July 1, 2025

Reply to Referee 1

I enjoyed reading this paper, very interesting, relevant research. I was very surprised to see a 10 m resolution backscatter product with such good performance measures. In my opinion a groundbreaking paper that shows the potential of S-1 SAR snow depth retrieval in mountain areas. Some adjustments are however necessary in my opinion to improve the quality even further.

Dear Jonas-Frederik Jans

Thank you for your detailed and constructive comments on our manuscript. In addition to improving the clarity of our article, they allowed us to obtain other interesting results. Please find below our replies (in blue) describing how we will address your comments in the revised manuscript.

General comments:

1. Would it be an idea to validate over an area where LIDAR or photogrammetric data is available? NSIDC has freely available ASO L4 Lidar snow depth maps where you can validate against. Also, Envidat has photogrammetric snow depth data available over the Alps (Yves Buhler will be able to help you further). Since the presented product has a very high resolution it would be very interesting to see how its performance holds in such a context. The validation presented now is based on a very small amount of datapoints.

   We initially considered the possibility of using the dataset you mentioned however those data were mainly collected in March or April and the Nagler's method detected the presence of wet snow. We agree that the only limitation of our approach relies in the fact that we did not consider a large amount of data, for this reason we managed to obtain an additional dataset consisting of photogrammetric data to validate our model. Such dataset was not available to us when we submitted the first version of the paper, at the end of this document we present the new methods and results. Anyway, we would like to highlight that, in contraposition to most papers present in the literature, our work focuses on high quality measurements to be able to precisely determine the effects of the snowpack variable and of the LIA on SAR signal depolarization. In our opinion this ensures that our manuscript represents a novelty in the literature.

2. Since the high amount of snow depth data available in the Alps maybe it would be better to select in-situ stations located in a range of incidence angles instead of assuming the snow depth being equal in a 4 km area around your in-situ station to extract your local incidence angles. In this context the photogrammetric and LIDAR datasets could also help. I think this will lead to a scientifically more robust relationship between LIA and the SIsar-index considering the use of 10 m backscatter data and the strong spatial variability of snow.

   As explained in the previous reply, we will add a validation carried out using photogrammetric data. Concerning the automatic weather stations for the calibration of our model, it is not possible to do what you suggest.

Firstly, AWSs are always located on flat terrain (in Italy and Switzerland this is a mandatory requirement), and so we realized that the range of LIA we could cover was very limited and bad distributed. Moreover, we remark that very often automatic weather stations are not well-calibrated, and we cannot perform reliable SNOWPACK simulations, while, on the other hand, the ones considered by us are checked every week by the Alpsolut team. Finally, we think that the relation between the SIsar index and the LIA derived is robust (see e.g. Figure 3) thanks to the fact that we were able to consider very precise measurements to validate our results. For example, the R^2 coefficient resulting from the derivation of $g(LIA)$ is equal to 0.695 (we will add this result in the revised version of the manuscript). The reliability of those measurements is demonstrated by the fact that they were used for several papers during the past years, see for example: (Monti et al., 2014), (Monti et al., 2016), *Simulation of snow management in Alpine ski resorts using three different snow models* (Hanzer et al., *Cold Regions Science and Technology*, 172, 102995, 2020). Anyway, the results that we will present deriving from the additional validation carried out using photogrammetric data will show that our model is robust, implying that the initial calibration that we made with the Livigno AWSs is valid.

3. Be careful when mentioning the use of dual-polarimetric data, since it can be easily confused with actual PolSAR data. Dual-pol polarimetric variables are derived from the Single-Look Complex (SLC) data, are then processed into covariance matrices to in the end extract metrics like the DpRVI. In this paper only backscatter, and no phase information, is used. I would emphasize this more throughout the text. It would also be interesting to analyze metrics derived from the SLC observations, however I understand this is out of scope.

   We will explain this better in the revised version of the manuscript. For example, we will add the following sentence in the Introduction: *The $DpRVI_c$ index is a simplified version of the $DpRVI$ index, adapted to be computed from a Ground Range Detected (GRD) Sentinel-1 product (Feng et al., 2024).* We agree that analyzing the metrics derived from the SLC observations would be interesting, however, since it is out of scope, we will not do so.

