August 25, 2025

Reply to Referee 2

This paper presents a novel approach to high-resolution snow depth monitoring by introducing the Snow Index SAR (SIsar), derived from dual-polarimetric Sentinel-1 data. The SIsar is defined as the difference between the Dual Polarimetric Radar Vegetation Index (DpRVIc) computed under snow-covered and average snow-free conditions. The study effectively demonstrates a correlation between SIsar and key snowpack variables, notably snowpack height and snow water equivalent. A critical finding and significant contribution of this work is the identification and subsequent correction for the influence of the LIA on the SIsar index, which improves snow depth estimations.

While the results are indeed promising and the methodology for LIA compensation shows great potential, the work, while novel in its application to Sentinel-1, resembles a substantial body of research conducted in the 1990s on C-band radar signatures of snow. Specifically, the seminal works by Kendra, Sarabandi, Ulaby, Strozzi, Wiesmann, and Mätzler (among the others) are directly relevant. These earlier studies explored C-band data collection under varying incidence angles and snow conditions, alongside the rigorous theoretical modeling of scattering mechanisms. The absence of these foundational references, and a more comprehensive literature review in general, is a significant oversight and detracts from the paper academic rigor. I strongly recommend the authors consult the (extensive) literature to provide proper context and build upon established and robust knowledge. It is highly recommended that the authors are revisiting and applying similar state of the art investigative principles for the most contemporary Sentinel-1 data. This would provide a much stronger theoretical foundation for the proposed SIsar index.

A fundamental question arises regarding the underlying electromagnetic justification of the proposed SIsar index. The paper suggests that SIsar is sensitive to snowpack properties, implying that the DpRVIc index effectively discriminates between volume and surface scattering through its relationship with VV and VH intensities and its capacity to describe depolarization (L68). However, this assertion requires further verification. Volume scattering is not the sole mechanism for depolarization; for instance, double-bounce effects, prevalent in very rough alpine terrain and due to strong individual scatterers, also may significantly depolarize the radar signal. The rationale for subtracting the summer mean DpRVIc from the snow-covered DpRVIc needs clearer justification. In alpine environments, surface scattering is a dominant factor, and its variability during summer, driven by soil moisture fluctuations, contrasts with its near-constant state in potentially frozen, high-altitude terrain during snow accumulation. Given this, it is difficult to see how this subtraction effectively isolates the scattering attributable solely to the snowpack. In this context, it is challenging to conceptualize how the specific proposed relationship between polarimetric intensities effectively discriminates between the different scattering mechanisms (surface and volume), particularly considering the observation (as highlighted in the response to Reviewer 1) that snow-free VV (across all LIAs) and VH (for LIAs from 0-25 and 70-90) backscattering are consistently higher than snow-covered backscattering, similar to findings by Strozzi et al. (1997). This indicates that the influence of snow volume scattering might be obscured by the prevailing ground contribution, consequently hindering the ability to derive meaningful insights into snow properties.

Despite these critical conceptual questions surrounding the method construction, the presented results are fairly impressive, much like those of the original Lievens et al. (2019) and following algorithms. To foster transparency and facilitate community understanding, I strongly recommend that the authors present the individual behaviors of the VV and VH signals in their paper (mean and std if more than one pixel is used and example also with only one pixel to see the impact of the speckle noise), allowing for direct comparison with the findings of Kendra et al 1998 and Strozzi et al, 1997, alongside the behavior of the DpRVIc index. Following the rationale and addressing the primary doubts raised in both Strozzi et al. (1997) and Kendra et al. (1998), I suggest that the calibration test sites should be as homogeneous as possible and thoroughly characterized. This means ensuring: uniform land cover, minimal presence of large scatterers (e.g., large rocks or boulders), absence of vegetation, consistent aspect angles, and comprehensive insights into snow conditions. If diverse "object classes" are necessary for the study, they should be clearly defined and analyzed separately, with distinct plots presented for each, similar to the approach adopted by Strozzi et al. (1997). This will provide clear evidence that varying snow depths produce significantly different backscattering responses in SIsar (even if the surface scattering change). Moreover, adopting such rigorous site selection and characterization will significantly enhance the scientific value of your work, providing critical elements for future research even in the absence of a definitive electromagnetic explanation.

Dear Carlo Marin

Thank you for your detailed and constructive comments on our manuscript, especially for the valuable literature recommendations. We believe your suggestions will significantly improve the quality of the manuscript and, in particular, of the introduction. We start our reply with some general comments.

