Authors' response to the reviews of

Seasonal evolution of suncup roughness describes broadband albedo decay on alpine snow

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RC: Reviewers' Comment, **AR:** *Authors' Response*, *Manuscript Text*

We thank our Editor, Francesco Avanzi, for promptly handling the review process of our manuscript. We have addressed all comments raised by the reviewers and detailed our responses point by point below.

1. Review #1

We thank Steven Fassnacht for the many positive and constructive comments on our manuscript. You correctly pointed out important methodological aspects that needed better clarification, while providing us with some interesting insights to strengthen our Methods and Discussion. This rigorous review will greatly improve the quality of our manuscript. Below, we address each comment, with the reviewer's comments in **bold black text** and our responses in *italicized blue text*.

1.1. Major comments

RC: How is z0 computed from Lettau (1969)? This is not trivial, yet it is not explained in the Methods section.

AR: *Thank you for this remark. We shouldn't have overlooked this important explanation.*

We estimated the aerodynamic roughness length z_0 from our Gaussian-detrended LiDAR digital surface models (DSMs) using the geometric method of Lettau (1969) adapted for raster grids, drawing conceptual inspiration from Neville et al. (2025). As specified in the manuscript, the interpolated DSMs have a native resolution of 5 mm, which we smoothed to 1 cm for computational ease and to remove small-scale roughness. The conceptual inspiration we drew from Neville et al. (2025) is to identify roughness elements as individual obstacles, which the authors resolve with an original watershed segmentation approach. We chose a simplified identification approach to reduce computational cost, given our need to process almost 10'000 DSMs. We used the 75th percentile of the elevation distribution to classify cells as obstacles, and merged neighboring obstacle cells into single roughness elements. A percentile threshold, rather than a fixed height, keeps the definition consistent while the surface amplitude changes throughout the season. This kind of threshold allows the identification of individual suncups as obstacles and avoids aggressively merging several suncups together. We also set a minimum threshold of 20 neighboring cells (i.e., 20 cm^2) for an obstacle element to be identified. Physically, the resulting obstacles correspond to the suncup walls, which produce most of the drag. The hollows between them are assumed to be mostly sheltered from the wind and therefore do not enter the calculation.

For each obstacle i, we computed the maximum height h_i above the mean detrended surface, the horizontal footprint area S_i, and the vertical area $s_{i,wd}$ facing each cardinal wind direction $wd \in (N, E, S, W)$, where a cell counts as wind-facing if it is higher than its upwind neighbor. For each wind direction, the roughness length is:

$$z_{0,wd} = C_d \cdot \overline{h} \cdot \frac{\sum_i s_{i,wd}}{\sum_i S_i}, \text{ with } \overline{h} = \frac{1}{N} \sum_i h_i \tag{1}$$

Where $C_d = 0.5$ is Lettau's drag coefficient and N is the total number of obstacles. Unlike Neville et al. (2025), we summed the areas first, which gives more relative weight to larger elements. Each scan thus yields four directional z_0 values, which we average to obtain the time series shown in Fig. 1(a–c).

We will add this explanation as a dedicated Methods subsection in the reviewed manuscript, and cite Neville et al. (2025) as the underlying conceptual framework. Following their approach, we will also complement the mean z_0 value with wind-rose histograms and comment on the directional dependence of this formulation.

RC: I not this below (lines 433 and 483) about "scavenging of impurities." This could be true but has not directly been seen. Instead, impurities, specifically dust, tend to accumulate at the surface. Within a suncup, they can be redistributed to the bottom and concentrated at the ridges. Consider how you write this.