4. To strengthen the claims made within this paper I would also analyze the behavior of the VV and VH backscatter with changes in snow depth or changes in incidence angle. This will allow the reader to understand the underlying processes related to a change in SIsar. So, in other words, it would be of interest to add an analysis that investigates the change of the components that make up SIsar-index to see exactly where the change in SIsar is coming from.

   We will add an evalutaion of the single components of our index (VV and VH backscatter coefficients) with respect to LIA in order to demonstrate our conclusions. See reply to Line 366.

5. I would in general mention the resolution of the product more, because sometimes I was confused whether 10 or 50 m was used (see specific comments).

   In the revised version of the manuscript we will mention the resolution of the product more. See the replies under the specific comments (Line 108;

Line 115; Line 117; Line 185) for a detailed description of the changes we will make.

6. I sometimes found it hard to follow the flow of the data and methods section. I would opt for a combination of both to improve flow. My suggestion would be to start with the study area, then go to the description of the SAR data and data processing, then followed by the weather and snowpack measurements and snowpack modelling, then the mathematical modelling to combine them both and then the model validation strategy.

   We do not agree on this. We would prefer to keep the sections separate because we believe they help to better present the topics covered. We would like to avoid writing sections that are too long and contain too much information of partially different nature.

Specific comments:

Line 36 and 37: I would change SAR signal by microwaves.

We will change the wording as suggested.

Line 44: This statement is not true for cross-pol. I would specify co-pol here.

We will specify co-pol as suggested.

Line 64: I would be careful with this statement because in the mentioned paper only backscatter was analyzed but no true dual-polarimetric indices (those derived from SLC-data).

We will specify better that DpRVIc and SIsar are just approximations of the real depolarization. However these approximations seems to be considered reliable in other works we cited.

Line 70: I would cite Feng et al. again here after Alps.

We will add this citation.

Line 108: What is the reason for working with a buffer zone of 50 m? Your backscatter data is processed at 10 m, is it not better to take the same resolution?

We considered 50×50 m areas, hence a 25 m square buffer (there was a misprint in the original paper). The pixels had a resolution of 10×10 m (standard GRD resolution), but considering the average value was the optimal choice to balance spatial resolution and temporal signal stability (similarly to what was done by Pettinato et al.(2014)). We will modify the final paragraph of Section 3.2 in the following way: *The $SIsar$ values in the time series were computed by averaging the values within the 50×50 m regression sampling areas for the two different AGs (similarly to Pettinato et al.(2014), we noticed that this was the optimal choice to balance spatial resolution and temporal signal stability). [...] For analyzing the dependence between our index and the LIA, we grouped and averaged the $SIsar$ values for LIA classes of $2°$ within the two LIA sampling areas (in this case, we considered the values of each 10×10 m pixel separately and no averaging was performed since each pixel was characterized by a different LIA). [...] The $SIsar$ values related to the 50×50 m validation sampling areas were again computed by averaging the values of each pixel.*

Line 115: independent as in your pixel AWS pixel was not taken into account for the LIA sampling? Since I saw your AWS pixel and LIA area overlap. Also, what is the resolution used for the LIA sampling is it 50 m, or 10 m?

You are correct, they are not perfectly independent. We will substitute that sentence with: *It is important to note that the regressions sampling areas were included within the LIA sampling areas, but the former represented only a small* *fraction of the latter. Therefore, despite the partial overlap, the results obtained from analyzing the second area were largely independent from those related to the first, as they were mainly driven by new additional data.*

Similarly, we will correct Lines 248 and 385 by substituting *independent* with *effectively independent* (or a similar expression).

The resolution of the LIA sampling area was 10 m, in order to be able to analyze each pixel separately, so that we could understand the influence of the LIA (each pixel is characterized by a different LIA, hence we did not perform any averaging process; see reply to Line 108).

Line 117: idem, why 50 m? Also, how many measurements were taken as validation points?

See reply to Line 108. The number of observation is reported at line 151.

