval for a matching orbit geometry is either 6, 12, or 18 days. (line 148-150).

Also clarified we used all available images. Appendix A contains details on the separation and normalization of images clarifying that we used ascending, descending, S1A, and S1B.

6. Line 136: I am not sure what you did here. I assume you want to compensate the effect of incidence angle for overlaps. Could you please clarify what you exactly did here and how it is supposed to help you.

We are following the methods described in Lievens et al. (2019, 2022). There is a full description of this choice in Lievens et al. (2019) and in Appendix A of our manuscript.

7. Line 189: remove extra dataFigure 2b x-axis should be snow depth, remove lidar

Removed extra “data” and adjusted x-axis label.

8. Figure 2b: it is not clear what dashed lines inside the histograms show.

We assume this comment is in reference to Figure 3b. Added to caption to clarify these refer to 25th, 50th, and 75th percentiles.

9. Line 263: higher volume scattering and higher SNR: the SNR is not defined, and we suggest using another term, as SNR normally refers to radar received signal compared to received noise. I think you are using S and N with a different definition. If so, please use

other term. Also, more volume scattering doesn't necessarily mean more depolarization, for instance for an isotropic volume it doesn't make it depolarized.

We have changed to "volume scattering" to "depolarization" (line 294). Added definition of SNR term as we are using it here (lines 250).

10. Line 284: need to provide a reference for east (more wind-deposit snow) and west (more direct solar radiation) facing comments.

Edited to clarify that south slopes receive more solar radiation and west slopes receive more solar radiation during the warmer afternoon. Added citation for east slopes (more wind-deposited snow) for the western US.

11. Line 305: you need reference for this. I don't think orbital error/variation in ground/vegetation properties affect the "noise" (not snow backscattered power). Normally speckle noise is the part that gets improved by taking looks.

Removed the reference to orbital errors here.

12. Line 352: remove extra be

Done

13. Line 365 a should be an

Done