AR: *Thanks for commenting on this. Our literature review shows that, although few in number, there are studies which have demonstrated the existence of this phenomenon. For example:*

- *Conway et al. (1996) loaded the surface of a melting snowpack with black carbon (BC) and noted that almost all hydrophilic and half of the hydrophobic BC was removed from the top 50 cm within 10 days. On the other hand, they also observed the immobility of hydrophilic, but larger (5 μm and more) particles.*
- *Based on a similar experimental design, Flanner et al. (2007) derived (low) scavenging coefficients for hydrophilic and hydrophobic BC. The authors indeed conclude that meltwater scavenging is a rather inefficient process. This paper is not cited in our manuscript.*
- *Doherty et al. (2013) Estimates scavenging efficiencies of 10-30% due to meltwater percolation in mixtures of BC and other light-absorbing particles (LAPs), depending on the particle size and hydrophobicity rather than composition, in line with Conway et al. (1996).*
- *Sterle et al. (2013) also documented a rapid release of meltwater that disproportionately removed BC and dust from the surface. This paper is not cited in our manuscript.*
- *Tuzet et al. (2017) summarized these previous findings and implemented a scavenging coefficient in the model CROCUS, which can be adjusted for each impurity type, that simulates LAPs scavenging to the underlying layer as a function of the mixing ratio with a conceptual bucket approach. Following Conway et al. (1996); Doherty et al. (2013), and especially Yang et al. (2015), the scavenging efficiency for dust is set to 0, as you rightfully remark, as these particles are too big to be scavenged.*

We will revise all sentences in our manuscript that present this phenomenon as more prevalent than it actually is.

RC: While it is impossible to disentangle impurities versus roughness, it should at least be discussed that some amount of impurities may not allow suncups to form (Fassnacht et al., 2009; cited in the paper) or may reduce roughness (Fassnacht et al., 2010; cited above). This could be briefly discussed in the paper.

AR: *Thank you for commenting on this. This aspect was indeed part of the Discussion in a previous version of the manuscript. In particular, we (partly) attributed the reduced development of suncups during the 2024 season to increased surface impurities, possibly associated with the algal bloom. However, after a round of internal revision, we decided to remove it because the consensus among co-authors was that our results cannot directly demonstrate that. Nevertheless, this process can be discussed only as existing, without forcing any conclusion about its occurrence if our results cannot support it. We will add an explanatory paragraph to the Introduction and/or the Discussion.*

1.2. Specific comments

RC: Lines 34-35: "since pyranometer-based measurements inherently integrate the effects of surface roughness within their radiative footprint" – also, consider which (intrinsic vs. apparent) impacts the energy balance

AR: *Thanks for this remark. Pyranometer-measured albedo is indeed apparent albedo, which also governs the energy balance by determining the actual absorbed shortwave radiation flux. All energy balance computations in SNOWPACK rely on apparent albedo. We will make this point clearer.*

RC: Lines 39, 107, 333, and elsewhere: it should be "elevation" and not "altitude." I know that people interchange these, but "altitude" is above the ground and here you are speaking to the "elevation" of the snowpack.

AR: *Thank you for pointing this out. We will correct the terminology throughout the manuscript.*

RC: Line 74: This is not crucial, but I would assume that nutrients in any dust, beside "Saharan" dust, may also drive algal blooms.

AR: *We agree. Saharan dust is the dominant deposition source at our site, but it is obviously not the only nutrient-carrying aerosol, and our phrasing should not imply this. We will generalize the sentence.*

RC: Line 90 (mid-line): the "s" is missing from "surface"

AR: *Thank you. We will fix this typo.*

RC: Line 133: I recommend using the word "slope" rather than "altitude," as it is the removal of the bias of the overall surface slope (in X and Y) on the surface roughness indices (Fassnacht et al., 2009; https://doi.org/10.1029/2008WR006986)

AR: *Thank you for these useful suggestions. We will correct the terminology throughout the manuscript.*

RC: Line 138: "the raster-based version of Lettau (1969)" can also be computed by Neville et al. (2025; https://doi.org/10.3390/rs17121984). Mostly as FYI here. Your method of applying Lettau (1969) is not explained.

AR: *Indeed. Please see our reply to the Major comment #1.*

RC: Line 144-146: Sastrugi fields are much direction than sun cups. Mostly as FYI here.