Line 127: 168 is descending captured in the morning, 15 is ascending captured in the evening . Idem on line 135 descending morning and ascending evening.

We will fix this error in the revised version of the manuscript.

Line 133: Mention the source of the downloads.

We will mention the source of the downloads.

Line 135: Mention why these dates are used.

We used these dates because in Tromso there was a snow expert who made several snow measurements and he was available at verifying our results (see Lines 116 and 150). For example, he confirmed that the snow was dry and no avalanche deposits were present in the area (see Line 151). We will add his name in the acknowledgments of the revised version of the manuscript.

Figure 1: I would add the validation area as well.

We prefer not to do this for three reasons. Firstly, a map of the calibration area is very important in Section 2.1 because it was chosen by us to calibrate the model, while, on the other hand, the validation area was a forced choice to incorporate the field measurements. Secondly, Figure 6 already contains a map of the validation area, with the simulated height of the snowpack. Finally, since the two areas are so far apart we do not like the idea of including them both in the same image. However, if our additional validation (see end of this document) is accepted, we will have to add a map of the new validation area. In that case, we will also add the map of this validation area to that image, so that there are two images: one dedicated to the calibration areas and one to the validation areas.

Line 169: What was the original resolution of your GRD product, is this 10 by 10 m? Why is resampling needed in line 175 or was it a reprojection to another CRS? If resampled, which technique was used?

The resolution was 10 by 10 m, we will clarify this at the beginning of Section 3.2. The resampling part was a misprint, we will remove it from the revised version of the manuscript.

Line 185: Indicate the resolution, also in section 3.4 indicate whether you also do an upscaling to 50 m. If this is the case it is maybe better to immediately indicate in section 3.2 that you are using a 50 m resolution to avoid confusion.

As explained in the replies to Lines 108 and 115, we will explain better in Section 3.2 the resolution we used.

Line 190: Indicate why you would exclude negative values and why you would make sure the data follow a normal distribution.

Concerning the normal distribution, it is not relevant, we will remove it from the revised version of the manuscript. On the other hand, we understand that we did not explain well how we dealt with negative $SIsar$ values, here we report the changes that we will make. We will modify the last paragraph of Section 3.2 as follows (see also reply to Line 108):

The $SIsar$ values in the time series were computed by averaging the values within the 50×50 m regression sampling areas for the two different AGs (similarly to Pettianto et al.(2014), we noticed that this was the optimal choice to balance spatial resolution and temporal signal stability). The pixels with negative $SIsar$ values along the snow season were excluded and we hypothesized that this anomalous effect was related to the LIA. For analyzing the dependence between our index and the LIA, we grouped and averaged the $SIsar$ values for LIA classes of 2° within the two LIA sampling areas (in this case, we considered the values of each 10×10 m pixel separately and no averaging was performed since each pixel was characterized by a different LIA). This last analysis was conducted under snow-free conditions as well as on several dates during the snow season. In this case, the pixels with negative $SIsar$ values were not excluded in order to verify our previous hypothesis. The $SIsar$ values related to the 50×50 m validation sampling areas were again computed by averaging the values of each pixel (in this case we did not remove the pixels with negative $SIsar$ values; the reason of this different choice will be clear in Section 4.2).

Hence, we will also modify Section 4.2 as follows:

Since we observe that for LIA values below approximately 30° the $SIsar$ index is always negative, even when HS is nonzero, we exclude all areas with $LIA < 30^\circ$ in the implementation of the final model (this confirms our initial hypothesis that those negative $SIsar$ values are related to the LIA; see Section 3.2). Areas with $LIA > 80^\circ$ are also removed because they are more prone to SAR shadowing errors. The resulting R^2 coefficient is equal to 0.695 and, due to these choices, Eq. (9) is well-defined since $g(LIA) \neq 0$ for all LIAs of interest.

Line 205: Explain what the acronym mdl stands for the first time it is used, same for msr.

We will explain this in the revised version of the manuscript: mdl stands for modelled, while msr for measured.

Line 225: Is this one a value for each orbit and regression area or is this everything combined in one a value?

