June 24th, 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 #2

General Comments:

This article reports on the evaluation of an algorithm for retrieval of snow depth based on C-band backscatter intensity images of the Sentinel-1 mission. The first version of the retrieval algorithm, presented by Lievens et al. (2019), applied an empirical change detection method using temporal changes of the cross- to co- polarization backscatter ratio (VH/VV) to compute a snow index that is rescaled by means of empirical parameters in order to obtain the snow depth. A modified version of the snow depth algorithm was applied for snow depth retrievals over the European Alps (Lievens et al., 2022). This version of the algorithm employs the VH/VV ratio and the VV backscatter intensity, accounts for the fractional forest cover, and uses several empirical scaling and weighting parameters. A critical constituent of the algorithm is the determination of empirical scaling factors that relate the backscatter intensity to snow depth.

A comprehensive independent validation of the algorithm has been lacking by now. The work by Hoppinen at al. addresses this open issue, evaluating the performance of Sentinel-1 snow depth maps derived by means of the 2022 version of the algorithm. Reference data for performance assessment are available from nine high-resolution airborne lidar acquisitions over extended study sites in the Western US, an excellent data set for algorithm validation. The scaling factors are derived from a subset of these data by optimizing the correlation coefficient and minimizing the mean absolute error.

The paper is a valuable contribution to the topic of SAR-based approaches for mapping snow depth and snow mass. It addresses a very relevant question in this context. The data analysis, results and conclusions are well described and conclusive. However, there are still some issues to be checked and clarified, addressed below. In particular Section 1.1 on the theoretical background needs major revision. Besides, information on the available Sentinel-1 coverage (orbits, repeat coverage) would be of interest.

We thank you the reviewer for these in-depth comments. We believe the manuscript changes, highlighted below, address all concerns and result in a much improved manuscript.

Specific comments

1. Section 1.1 SAR volume scattering snow depth retrieval theory:

   This section refers to the radar signal interaction with snow. The description of processes having an impact on the C-band backscatter signal for snow over ground is incomplete and lacks quantitative information. Volume scattering cannot be considered as a stand-alone process for deriving physical snow parameters from backscatter intensity. In support of interpretation and discussion of the results of the study, I recommend including a concise description of the main contributions to the observed backscatter signal (including not only snow, but also ground and vegetation) and their impact on retrievals of snow depth. Because large parts of the study sites are covered by forest, the impact of forests on C-band signal propagation should be addressed. Figure 1 needs to be revised. The information to be conveyed by this figure is unclear. It implies that the incoming radar signal is reflected within the snow volume as a main source and the signal increases directly with increasing snow depth. Besides, the figure shows incoming and reflected beams in bistatic configuration.

We have made major revisions to our initial discussion of SAR theory, including specifically mentioning the various other contributors to SAR backscatter over snow-covered ground (line 84-86). We present a generalized discussion of which scatterers are relevant and the relative importance of these scattering factors at non-grazing incidence angles (line 88-93). We have also highlighted that C-band SAR snow depth estimation methods rely on the assumption that there are minimal changes in vegetation or ground surface scattering characteristics throughout the snow covered period of observation (line 100).

We have also modified Figure 1 to include non-snow related contributors to volume scattering and signal depolarization and have separated the two acquisitions to clarify that they are two monostatic time acquisitions.

2. Line 12 (Abstract): the term “cross-polarization backscatter ratio” should be defined at its first usage.

   Added formula after this usage to define.

3. Line 13 (Abstract): Please provide a number for “significant correlation”

This is quantified by wilcoxon-box tests with a p-value below 0.0001. We have rephrased this sentence due to other reviewer comments (Reviewer 3, minor comment #5) that it was unclear and removed the “significant correlation” as part of those changes.

4. Line 99, 100: “as new snow increases the cross-polarized energy that is backscattered toward the sensor” This is not in accordance with experimental data and theory. Due to the low C-band scattering albedo of fresh snow the backscatter signal of a medium below with higher scattering albedo (e.g. coarse-grained metamorphic snow, refrozen snow) is attenuated when propagating through fresh snow.

Agreed, the choice of “new snow” here was misleading. We have changed “new snow” to “snow depth increases” to better capture that the current theory and literature is unclear on the exact rationale for the increased depolarization by snow at C-band, but that new snow is certainly not the only factor.

