Revision to the manuscript preprint EGUSPHERE-2025-974

The Authors would like to thank the Editor and the two anonymous Reviewers for the positive feedback to our study and for their detailed and constructive comments which will improve the quality of this manuscript. We are aware that reviews are a significant time investment and therefore especially appreciate their effort and feedback. The manuscript will undergo revision according to the Reviewers' comments. Please see below our responses, which are highlighted in blue.

Response to the Reviewers

Reviewer 1

Reviewer Comment 1.1 — L376 – It was unclear to me how many layers were used for the SMRT simulations. From the text it seems layers were varied to account for the C-band wavelength, which is fine. However, a short statement outlining the range of layers or some statistics on this parameter would be good to see.

Reply: We thank the reviewer for pointing this out. We did not properly clarify this aspect in the text. We prepared Fig. 1, which we will insert into the Appendix. Fig. 1 represents the variability of the number of SMRT layers used for each simulation day as a function of the melting phase and the campaign year.

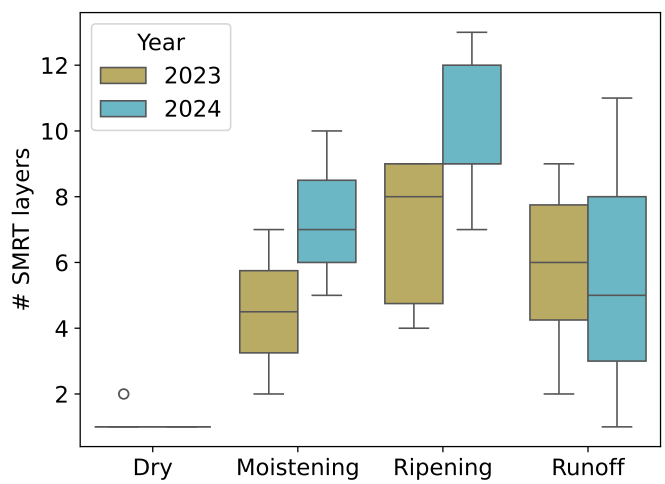

Figure 1: Variability of the number of SMRT layers used for each simulation day as a function of the melting phase and the campaign year.

At L379, we state: *To reduce the aforementioned sources of uncertainty, we chose to model the snowpack structure by stacking layers with a minimum thickness corresponding to the C-band wavelength, ensuring each layer had consistent average physical properties. These property-based layers* *were identified automatically by means of a simple algorithm and then refined manually, with particular emphasis placed on LWC over the other variables.*

We will expand this section referring to Fig. 1 and explain the following concepts:

- The number of identified SMRT layers for each simulation day depends on the stage of the melting process. Fig. 1 shows that in dry snow conditions, the densely measured snow properties are often averaged into one single layer, given the absence of liquid water. As the snowpack starts moistening, the number of identified layers increases, as a function of the first formation of liquid water within the snowpack. Generally, the highest number of SMRT layers to model the snowpack is used during the ripening phase, as the LWC layering is at its most heterogeneous state during this phase, as a consequence of the progression of the wetting front. Later in the runoff stage, with the snowpack being fully saturated, the number of used SMRT layers decreases again, as a consequence of a more homogeneously moist snowpack.
- The number of identified SMRT layers also depends on the campaign year. Fig. 1 shows that the first campaign year has been modeled using ∼10% less layers than the second, on average. The presence of ice lenses helped to homogenize the distribution of liquid water within the snowpack, resulting in more uniformly wet layers near the surface and consistently drier sections toward the bottom. Without ice lenses, in 2024, the progression of liquid water into the snowpack was more heterogeneous, therefore requiring more modeling layers to remain as true as possible to the conditions observed in the field.

Reviewer Comment 1.2 — Fig 2. - The colours in panel a) are not clear. The white area is skiable domain, however, has a greener shade to it. Recommend using a hash or some sort of pattern to denote this area.

Panel b) and Panel c) – I realize that if I zoom in the legend is more visible, however, it is difficult to see at 100% zoom. The legend is also identical for both panels. Therefore, one larger legend with font size increased would be an improvement.

Reply: We will improve the readability of manuscript's Fig. 2 according to the reviewer's suggestions.

Reviewer Comment 1.3 — Figure 4. – This shows the setup for one SMRT simulation; however, nothing is marked as the SMRT layers used. Would this be possible to provide? Addresses my previous comment as well as providing some sort of example relate to the number of SMRT layers used.

Reply: We thank the reviewer for this remark. What we call "Physical layering" (horizontal yellow dashed lines) in manuscript's Fig. 4 are indeed the layers used in SMRT to simulate the radar backscattering on that specific day. We will modify the legend, the caption of the plot and the reference in the text in order to make this point more clear.

Reviewer Comment 1.4 — Figure 5/6. – I think these are good figures illustrating the change in variables across the campaign. However, the text is not readable. Suggested changes: the different stages of snow melt could be included at the stop of only one pane as they are identical throughout the rest of the figure. Ideally the vertical lines could also be colour coded. Dates can be provided on only one pane as they are identical across. Also, font size is okay but a little small.