The value of a was derived combining all the data. By doing so we obtained a more reliable value that should be independent on the date and the location. Figure 5(a) shows the corresponding graph, highlighting that all the data were used. Moreover, in Section 3.2 is explained that both regression sampling areas and satellite orbits were used.

Line 233-235: This is a very tricky statement to make which I would not necessarily agree with. See general comments 2.

We understand that it could be seen as a forced assumption but we ensure you that it is not. The great advantage of the areas we selected for the analysis of the LIA and of the influence of the snowpack properties is that they are close to the Alpsolut avalanche center and hence carefully monitored by several (about five) avalanche forecasters (some of them are among the authors of our manuscript). As explained in the paper (Section 2.3), the avalanche forecasters of the Alpsolut avalanche center carry out several snow measurements every week in different locations and the LIA sampling areas were defined taking into account their reports.

Line 243: Did you exclude values from your validation set? Or is this sentence related to the calibration? If it is related to the calibration, I would leave this sentence out because it is a bit confusing, otherwise there is a mistake here since excluding data from your validation set because they have negative values is not good. It will lead to performance metrics that are unrealistic since you have tuned it to artificially create better results.

We excluded values from the calibration set. We agree that we should remove this sentence: not only it is confusing but we will provide more details regarding how we dealt with negative $SIsar$ values in the previous sections (see reply to Line 190). Obviously we did not manipulate the data to obtain better results. We also modified the second paragraph of the appendix as follows (recall the reply to Line 190):

Furthermore, we assessed the model's capability to distinguish between snow-free and snow-covered conditions. In Fig. A1 are shown the values of HS_{mdl_LIA} during the summer 2023 computed for the Gessi regression sampling area. During the observed summer period, the RMSE was 30.5 cm and the MAE 25.6 cm, consistent with snow-covered seasons. Negative $SIsar$ values were common and expected, as even small model errors can yield negative estimates when snow depth is zero; this behavior is not related to the LIA (see Sections 3.2 and 4.2). Due to the modelled HS fluctuations, it is not possible to detect snow-free conditions with our model. Anyway, on average $HS_{mdl\_LIA} \simeq 0$, which is in line with the results presented in Section 4.2. Similarly, we noticed the presence of pixels characterized by negative $SIsar$ values under snow-covered conditions when the snowpack is very thin.

Line 266: Would it be an option to set an extra parameter that takes care of the slope? Why does it necessarily have to be 0?

We think you meant the intercept, not the slope. We decided to set the intercept equal to zero for two reasons. Firstly, we wanted to develop an index satisfying the theoretical property $SIsar = 0$ in absence of snow (see Section 3.3 and Eq. (6)). Therefore we looked for a relation of our index with a snowpack variable satisfying this condition. Obviously, not necessarily such variable exists. However, we demonstrated through a statistical analysis (see Table 1) that the snowpack height satisfies such property and hence we set the intercept equal to zero. Indeed, the p-value showed that the intercept is not significant, implying that considering it would have led to almost equal performances (see Line 268) but also to a weaker model under a theoretical point of view. We also discussed (see Section 5.1) why only for HS the intercept could be set equal to zero, explaining the difficulties of deriving other snowpack information from SAR data with our approach.

Line 268: I would add the sentences on line 315-316 here to immediately bring clarity on why you only take one variable.

We do not agree on this. In Sections 3.3 and 4.1 we already explained in depth why we only took one variable.

Line 302: errors drop significantly, did you do a statistical analysis?

Yes, the subsequent sentence explains that the errors decrease by about 39 % (we think that clearly this represents a significant reduction in errors).

Line 307: I do not think I understand this well, what does c1 * g_bar explain? You could make it equal to a if c1 would be lower but then you would just have a worse correlation between HSmsr and HSmdl_LIA. I do not see the added value of this sentence.

We agree. We will remove this sentence in the revised version of the manuscript.

Line 321: larger snow mass and more grains per unit volume of snow.

We will change the wording as suggested.

Line 327: Can this be related to signal attenuation? I think I would mention it here.

Absolutely! Thanks for the suggestion, we will mention this fact in the revised version of the manuscript.

Line 332: Is this not because your SIsar index is also dependent on soil backscatter processes?

