

Response to the major comment

We thank the referee for the detailed and constructive review and for recognizing the originality of the field observations and the objective of the paper. We agree with the principal concerns raised by the referee. In particular, we agree that the manuscript should be framed explicitly as an event-scale case study rather than as a general conceptualization of snowpack response to ROS.

In response to these comments, we will revise the manuscript in several ways. First, the Abstract, Introduction, and Discussion will be revised to state more clearly that the observations represent a single natural ROS event and that the quantitative characteristics reported here should not be generalized to other rainfall amounts, snow depths, or initial snowpack conditions without further observations, experiments, or modelling.

Second, we will reorganize and expand the Materials and methods section to provide a clearer description of the study sites, meteorological observations, snow-pit measurements etc. We will also clarify which characteristics were measured directly and which interpretations were derived from post-event observations.

Third, we will strengthen the treatment of uncertainty. In addition to discussing the limitations associated with the single event and differences among the four sites, we will quantify measurement variability and uncertainty where the available observations permit this. We will also discuss more explicitly the limitations related to manual feature delineation and to the exploratory protocol used for destructive measurements of individual percolation columns.

We will also reorganize and expand the Discussion. We will compare our observations more explicitly with previous studies of ROS-induced snowpack changes in boreal forests and layered snow, distinguish more carefully between direct observations and process interpretations, and discuss the implications of the observed sub-metre heterogeneity for multilayer snow, hydrological, and land-surface modelling. The methodological value of smartphone-based mapping will be retained, but presented as a secondary contribution rather than as the principal scientific implication of the study.

In addition, we will address each of the specific comments raised by the referee individually and revise the corresponding sections, figures, and references where appropriate. We believe that these revisions will improve the transparency of the methods, strengthen the treatment of uncertainty, and clarify the positioning of the manuscript as a detailed field case study of spatially heterogeneous snowpack response to a natural ROS event.

Response to Specific comments

32: Please correct the reference in the text: Pomeroy and Brun, 2001.

We corrected the typo in the publication year from Pomeroy and Brun (2021) to Pomeroy and Brun (2001).

41 to 44: I don't see the opposition between these two statements. I suggest rephrasing to emphasize that capillary barriers can form in the presence of a fine-to-coarse layer superposition (see Avanzi et al. 2026 (<https://tc.copernicus.org/articles/10/2013/2016/>))

We revised this to clarify that capillary barriers can form where fine-grained snow overlies coarser snow and can promote water accumulation and lateral redistribution. We also added Avanzi et al. (2016) to support this mechanism.

Fine-grained snow generally has stronger capillary retention than coarse-grained snow and may support more uniform wetting under some conditions, but the actual flow regime depends on water input rate, initial snow structure, and hydraulic contrasts between layers (Albert et al., 2000; Katsushima et al., 2013).

Fine-grained snow generally has stronger capillary retention than coarse-grained snow. Where fine-grained snow overlies coarser snow, this contrast can create a capillary barrier that impedes downward water transmission and promotes localized accumulation and lateral redistribution at the layer interface (Avanzi et al., 2016). In addition to snow structure, the flow regime depends strongly on the rate of liquid-water input (Albert et al., 2000; Katsushima et al., 2013). Together, these controls determine whether water moves relatively uniformly through the snow matrix or becomes concentrated into preferential vertical or lateral pathways (Hirashima et al., 2014; Leroux and Pomeroy, 2017).

62 to 64: In my view, this paragraph essentially addresses vertical ice features in the snowpack. However, this sentence mentions exclusively an effect of the horizontal structures such ice crusts of internal ice layer on wildlife access to resources. I recommend you reorganize this part of your introduction to present sequentially the effects of 1) horizontal structure and 2) vertical percolation features.

We reorganized this part of the Introduction to distinguish more clearly between the effects of horizontal ice crusts and internal ice layers and those of vertical percolation features. The revised text now presents these effects sequentially, as suggested.