Based on the theory and our observations we agree on the fact that the $DpRVI_c$ index alone cannot discriminate the snow backscatter contribution, since depolarization occur also due to other mechanisms. However, the snow volumetric backscatter can be significantly isolated if you work in change detection, so detracting the depolarization contribution of soil macroscopic properties characteristics (i.e., the land cover, such the presence of large boulders or shrubs). As better detailed in the reply to Eq 1 and Eq 2, by normalizing each scene with a summer reference, representative of the average $DpRVI_c$ index values due to land cover, we can reasonably assume that the effect of soil and vegetation cover is largely negligible. Note that the work of Feng et al. (2024) follows a similar idea. Regarding the working principle of $SIsar$ and $DpRVI_c$ indices for snow depth (HS) retrieval, we have also provided explanations in the responses. However, we would like to highlight a few additional points. Firstly, we demonstrated that gammaVH is slightly higher for certain LIA angles in the presence of snow than in summer (see Figure 2 below): these specific LIA values are precisely those for which we found $SIsar$ suitable for HS retrieval. Secondly, we refer to the recent work by Brangers et al. (2024), where a tower-based C-band radar system was used in conjunction with Sentinel-1 and snow stratigraphy observations: that study demonstrates not only that snow depth retrieval using C-band SAR can perform well, but also that:

- A volumetric backscatter contribution is present in the snowpack and is reflected by an increase in gammaVH, whereas gammaVV is generally not sensitive to increases in snow depth;

- This volumetric dry snow backscatter can, in some cases, be of the same magnitude as the ground contribution;
- The use of VH/VV ratios or derived indices (such as $DpRVI_c$) is a sound approach, as it can help mitigate the influence of varying snowpack properties, and increase the sensitiveness of the VH band alone to the snow depth.

These results align with our findings, especially when looking at the trends in VV, VH, $DpRVI_c$, and ultimately $SIsar$ with LIA and HS, as presented in the manuscript and in our response to Reviewer 1. Furthermore, after reviewing the work of Strozzi et al. (1997), we can show that our results are consistent with their findings. To support this, we include a comparison using VV, VH, $DpRVI_c$, and $SIsar$ values directly derived from the data presented in that paper (see Figure 1, to be compared with Figure 2 below). Please note that in this validation the $SIsar$ index is approximated from the available data in Strozzi et al. (1997) where only one set of backscatter coefficient observations for varying LIA values was available for summer, which was used as the summer reference. Nonetheless, the trends observed for the $SIsar$ index in our study closely resembles the one derived from Strozzi's data. Based on these new observations, we can confidently propose the following interpretation of the working principle:

- gammaVV is not sensitive to snow depth, except at low LIA values, as also demonstrated in previously cited works;
- gammaVH is sensitive to snow depth and may even exceed summer values for certain LIA ranges (see Figure 2 below);
- the variations of such quantities are limited and therefore they are not suitable for HS retrieval alone. For this specific reason we believe that Strozzi concluded that at C-band is impossible to sense variation in snowpack depth.

However, combining gammaVV and gammaVH in the $DpRVI_c$ index potentially allow to estimate HS, indeed for sufficiently high $LIAs$ its values in summer are consistently lower than those observed under snow-covered conditions, both in our data and in Strozzi's measurements. Based on this hipothesis we based our study and we reserved the demonstration of this with our results. The only exception, as explained, is for $LIA < 30°$, where this fact is not true and the $SIsar$ index becomes negative, limiting its reliability for HS retrieval. In general, we will be more caution in the final version of the manuscript with the explanations of the backscatter behaviour, which as suggested would require more detailed and specific experiments. As a consequence we will modify and clarify these reasonament within the introduction and the discussion.

Concerning, the calibration sites, see reply to L235 and, in particular, reply to referee 1.