We do not believe this is a significant issue – or rather, we believe that the soil effect can be neglected. This is because, in our analysis of S1-SAR data, we are focusing on variations every 12 days, and we can reasonably assume that soil properties do not change during the winter season, when the ground is covered by a significant amount of snow.

Line 321-335: This is quite speculative since your index depends on both VV and VH but you only describe HH here, what about the change of VH during those periods of GS increase and density increase?

Yes, the interpretation we propose is indeed speculative. We acknowledge that we are offering a possible explanation, and we believe it is clearly presented as such in the manuscript (see Lines 320, 323, and 327). We have chosen to retain this section because it highlights a potentially valuable line of inquiry that could be of interest to other researchers. In our view, this kind of exploratory reasoning can help stimulate further investigation and discussion within the field.

Line 360-362: I agree but I do not think this is relevant here since your SIsar measurement is a measurement over time and this sentence only explains what happens at one point in time. It is the change in VV under a certain LIA that is critical.

In fact, here we are discussing how the coefficient varies with the LIA at a specific point in time, not how it changes over time. In our opinion, this is important and it's already explained clearly, as we specifically mention "constant HS".

Line 366: The statement that VH backscatter increases is not correct. Depolarization will increase but only because VV backscatter will decrease more than VH backscatter if you go to higher local incidence angles. The specular reflection captured by the sensor will decrease leading to a decrease in both VV and VH (but a stronger decrease in VV than VH). See figure below for the effect of LIA on the volume scattering component of a snowpack (simulated using a radiative transfer model DMRTBic on a snowpack of 2 and 4 m):

We performed the proposed analysis and we will add this within the revised version of the manuscript, in the main sections. Here we will anticipate you the results (see figure below, here named Figure 0).

In Section 4.2, at the end of the second paragraph, we will add : *Figure 0 reports the behaviors of the single backscatter components* γ^0_{VV} *and* γ^0_{VH} *as the LIA varies and under the same conditions of Fig. 3. Note that* γ^0_{VV} *increases with HS for LIAs below approximately* $20°$*, while it appears to be independent on HS for higher LIAs. However, it decreases as the LIA varies between* $20°$ *and* $80°$*. On the other hand,* γ^0_{VH} *shows a more complex behavior as the LIA varies. Interestingly, for LIA values below* $30°$*, it reaches lower values under snow-covered conditions than under snow-free conditions.*

In Section 5.2 we will write: *The negative values of the SIsar index for low LIAs can be firstly explained by the increase of* γ^0_{VV} *with HS, as reported in Fig. 0(a). This behavior of* γ^0_{VV} *for low LIAs is consistent with the findings of Jans et al. (2025), which highlighted that the Sentinel-1 co-polarized backscatter coefficient (*σ^0_{VV}*) increases with HS only for small incidence angles. Secondly, at these low LIAs, Fig. 0(b) shows that* γ^0_{VH} *assumes lower values under snow-covered conditions compared to snow-free conditions, implying a decrease in the* $DpRVI_c$ *index. [...] For LIA values between approximately* $30°$ *and* $50°$*, the increase of the SIsar index with the LIA for a constant HS can be explained by the fact that, as the LIA increases,* γ^0_{VV} *significantly decreases (Keskinen et* *al., 2022), while γ^0_{VH} slightly increases in presence of snow (see Fig. 0). Furthermore, in this range of LIAs, γ^0_{VV} is not sensitive to HS, while γ^0_{VH} tends to increase as HS increases because a larger volume of snow is traversed by the SAR signal, leading to more opportunities for signal depolarization and different interactions with layered structures. The fact that γ^0_{VV} shows no dependence on HS indicates that it is primarily influenced by soil properties; this supports a hypothesis proposed by several authors (e.g., Lievens et al. (2019), Dunmire et al. (2024), and Feng et al. (2024)), who suggested that the cross-polarization ratio $\gamma^0_{\mathrm{VH}}/\gamma^0_{\mathrm{VV}}$ or the $DpRVI_c$ index are more effective for HS retrieval than relying on γ^0_{VH} alone, as these indices are designed to reduce the influence of soil, present in both γ^0_{VV} and γ^0_{VH}, and isolate the snow contribution.*

Note that the behavior of γ^0_{VV} reported in Fig. 0(a) is similar to the one of your numerical simulation. On the other hand, the behavior of γ^0_{VH} is different, however it is in line with our findings.