5. Line 125: Please check these two references: “Frerebeau et al., 2023; Lebrun et al., 2020”; both refer to “Dose Rate Estimation from in-Situ Gamma-Ray Spectrometry” and not to SAR.

Changed to correct citations for GAMMA and Sentinel-1 for GAMMA

6. Line 126: “European Space Agency, 2021” missing in the reference list

It was incorrectly abbreviated to Agency, E.S. We have changed this to “European Space Agency” in references.

7. Line 130: The speckle-related uncertainty of the selected grid size would be of interest.

We have chosen our grid size(s) to match Lievens et al. (2022). Future work on SAR speckle uncertainty as a function of grid size is an interesting topic, but we believe this to be outside the scope of this analysis and would distract from the primary conclusions. We have added a sentence to future work discussing the need for this analysis.

8. Line 133 to 135: The backscatter intensity at different incidence angles is not an “artifact” but contains relevant information related to physical properties of a medium which is suppressed of data with different incidence angles are merged.

We have changed “artifact” to “differences” and added a future work sentence suggesting the need to assess the relationship between incidence angle and backscatter/depolarization (line 411).

Since we are following the methods in Lievens et al. (2019, 2022) which use this incidence angle normalization we also performed this normalization to replicate the published method.

9. Line 175 to 177: According to this information a single S1 image per lidar acquisition data set (a subset of the total S1 data set) is used for deriving optimized scaling parameters. However, the snow depth retrieval algorithm is not based on single images but on changes of backscatter intensity in time over an extended period (Appendix A).

The single S1 image is the result of the cumulative time series to that point and should be capturing information from all the relevant S1 backscatter changes that had snow coverage in the IMS data.

10. Line 208, 209: From the histograms in Fig. 3b, the agreement between the medians of these three sites is not obvious (due to the log-scaling). Besides, Banner 2020 and Fraser 2020 show a rather high negative bias for average snow depth (Table 2).

We agree this line is over-optimistic and qualitative. Changed to "At most other sites snow depth is strongly underestimated by the S1 retrieval and retrieved snow depths exhibits much larger dynamic ranges compared to lidar" (line 234-235).

11. Line 209: The acronyms "ICC", and in Fig 3b "LCC", refer probably to Little Cottonwood?

Corrected.

12. Fig. 3a: The linear trendline (on which the correlation coefficient is based) should be included, as its slope is an indication for the S1 sensitivity in respect to snow depth.

Added in linear trend line along with equation.

13. Line 240 to 244 and Fig.7: The SNOTEL snow depth times series should be compared to the Sentinel-1 CR data of areas in the vicinity of the SNOTEL stations rather than to the site-wide mean cross ratio. Surface elevation has a major impact on the state of the snowpack and its backscatter properties.

Changed figure 7 to include a 1 km buffer around the SNOTEL to get CR. Changed appropriate lines of text and caption to reflect this change.

14. Line 245 to 248 and Fig. 8: The two classes with deep snow (2.5-3, 3+) showing a distinct rise in delta-CR comprise only 1.2 % of the total sample. Hardly a suitable basis for a statistically significant conclusions regarding the retrieval performance for deep snow.

While these two classes are a small portion of the pixels, they also represent ~800 pixels and we believe that plotting them provides useful information about the relationship between CR and snow depth. We also quantified this relationship in figure 8 and found a significant change in means (line 272-275). Future work exploring a dataset with more deep snow lidar acquisitions would be valuable.

15. Page 14, Fig. 4 caption: the labels for FC and elevation in the figures and caption are mixed up.

    Fixed.

16. Page 15, Fig. 5: These histograms are probably also log-scaled.

    These are linear not log-scaled.

17. Line 252: The correlation coefficient is not a suitable parameter for assessing the performance of the spatial distribution.

    We chose this statistic to allow for direct intercomparison with Lievens et al. (2022).

18. Line 255: “Frasier” typo

    Fixed.

19. Line 261-262: Fig. 6a shows a decrease of the relative error with lidar snow depth, but the absolute error increases with snow depth for snow depth > 1 m. Also, in Fig. 5a the S1 and lidar histogram for snow depth > 2 m show the largest disparity. This is not a clear evidence for improved performance for deep snow.

It is certainly suggestive when combined with Figure 7 and 8, previous work showing more response at deeper snow depths, and a theoretical understanding of C-band volume scattering. Clarified that this understanding relies on not only on Figure 6a but other previous work and figure 7 and 8 in line 384.