To conclude, we would like to reiterate that the present study is focused on highlighting and verifying the existence of a dependence between the $SIsar$ index (or $DpRVI_c$) and the LIA, and on proposing a method to compensate for it within an algorithmic framework, with the final aim of improve the spatial resolution of snow depth retrieval algorithm. We acknowledge that the backscattered mechanism in dry snow is complex and still poorly understood, and further studies—supported by specific and precise ground-truth data on snowpack properties across varying $LIAs$, snowpack and ground conditions—are necessary to

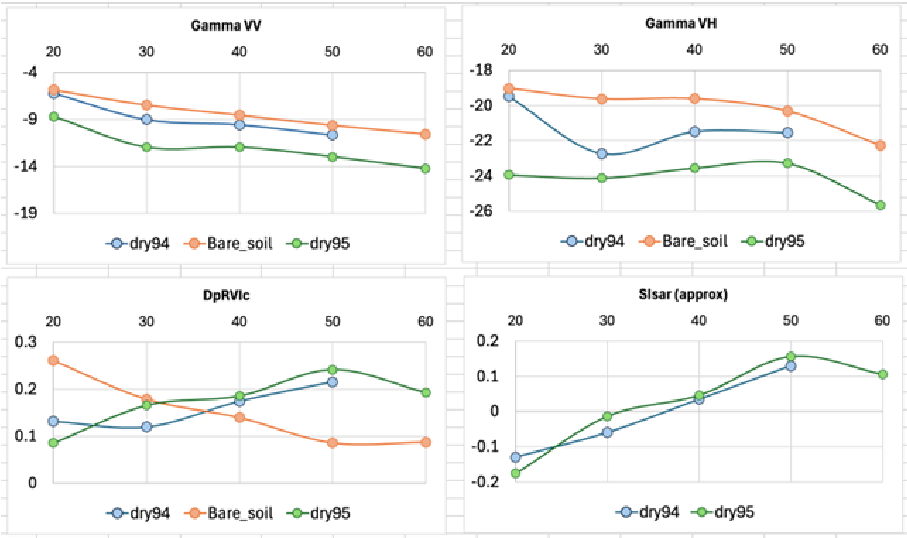

Figure 1: Revised results from Strozzi et al. (1997).

refine the relationship between $DpRVI_c$, SAR backscatter, and $SIsar$ with LIA and snowpack properties.

Finally, please find below our replies (in blue) describing how we will address your comments in the revised manuscript.

Detail comments:

The current title seems a bit too generic. I suggest incorporating your finding of a quadratic relationship between the SIsar index and LIA directly into the title (specifying what a mathematical model is). This would immediately highlight a significant contribution of your work.

We will think about this at the end of the peer review process. The problem of talking about the quadratic relationship in the title is that it requires the knowledge of the $SIsar$ index, which is introduced in our paper.

L35: From a scientific standpoint, the initial question revolved around whether microwave can effectively be employed to extract pertinent information from snow. The selection of SAR in this context is primarily driven by its inherent capability to provide the requisite spatial resolution for detailed analysis especially in mountain areas.

We will explain better the "history" of snow remote sensing in this part of the Introduction, highlighting better the potential of SAR. We will also cite additional papers, as you previously suggested.

L38: for SAR signals interactions with snow please read the fundamentals literature and books from Ulaby, Mätzler, Picard, Löwe, Tsang and many others. The dielectric constant, which is the real part of the permittivity is only one small ingredient. I suggest to read the review from Mätzler "Applications of the interaction of microwaves with the natural snow cover" (written in 1989!) to better shape the introduction.

We will read the suggested books and articles in order to improve the introduction. We will also cite some of such papers in the revised version of the manuscript.

L47: this seems not be true. See Strozzi et al, 1997.

In the conclusions of Strozzi et al. (1997) they found that that the backscatter at Ku-band increases more than the backscatter at C-band in presence of a snow cover. In particular, it is reported that *the increase in backscatter is pronounced only at ku-band*. The sentence at L47 comes directly from the conclusions of the cited paper (Tsang et.al. 2022), which agree with the findings of Strozzi et al. (1997). However, none of the mentioned work clearly talk about volumetric backscatter, so we will modify the sentence talking in general about backscatter and not volumetric backscatter (we will also check the reminder of the paper to be sure that this refuse will not be present in the revised version of the manuscript).

L68: While I am not a polarimetry expert, I question whether volume scattering is the exclusive source of depolarization, as individual scatterers or very rough surfaces can also induce polarization rotation.

We fully agree with this observation; however, it is important to note that, both in our study and in the work by Feng et al. (2024), where the $DpRVI_c$ index was shown to outperform other indices in snow depth (HS) retrieval, such index is not used in its absolute form, but always in a relative manner. Specifically, in our case, and precisely for this reason, we subtract the average $DpRVI_c$ index value observed under snow-free conditions from the $DpRVI_c$ index value of the winter scene. This approach allows us to effectively remove any depolarization contribution from the underlying soil, under the assumption that its macroscopic properties do not change significantly between the summer and winter acquisitions. In particular, the surface roughness of the soil is assumed to remain constant, considering the short temporal interval between the summer reference acquisition and the corresponding winter scenes.