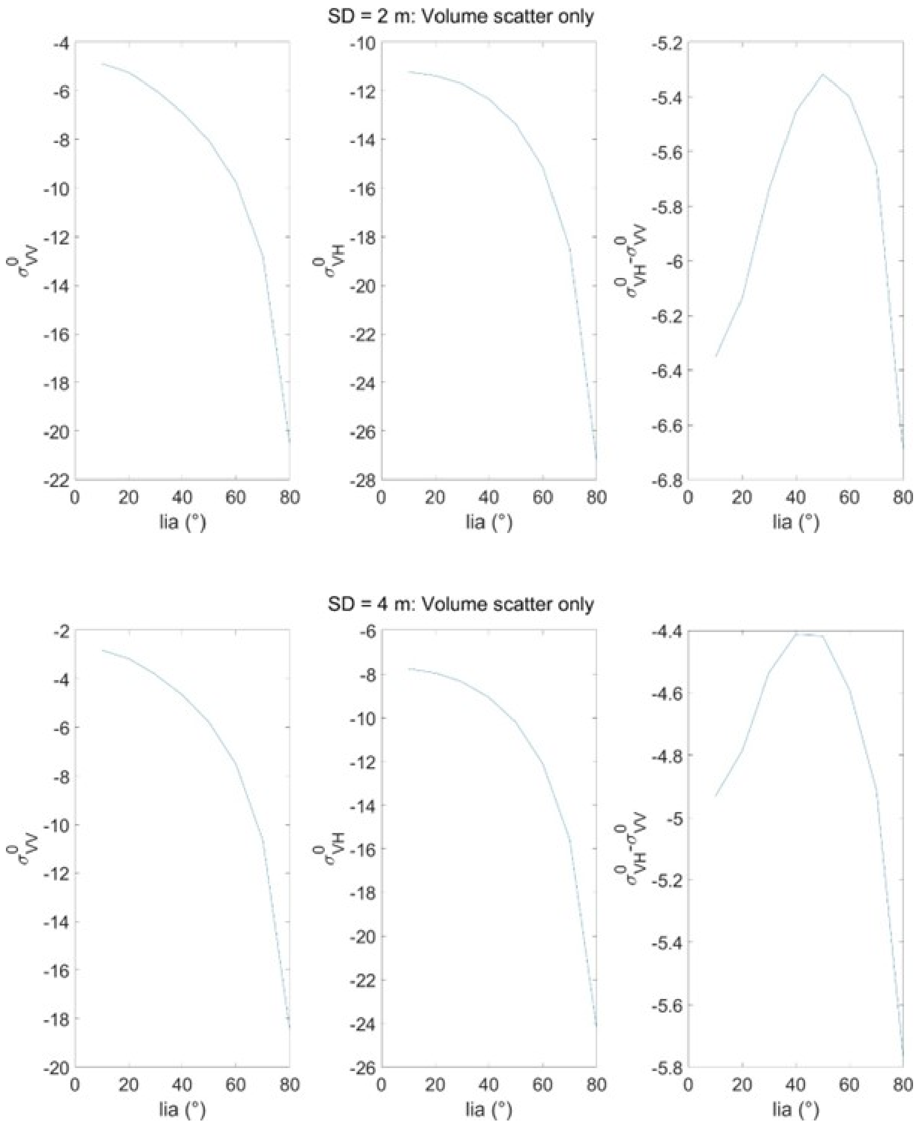
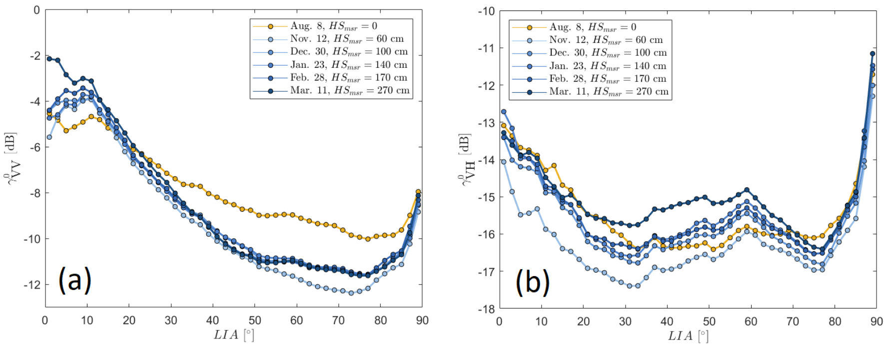