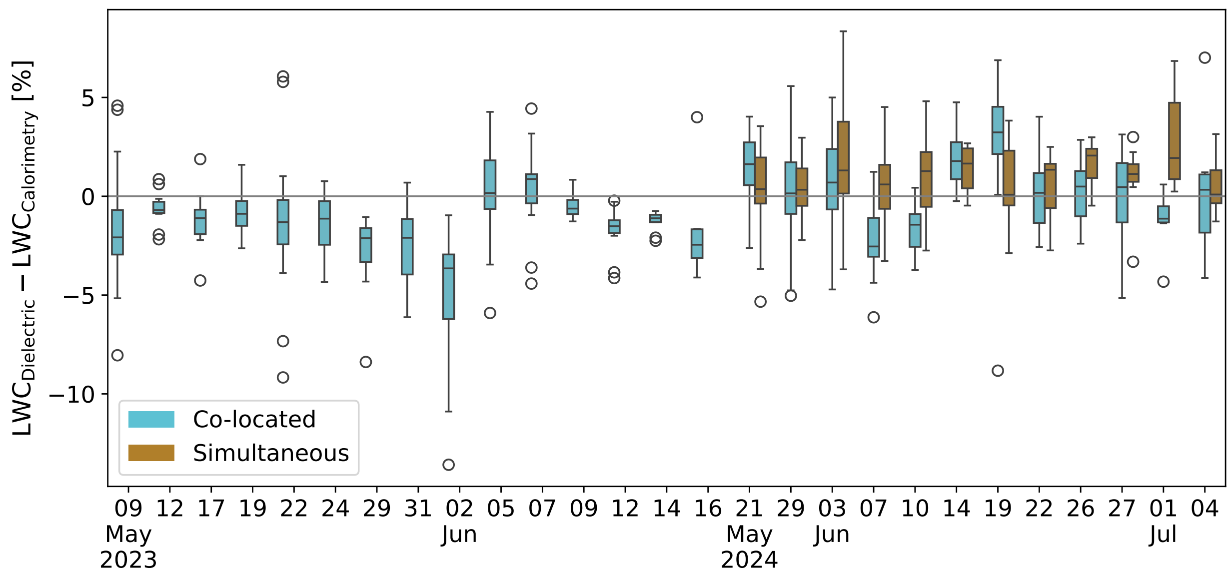

Figure 2: Modified version of manuscript's Fig. 8.

Reply: We thank the reviewer for these suggestions. We propose to leave the names of the melting phases at the top panel only and remove them from the rest of the panels, only leaving the separating dashed vertical lines. Our opinion is that by adding more color-coded lines, these plots might become overwhelming, therefore our preference is to leave the separating dashed vertical lines in gray. We agree that the dates on the x-axis of each panel might look redundant; however, in our opinion, they help improving the readability of the Figures, since we often mention different date ranges referred to different measured snow properties throughout the text. We will try to increase the font size.

Reviewer Comment 1.5 — Figure 8 – Only one y-axis is needed between the figures. This would allow the size to be increased and increase readability.

Reply: We propose Fig. 2 as the new version of manuscript's Fig. 8, modified according to your suggestions. We merged the two panels into a single one, increasing the font size for better readability.

Reviewer Comment 1.6 — Table 3 – Table layout is somewhat difficult to read. Horizontal outlines would aid in the interpretation of the data.

Reply: We will add horizontal outlines to improve the readability of the table.

Reviewer Comment 1.7 — Figure 9 – This is a great figure. However, it is difficult to read. Two suggestions, 1) accompany the figure with a table which includes the specific explanations that way the table would be less busy, 2) use numbers or letters to point to specific events rather than having the arrows across the figure. This would again aid in improving the readability. Currently it takes too long to interpret it.

Reply: We agree with the reviewer that manuscript's Fig. 9 contains a great amount of information. It was rather hard to think about a way of conveying it while trying to avoid a chaotic result. We prepared a variation of this Figure according to your suggestions – see Fig. 3 and Tab. 1.

Table 1

Group	Measured snow properties			Possible reasons for deviations
	TWC	LWC	RMSH	Possible scattering mechanisms
1a	–	–	–	Backscattering increase due to soil thawing
2a	<10 mm	<3%	∼1 mm	Uncertainty in spatiotemporal LWC/TWC Scattering from surface structures due to melt-refreeze Underestimation of surface roughness Scattering from the wet soil
3a	>10 mm	>3%	1 → 4 mm	Uncertainty in spatiotemporal LWC Uncertainties in roughness measurements Uncertainties in IEM modeling
4a	>10 mm	>3%	3∼4 mm	Uncertainty in spatiotemporal LWC Uncertainties in roughness measurements Uncertainties in IEM modeling
5a	>10 mm	>3%	∼1 mm	New snowfall on a rough surface: ”buried roughness”
6a	<10 mm	<3%	∼1 mm	Cold spell Uncertainty in spatiotemporal LWC Scattering from surface structures due to melt-refreeze Underestimation of surface roughness Scattering from wet soil
7a	>10 mm	>3%	>4 mm	Uncertainty in spatiotemporal LWC Uncertainties in roughness measurements Uncertainties in IEM modeling
1b	–	–	–	Backscattering increase due to soil thawing
2b	<10 mm	<3%	∼1 mm	Uncertainty in spatiotemporal LWC/TWC Scattering from surface structures due to melt-refreeze Scattering from the wet soil
3b	>10 mm	>3%	∼1 mm	Uncertainty in spatiotemporal LWC Scattering from surface structures due to melt-refreeze Underestimation of surface roughness
4b	Varying, <10 mm	<3%	∼1 mm	Cold spell Uncertainty in spatiotemporal LWC Scattering from surface structures due to melt-refreeze Underestimation of surface roughness Scattering from wet soil
5b	>10 mm	>3%	∼1 mm	Uncertainty in spatiotemporal LWC Scattering from surface structures due to melt-refreeze Underestimation of surface roughness
6b	>10 mm	>3%	∼3 mm	Uncertainty in spatiotemporal LWC Uncertainties in roughness measurements Uncertainties in IEM modeling
7b	>10 mm	>3%	∼1 mm	New snowfall on a rough surface: ”buried roughness”
8b	>10 mm	>3%	>4 mm	Uncertainty in spatiotemporal LWC Uncertainties in roughness measurements Uncertainties in IEM modeling