AR: *Thank you for commenting on this. We will include this in the sentence.*

RC: Table 1: should appear after it is cited on line 174

AR: *Noted. We will edit the manuscript accordingly.*

RC: Table 1: is there a difference between Black Carbon versus Dust modeled in SNOWPACK, i.e., should there be scenarios where one is 0 and the other is > 0?

AR: *Unlike CROCUS, SNOWPACK in its current version doesn't allow the simulation of LAPs surface enrichment and/or scavenging. Our scenarios are only applied to TARTES simulations, which are initialized with SNOWPACK-simulated surface properties with added impurities. Importantly, this configuration does not allow impurities surface concentration to change over time. This is an important limitation that we acknowledged in the manuscript (L426–429): (...) The systematic co-existence of enhanced LAPs concentration and suncup growth has been addressed by our study, but remains a clear limitation of our modeling approach. The presence and evolution of impurities is not accounted for in SNOWPACK, which therefore neglects the enhanced SSA decrease derived from LAP-accelerated metamorphism (Skiles and Painter, 2019; Tuzet et al., 2017) (...).*

However, you raised an important point here. In Tuzet et al. (2020), cited in our manuscript, it is very clear that the concentration of BC shows quasi-continuous behavior, in contrast to episodic dust. Réveillet et al. (2022) showed that dust dominates the radiative forcing in dust-event years, while BC dominates in others. We overlooked this paper in our literature review. On the other hand, Gabbi et al. (2015), cited in our manuscript, shows how both species are always present in the Swiss Central Alps, although at strongly varying ratios.

Our "low" and "medium" scenarios are already covering BC variability quite well (L429–434): (...) Since we had no measurements of impurity content at our site, the scenarios we defined rely on literature values available for LAPs concentrations on seasonal Alpine snow. Specifically, our medium black carbon concentration matches the highest value measured in the two-seasons campaign of Tuzet et al. (2020), and is consistent with previous assessments in the Alps and beyond (Kau et al., 2026; Gabbi et al., 2015; Doherty et al., 2013; Painter et al., 2012). These campaigns included the melting season, when impurities that are not scavenged through percolating meltwater accumulate at the surface through melt amplification. (...)

Therefore, we probably need to better design our "high" scenario to represent a strong dust deposition event with an average concentration of BC. We will change this in the reviewed version of the manuscript.

RC: Line 161-162: How is "[s]uch information is combined with the nearest acquisition of surface roughness in time?"

AR: *Thank you for this remark. This sentence is poorly written and likely misplaced. We intended to explain that our dataset combines LiDAR-derived surface roughness acquisitions with SNOWPACK-simulated surface properties. For computational efficiency, the simulated properties are only written every 3 hours (00, 03, ..., 21 UTC). Whenever the two are used together (e.g., in the Löwe and Helbig (2012) parametrization), each roughness acquisition is paired with the nearest 3-hourly SNOWPACK output in time. While explaining this, we acknowledge that this sentence does not belong to the SNOWPACK section (2.1), but rather to the effective albedo parametrization (2.4) section. We will improve the sentence's clarity and move it to the relevant section.*

RC: Line 214: "Multiple reflections are not accounted for under either condition in the parametrization

of Löwe and Helbig (2012)." Understandable that this is not included due to the computations burden. But are the implications of not doing this discussed? This could/likely further reduces albedo.

AR: *Thank you for this comment. We think we did indeed stress the importance of this aspect throughout the manuscript:*