Their presence is also important for remote sensing and modelling because vertically and laterally heterogeneous wet or refrozen structures can modify microwave propagation, radar backscatter, penetration depth (Colbeck, 1972; Hirashima et al., 2010; Donahue and Hammonds, 2022; Stroeve et al., 2022), and the interpretation of snow wetness, snow water equivalent, and internal layering (Wiesmann and Mätzler, 1999; Montpetit et al., 2013; Zhu et al., 2021).

Horizontal ice crusts and internal ice layers can have important ecological consequences by limiting access to vegetation, contributing to large-scale mortality in reindeer and caribou populations and affecting local livelihoods (Bartsch et al., 2010; Forbes et al., 2016; Serreze et al., 2021). Vertical percolation features, in turn, concentrate liquid water and refrozen ice within localized pathways, increasing small-scale heterogeneity in snow properties. Such vertically and laterally heterogeneous wet or refrozen structures are also relevant for remote sensing and modelling because they can modify microwave propagation, radar backscatter, and penetration depth, complicating the interpretation of snowpack properties (Colbeck, 1972; Hirashima et al., 2010; Donahue and Hammonds, 2022; Stroeve et al., 2022).

75 to 81: I suggest to clearly state the objective of your work at the end of the introduction: ‘Overall, the objective of this study is to ...’.

We revised the end of the Introduction to state the study objective explicitly.

Despite this relevance, the formation, morphology, and spatial distribution of percolation columns remain poorly studied under natural ROS conditions, particularly in cold, predominantly fine-grained snowpacks. Here, we present direct in situ observations of percolation-feature formation during a ROS event in the southern taiga. We analyze snow properties, stratigraphy, and meteorological forcing before and after the event, and examine how local surface conditions, vegetation, and slope influenced the spatial distribution and variability of preferential flow. The results provide field evidence of small-scale snowpack heterogeneity after ROS and have implications for snow hydrology, ecological interpretation, modelling, and remote-sensing applications under changing winter conditions.

Despite this relevance, the formation, morphology, and spatial distribution of percolation columns remain poorly studied under natural ROS conditions, particularly in cold, predominantly fine-grained snowpacks. Overall, the objective of this study is to document the development and spatial organization of percolation features following a natural ROS event in the southern taiga and to examine how their expression varies among nearby sites with contrasting surface conditions, vegetation, and slope. To address this objective, we analyze snow properties, stratigraphy, and meteorological forcing before and after the event and compare the spatial distribution and variability of preferential flow among the study sites. The results provide field evidence of small-scale snowpack heterogeneity after ROS and have implications for snow hydrology, ecological interpretation, modelling, and remote-sensing applications under changing winter conditions.

82 to 85: I like this idea of explicitly describing very close expressions in the introduction, but I think that it would improve the readability if these expressions were defined in the running text after the first mention.

We would prefer to keep as is in the introduction.

Not that I want or ask you to cite my work, but I think that your manuscript is closely related to Bouchard et al. 2024 (<https://doi.org/10.5194/tc-18-2783-2024>), in particular, in section 4.1.2 where snowpack observations following ROS are thoroughly documented and supported with photos. Based upon the analysis and the hypotheses exposed by Bouchard et al. (2024), how does your study contribute to deepening the understanding of the impact of ROS in boreal forests? I think that this would reinforce the introduction and the first part of the Discussion. Other studies relevant to your work include Teich et al 2019 (<https://doi.org/10.1016/j.foreco.2019.01.052>) and Bouchard et al 2022 (<https://doi.org/10.1002/hyp.14681>). Perhaps you can have a look at these.

We added Bouchard et al. (2022, 2024) and Teich et al. (2019) to the Introduction and expanded Section 4.1 to compare our forest observations explicitly with previous boreal-forest studies. In particular, we now discuss the contrast between preferential sub-canopy flow reported by Bouchard et al. and the sparse features concentrated mainly in a canopy gap at our birch-forest site, while emphasizing differences in forest and event conditions. We also clarify that our study extends this perspective by comparing forest, meadow, field, and slope settings exposed to the same ROS event.

86: I recommend sorting this section (Materials and methods) into two subsections: 1) study site and 2) Experimental protocol. Moreover, this section should include a map of the site. Perhaps, you can move figure S1 to your study site subsection and complement it with an overview of the site location relative to the three weather stations and the town of Khotkovo with a relevant background map.