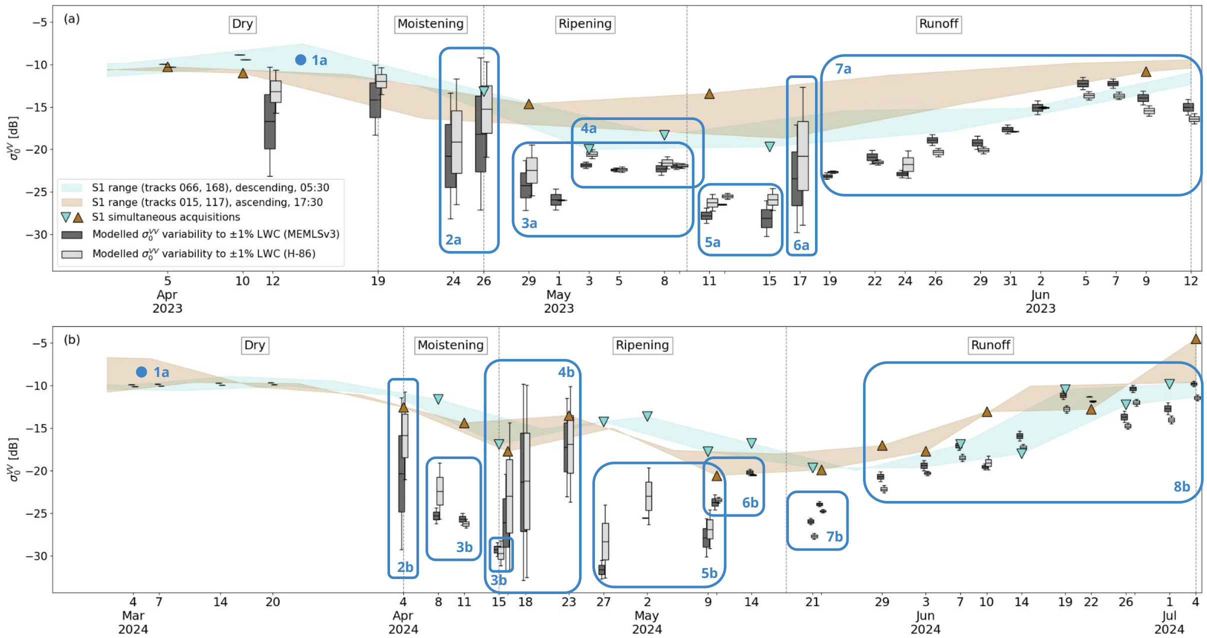

Figure 3: Modified version of manuscript's Fig. 9.

Our opinion, however, is that by off-loading the descriptions to a table the readability doesn't necessarily improve. In the old version, all information is on the same figure – albeit a figure which takes time to interpret. In the new version, the figure is indeed lighter, but the reader has to toggle between the figure and the table, henceforth the interpretation of the information is not necessarily faster or easier.

Reviewer Comment 1.8 — Table 4 – Similar comment to table 3.

Reply: We will add horizontal outlines to improve the readability of the table.

Reviewer Comment 1.9 — Figure 11 – The bottom pane on this figure is unnecessary – it does not add anything to the interpretation. Simply providing the LWC for the top layer would be sufficient. Also, the LWC legend is unnecessary on the bottom. The RMSH legend is good.

Reply: We will modify manuscript's Fig. 11 according to the reviewer's suggestions.

Reviewer 2

Reviewer Comment 2.1 — The radiometric accuracy for the backscatter data of the reference cell needs to be specified in detail. The reference to Marin et al. (2020) does not provide specific information on radiometric uncertainty for this particular cell of 20 m x 20 m extent. In S1 IW mode data it covers only about 4 independent samples (looks), resulting in high speckle-related uncertainty. Marin et al. apply for speckle reduction a multitemporal filter of 11 x 11 pixels and a gamma-MAP filter of 3 x 3 pixels. In both cases the radiometry is preserved if the window covers a homogeneous distributed target. This is not the case in the area surrounding of the refence cell. For the multitemporal filter different temporal response within the window may introduce additional radiometric biases for sigma-0 of individual pixels and dates.

Reply:

We appreciate the reviewer's insightful comment regarding the characterization of radiometric uncertainty, which allows us to provide important clarification. We agree that thoroughly addressing radiometric uncertainty is crucial for our study, especially concerning the impact of speckle.

Indeed, our signal analysis and subsequent comparison with model results are influenced by several sources of uncertainty. As the reviewer rightly points out, the radar instrument itself is a significant contributor. ESA calibration campaigns indicate a nominal uncertainty within a 3σ of 1.0 dB [1–3]. However, additional pre-processing steps also contribute to the overall uncertainty. These include not only the application of despeckle filters but also crucial operations like terrain correction and radiometric normalization, which are particularly challenging in mountainous regions due to complex topography. Other steps such as thermal noise removal (important in conditions of high absorption like wet snow) and spatial interpolations further add to this complexity. Consequently, a detailed specification of the overall radiometric accuracy becomes quite complex and is beyond the scope of this paper, which we have earmarked for future dedicated work.

