

Authors' response to the reviews of

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

Francesca Carletti, Nora Helbig, Nander Wever, Loïc Brouet, Mathias Bavay, Benjamin Walter, and Michael Lehning

The Cryosphere, <https://doi.org/10.5194/egusphere-2026-3049>

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 has greatly improved the quality of our manuscript. We addressed each comment below, with the reviewer's comments in **bold black text** and our responses in *italicized blue text*.

1.1. Major comments

RC: How is z_0 computed from Lettau (1969)? This is not trivial, yet it is not explained in the Methods section.

AR: *Section 2.2 (Data and Methods – L140-177) of the revised version of the manuscript now provides a detailed explanation of how all the used roughness descriptors are computed and which assumptions are made.*

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: *In the revised manuscript, we have toned down this claim, specifying that the effectiveness of the scavenging mechanism is low, e.g. (Introduction – L66-68): Specifically, as the snow melts, black carbon and dust are retained at the snow surface because the scavenging effect of percolating meltwater is generally inefficient (Doherty et al., 2013; Sterle et al., 2013; Flanner et al., 2007; Conway et al., 1996), especially for particles of greater size like aeolian dust (Tuzet et al., 2017; Yang et al., 2015).*

Throughout the manuscript, we corrected our use of the word “scavenging” whenever “redistributing” was intended.

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: *In the revised manuscript (Discussion – L466-468), we report such results as part of our discussion: Interestingly, under turbulent conditions, Fassnacht et al. (2009) observed entirely smoothed-out suncups over "red" snow, and reduced suncup amplitude following the deposition of coarse aeolian debris (Fassnacht et al., 2010), suggesting that not only concentration, but also distribution and grain size of impurities may influence how suncup roughness evolves on snow.*

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: *In the revised manuscript, the sentence now reads (Introduction – L30-33): In the context of surface energy balance, the effective albedo is the physically meaningful quantity, because it determines the net shortwave radiation absorbed by the rough snow surface. Pyranometer-based measurements capture the effective albedo, as they inherently integrate roughness effects into their radiative footprint (Bair et al., 2016, 2015).*

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: *In the revised manuscript, all incorrect terminology remarked by the Reviewers has been corrected.*

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: *In the revised manuscript, the sentence now reads (Introduction – L74-75): Nutrients supplied by aeolian dust deposition may also drive algal blooms*

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

AR: *In the revised manuscript, all typos have been corrected.*

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: *In the revised manuscript, all incorrect terminology remarked by the Reviewers has been corrected.*

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: *Please refer to our reply to Major comment #1.*

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

AR: *In the revised manuscript, we dropped the reference to sastrugi, because they do not form at our site.*

The sentence now reads (Data and Methods – L170-171): A ripple field exhibits large horizontal and moderate-to-low vertical descriptors: its reliefs are spatially extended, widely spaced, and strongly directional, giving large α_{CL} .

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

AR: Done.

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: *In the revised manuscript, we split our scenarios into two families: mixed scenarios (where black carbon and dust co-exist at varying, comparable levels), and dominated scenarios (in which either one or the other species prevails). Specifically, (Data and Methods – Tab.1, L203-208): As impurity content measurements were unavailable at our site, we designed five scenarios (Tab. 1) building upon the existing monitoring campaigns in the Alps, assuming that LAPs absorption is only attributable to black carbon and dust (Tuzet et al., 2020), which systematically co-exist in the Central Swiss Alps (Gabbi et al., 2015), although at strongly varying ratios. Specifically, it was shown that dust dominates the radiative forcing in dust-event years, while black carbon dominates otherwise (Réveillet et al., 2022). Therefore, we divided our scenarios into two families: mixed scenarios, in which black carbon and dust co-exist at comparable levels, and dominated scenarios, in which one species clearly outweighs the other.*