We divided the Methods section into two subsections and added a detailed map along with the site coordinates.

87: Please mention the coordinates of your site (l. 101) here.

We added the coordinates.

89 to 91: Please be more specific here. What is the mean winter temperature and precipitation here?

We added this:

Long-term mean air temperature from November to March ranges from about -4.8 to -6.4 °C across the region, with average precipitation of approximately 220 mm (Komarov, 2022)

91 to 93: How variable are snowpack properties from year to year? Some references are indicated but it would be necessary to mention the information explicitly here.

We added quantitative information on the interannual variability of snow depth and SWE in the region.

Regional maximum snow depth averages about 42 cm, with a standard deviation of approximately 14 cm, while maximum SWE averages about 100 mm with interannual variability of 40–45 mm (Komarov, 2022). Snow density at the time of maximum snow depth averages $225\text{--}250\text{ kg m}^{-3}$ but varies widely, from about 120 to 350 kg m^{-3} .

96: at what height are the met data measured?

Each parameter was measured at a height of 2 m above the ground.

103: Is the site surrounded by topographic features (e.g. hills, mountains) that would alter wind or radiation?

The surrounding terrain was generally flat, without major topographic features that would substantially alter local wind or radiation conditions.

105: what height are the surrounding trees?

This site was surrounded by 12–15 m tall trees, which protected it from wind and limited snow redistribution.

111: Please indicate explicitly the dates of snow pit measurements

At the main site, snow-pit measurements were conducted on 24 November and 2, 8, and 14 December 2024, followed by detailed measurements after the ROS event on 23 and 24 December.

115. Were snow density and temperatures measured as profiles (at what vertical precision?) or based on the identified snow layers? Please specify.

Snow temperature and density were measured in stratigraphic layers rather than at fixed vertical intervals. Temperature was measured at the center of each layer using a manual temperature probe, while density was measured at least three times per layer at lateral positions of 20, 50, and 80 cm using a standard 100 cm³ density cutter. Thus, the vertical resolution of these measurements corresponded to the thickness of the individual snow layers.

116 to 117. This information should appear later (l. 130) where you describe the related protocol.

Moved downwards.

125 to 128: this should be included in the running text.

Changed to running text.

130 to 135. The description of your protocol here is slightly elusive. I would expect more precision on the morphological characteristics that you aim to quantify. Is it the length of the percolation fingers, their diameter, the tortuosity? Also, as you mention, this approach includes important limitations (l. 134). The uncertainty on the estimation of the density, SWE and contribution to the mass balance should be quantified.

The draft already states that each percolation column was excavated, photographed, measured, separated into structural components (e.g. ice crusts and column body), and that the dimensions and mass of each

component were recorded. It also states that funnel diameter/area and surface coverage were quantified from the spatial mapping.

The updated version of this paragraph:

At the meadow and field sites, we conducted detailed analyses of individual percolation columns, including their morphology, density, SWE, and contribution to the snow mass balance. Each column was carefully excavated by removing the surrounding snow with a shovel and soft brush, photographed, and separated into its main structural components, including the surface funnel, column body, and associated ice crusts. For each component, vertical and horizontal dimensions were measured with a ruler, and volume was estimated assuming a circular cross-section. Components were then weighed, and their density and SWE were calculated from the measured mass and estimated volume. The ruler and balance had resolutions of 1 mm and 0.1 g, respectively. However, the total uncertainty could not be quantified because the irregular geometry of the structures required simplified assumptions for volume estimation; therefore, the reported density and SWE values should be considered approximate. Additional measurements were performed on a steep slope near the study area to examine linear drainage formation.

136: a reference or a link to the software should be added to your text.

To assess the spatial distribution of percolation features, we used Scaniverse (Niantic Spatial; <https://scaniverse.com/>), a smartphone application that reconstructs three-dimensional scenes using LiDAR and photogrammetry.

136 to 143: Please detail more thoroughly what Scaniverse does and how it is used to identify the percolation funnels. A reference would be helpful here. Does Scaniverse use a objective threshold to determine or is it set by the user? Also, be more specific about the analyses that you perform with the ‘Python libraries’. Maybe the link to Zenodo should be already available.