20. Line 267: “SAR signals primarily interact with layers within the snowpack rather than individual snow grains”. Please check this statement. The scattering elements within layers are grains and grain clusters.

Added “anisotropic grains and grain clusters”. The cited Brangens et al. paper suggests that scattering at layer interfaces within the snowpack also play a role.

21. Line 267-271: Experimental data on propagation losses show for C-band power penetration length in dry seasonal snow typical numbers in excess of 10 m. Consequently, backscatter contributions of the subnivean ground and snow/ground interaction play also a role.

Agreed though skin depths for C-band into soil are relatively limited. Added in this contribution to the radar theory section (line 88) and into Figure 1.

22. Line 304-305: The Sentinel-1 orbit accuracy is very high so that orbit errors do not play any role. For estimating the impact of SAR speckle, estimates for the speckle-related uncertainty would be useful.

Removed orbital errors from this sentence. See response to #7 for speckle.

23. Line 317-318: " ... S1 snow retrievals agree best with lidar snow depth measurements in regions with snow packs deeper than 1.5 m …" this refers to the local snow depth relative to the mean value, not to the magnitude of snow depth. This should be stated. See comment line 261.

Not quite sure what you mean "local snow depth relative to the mean value". Do you mean we show relative error rather than absolute error?

24. Line 321-323: Ku-band and X-band are mentioned here. Why not L-band, for which the InSAR phase delay is applicable for SWE retrievals also in deep snow?

Since this paper has primarily focused on backscatter based approaches for which L-band is unsuitable we omitted mentioning it here. See revised introduction paragraph (lines 62-73) where we have added a new paragraph discussing other SAR retrieval techniques including many time-of-flight based approaches for which L-band is suitable.

25. Line 324 ff, Section 4.2: The analysis of the CR time series suffers from the spatial disparity between point measurements (SNOTEL) and spatial average S1 data of large test sites extending over different elevation zones (see comment line 240ff).

See response to comment line 240. We have changed the line in the figure/analysis to show the CR behavior in a 1km box around the SNOTEL sites instead of the mean-site CR and altered the appropriate text.

26. Line 373 to 376: A conclusive time series analysis of CR is lacking. See the comment above.

    See response to comment line 240.

27. Line 375: Please provide numbers for “significant relationship”.

    This relationship comes from notched boxplots so we can say the increase has a p-value < 0.05. We have further quantified with a wilcoxon signed rank test (line 273-275)

Appendix B:

28. Line 459, Table B1: Units?

    Added units to this table and text. Omitted units on text that are completely dimensionless.

29. Line 466, 467: “Since C simply scales values in the final step of the retrieval, this parameter can be optimized efficiently and should be adjusted first when applying this technique at a new site”. This indicates that for any site an individual calibration of the scaling parameters is needed to obtain useful results. To this end representative and reliable snow depth data from other sources are needed. Furthermore, due to interannual changes in permittivity and structural properties of the snow cover ground, the value of the scaling parameter may change from year to year. This questions the feasibility of the retrieval approach for regular applications over extended areas.

    Correct and one of the weaknesses of this algorithm. We chose to move this to the appendix since we advocate for an entirely new algorithm but originally this was a major weakness we planned to highlight.

30. Line 484, Fig. B1: Please check the sign for the % change in snow depth in Figs a, b, c. Would not C = 0.0 result in zero snow depth (-100%), rather than in +100% ?

    Thank you for catching this error. We had the signs reversed for the three subpanels in this figure. This has been corrected in the revised version and C = 0.0 does result in -100% snow depth as expected.

31. Page 28, Fig.B2: Figs. B2a and B2c show low RMSE for the wet snow threshold -1 dB and high RMSE for the threshold -3dB. As a lower wet snow threshold reduces the number of misclassifications for dry snow, the opposite behaviour is expected.

Respectfully, we believe the expected behavior is shown: the aggressive wet snow threshold of -1 dB will mask out more pixels, misclassifying a greater amount of truly dry pixels as instead falsely wet and thus removing all but the driest pixels from the RMSE analysis. The more conservative -3 dB will misclassify more truly wet pixels as falsely dry, including them in the RMSE analysis. With this more conservative -3 dB threshold, the misclassification of a greater number of truly wet pixels as falsely dry will drive the RMSE up, as we expect reduced algorithm performance in wet snow conditions.