Figure 7: The change in backscatter from one LIA to the other is not well



represented here, change the magnitudes of VH and VV accordingly. The depolarization depends on the ratio of the two. (see previous comment and see comment 4 in the general comments section).

We agree and we prefer to remove this figure from the revised version of the manuscript for three reasons: it has a lower quality compared to the other images; it is probably not necessary; we will add other images due to the additional results we obtained following your suggestions and we do not want to insert too many figures.

Line 376: Is this a valid approach if HS is considered equal over the entire study area where LIAs were extracted? See comment 2 in the general comments section.

Yes, since HS was constant, by dividing the $SIsar$ index by it we isolated the function $g(LIA)$, which represents the influence of the LIA (see Eq. (8) and the final part of Section 3.3). Recall that to derive the function g we considered only data deriving from the beginning of the snow season, when we could assume that HS was representative of the entire LIA sampling areas (see, for example, the caption of Figure 4 and see also the reply to Line 233–235). The assumption of generally constant HS was also observed by our avalanche experts in that period.

Line 414: Is this a hypothesis or did you see this in the data, if you saw it in the data, please include it, very interesting stuff.

We saw this in the data and we will add this.

Technical corrections:

Line 19: Set snowpack to snow.

We will change the wording as suggested.

Line 23: I would rephrase to snow monitoring is critically important for avalanche forecasting

We will change the wording as suggested.

Line 78: sentence difficult to read, I would rephrase.

We will rephrase the sentence as follows: *The aim of this study is to provide a detailed analysis of the capability of the $DpRVI_c$ index to retrieve snow depth and other snowpack properties at a small spatial scale.*

Line 123: are available and is right-looking

We will change the wording as suggested.

Line 233: derived instead of deriving

We will change the wording as suggested.

Line 423: high number of pixels

We will change the wording as suggested.

Photogrammetric data

We conclude by presenting an additional validation of our model based on photogrammetric data. In the following, we will present methods, results, and discussion related to this new validation. Obviously, small changes must be made to the entire paper in order to add these new parts and to link them with the rest of the analysis, however here we will not report all of them for brevity and because they are limited to few sentences in abstract, introduction, and conclusions.

2.1 Study areas

The second model validation area is located near Davos, Switzerland ($46°49'20''$ *N* $09°50'02''$ *E) and corresponds to the south-facing slope of Salezerhora peak* (2537 *m a.s.l.). This region ranges in altitude from* 2500 *m a.s.l. to* 1660 *m a.s.l. and is characterized by alpine meadows, with a few small forested sections that have been excluded. [...] Concerning the Davos validation area, we considered an area of approximately* $2\ \text{km}^2$ *were a photogrammetric snow depth product was available.*

We will add an image containing both validation areas (see also reply to Figure 1).

2.2 SAR data

For the Davos validation area, we downloaded Sentinel-1 GRD products acquired on 9 January 2022 at 5 p.m. UTC (AG 15).