Study area: The land cover and soil type require characterization. Which DEM was utilized?

We will provide more details in the revised version of the manuscript. We will add some details related to the characteristics of the soil when buried under snow, which varied over the seasons.

Snowpack modeling: To ensure clarity and adhere to established standards, I suggest adopting the symbols from Appendix D of The International Classification for Seasonal Snow on the Ground. For instance, E is typically used for grain size, and ρ for the density. Presently, ρHS might be confused with SWE.

We will make these changes in the revised version of the manuscript.

L166: Regarding the measurement, could you specify whether the units are in percent by volume or millimeters? It is also worth considering that averaging across the entire snowpack might introduce inaccuracies, given that microwave signal attenuation, particularly in the presence of significant wet superficial layer that can limit the penetration depth.

We will specify the unit of measure in the revised version of the manuscript. We are aware that averaging trough the entire snowpack is a limitation, however this choice is due to the fact that we used snow properties simulated with the snow cover model SNOWPACK. Indeed, finding an index suitable for our random forest and statistical analysis which considers both the LWC and its position within the snowpack is not possible. Anyway, the statistical analysis is solely intended to identify the snowpack variable most strongly correlated with the $SIsar$ index, not to verify the precise signature of each parameter to the backscatter. Moreover, we highlight that the results of the statistical analysis are in line with the findings of other cited works (as reported in our manuscript). Concerning the presence of wet layers, we analyzed their effects in Appendix A.

L170: did you use the “projected LIA” from SNAP?

We used the local incidence angle (LIA), not the projected local incidence angle ($PLIA$) from SNAP. The backscatter strongly depends on the true orientation of the surface relative to the radar beam, especially in non-flat terrains like snow-covered slopes. The LIA accounts for the local surface normal, incorporating slope and relief, and is therefore more physically consistent with the radar–surface interaction. Snow is a highly anisotropic medium in terms of radiometric behavior: even small changes in local incidence angle can lead to significant differences in the radar return. For this reason, we opted for the LIA, which better reflects the physical scattering conditions at the surface. Finally note that the LIA has beed used in other works for snow avalanches mapping (Tompkin and Leinss, 2021; Keskinen et al., 2022).

L174: Could you please explain the rationale for using the refined Lee filter?

Firstly, we got inspired by the majority of work in which preprocessing of SAR data for snow or avalanche detection purpose is presented. It would be very interesting to do a sensitive analysis changing the filtering technique in the preprocessing phase, however we believe that this is out of scope for our manuscript. We also had a closer look to temporal filtering techniques, however we decided to not use this for mainly two reasons:

1. Our work aims to improve the HS detection for operative scopes, like the support of avalanche forecasting activities. Temporal filter requires a certain number of acquisitions for the preprocessing and this can require a very high computational cost, especially for large areas.

2. The temporal filter requires a certain temporal radiometric stability in the scatterer, and we assume that this condition is missed in presence of dry snow.

In conclusion, in this study, the Refined Lee Filter was chosen to ensure consistent, per-image noise reduction, without introducing artifacts that could arise from temporal averaging across dynamically changing scenes.

L175: Given the S1 SAR sensor native resolution of 5m x 20m and the subsequent application of spatial filtering, a final resolution of 10m appears optimistic. A resolution closer to 20m seems more analytically consistent with these parameters.

The 10 m resolution results from the preprocessing activity performed in SNAP as described. The word "resampled" is a refuse which will be corrected in the final version of the manuscript, as described in the Reply to Referee 1. Anyway, we think that this highlights the validity of our model: even without an high resolution we were able to obtain promising results.

Eq 1 and Eq 2: can you better justify why you choose this index and why you made the difference between the indexes? What does it change if you change the reference? Would it be more appropriate to select as reference the initial backscattering after a significant accumulation, accounting for soil insulation that will persist for all the season long (if no permafrost is present)?

Eq. 1: As described in the introduction, the $DpRVI_c$ index follows directly from the work of Feng et al. (2024), who recently demonstrated that this index, which can be simply approximated from a GRD product (a great advantage from a computational point of view) outperforms all other dual-polarimetric indexes in snow depth retrieval.