Nonetheless, we believe it is valuable to illustrate the temporal behavior of backscattering over the target cell and demonstrate the importance of speckle denoising. For this study, we opted for the multi-temporal filter proposed by Quegan and Yu in [4] to mitigate speckle impact. The Quegan and Yu filter offers a powerful yet simple approach for denoising multi-temporal stacks. Its original implementation involves local averages of intensity values for each date (akin to local spatial multi-looking), this can lead to a loss of resolution, blurring strong targets and edges, and ultimately impacting the global multi-temporal result as pointed out by the reviewer. Indeed, we acknowledge that more complex and robust despeckling methods are now available (e.g., [5]) and can be considered for future work. However, when the target is presumed to be time-invariant, such as during the winter period (with stable scatterer positions, snow cover, temperatures well below 0°C, and frozen soil ensuring no soil moisture variation), the pixels in our study exhibited stable behavior. This stability was also observed during dry periods in summer. In these specific cases, the standard deviation was within 1.0 dB, which aligns well with S1's nominal radiometric uncertainty.

On the other hand, Figure 4 illustrates the behavior for track 168 during the melting period when differences in time are present due to wetting of the snow. As evident, the differences between various window sizes and the unfiltered signals are minimal, justifying our pragmatic choice of an 11 x 11 pixel window: this choice aims to reduce uncertainty due to speckle while (trying to) introducing minimal bias. Crucially, we want to clarify that the gamma-MAP filter (as used in Marin et al. 2020) was not applied in this study – we will make this distinction clearer in the revised version of the manuscript.

Finally, we acknowledge that the primary source of radiometric uncertainty, particularly during the melting period, originates from the presence of liquid water content (LWC). Estimating radiometric uncertainty under such conditions is extremely difficult, given LWC high heterogeneity across the entire resolution cell, as highlighted already in the manuscript. Therefore, a rigorous uncertainty estimation without a precise reference for LWC is inherently challenging and falls outside the scope of this paper. We will introduce the discussion of radiometric uncertainty more thoroughly in the revised manuscript and clearly outline this as a limitation of our current study, highlighting it as an important direction for future dedicated research.

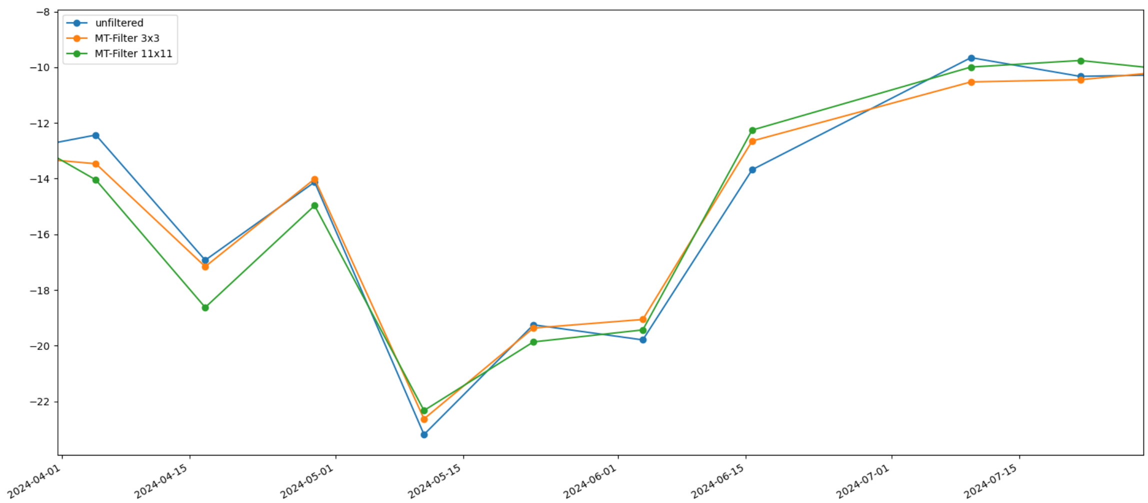

Figure 4: Effect of the multitemporal filter, with different window sizes, to the backscatering signal for track 168 during the melting season of 2023-2024.

Reviewer Comment 2.2 — The representativeness of the S1 signatures of the reference cell in respect to the area in its surroundings should be assessed by analysing also data of other cells, preferably covering some larger area. Considering Fig. 2, it is unclear why cell 39 is used as single reference case. The data of cell 39 do not show a distinct incidence angle dependence and track 117 shows a different behaviour in the two years. The data of cell 18 of the two years, for example, are consistent. According to the elevation contour lines, this cell is located on an east-facing slope, and the two tracks of the ascending orbit show lower sigma-0 values, as expected for a back-slope. This may offers a possibility for studying impacts of the incidence angle.

Reply: We thank the reviewer for their comments on the representativeness of the selected S1 cell. These remarks helped us to corroborate the premises of our analyses.

In the manuscript (Sec. 4.2), we tried to highlight the heterogeneity of LWC measurements (even when measuring simultaneously). Such heterogeneity has been previously analyzed and consolidated in both laboratory settings and field studies [6, 7]. It is true that the representativeness of the chosen S1 reference cell would benefit from the comparisons to other neighboring cells. However, the range of locations within the field site being undisturbed, potentially matching the snow characteristics measured in the snow pit and not affected by double-bounce effects from big metallic structures nearby is rather small and likely limited to cells 32, 38, 39, 40. In the reviewed version of the manuscript, we will show a comparison between the S1 signatures of the selected reference cell with respect to these neighboring cells.