2.3 Weather and snowpack measurements

To the Davos validation area is associated a snow depth raster (as will be shown later in the paper, our analysis focused on snow depth retrieval) obtained by differentiating a summer digital surface model (DSM) realized with a UAV–photogrammetric survey with another DSM carried out on 12 January 2022 in dry snow condition (Bühler et al., 2022). The dataset has an original resolution of 10 *cm. These measurements were made three days after the corresponding SAR acquisitions, but the snowpack did not change significantly during that period, so the temporal offset is not expected to strongly affect the comparison.*

3.2 SAR data processing

The $SIsar$ *values related to the* 50×50 *m Norwegian validation sampling areas were again computed by averaging the values of each pixel. The Swiss validation area was treated in a similar manner: we computed* $SIsar$ *values over the SAR scene at* 50 *m spatial resolution and applied a 3×3 pixel median filter to reduce outliers and to handle for a few missing values in the photogrammetric raster. Subsequently, the photogrammetric snow depth raster was resampled to the same resolution using bilinear interpolation. The presence of dry snow was verified with the Nagler's method in all validation areas, moreover for the validation we did not remove the pixels with negative* $SIsar$ *values (the reason of this different choice will be clear in Section 4.2).*

3.4 Model validation strategy

Finally, using again Eq. (9), we computed the values of Xsv_{mdl_LIA} for the Swiss validation area. Those values were compared to the photogrammetric data to validate the model with a large dataset.

4.3 Models comparison and validation

Finally, using Eq. (9) to estimate HS in the validation area around Davos (the Swiss validation dataset; see Fig. 1) we obtain an RMSE of 22.4 *cm and a MAE of* 18.1 *cm.*

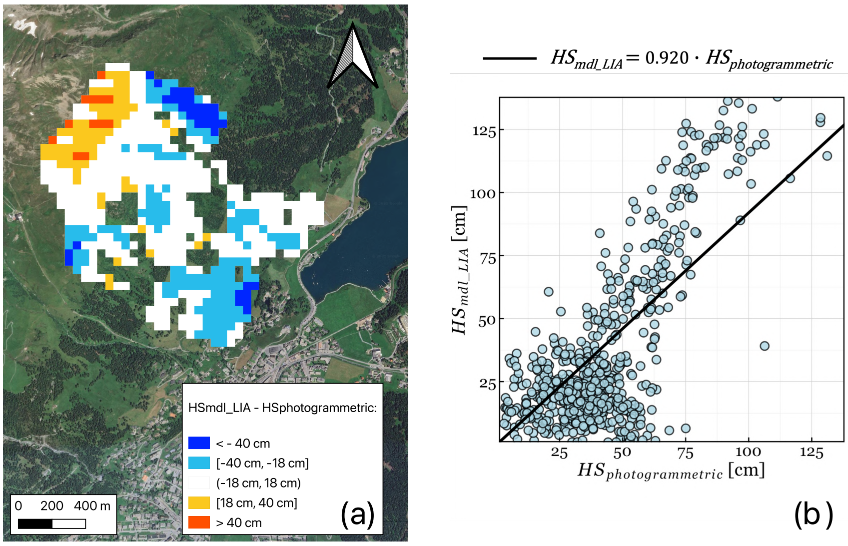


Figure 1: *Davos validation area. (a) Difference between HS_{mdl_LIA} and $HS_{photogrammetric}$, derived from the photogrammetric survey. (b) HS_{mdl_LIA} vs. $HS_{photogrammetric}$.*

5.3 Effectiveness of considering the local incidence angle within the model

The quality of snow depth retrieval at the slope scale is confirmed by the validation in Davos, where the HS mapped with our model was compared with measurements derived from a photogrammetric survey. Indeed, the RMSE and the MAE were even lower than the ones related to the Tromsø validation area. From Fig. 1(a), it is evident that the largest overestimations are concentrated in the upper part of the mountain slope, where the steepest gradients occur and the snowpack may have experienced variations (e.g., snow creep) during the three days between the Sentinel-1 GRD acquisitions and the UAV photogrammetric survey. On the other hand, the largest underestimations are found in the lower part of the slope, where the snowpack is very thin.

5.4 Model limitations

Finally, when mapping HS over the validation area near Davos, we observed the presence of few patchy outliers. These anomalous pixels likely resulted from the sensitivity of the $SIsar$ index to local variations in snow cover conditions *or changes in soil properties between the snow-covered and summer reference acquisitions. However, these outliers could be effectively reduced by applying a median filter over a small pixel window (see Section 3.2). We therefore recommend using this filter as a post-processing step.*

On behalf of all the authors,

Alberto Mariani