- *Introduction (L57–62): (...) The multiple reflections effect activates because, on a rough surface, the probability of photons being trapped inside concavities rather than being scattered away is higher than it is for a flat surface. After each collision with the roughness walls, the probability of light being absorbed rather than reflected increases. This causes a systematic increase in absorption, especially in conditions where reflection and absorption are balanced, i.e., for values of albedo close to 0.5 in the near-infrared (NIR, 700-1100 nm) (Bair et al., 2022; Larue et al., 2020). This effect is active under both direct and diffuse illumination conditions. (...)*
- *Discussion (L421–425): (...) Under diffuse illumination, however, the shading factor vanishes by definition and the only remaining roughness correction is the redistribution of irradiance between sky and terrain fractions through the sky-view factor. This single exchange does not capture subsequent multiple reflections, causing the parametrization to converge toward flat-surface TARTES values and leaving the multiple reflections effect under diffuse conditions unaccounted for. This is particularly relevant in the presence of LAPs, where each subsequent reflection carries an increased probability of absorption. (...)*

RC: Line 230: what do you mean by "flattens to a near-zero trend?" In particular, the word "trend" seems unclear.

AR: *Thank you for this comment; the sentence is poorly written. What we wanted to explain is that one of the suncup roughness signals is the surface becoming highly isotropic ($\sigma_{\Delta CL} \sim 0$ as a result of $CL_x \simeq CL_y$). We will improve the clarity of this sentence.*

RC: Figure 1: I really like this figure. It is very informative. Would it be worthwhile putting (a) to (f) on a log scale to highlight the difference between vertical and horizontal being dominant?

AR: *Thank you for this input. The log-scale in Figure 1(a–f) conveys the results much better. We will update the figure accordingly.*

RC: Figure 2: Is it worth it to denote when in the time series (g) to (u) the photos and surface geometry from (a) to (f) where taken? A vertical line provide context.

AR: *Thank you for this input. We will update the figure accordingly.*

RC: Line 252: why a 5x5 m sub-domain and not the 10 x 4 m lidar domain?

AR: *Thank you for the remark. We indeed didn't justify this choice. The reason is simply that during snow ablation, the snow starts disappearing a few days earlier in the easterly region of the domain. In that region, at the very end of the season, the computed roughness would be therefore the roughness of the terrain, not of the suncups. We therefore defined a smaller domain to study the suncup evolution over a slightly longer time frame.*

RC: Lines 277-280: these are Methods, not Results

AR: *You are right. We will move this sentence to the Methods section.*

RC: **Lines 337-339, especially the part of the sentence "it provides a simple diagnostic . . . " is very insightful. No action necessary.**

AR: *Thank you very much for the positive feedback!*

RC: **Lines 433, 483 (and I think elsewhere?): "impurities that are not scavenged through percolating meltwater." This really depends on the size, color (albedo) and composition of the impurities. Larger impurities "melt" into the snowpack (Fassnacht et al., 2010; https://doi.org/10.1002/hyp.7661) but dust generally tends to stay close to the surface and "accumulates" as the snow melts down to subsequent dust layers. This is not crucial, but the processes is not the same as ionic elution (e.g., Bales et al., 1989; https://doi.org/10.1029/WR025i008p01869) where the ions move through the snowpack. In line 483, redistribution within the suncup is understood, but scavenging may not be correct. Reconsider how this is written.**

AR: *Please see our reply to the Major comment #2.*

RC: **Lines 454-455: "tracking both suncup geometry and LAPs distribution at the same fine spatial resolution" –does your TLS not collect RGB (or red reflectivity if the TLS is 532 nm) data at the same time and resolution at the geometry? While I don't expect the analysis of said data (to overcome this fundamental obstacle), it should at least be mentioned here that some of these data are available. Or not?**

AR: *This is a very good point, which we did not previously consider. Indeed, reflectivity data exist, and they were used to study the spatial and temporal evolution of surface hoar formation in a wind tunnel under laboratory conditions. The results are published within the proceedings of the 2023 International Snow Science Workshop (Walter et al., 2024). We will mention this in the reviewed manuscript.*

RC: **Line 463: change the word "lower" to "smaller" as the former may indicate height**

AR: *Again, many thanks for these very useful remarks on terminology. We will change this there and elsewhere.*