As explained in Section 3.1 of the manuscript, cell 39 was initially chosen primarily because it is closest to our measurement field and therefore most likely to represent the snow conditions accurately. However, as the reviewer rightfully points out, the incidence angle generally plays an important role in wet snow conditions. Therefore, in the reviewed version of the manuscript, we will change the reference cell to cell 40. Cell 40 covers a flat area right next to our measurement field, and the snow surface there is totally undisturbed because of the presence of a terrestrial laser scanner taking point cloud acquisitions of the snow surface at high temporal resolution. Therefore, we could hypothesize that the snow properties are as similar as possible as those we have measured. Besides, the data of cell 40 also show relatively distinct incidence angle dependence as for cell 18. We have three major concerns with the choice of cell 18 as a reference cell. In the first place, although it lies officially outside of the skiable domain, it is not uncommon for skiers to traverse that area, especially for employees on duty for daily snow measurements throughout the snow season. This highlights the need for a more precise delineation of the skiable domain in Fig. 2 of the manuscript, which we will address in the revised version. On the other hand, cell 18 belongs to a east-faced slope (∼11°), where the snow cover becomes patchy and disappears earlier with respect to the flatter area (∼5.5°) where the preferred cells are situated (see Fig. 6). This could introduce ambiguities in our discussion of backscattering increases related to the development of surface roughness. Finally, cell 18 lies ∼90 meters away from the area where we performed our snow pits. To our knowledge, the greatest horizontal distance over which LWC variability has been characterized is 5 meters on relatively flat terrain [6]. Therefore, selecting a greater horizontal distance and slope difference than what we used could potentially introduce additional uncertainties in our analysis that we would be unable to quantify.

The impact of incidence angles was not explicitly designed as a part of this study, primarily because this topic has already been extensively investigated in the literature already cited in the manuscript – and further complemented by the reviewer's suggestions [8–10]. These studies primarily rely on tower-based radiometers and radars. Tower-based settings offer a level of precision and control that is not feasible for radars installed on satellite platforms overlooking 20 x 20 meter cells. In our case, the region matching the characteristics of the snow pits' area is small and primarily flat, therefore the range of local incidence angles suitable for a comprehensive study is relatively constrained (see Fig. 5), thus limiting the possibility to investigate its impact.

Moreover, due to the significant heterogeneity of LWC, a comprehensive study covering a sufficiently wide range of local incidence angles would ideally require a separate ground reference for each cell. However, this was not feasible given the time and resource demands already involved in conducting the analysis at a single location. The use of different tracks was done primarily in order to maximize the number of possible comparisons under different snow conditions, also given the failure of Sentinel-1B. As explained in Sec 3.2 of the manuscript, the incidence angle is not responsible for deviations between backscattering simulations and S1 recordings. This is because the SMRT model is provided with either the exact incidence angle of S1 (when measurements and satellite overpasses coincide) or the incidence angle of the temporally closest overpass (when they do not). However, as the reviewer correctly notes in the following Comment 2.3, the incidence angle is relevant when analyzing roughness effects on wet snow surfaces, as we do in Section 4.3.1 of the manuscript by means of synthetic experiments. In the reviewed version, we will improve this section by repeating such experiments across the range of incidence angles of cell 40. The results will be discussed in the context of the prior studies that the reviewer mentioned [8, 9] – although the surface roughness was not measured in these studies, but only assessed.

We will ensure that each of the above points is thoroughly addressed and that our choices are clearly justified in the revised version of the manuscript.

Reviewer Comment 2.3 — For relating the backscatter modelling results to specific melt phases and for interpretation of the observed backscatter signatures, first of all it is important providing information on the backscatter contributions of the individual snow layers in dependence of the liquid water content. The relative contributions to total backscatter in dependence of depth below the snow surface should be specified for typical snow profiles shown in Figs. 5 and 6. Another concern is the limited information regarding properties of the scattering elements in the individual layers and the parametrization of the dense medium effect. In particular for the early melt phases, the properties of the top snow layer can be quite different, depending on size and shape of water inclusions (Arslan et al., 2003). Furthermore, information on the impact of incidence angle dependence of backscatter would be of interest, being of relevance for assessing roughness effects of wet snow surfaces.

Reply: We thank the reviewer for these valuable comments, which highlight issues that were not sufficiently addressed in the initial version of the manuscript.

In a previous version of our work, we included a section addressing the relative contribution to the total backscattering of individual snow layers, as a function of the measured liquid water content. We finally decided to remove it, because the solidity of such analysis is strongly dependent on two main aspects. One of them is the lack of a consolidated wet snow permittivity formulation, which as explained in Section 3.2 of the manuscript, remains an unresolved issue in the field of electromagnetic modeling of wet snow surfaces. In the course of our analysis, we compare two formulations which were previously validated against real-world C-band data [11–13]. However, these two formulations diverge significantly at C-band, especially for what concerns the prediction of the imaginary part of the permittivity, which governs absorption losses. Such divergence becomes more pronounced for increasing values of LWC, as we show in Fig. 3 of the manuscript. On the other hand, we were unable to measure and model larger (superficial or internal) snow structures potentially causing additional scattering effects, such as crusts, which might have an important effect on the total recorded backscattering at the cell-scale. In the reviewed version of the manuscript, we will make sure to highlight this issue more clearly.