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

AR: *This was an editorial mistake on our side, which we removed.*

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: *In the revised manuscript, the sentence reads (Data and Methods – L280-283): Neglecting multiple reflections omits the photon-trapping contribution to albedo reduction. Bair et al. (2022) quantified this effect in the field at roughly 5% of the total roughness-driven albedo decay on average, but found it exceeds the 20% for late-ablation suncups: a regime our observations fully cover. Its omission, therefore, likely underestimates the modeled albedo reduction that increases with suncup growth.*

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

AR: *In the revised manuscript, the sentence reads (Results – L297-298): Conversely, the horizontal roughness signal becomes highly isotropic, with α_{CL} shrinking towards 1.*

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: *In the revised manuscript, the y-axis of Fig.2a–f is on a log scale.*

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: *In the revised manuscript, Fig.3g–u were updated with a dot and a vertical line indicating the time of*

acquisition of the webcam images (a–c) and the LiDAR scans (d–e).

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

AR: *In the reviewed version of the manuscript, this aspect is clarified (Data and Methods – L128-130): During ablation, snow generally disappears earlier in the south-eastern part of the domain; therefore, to track suncup evolution over a longer period, we defined a smaller 5 × 5 m sub-domain within the north-western area of the ROI.*

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

AR: *In the revised manuscript, this has been moved 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: *We thank the Reviewer 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 refer to our reply to 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: *In the revised manuscript, we addressed this point directly (Discussion – L520-531): The progressive surface LAP enrichment monitored by Tuzet et al. (2017) reflects the suncup growth we observed, but, in contrast, it shows no consistent interannual trend and varies significantly across measurement techniques (Dumont et al., 2017). Disentanglement would therefore not depend solely on an increase in LAP measurement frequency. Geometry brings further complications: because suncup growth drives differential LAP accumulation between hollows and ridges (Rhodes et al., 1987), partitioning their contributions to albedo decay would require co-registered tracking of surface topography and LAP distribution at the same fine spatial resolution. LiDAR scanners like ours can resolve the spatiotemporal evolution of surface reflectance and have been deployed for this purpose (Walter et al., 2024), but they record backscattered intensity at a single wavelength in a fixed directional geometry and are rarely radiometrically calibrated. They yield, at best, a relative single-wavelength directional reflectance rather than the hemispherically and spectrally integrated quantity that albedo represents. Resolving the suncup–LAP problem would thus require instrumentation capable of tracking LAP evolution not only in space, time, and vertical distribution within the surface snow, but also across wavelengths. Even then, spectral resolution alone may not fully separate the two sources; until then, this remains a fundamental obstacle for future work.*

RC: Line 463: change the word “lower” to “smaller” as the former may indicate height

AR: *In the revised manuscript, all incorrect terminology remarked by the Reviewers has been corrected.*

RC: Lines 489-490: “these results highlight the potential value of assimilating surface roughness observations into snow energy balance models” – z_0 varies but is considered a constant in most (all?) models. z_0 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: *In the revised manuscript, we made this point clearer (**Discussion – L552-556**): Second, suncup roughness is not only an optical property governing albedo but also an aerodynamic one, with direct implications for the ablation-season energy balance. Sanow et al. (2024) showed that varying the aerodynamic roughness length in SNOWPACK substantially changes cumulative sublimation, sensible heat, and snow water equivalent. Although dynamic z_0 has been implemented in SNOWPACK to study the seasonal effect of wind (Amory et al., 2017), during ablation it is almost universally kept constant, site-specific at best.*

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 was an editorial mistake on our side, which we removed.*

2. Review #2

We thank Anonymous Referee #2 for the many positive and encouraging comments on our manuscript. Your rigorous review helped us improve and clarify many definitions in 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 improved the manuscript's quality and depth. We addressed each comment below, 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: *In the revised manuscript, all typos have been corrected.*

RC: Line 90: correct “urface roughness” with “Surface roughness”.