RC: **Lines 489-490: "these results highlight the potential value of assimilating surface roughness observations into snow energy balance models" – z0 varies but is considered a constant in most (all?) models. z0 was varied in SNOWPACK (Sanow et al., 2024; https://doi.org/10.1016/j.polar.2024.101110) and this yielded quite different simulated turbulent fluxes.**

AR: *Thanks for this comment. We are aware of this paper, as we are currently working on a similar project. We will add the citation and briefly comment on the relevance of assimilating time-dependent aerodynamic roughness length into energy-balance models.*

RC: **Lines 491-501: This last paragraph seems to be a repeat of previous statements. Consider making this its own subsection, as I think it is an important summary. Maybe it is the last paragraph of the Conclusions?**

AR: *This is an editorial mistake on our side. The last paragraph of the Discussion is indeed repeated twice. We will fix this.*

2. Review #2

We thank Anonymous Referee #2 for the many positive and encouraging comments on our manuscript. Your rigorous review will help us improve many definitions throughout our manuscript, as well as explanations and interpretations of our results. You also provided insightful comments that helped us deepen our understanding of the interplaying processes, which will improve the manuscript's quality and depth. Below, we address each comment, with the reviewer's comments in **bold black text** and our responses in *italicized blue text*.

2.1. Minor comments

RC: Line 47: after Betterton (2001), replace ";" with "and".

AR: *Thanks, we will.*

RC: Line 90: correct "urface roughness" with "Surface roughness".

AR: *Thanks, we will fix this typo.*

RC: Line 148: The "mean planar extension" is later referred to as "mean equivalent diameter". I recommend to provide a consistent definition and also explain how it is calculated.

AR: *Thank you for pointing this out. We switched between the two indices during the internal review and forgot to update. We will be consistent with the mean equivalent diameter and provide its definition (i.e., the equivalent diameter of a circle with the same surface area as each suncup).*

RC: Line 223: could you please explain how you calculated the horizontal roughness index CL from the correlation lenghs CL?

AR: *Thank you for this comment. In the caption of Fig. 1, we define: (...) (d–f) Horizontal roughness index, expressed as the standard deviation of the difference between CL in both directions $\sigma_{\triangle CL}$ (...)*

Essentially, it flags suncup growth by indicating the surface becoming isotropic ($\sigma_{\triangle CL} \sim 0$ as a result of $CL_x \simeq CL_y$). However, the definition should appear and be clearly explained in the manuscript's body, not in a figure caption. We will fix this.

RC: Caption of Figure 1: Please specify the temporal interval of all the time series (it is specified only for wind speed).

AR: *Thanks for pointing this out, we will.*

RC: Line 260-261: "depth and size reached peak values of approximately 30 mm and 10 cm". Please use the same unit for both numbers (cm, as in Fig. 2). 3 cm is not a peak in suncup depth: Fig 2m shows peaks around 11 cm. Do you mean here that when the equivalent suncup diameter was largest (after snowfall) the mean suncup depth decreased to about 3 cm? Please clarify.

AR: *Thanks for commenting on this. This is likely an editorial mistake on our side. We will correct this.*

RC: Line 261-273: "and depth growth rate was also considerably faster" compared to the other years? please specify. From here until the end of the paragraph, I recommend providing quantitative estimations of the differences between the years, instead of qualitative statements that are difficult to extract from the sublots of Fig 2. Also, please clarify what is (the standard

deviation of wind direction?).

AR: *Thank you for the useful suggestion. Yes, with σ here we refer to the standard deviation of the wind direction. We will state the definition clearly and corroborate the paragraph with quantitative statements.*

RC: Caption of Figure 2: please specify that plots g-o and s-u show hourly time series smoothed with a 3h moving average, and explain what the time interval of the data in v, w, x is.

AR: *Thank you once again for commenting on this. The resolution is hourly unless otherwise specified. We will write this explicitly, here and elsewhere.*

RC: Caption of Figure 3: to simplify, after (d-f) I recommend replacing "Measured and TARTES flat-surface broadband albedo for pristine and impurity-loaded snow under" with "and".

