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 #3: Benoit Montpetit

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.

We thank the reviewer for their insightful comments. The comments were thorough and helpful and have certainly improved the writing and analysis. The suggestion to include delta VV and VH in the boxplots was especially well worth the work.We have highlighted changes made to the manuscript below in response to the reviewer's suggestions.

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).

We appreciate these comments and strongly agree that much more research is necessary to understand the relationship between different snow properties and signal depolarization. As we felt the primary focus of this paper was to evaluate a specific algorithm and platform for snow depth retrievals we didn't want to get too deeply into the DMRT and theory behind C-band depolarization but instead to give a general understanding of the theory and some citations for those interested in the radiative theory. We have added in more discussion of this depolarization theory and citations for some of the fundamental works on this depolarization theory (line 88, 93, 97) along with a few textbook and review papers (line 71, 88).

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.

We have made significant revisions to Figure 8 in response to your minor comment #34 and agree that an analysis of the polarization individually for this part was a very valuable analysis and have consequently also revised our discussion of this Figure (line 377-384).

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.

We agree that the empirical nature of this algorithm is a challenge for future global implementation and specifically address the need for either a more complex model that can better capture these landscape parameters or is more physically based (lines 397-404). We originally had the analysis of parameters from Appendix B in the main body of the paper but decided to move the major of that discussion to the Appendix since it confused the primary takeaways of the paper and are of interest to a much smaller group of researchers than the primary evaluation of this S1 algorithm. Overall, we believe that the algorithm needs such dramatic changes that a specific analysis of the A, B, and C parameters specifically is probably not a helpful comparison for most researchers.

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

We have quantified these relationships. See our responses to minor comments #32, 33.

Minor comments

1. 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.

We believe these are two separate ideas. One is an open source algorithm that the whole community can work with and evaluate openly. The other is an open-source framework for future work on C-band volume scattering.

2. 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.

We chose to revise this wording to highlight that this RMSE is significantly worse than the requirements for remotely sensed observations of snow which are usually given as an RMSE (Line 10).

3. 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.

We find the phrase cross- to co-pol backscatter ratio to be a pretty clear and succinct phase for this idea, and have added it in as a general descriptor in the abstract (line 12) but have continued to use cross-ratio afterwards since we are comparing explicitly to Lievens et al. (2022).

4. L.13 remove “cross”.

We have clarified that this CCPR is called Cross-Ratio in line 12 and use that term here to match the current literature usage.

5. 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.

We have reworded this sentence for clarity. This sentence now reads: "We find the cross-ratio increases through the time series for snow depth over ~1.5~m but that the cross-ratio decreases for snow depths less than ~1.5~m."

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

   Corrected

7. 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.

There are definitely other important variables for water management. Changed to "one of the defining…"

8. 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.

We have split this sentence into three sentences to improve readability. We have also changed "correlation length" to "spatial autocorrelation" to clarify our meaning. This now reads: "Networks of in-situ weather stations (e.g., SNOTEL in the United States) make point measurements of snow depth with high temporal resolution. However, accurate spatial interpolation required to generate distributed products presents a significant challenge (Dressler et al., 2006; Bales et al., 2006; Schneider and Molotch, 2016). This challenge is largely due to snow's typical spatial autocorrelation length of 50–200 m (Trujillo et al., 2009)." (line 31-34)

9. 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.

   Added "using the typical 37 GHz" (line 37)

10. 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.

In response to reviewers' comments we have now added a paragraph discussing other SAR-based snow estimation techniques and relevant review papers including Tsang et al. (2022). Please see our response to Reviewer #1, comment 4 and lines 62-71.

11. L.53: change “a type of” to “an”.

    Changed.

12. L.57: add “and polarization” to “SAR signal’s frequency”.

    Added.

13. L.57: replace“”to“to retrieve”.

    Replaced.

14. L.67: delete “a”.

    Deleted.

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

    We have changed this range from “1-300 GHz” (line 83)

16. L.74: Needs a textbook reference here.

Added textbook references (Ulaby 1980, Long, 1975), tower comparisons (Cihlar and Ulaby 1974, Naderpour et al. 2022, Branger et al. 2023) (line 87, 88, 92, 93, 96)

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

    Fixed.

18. L.79: delete substantial.

    Deleted.

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

    Replaced.

20. 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.

    Added this to the list of potential effects leading to higher depolarization. (line 123)

21. 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".