At each site, the snow surface within an approximately 3 m radius was scanned to produce a three-dimensional representation of the surveyed area. Percolation funnels were identified and manually delineated from their visible surface expression; no automated detection threshold was applied. The annotated features were then analyzed in Python to calculate their number, area, relative surface coverage, nearest-neighbour distances, kernel density estimates (KDE), and Ripley’s L-function relative to complete spatial randomness (CSR). At the slope site, elongated flow paths were analyzed separately based on their length and orientation. The analysis code and processed data are available at Appendix 2.

The code will be available via Zenodo upon acceptance. For now it is be available at GitHub.

152: As a scalar, wind speed is bounded to 0 m s⁻¹ by definition. Saying that it fluctuated between 0 and 5 m s⁻¹ sounds a bit clumsy. Maybe say that wind speed reached 5 m s⁻¹.

Changed to: air temperatures ranged from -18 °C to +2 °C, while wind speeds reached 5 m s⁻¹.

155 this sentence is not useful. Please remove.

Ok. We agree and removed it.

150 (Weather conditions): This whole section includes results that do not belong here as are described the effects of weather on snow stratigraphy (l. 155 to 162; l. 163 to 165; 167- 170). These results should be moved to the following section, and this section 3.1 should be thoroughly reorganized to include only results related to weather conditions: temperature, wind speed, precipitation and snow depth.

We reorganized Sections 3.1 and 3.2. Section 3.1 now contains only meteorological conditions, including air temperature, wind speed, precipitation, and snow depth. The observed effects of these conditions on snow stratigraphy and preferential-flow development were moved to Section 3.2.

171 (Figure 1): Where does snow depth measurement shown in Figure 1 come from? Was it measured at one of the weather stations or is the snow depth time series retrieved from your field measurement; I am not sure if this was mentioned. Also, can you explain why the initial snowfall caused the formation of a compact basal layer (l. 157) but Fig 1c shows that snow completely melts out on December 2?

We clarified that the snow-depth time series in Fig. 1 was measured at the Pushkino meteorological station and is shown only as regional meteorological context, whereas the snow stratigraphy discussed in the text was measured at the study site. The early basal snow layer persisted only at the meadow site, so its presence does not conflict with the temporary disappearance of snow at Pushkino on 2 December.

175 (Figure 2): I think that this figure should be removed as it repeats what is shown in Figure 1.

We agree that Fig. 2 presents the same meteorological variables as Fig. 1, but we prefer to retain it because it shows the ROS event at hourly rather than daily resolution. This finer temporal resolution is important for resolving the temperature, precipitation, and wind conditions during the event.

179 to 180: This was already mentioned in Methods. Please remove.

We removed this sentence from the Results.

178 (Section 3.2): This section essentially presents how the snow stratigraphy evolves until the ROS of 21 December. It gets a bit hard to follow since all 4 sites are presented at once. Maybe you should specify at which location were measured the lower and upper bounds of snow density ranges that you indicate.

We revised this section to make the temporal and spatial sampling clearer. In particular, we clarify that repeated detailed pre-ROS snow-pit measurements were available only at the meadow site, whereas detailed measurements at all four sites were conducted after the ROS event; where density

ranges among sites are reported, the lower values were generally measured at the meadow and the higher values at the field.

183 to 184: Is this what you show on Figure 1. Please specify.

We clarified the corresponding figure references in the text. Snow-depth evolution is shown in Fig. 1, while the detailed snow stratigraphy and snow properties measured after the ROS event are presented in Fig. 3.

198 (Figure 3): Here you only show snow profiles after the ROS despite describing mostly conditions before in the text. It would be insightful to have the profiles (grain type, density, SWE, temp) before and after. Moreover, the density profile should be presented as discretized values as each point is representative of one layer. Then I don't think it would be necessary to present the profile of SWE, but rather indicate the bulk SWE of the snowpack (before and after ROS).