AR: *Thank you for this useful remark. We will adopt this shorter and clearer formulation.*

RC: Line 283: "As the snow wets and SSA decreases, prior to suncup development, the overestimation of albedo in TARTES increases". The verb "increases" does not seem appropriate. TARTES underestimated albedo in dry conditions, whereas overestimated it in wet conditions. Hence, rather than an increase in bias from dry to wet conditions, there was a change of sign in the bias.

AR: *Thank you. This is correct, and there was a mistake on our side. We will correct this sentence here and elsewhere.*

RC: Line 285-290: "We interpret this as the signal of wetting and metamorphism (wet,smooth)" What "this" refers to? I guess it is the bias between measured and modelled albedo, and not the RMSE, right? Please clarify. Also, I recommend improving the explanation of what wet,smooth represents. Rather than "the signal of wetting and metamorphism", I would explain that it a bias due to TARTES misrepresentation of wet snow metamorphism over a flat surface. This will help in understanding your use of wet,smooth in the calculation of the suncup signature.

AR: *Thank you for bringing this up. Correct, $\beta_{wet,smooth}$ is the bias between modeled and measured broadband albedo. Your suggested explanation provides more clarity. We will reformulate this concept here and elsewhere.*

RC: Line 293: "This suggests that roughness and impurities jointly contribute to the observed albedo deficit that TARTES cannot reproduce from optical properties alone". Instead of "from optical properties alone" I recommend using something like "when assuming a flat surface" which I guess is what you mean (optical properties could be parameterized including the effect of roughness, and in that case the observed albedo deficit could disappear just by applying those optical properties).

AR: *Thank you for specifying this; you are correct. We will reformulate this sentence.*

RC: Line 395-397: "It is only for extremely high impurity contents or algal bloom that the suncup signal partially decouples from the impurity signal. In such cases, the suncup signal either only slightly persists or disappears (Fig. 3m)." I don't understand what you mean: in my view, the fact that the high impurity content cancels the suncup signature suggests that the combined effect of suncups and impurity content on albedo is comparable to that of the simulated (and possibly too high?) impurity content over a perfectly flat surface. I don't see how the suncup signal decouples from the impurity signal. Could you please clarify?

AR: *Thank you for pointing this out. This is indeed what we intended to convey, but we agree that our original wording was poor. Our point was that, at very high impurity concentrations, the albedo reduction caused by impurities becomes so dominant that the additional contribution of suncups is no longer distinguishable in the albedo signal. In other words, the combined effect of suncups and impurities becomes comparable to that of a sufficiently high impurity concentration over a flat surface, such that the characteristic suncup signature is effectively masked, as you rightfully say. We will revise the text.*

RC: Line 304: "High impurity and algal bloom scenarios are, on the other hand, likely on the extreme side, and the fact that residuals fall below zero in such cases seems to support the hypothesis" What do you mean by "on the extreme side"? "Unrealistic"? Please clarify. You are interpreting your results more than making a hypothesis, so I would rephrase the last sentence with something like "as suggested by the fact that residuals fall below zero in such cases".

AR: *Yes, what we meant here is "unrealistic". We will rephrase the sentence according to your suggestion. Thank you.*

RC: Caption of Figure 4: Please specify date and time of the photos in subplots d and e.

AR: *Thank you for the suggestion; we will add the date and time to all photos in this Figure.*

RC: Line 315: "At early stages, visible impurities are distributed relatively homogeneously across the snow surface." I partly disagree. Looking at your Fig 4b, it seems to me that the impurities are concentrated on the upwind side of parallel waves. This does not contradict your conclusion that impurities are more concentrated into the hollows at the final stage of the suncup formation and therefore have less effect on albedo than at the early stage. In fact, it strengthens your conclusions. I would spend few words describing the wavy pattern of the early snow roughness, with parallel ripples aligned perpendicular to the wind (very typical of the interaction of a fluid with the underlying granular material). I think that this is very important, because it explains how impurity and roughness are coupled from the very beginning, and it also makes it easier to understand the further evolution of the roughness into suncups.