Furthermore, we agree with the reviewer that our dataset lacks information about the shape of the water inclusions per layer, and we thank them for providing this reference, which we overlooked in our literature review. To our knowledge, neutron radiography can be used to distinguish water from ice, however, it requires either prior knowledge of the dry density or other measurements such as energy-selective neutron radiography [14]. In such experiments, the initial conditions need to be carefully controlled, therefore requiring a laboratory setting. [15] recently presented the nuclear MRI rapid profiling technique, which allowed measuring different states of wetting snow and therefore the shape of the water inclusions at unprecedented resolutions. This technique also depends on a controlled laboratory environment. At the time our measurement campaign was designed and conducted, these methods did not yet exist – let alone their applicability in the field, which is still entirely unknown. However, this would be an extremely promising development for future research and similar studies. In the revised manuscript, we will acknowledge the additional source of uncertainty arising from the practical infeasibility of characterizing the shape of water inclusions in an experimental field study, and suggest exploring the feasibility of methodologies as [15] in the field as potential future developments.

Regarding the incidence angle effects, we refer to our previous Reply 2.2.

Reviewer Comment 2.4 — Introduction and Discussion: During three winters Strozzi and Mätzler (1997; 1998) performed at the same test field above Davos C- and Ka-band backscatter measurements. Reference 1 (1997) is briefly cited in the manuscript, reference 2 (1998) is not cited. Results of these measurements are relevant within the context of the work presented by Carletti et al. and key points should be mentioned. Among issues addressed in the two papers are impacts of surface roughness and refrozen snow crusts based on measurements at different incidence angles, and the response to liquid water content.

Reply: We thank the reviewer for this comment – we have missed the above mentioned article in our literature review. We will make sure to reference it in the Introduction and Discussion of the revised version of the manuscript.

Reviewer Comment 2.5 — P4, L121: The sensitivity to snow wetness depends on the local incidence angle, not directly on slope steepness. On steep fore-slopes the sensitivity is low.

Reply: We thank the reviewer for this comment. We will correct this sentence.

Reviewer Comment 2.6 — P8, L212: A cutter of 55 cm length may not be suitable for resolving the density differences between individual layers that may be quite thin during the different melt phases. Please explain the limits in vertical resolution.

Reply: The cylinder cutter was only used for manual measurements of bulk snow water equivalent, which served as validation for the automatic sensor. As explained in Section 2.1.2 of the manuscript, manual density measurements were performed using a box cutter with a vertical dimension of 3 cm. This method allowed us to precisely measure density differences between individual snow layers (see Fig. 4 of the manuscript). The resulting density profiles were then used as part of the inputs to the radiative transfer model to simulate the S1 backscattering.

Reviewer Comment 2.7 — P9, Table 1: The elevation contour lines of Fig. 2a indicate different slope steepness within the test field. Please explain to which points the cited incidence angles refer and show the overall range of angles for the test field.

Reply: We thank the reviewer for noting that in the original text there was ambiguity between "test field" and the reference cell actually chosen for the analysis. We will clarify that the incidence angles reported in Tab. 1 of the manuscript refer to the selected reference cell. Moreover, to address the reviewer's concerns about the influence of the incidence angle in Comment 2.2, we prepared Fig. 5, which we will include in the Appendix.

Reviewer Comment 2.8 — P11, Fig: 2: Incidence angles of cells on sloping show lower sigma-0 for ascending orbits, as to be expected for back-slopes. Consequently, the difference in sigma-0 between individual tracks may offer the possibility for exploring incidence angle effects.

Reply: We thank the reviewer for this comment. In Reply 2.2, we have explained why studying the effects of the incidence angle is quite impractical in a study designed like ours, primarily due to the extreme heterogeneity of the LWC. However, we will propose this topic as one of the future developments of our work.

Reviewer Comment 2.9 — P12, L328: The characterization of snow microstructure is a critical issue for snow backscatter modelling. Exponential correlation functions have major deficiencies, in particular for multi-size and sticky cases (Chang et al., 2016). Furthermore, the phase functions of snow with liquid water inclusions are quite different from that of dry snow (Arslan et al., 2003).

Reply: We thank the reviewer for their comments about the characterization of snow microstructure, and for providing important literature reference which we overlooked.

The choice of an exponential correlation function is explained at P12, L325-330 of the manuscript. In these lines, we refer to the study of [16], specifically to Section 2.2, where the authors illustrate the unifying role of the microwave grain size (ℓ_{MW}) at low frequencies such as the C-band. In detail, the authors explain how ℓ_{MW} can be computed for different analytical forms of various auto-covariance functions (i.e. the exponential and the sticky hard spheres) and then related to specific parameters of such forms. Notably, it is not guaranteed that such analytical forms always exist, but they do for the aforementioned formulations. In such cases, the analytical expressions of ℓ_{MW} make the different representations of microstructure comparable. Most importantly, it is guaranteed that different microstructure representations predict the same scattering amplitude in the low frequency limit when the same value of ℓ_{MW} is used as input. Therefore, according to these findings, the choice of the best representation of snow microstructure becomes a secondary problem with respect to measuring ℓ_{MW} in order to predict snow scattering in the microwave domain. ℓ_{MW} is computed as the product of the Porod length ℓ_P and the polydispersity k. As explained at P12, L325-330, we computed ℓ_P from our detailed field measurements and chose an empirical value of k according to the findings of [16], which are based on a comprehensive set of μ-CT scans covering a wide variety of Alpine snow samples with convex grains, among which rounded grains and melt forms.