Eq. 2: As already explained in a previous answer (see reply to L68), as observed by Feng et al. (2024), the $DpRVI_c$ index cannot isolate the depolarization resulting from the snowpack since there are also other sources of depolarization (land cover and ground properties). We therefore decided to subtract to it the average $DpRVI_c$ index value obtained under snow-free condition. In this way we could assume that the $SIsar$ index variations ($SIsar = DpRVI_c - DpRVI_c^{summer\ reference}$) are only due to variations occurred between the summer reference and the subsequent winter, which are negligible at least in terms of land cover and macroscopic ground properties. Moreover, we believe that the usage of an average between many snow free images related to the closest-in-time summer reduces the effects of soil properties variations. This technique is widely used and recognized in several SAR-based snow monitoring approaches, like the famous Nagler's method for wet snow retrieval. Furthermore, the usage of an average summer reference does not require any knowledge on the temporal weather condition, which is fundamental in the poorly monitored areas. It is interesting that a relationship between the $DpRVI_c$ and HS like Eq. (8) cannot hold. Indeed, as reported in the following image, that we will also add in the revised version of the manuscript, such index shows a dependency on the LIA even in absence of snow (see the yellow curve in subgraph (b)). This further supports our approach.

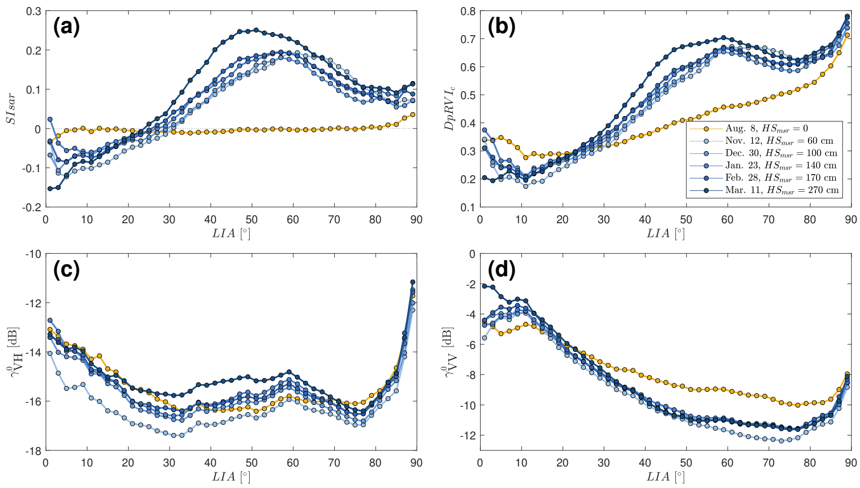

Figure 2: Mean $SIsar$ (a), $DpRVI_c$ (b), γ^0_{VH} (c), and γ^0_{VV} (d) values, sampled from the LIA sampling areas, grouped into LIA classes of 2° width. The blue color scale represents the increasing average HS values measured by the two AWSs for different acquisition dates along the 2023-2024 season. Yellow points represent the values from an acquisition date in summer 2023.

Finally, we decided to not subtract the initial backscatter after each accumulation for a purely strategic reason. As previously explained, our work is aimed at applications (avalanche forecasting for remote regions, for example). With such method, in order to derive the snow depth in a certain date within the winter season, one must analyze several seasons, significantly increasing the computational and storage costs. Thus we decided to use this simplified method and we showed the benefits. The only other way to avoid the preprocessing of an entire time series is using the average $DpRVI_c$ index value of the last images before the snow accumulation start in autumn/early winter. This would produce a reference more representative of the soil conditions below the upcoming snowpack, but again it requires a continuous knowledge of the snow cover (which is not possible to have for remote regions). We will explain this in the discussion of the revised version of the manuscript.

It would be beneficial to understand why the aspect angle i.e., the angle between the sensor flight direction and the surfaces orientation, was not considered into the analysis. This raises the question of whether the surface and snow are being implicitly assumed as isotropic mediums

As explained in the Introduction (L85), our initial objective was trying to understand what happens, at a small-scale (e.g., single mountain slope), when the SAR signal penetrates more or less snow (i.e., the LIA varies). We acknowledge the importance of the aspect angle in radar backscatter, however, in our study, our aim is to use SAR observations to detect and characterize variations in the snowpack properties themselves, without introducing assumptions about their directional dependence. Including aspect angle in the analysis would imply incorporating a prior model of directional variability (e.g., different snow or surface behavior depending on slope orientation). But in our case, we intend to observe whether such variability exists and how it manifests in the SAR response. In other words, considering aspect would mean assuming that snow characteristics change with orientation, but our goal is to assess whether and how they change, not to impose it beforehand. We therefore opted to analyze the data using only the LIA, which accounts for slope steepness but not orientation.