We revised Fig. 3 to present density as discrete layer measurements and bulk SWE rather than as a continuous SWE profile. Unfortunately, directly comparable pre- and post-ROS profiles are not available for all four sites because detailed pre-event snow-property measurements were conducted only at the meadow site, with the last detailed profile measured on 14 December. We have clarified this limitation in the text.

203 to 204: Please review the syntax of this sentence

We corrected the syntax and rephrased the opening paragraph of Section 3.3 for clarity:

The development of percolation features during the ROS event differed markedly among the four sub-sites. At the three flat sites, percolation was expressed mainly as discrete funnels and vertical columns, whereas at the slope site it formed elongated linear pathways. These contrasting spatial patterns were associated with differences in local ground microtopography, vegetation, and slope, resulting in substantial variability both among the sites and at the microscale within individual sites (Figs. 4 and 5).

207 to 210: This belongs to methods. Please remove.

This part was removed as repetitive:

To quantify this variability at the three flat sites, we analyzed in situ data and photogrammetric images acquired with Scaniverse. We measured the diameter and area of percolation funnels, representing the surface expression of percolation columns, as well as the mean nearest-neighbor distance between them. The slope site was analyzed separately using the length and orientation of linear flow paths.

212 (Figure 4): Were the KDE maps normalized. I find it curious that it reaches 1.

Yes, the KDE maps were normalized from 0 to 1 for visualization, which explains the maximum value of 1. We clarified this in the Methods and the Fig. 4 caption.

Fig. 4 Spatial distribution of mapped percolation funnels and corresponding normalized kernel density estimation (KDE) maps for the floodplain meadow (a), watershed field (b), and birch forest (c) sites. Left panels show the detected funnels within the analysis circle (3 m radius), and right panels show the corresponding centroid density fields. KDE values were normalized from 0 to 1 for visualization, with 1 representing the maximum estimated density within each map.

226 to 227. The standard deviation of the nearest-neighbor distance would be needed here to support this statement.

We added the standard deviation of the nearest-neighbor distance and removed the interpretation based solely on the mean distance. The mean nearest-neighbor distance was 38 ± 12 cm (mean $\pm$ SD).

228 to 230: Can you elaborate on the Ripley's L-Function and the CSR? Maybe it is just me, but this approach does not seem to be mainstream. I think many readers (like me) may get lost here.

We added this explanation to the methods:

To assess whether percolation funnels were clustered, randomly distributed, or regularly spaced, we calculated Ripley's L-function. Complete spatial randomness (CSR) was used as the reference. Values above the CSR expectation indicate clustering, values below it indicates more regular spacing, and values close to it indicate an approximately random pattern. Unlike nearest-neighbor distance, Ripley's L-function considers spatial patterns over a range of distances.

232 to 234: I feel like this belongs more to the discussion than the results

We agree and moved this interpretation and the discussion of future LiDAR measurements to the Discussion section.

241: Very interesting!

It is!

255: Please elaborate on that and specify what you mean by 'directly influenced by tree canopies'

It isn't probably the best formulation so we adjusted this para as follows:

The birch forest showed the strongest localization and lowest abundance of percolation features. Despite having microtopographic variability comparable to the meadow, only a few columns developed, and these were mainly located in an open gap between two adjacent birch trees. In contrast, no percolation features were observed beneath the birch crowns within the canopy-covered part of the study area. This suggests that forest vegetation and canopy-related surface conditions were associated with the localization of water entry into the snowpack.

272: It would be interesting to show a photo with the analysis from Scaniverse of these elongated flow pathways similar that shown on the left side of Figure 4.

Thank you for the suggestion. We prefer to keep Fig. 4 focused on the three flat sites because the slope analysis did not include a KDE map, and adding the slope image there would make the figure less consistent. However, we agree that the image would be useful and will consider adding the Scaniverse view of the elongated flow pathways to Supplement 1.

275: What is the aspect of the slope?

We added the slope aspect to the site description. The slope was approximately 25° and faced south-west.

280: Study from Eiriksson et al 2013 (<https://doi.org/10.1002/hyp.9666>) is of interest here.

Thank you for this relevant reference. We added Eiriksson et al. (2013) to the Discussion when interpreting the elongated downslope flow pathways observed at the slope site.