The reviewer correctly points out that phase functions of wet snow differ from those of dry snow. We will address this source of uncertainty in the revised version of the manuscript.

Reviewer Comment 2.10 — P14, L363: In particular during a main part of winter 2023-2024 the base of the snowpack shows zero deg. temperature, implying unfrozen ground. This goes on throughout the snowmelt periods.

Reply: We thank the reviewer for this remark. The sentence they refer to is actually formulated poorly. Indeed, we do not model the soil as a frozen surface. Using the functions available in SMRT, we model the substrate as a reflecting surface with a given value of backscattering. In dry snow conditions, on days when manual measurements and satellite overlooks coincide, we assign the S1 recorded backscattering value to the substrate, assuming that dry snow is transparent to radar waves at C-band and that therefore the soil is the only contribution to the total backscattering. In wet snow conditions (or in dry snow conditions, when there is no concomitance between measurements and satellite overlooks), we assign a fixed value of backscattering to our substrate, which we compute as the average value in dry snow conditions of each individual track (incidence angle). Notably, SMRT offers the possibility to compute the backscattering from the soil, however, it requires a series of detailed information that are spatially heterogeneous and would have been nearly impossible to retrieve continuously over the course of our campaign. These properties include the soil moisture, the relative sand content, the relative clay content, the soil content in dry matter, and other geometrical parameters such as the roughness and the correlation length.

Reviewer Comment 2.11 — P16 Fig. 5: Between mid-April and early June 2023 sigma-0 of the two ascending tracks shows consistent differences of 3 to 5 dB. Please explain the reason. The high sigma-0 values are probably from track 117 which shows high variance in 2023-2024, differing from 2022-2023 (Fig. 2b).

Reply: We thank the reviewer for this remark, which we will address in the revised version of the manuscript.

Reviewer Comment 2.12 — P20, Table 2: Please specify the incidence angle. Besides, the validity of these numbers in respect to other incidence angles would be of interest.

Reply: We thank the reviewer for this comment. The values in manuscript's Tab. 2 were averaged over the 4 incidence angles. We enhanced this analysis and highlighted the dependence on the incidence angle.

Reviewer Comment 2.13 — P 27, Fig. 11: Please specify the incidence angle. Strozzi and Mätzler (1997) show the incidence angle dependence of backscatter of wet snow (for smooth and rough surfaces) based on backscatter measurements at the same test site.

Reply: The used incidence angle is specified in the text, however, according to your suggestion, we will improve our analysis by repeating the simulations for the incidence angles overlooking cell 40. While comparing the various datasets is of great interest, the considerable LWC heterogeneity complicates direct comparisons. Moreover, our dataset includes time series of measured surface roughness, whereas [8, 9] provided either visual estimates or just estimates.

Reviewer Comment 2.14 — P29, L625: The development of surface roughness after start of the snowmelt period depends also on the sequence and intensity of snowfall events, varying from year to year.

Reply: We thank the reviewer for this remark, we will rephrase the sentence.

Reviewer Comment 2.15 — P29, L641ff: The parametrization of the scattering elements may as well be a reason for differences between recorded and modelled backscatter (see e.g. Arslan et al., 2003; Chang et al., 2016).

Reply: As discussed above, we will make sure to mention this limitation in the Discussion.

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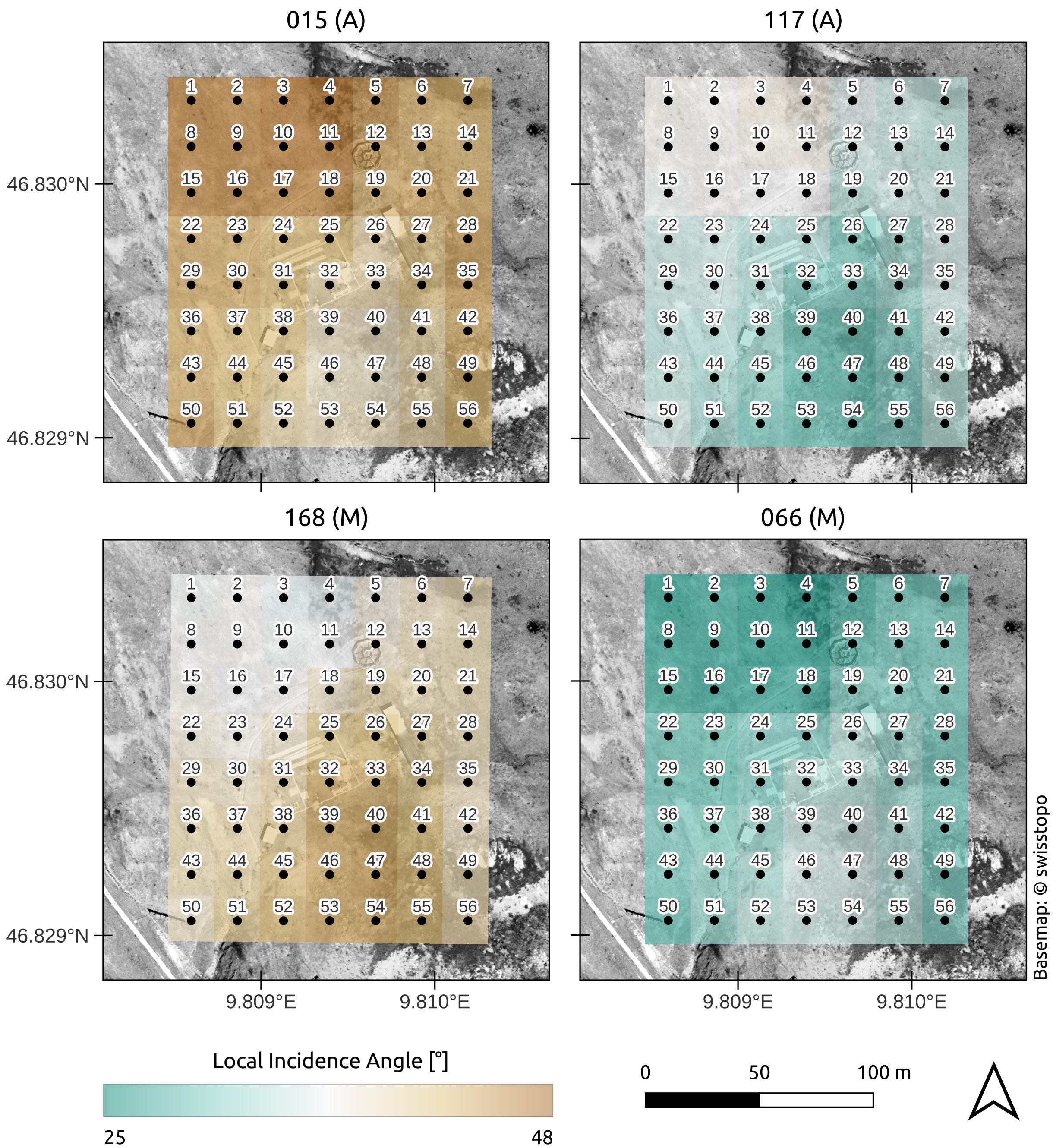