L204: It is important to carefully review the symbols used here to ensure they are consistent across the entire paper.

We did this and we ensure you that there are no errors.

L206: I am seeking further clarification regarding the methodology. Specifically, the justification for integrating the Random Forest algorithm to find the most predominant features, with a Least Squares method later, needs more comprehensive explanation (at least to me in the present form). Can you do the same using only one method?

In order to understand which snowpack variables have a strong relationship with our $SIsar$ index we used two different approaches. The random forest was used to obtain an initial idea of the presence or the absence of such relationship. We say "idea" because the sample size is not very large. We therefore performed a statistical analysis to study such relationships with another mathematical technique. The results of the two analysis are in line with each other (as observed in our paper) and they strengthen each other. However, we explain that the second analysis is the most important one for two reasons: firstly, through several statistical tests (for example, the p-value) we were able to prove that those results are reliable; secondly, through other tests (for example, the Pearson's correlation test) we were able to have an idea of the kind of relationship. At the end, we used the least squares method to derive the precise relationship. Obviously, we could directly do a linear or quadratic regression but in that case the results would not have been validated with a statistical analysis.

L235: It is important to note that snow distribution is highly preferential, influenced by both topography and meteorological conditions. This inherent variability is precisely why utilizing such high-resolution data is crucial for accurate analysis.

This is a limitation that also Referee 1 was concerned about (see also that reply). In general, we explained that the reliability of those measurements is demonstrated by the fact that the avalanche forecasters of the Livigno Avalanche Center carry out weekly measurements in different places of the valley and that similar data were already used for different 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). Moreover, we decided to carry out an additional validation with photogrammetric data (see again reply to referee 1) to validate our model with a large dataset: remarkably, with this additional validation we obtained errors even lower than with the other validations, further supporting the validity of the calibration of our model.

L297: just shadow.

We will change the wording as suggested in the revised version of the manuscript.

Fig 2. Following the criteria outlined by Nagler and Rott 2000, the presence of layover is identified for LIA less than 17° and shadow regions for LIA greater than 78°. Ensuring consistency with these thresholds throughout the analysis is appreciated for better reading the plots. Additionally, please standardize the LIA classification scheme across all plots for coherence. Could you also provide comments on the source of the standard deviation and outliers, as raised in the general comments? Finally, it would be beneficial to discuss the influence of aspect angle and the different land cover types on the results.

We will add the following sentences in the revised version of the manuscript (in Section 5.2, Line 353). *As shown in Fig. 2, the presence of outliers in the $SIsar$ index values across the LIA classes is notable. However, such variability is common in this type of experiment and can be attributed to various sources of error, including wind redistribution effects, speckle noise, and spatial variability in snowpack properties.* Concerning the LIA classes, we chose classes of 5° width for the box plot just to improve graph visualization since a 2° width would lead to a graph difficult to read. However, for the mathematical analysis and the subsequent graphs we chose 2° classes to improve the accuracy of the model. We discussed in the paper (L299) why it is not possible to consider also $LIAs$ lower than 30° in the implementation of the final model. This fact is also further supported by the behaviors of γ_{VV} and γ_{VH} (recall Fig. 2 above), as already discussed in the reply to referee 1 (see reply to Line 366). Finally, for the aspect angle see a previous reply.

Fig3. Could you please explain why, beyond an incidence angle of 60 degrees, the backscattering for a snow depth of 60 cm becomes higher in terms of SIsar?

Unfortunately, a clear explanation for this behavior is not readily available. One hypothesis is that, on 12 November 2024, the basal layer of the snowpack was generally wet, as indicated by both simulations and manual observations. As reported in Appendix A, such conditions can lead to overestimations. However, this wet basal layer cannot be assumed to be homogeneous across the entire study area. Therefore, we cannot exclude the possibility of preferential patterns in this snowpack property with respect to LIA on this date. In this analysis, we considered these values as outliers. We acknowledge that further studies, supported by specific ground-truth data on snowpack properties across varying LIA conditions, are needed to better refine the relationship between the $DpRVIc$ index, SAR backscatter, and the $SIsar$ index as a function of the LIA. However, this lies beyond the scope of the present study, which aims to highlight and verify the existence of a dependence, and also present a way to compensate it in an algorithm.