AR: *This is a very interesting interpretation which we didn't consider in our analysis. To verify it, we carefully inspected our archive of webcam images under diffuse conditions, to avoid optical artifacts caused by illumination. At the same time, we inspected the predominant wind direction prior to each webcam acquisition. Two relevant acquisitions are compared in Fig. 1.*

Fig. 1a shows the webcam acquisition of 2023-06-03 at 20:00:00, with $z_0 = 8.7$ *mm. In the prior week, winds blew predominantly from the NW, with a speed-weighted average direction of 305°, prevailing for 77% of the total hours. Dark, rock-like debris is visible on the upwind section of early-stage suncup roughness, as you rightfully point out. Interestingly, the NW of the research field is surrounded by steep rock faces, which become often patchy and snow-free well in advance with respect to the research field itself, where snow generally persists longer because of a slight local terrain depression. The surface darkening may therefore be partly attributable to debris mobilized from these surrounding slopes.*

Fig. 1b shows the webcam acquisition of 2024-06-17 at 20:00:00, with $z_0 = 8.3$ *mm. In the prior week, winds blew predominantly from the SE, with a speed-weighted average direction of 147°, prevailing for 70% of the total hours. Although the suncup roughness is almost identical to that of the previous year, and therefore likely provides a comparable debris interception efficiency, the upwind dark debris is far less apparent here – largely absent, or confined to the main early-formed melt channels, as it was the year before. Unlike the NW sector, the SE side of the research field opens onto the valley and is not overlooked by steep rock faces.*

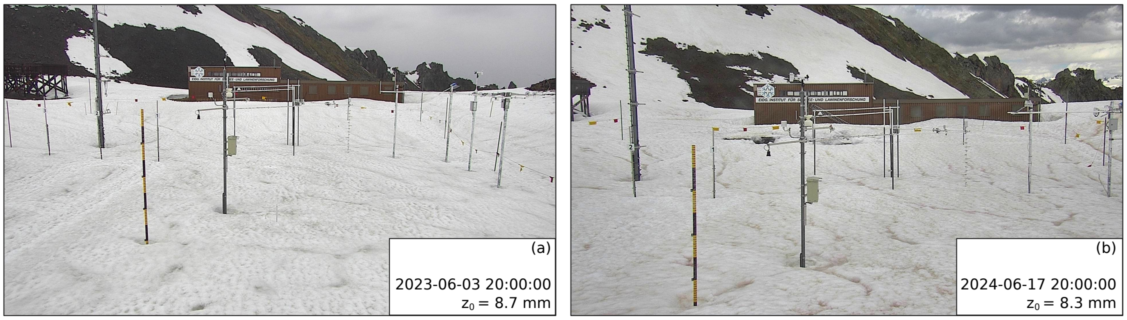

Figure 1: *(a) Webcam image from 2023-06-03, 20:00:00, under overcast (diffuse) illumination, with $z_0 = 8.7$ mm. Winds over the preceding week were predominantly from the NW (speed-weighted mean direction 305°, prevailing for 77% of the total hours). (b) Webcam image from 2024-06-17, 20:00:00, under overcast (diffuse) illumination, with $z_0 = 8.3$ mm. Winds over the preceding week were predominantly from the SE (speed-weighted mean direction 147°, prevailing for 70% of the total hours).*

We believe this could be a sufficiently convincing evidence of the process you have hypothesized. As you rightfully point out, it does indeed strengthen the conclusions of our manuscript. We will comment on this in our Discussion. Many thanks for providing this insight.

RC: Line 472: I suppose that Manninen et al (2012) should be corrected with Manninen et al (2021).

AR: *Indeed, thanks for the remark.*

RC: Line 491-501: this whole paragraph seems to be a repetition of the previous one. I suggest removing it.

AR: *This is an editorial mistake on our side. The last paragraph of the Discussion is indeed repeated twice. We will fix this.*

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