Figure 5: Range of local incidence angles over the Weissfluhjoch measurement station for each S1 afternoon (A) and morning (M) track.

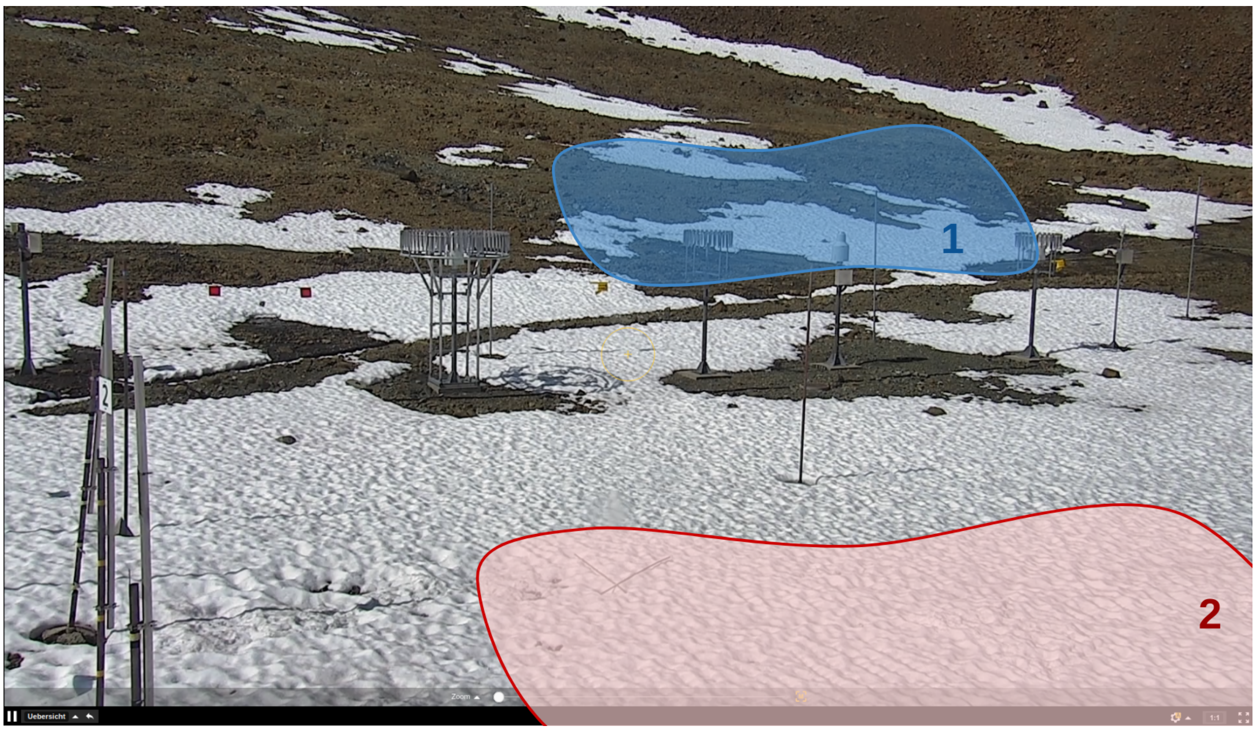

Figure 6: Webcam acquisition of the field site of Weissfluhjoch (June 11^{th}, 2025 at 16:00:00). Area 1 (in blue) indicates the approximate location of cell 18. During snow ablation, this section shows earlier snow disappearance with respect to the flatter locations where cells 32, 38, 39, 40 belong, highlighted by area 2 (in red).