Regarding Figures 5 and 6, were these HS measurements obtained manually?

As explained in the methods section, these HS values are obtained manually for the Tromso validation area (Figure 6), while they derive from automatic weather stations measurements for the Livigno area (Figure 5).

L321: Could you elaborate on the statement that the snowpack contains a larger ice component (if I understood what a "snow mass inside a snowpack" is)? Specifically, how was it determined that this implies a higher concentration of grains and discontinuities? Furthermore, I am not entirely convinced that snow layers with varying relative permittivity alone can depolarize the signal; this would be an interesting hypothesis to demonstrate.

At L321 we have taken up an idea already presented in the work of Lievens et al. (2019), cited in the subsequent sentence, where it is suggested the V band signal depolarization increase with increasing discontinuities within the snowpack. A similar behavior was confirmed in the work of Keskinen et al. (2022) on avalanche deposits detection, we will add this citation at the end of the sentence. However, as suggested by Referee 1, we will change the wording in the following way: *One possible explanation for this increase is that thicker and denser snowpacks contain larger snow mass and more grains per unit volume of snow.* The fact that snow layers with varying relative permittivity alone can depolarize the signal is not obvious and out of the scope of our paper, indeed, as explained in the manuscript, this is just a possible explanation. Finally, the fact that thicker and denser snowpacks contain a larger snow mass, more grains, and more potential discontinuities follows from the fact that they have a large number of different layers, each characterized by different physical properties.

L322 and on. For a more pertinent comparison, I suggest referencing Kendra et al. (1998) and Strozzi et al. (1997), given their direct relevance, as opposed to broader X-band research.

We will do so in the revised version of the manuscript.

L355: Given the likely significant disparity in scatterer size between vegetation and snow, I am not convinced that a direct or simple analogy between their scattering properties is entirely straightforward

This analogy was proposed by Feng et al. (2024), we will report a citation at the end of the sentence in the revised version of the manuscript.

Table 2: Could you please specify the total number of validation points

We will add a column with that number in the revised version of the manuscript.

Fig 7: I am not certain I have fully understood the representation in this figure. It does not appear to align with the solutions of any radiative transfer equations commonly used for the snow problem that I am familiar with. Specifically, could you explain why the VV polarization appears unaffected by the presence of snow? Furthermore, the contribution of surface scattering, which could be dominant, seems to be unrepresented. Please refer to Figure 6 of the paper of Kendra et al, 1998 for a complete first order volume scattering mechanism representation for a snow layer.

As explained in the reply to Referee 1, we will remove this picture from the final version of our manuscript. See that reply for details.

L401: If the geographical aspect varies E-W (or W-E) while the slope remains constant, the LIA should remain the same.

Absolutely. With that sentence, our intention was simply to highlight that the different field measurements correspond to different $LIAs$ because they were collected on various mountain slopes, which are naturally not homogeneous. We did not mean to say that a change in aspect implies a change in the LIA; rather, our point is that when the slope itself changes, the associated $LIAs$ almost certainly differ as well, since the slopes are not identical. In any case, we will revise this sentence in the updated manuscript to clarify that the field measurements were taken from different mountain slopes and therefore span a wide range of $LIAs$.

References:

J. R. Kendra, K. Sarabandi and F. T. Ulaby, "Radar measurements of snow: experiment and analysis," in IEEE Transactions on Geoscience and Remote Sensing, vol. 36, no. 3, pp. 864-879, May 1998, doi: 10.1109/36.673679.

T. Strozzi, A. Wiesmann and C. Mätzler, "Active microwave signatures of snow covers at 5.3 and 35 GHz," in Radio Science, vol. 32, no. 2, pp. 479-495, March-April 1997, doi: 10.1029/96RS03777

C. Tompkin and S. Leinss, "Backscatter Characteristics of Snow Avalanches for Mapping With Local Resolution Weighting," in IEEE Journal of Selected Topics in Applied Earth Observations and Remote Sensing, vol. 14, pp. 4452-4464, 2021, doi: 10.1109/JSTARS.2021.3074418

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On behalf of all the authors,

Alberto Mariani