*Eiriksson, D., Whitson, M., Luce, C. H., Marshall, H. P., Bradford, J., Benner, S. G., Black, T., Hetrick, H., and McNamara, J. P. (2013). An evaluation of the hydrologic relevance of lateral flow in snow at hillslope and catchment scales. *Hydrological Processes*, 27, 640–654. <https://doi.org/10.1002/hyp.9666>*

282 (Section 3.4). In addition to the schematic diagram (Figure 6), It would be relevant to add photographs mentioned earlier (l. 131) in this section.

Photographs of individual percolation columns are provided in Supplement 1, and we have added a clearer reference to these photographs in Section 3.4.

297: Figure 6

Panel c (forest site) should be removed as this was not supported by field evidence (at least not that is mentioned in the text).

We removed panel (c), representing the forest site

Indicate clearly on top of each frame which is the meadow and which is the field.

We added clear labels above the two remaining panels identifying the meadow and field sites.

If you have any photos of the snow pit wall showing the percolation columns (such as Fig. 5 from Bouchard et al. 2024), add it here.

Photographs of the observed percolation columns are provided in Supplement 1. We will also add a representative photograph of an exposed percolation column in Section 3.4 to facilitate comparison between the field observation and the schematic representation.

Can you explain why in Figure 6a, you have dry FC on the same layer as wet FC next to your percolation column? Please also correct FC crystals (the C already stands for crystals). Also specify what do you mean by wet FC, as FC is typically specific to dry snow.

We agree that the term “wet FC” could be misleading. What we intended to represent was a coarse-grained layer with pronounced pre-existing faceting in which the coherent percolation-column body became less distinct and separated into several smaller percolation fingers. Wetting was assessed visually during excavation, and no liquid-water-content or dielectric measurements were available. We therefore removed the term “wet FC,” corrected “FC crystals,” and revised both the text and Fig. 6 to describe this layer as faceted grains affected by localized percolation:

Both columns penetrated a sequence of distinct snow layers, including precipitation particles near the surface, a decomposed crust, rounded grains, and faceted-grain layers. Below the lower crust, the main percolation column became less distinct and transitioned into several smaller percolation fingers penetrating a coarse-grained layer with pronounced pre-existing faceting.

309: Can you explain how macropores were formed in the snowpack before it got wet?

We didn't imply that the macropores were present before wetting. We interpret the observed channel-like voids within the columns as likely resulting from concentrated liquid-water flow, although their formation was not directly observed. We revised the text to make this distinction clear.

Channel-like voids were observed in the upper and central parts of the percolation columns. We interpret these features as likely having formed during concentrated liquid-water flow through the snowpack rather than as pre-existing structures.

314: Can you confirm that you have observed more than only one column at each site, and perhaps rephrase? As I read this, it sounds that only one column at the meadow and fields sites was identified and this wouldn't be enough to draw a generalized conclusion.

Multiple percolation columns were observed and measured at both the meadow and field sites; Fig. 6 shows representative examples rather than the only columns identified. We revised the wording to make this clear and added photographs of additional observed columns to Supplement 1.

321: I don't understand here. Did you assess percolation columns also at the forest site or not? It is confusing.

Percolation columns were observed at the forest site, but their detailed volumetric and internal morphological characteristics were not measured in the same way as at the meadow and field sites. We removed the forest site from this detailed morphological comparison and clarified this distinction in the text.

328: have you observed merging of preferential flow pathways at transition layers like Bouchard et al 2024 did?

We observed lateral branching and changes in the geometry of preferential-flow pathways near stratigraphic transitions. However, we did not directly document whether several smaller pathways merged into a larger pathway, as illustrated in Bouchard et al. (2024).

344 (Figure 7): How I interpret this figure is that after some lateral distance water penetrates the snowpack vertically under homogeneous flow rather than preferential flow as is the case for the meadow and field site. Am I correct? If so, this should be somehow highlighted as it would be, in my opinion, an important finding of your work. Also, have you observed from a snow wall dug in the direction of the slope these ladder-like features of percolation or is this what you infer from your three snow pit observations? This should be specified.