    We have changed "new snow" to "snow depth increases" and included this citation. (line 126)

22. 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.

    We agree this original figure was unclear. We have separated the two time steps to clarify that we are discussing a monostatic configuration and highlighted some other components of volume scattering (vegetation and ground) to this conceptual figure. Also changed the caption to clarify this figure is showing an idealize conceptual figure.

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

    Sticking with cross-ratio since that is the term used in the literature.

24. 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).

    Rephased.

25. 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?

    Yes, we feel that unless otherwise specified multi-looking refers to this block averaging.

26. 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.

We have experimented with these subplots but want to maintain the full view of the study sites presented.

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

Agreed, we have deleted these lines.

28. 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.

We originally had this configuration but after deciding that we felt that a whole new algorithm was the best approach going forward, we decided to move the algorithm discussion to the appendix to avoid distracting readers with too many details on an algorithm we ultimately suggest is inadequate to capture the snow information in the S1 backscatter. (see response to major comment #3.

29. L.186-195: This section should be in results/discussion Table 2: I would include the nRMSE as well.

While an analysis of the differences between the stated algorithm in Lievens et al. (2022) and the current CSNOW product would be an interesting comparison we feel it would distract from the overall focus of this paper (how the published methods in Lievens et al. (2022) and the Sentinel-1 CR values relate to lidar snow depths) to move these into the results and Table 2.

30. 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.

Agreed. Some of this analysis is discussed in Appendix B when we look at the impact of FC on parameterizations. We know that the results for deeper snow are not biased by the class distribution since the histograms in Figure 3b show we have generally lower snow depths but a lower nRMSE at higher snow depths. Added histograms to show distributions to figure 6 to help clarify that we don't have imbalanced dataset affecting these values. Since elevation quantile and spatial resolution are both even distributions we omitted them for figure space.

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

    Agreed. See discussion of this very idea (lines 334-341).

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

    We have quantified for the lidar which has a much larger dataset than these sparse SNOTEL sites (lines 273-274)

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

    Added in text quantifying this relationship (line 273-274).

34. 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.

    Added in both VH and VV boxplots along with mean values and quantified relationship for all in text. Also added in a discussion for the new subpanels in results (lines 276-281).

35. 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).

    Added in clarifying definition for SNR (increased snow depth change related signal relative to other sources, such as thermal noise or radar speckle). (line 295)

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

    Added in correlations for 0-1 meter group and 2+m group. (line 297-298)

37. 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.

Intersnowpack layer interfaces have been shown to cause depolarization and VH backscatter (see Brangers et al. 2023). Edited to clarify we are discussing within the snowpack and added in anisotropic grains and grain clusters. (line 301-303)

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

We think this makes more sense in the discussion of challenges of this specific approach rather than in the SAR theory section which focuses more on general concepts to acquaint the reader rather than tell them specific challenges of this technique.

39. 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.

See the added histograms in Figure 6a, b,c to see we have a pretty even distribution of FC that seems to be unrelated to improvements in algorithm performance.

40. 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.

This is definitely true. There is a wet snow masking algorithm that is implemented based on Lievens et al. (2022) and addressed in appendix B.

41. 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.

The parameters are fixed based on the 90m scale data. This discussion has to do with the impacts of using a C parameter at 90m resolution to minimize MAE and then as you coarsen you tend to decrease the spread of the distribution leading to a better fit of the data.

42. 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.

We could only find this paper by Madore et al. (2023) (https://www.researchgate.net/publication/372886204_Temporal_Analysis_of_Snow_Stratigraphy_and_Melt-Freeze_Crusts_Using_a_24_Ghz_Frequency_Modulated_Continuous_Wave_Fmcw_Radar_in_Avalanche_Terrain) and it seems to be a preprint at this point with no chapter 6. Could you please provide more information?

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

    All data was used for parameters optimization.

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

    Included 3 possible explanations. (line 374-376)

45. 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.

It is included in line 105 that our understanding of C-band - snow interactions is poorly understood. Though it is not specifically called out as a weakness.

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

Yes, it would certainly only be part of the solution which is the reason we suggest multiple possible solutions and the need for more research.

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

    Included (line 395).

48. 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.

Thank you for these comments. They have certainly improved the writing and analysis. Especially the suggestion to include delta VV and VH in the boxplots was well worth the work.