The ladder-like structure shown in Fig. 7 was not observed in a continuous snow wall excavated along the slope. It represents a conceptual interpretation based on observations from three snow pits positioned along the flow path. These observations indicated lateral water movement along crusts or stratigraphic interfaces, with localized vertical penetration between them. However, our measurements do not allow us to determine whether this vertical component represented homogeneous matrix flow or preferential flow. We have revised the text and Fig. 7 caption.

346 to 362: This should appear before Figure 7 and the corresponding paragraph.

We moved the volumetric results (former lines 346–362).

366: If liquid water refreezes on contact with snow surface is either because it was freezing rain or because the snow surface was below zero. Looking to Figure 2, We see that the temperature rises above zero before precipitation is recorded within the identified ROS period. Can you elaborate on that and address the role of pre-ROS snowpack conditions on water transport (vertical and lateral). In other word, what one can expect from ROS in the specific locations that you have presented if we were later in the season or in spring, when the snowpack is at 0 C?

We agree that the initial freezing mechanism cannot be uniquely identified from our measurements. Although air temperature was above 0 °C at the onset of rainfall, the snowpack had experienced prolonged cold conditions and likely retained substantial cold content. Because snow-surface and precipitation temperatures were not measured, we cannot distinguish freezing caused by contact with subzero snow from possible supercooled precipitation. We revised Section 4.1 accordingly and added discussion of how a later-season, isothermal snowpack would have lower refreezing capacity and therefore likely allow greater liquid-water transmission and drainage.

386: I highly recommend you expand the discussion on limitations as these are numerous in your study. This starts with the fact that your conclusions rely on a single event at only one site. How can this be

transposed to other locations under different ROS conditions? Also, your assessment of percolation column characteristics does not rely on any proven protocol and no quantification of uncertainty is provided. This lacks robustness and should be corrected.

We substantially expanded Section 4.2 to address the limitations of the single-event, single-area study design, the absence of direct liquid-water measurements, uncertainty associated with reconstructing flow pathways after refreezing, and the non-standardized protocol used to quantify percolation-column properties. We also quantified the approximate instrumental contribution to the volumetric uncertainty and explicitly discuss the larger, unquantified uncertainty associated with irregular column geometry.

406 to 410: The low-cost attribute of the photogrammetry approach is emphasized as a strength of this work (also in the abstract) but in fact, the Scaniverse software used in this study is not supported by any reference how can we know that this method was proven and validated? This must be address for an eventual publication.

We agree that the original manuscript overstated the methodological significance of the smartphone approach. We now distinguish our field consistency check from formal validation, added literature on smartphone LiDAR/Scaniverse performance, expanded the discussion of scale and operator-related uncertainty, and clarified that the Scaniverse measurements are used primarily as comparative indicators of spatial organization rather than precise topographic measurements.

405 (4.3 Broader use and future potential: The only point that is discussed here is the use of low-cost phone-based photogrammetry. However, I don't think that this is the most important takeaway of your work. In fact, I would expect to discuss how your findings, i.e. the differences observed between locations, can be used in snow, hydrological and land surface models. How can this be integrated into a multi-layer snow models and how to upscale those sub-grid processes at larger modeling scale? I invite the authors to refer to the work of Mazzotti et al (2024) for some ideas (<https://doi.org/10.5194/tc-18-4607-2024>)

We substantially reorganized Section 4.3 so that the broader implications for snow, hydrological, and land-surface modelling are now the primary focus. We discuss how the observed sub-meter heterogeneity could inform preferential-flow and sub-grid representations in multilayer models and added Mazzotti et al. (2024) as an example of spatially explicit modelling used to investigate snowpack heterogeneity and unresolved sub-grid variability. Smartphone-based mapping is now presented as a secondary methodological contribution rather than the principal broader implication of the study.

451 (Bibliography). Many items in the bibliography are not cited in the text (I have counted 3). Please make sure that the list of references matches the entries the manuscript.

Except for Fig. S1, none of the figures in the supplement are indicated in the manuscript.

Thank you. We revised the bibliography to ensure that all references are cited in the manuscript and that all in-text citations are included in the reference list. We also added citations to Supplement 1 at the relevant locations in the manuscript.