AR: *In the revised manuscript, all typos have been corrected.*

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: *In the revised manuscript, we kept the consistency with mean planar extension (suncup area). All definitions are now given in the dedicated Section 2.2 (**Data and Methods – L140-177**).*

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

AR: *In the revised manuscript, we dropped the old indicator for horizontal roughness, replaced it with a_{CL} and provided detailed description in the dedicated **Section 2.2 – L160-173**: Horizontal descriptors characterize the lateral scale of surface relief. To represent this scale, we use the correlation length (CL), which expresses the horizontal distance over which surface height fluctuations remain statistically correlated (Manninen, 2003; Huang, 1998; Manninen, 1997). We computed CL separately across (CL_x) and along (CL_y) the TLS beam direction. The TLS is facing north; therefore, the y-axis is along the dominant wind direction. We define the anisotropy between CL_x and CL_y :*

$$a_{CL} = \frac{\max(CL_x, CL_y)}{\min(CL_x, CL_y)} \quad (1)$$

The ratio a_{CL} describes surface directionality: anisotropic, preferentially oriented reliefs exhibit large values, whereas $a_{CL} \simeq 1$, (i.e., $CL_x \simeq CL_y$) indicates isotropy. The vertical and horizontal descriptors provide complementary information about the snow surface state. A ripples field exhibits large horizontal and moderate-to-low vertical descriptors: its reliefs are spatially extended, widely spaced, and strongly directional, giving large a_{CL} . Suncups, conversely, show smaller horizontal and larger vertical descriptors, since reliefs of similar amplitude arrange periodically over short distances with generally no preferential direction: the surface tends towards isotropy, giving $a_{CL} \rightarrow 1$.

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

AR: *In the revised manuscript, all time series are now provided with the specification about temporal resampling.*

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: *In the revised manuscript, this entire section has been rewritten – please see comment below.*

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 subplots of Fig 2. Also, please clarify what is (the standard deviation of wind direction?).**

AR: *In the revised manuscript, Section 3.2 has been entirely rewritten according to this suggestion (**Results – L320-357**). We additionally provide Tab. 2, which summarizes suncup geometry, meteorological, and radiative conditions averaged over the suncup growth window across all seasons.*

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: *In the revised manuscript, all time series are now provided with the specification about temporal resampling.*

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: In the revised manuscript, Fig.4 has been revised and the caption updated.

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: In the revised manuscript, we abandoned RMSE scores – which originated unclear sentences – and kept biases instead. The new sentence now reads (**Results – L359-363**): Under smooth, dry conditions, TARTES overestimates and slightly underestimates measured albedo under diffuse and direct illumination conditions, respectively, with biases $\beta_{dry}^{smooth} +0.04$ and -0.01 (Fig. ??a–c,d–f), which may reflect inaccuracies in the input snowpack and model limitations. As the snow wets and SSA decreases, but prior to suncup growth, TARTES overestimates albedo under all illumination conditions, with average biases across years $\beta_{wet}^{smooth} = +0.06$ (diffuse) and $+0.04$ (direct).

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 is 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: In the revised manuscript, we carefully defined in the Methods all indices that are used to explain Results. Specifically, (**Data and Methods – L240-261**): To determine the effect of suncups on the apparent broadband albedo, we compare measured time series with TARTES simulations in diffuse and direct illumination conditions for all years (Fig. ??a–f). Because TARTES neglects the presence of liquid water, relying on pure-ice refractive indices, we evaluate the quality of the simulations in dry and wet conditions separately (Fig. ??g–i). Wet conditions are initiated when the daily average of the modeled LWC exceeded 1% in the top 30 cm (2022), or based on direct field measurements through 2023 and 2024 (Carletti et al., 2025a). Because suncups form on a wet snow surface (Carletti et al., 2025b), any misrepresentation of wet-snow metamorphism by SNOWPACK or TARTES must be ruled out for the earlier flat-surface stage. Despite SNOWPACK sometimes underestimating surface SSA and systematically underestimating density (Fig. ??k–l,n–o), TARTES simulations forced by either SNOWPACK or measured surface properties show mostly negligible differences in broadband albedo (Fig. ??). This indicates that the simulated albedo is insensitive to these input biases in the wet phase, prior to suncup development. We therefore attribute the deviation from measured broadband albedo primarily to TARTES not accounting for the optical effects of liquid water and wet-snow metamorphism on a flat surface. To do so, we define the index $\beta_{wet}^{c,smooth}$, which is the mean offset between TARTES simulations and broadband albedo measurements, computed separately for each year and illumination condition, over the wet-smooth phase. Therefore, for each year y , the wet-smooth window $\mathcal{W}_y^{smooth} = \{t : t_{wet} \leq t < t_{rough}\}$ defines the evaluation set $\mathcal{S}_y^{smooth}$ containing pairing broadband albedo values $\alpha_{TARTES}(t)$ and $\alpha_{measured}(t)$. The offset is the mean difference over the evaluation set:

$$\beta_{wet}^{smooth}(y) = \frac{1}{|\mathcal{S}_y^{smooth}|} \sum_{t \in \mathcal{S}_y^{smooth}} (\alpha_{TARTES}(t) - \alpha_{measured}(t)) \quad (2)$$

which we evaluate separately for direct and diffuse illumination conditions. Consequently, and similarly,

we define $\beta_{wet}^{c,rough}$ as the additional offset that appears once the suncups develop:

$$\beta_{wet}^{rough}(y) = \frac{1}{|\mathcal{S}_y^{rough}|} \sum_{t \in \mathcal{S}_y^{rough}} (\underbrace{\alpha_{TARTES}(t) - \alpha_{measured}(t)}_{\text{mean raw offset over } \mathcal{W}_y^{rough}}) - \beta_{wet}^{smooth}(y) \quad (3)$$

By subtracting $\beta_{wet}^{c,smooth}$ from albedo residuals in the rough phase, we assume that the systematic errors deriving from wet-snow representation in TARTES are accounted for to the first order, leaving a residual signal that we can attribute solely to the co-evolution of suncup roughness and impurity concentration.

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: *In the revised manuscript, the sentence now reads (Results – L373-375): This suggests that suncup roughness and impurities jointly contribute to the albedo deficit that TARTES cannot reproduce when assuming a flat surface.*

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: *In the revised manuscript, we reformulated this concept with this new sentence (Results – L375-378): When assuming mixed-high scenarios (or the dust-dominated scenario, which reduces albedo comparably) and algal bloom scenarios, β_{wet}^{rough} tends to, and often falls below, zero under direct illumination conditions (Fig. ??p). These impurity scenarios cancel the suncup signature, suggesting that the combined effect of suncups and impurities is comparable to that of the hypothesized impurity content on a flat surface.*

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: *In the revised manuscript, we reformulated this concept with this new sentence (Results – L405-407): The mixed-high, dust-dominated, and algal-bloom scenarios yield residuals that fall systematically below zero ($\beta_{wet}^{rough} = -0.04$ on average). Because β_{wet}^{rough} is averaged over the entire rough period, this suggests these high impurity concentrations are unrealistic as a whole-phase average, though plausible for the late ablation stage, when surface impurities have become most concentrated.*

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

AR: *In the revised manuscript, all webcam and LiDAR acquisitions are accompanied by the date and time of acquisition.*

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: *In the revised manuscript, we have incorporated this remark into our Results by modifying Fig.5 (Results – L423-432): Aeolian debris transport also appears to contribute to surface darkening. Fig. ??h (Jun 03rd, 2023; $z_0=8.7$ mm) corresponds to a week in which 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 the early-stage suncups. The NW sector of the research field is enclosed by steep rock faces where snow generally disappears well in advance of the field itself, which sits in a slight local terrain depression that allows snow to persist longer. The surface darkening is therefore consistent with rock debris mobilized from the surrounding slopes. Conversely, Fig. ??i (June 17th, 2024; $z_0=8.3$ mm) corresponds to a week of predominantly SE winds, with a speed-weighted average direction of 147° prevailing for 70% of the total hours. Although the suncup roughness is nearly identical to that of the previous year, the upwind dark debris is markedly less apparent. Unlike the NW sector, the SE side of the research field opens onto the valley and is not overlooked by steep rock faces, consistent with a reduced local debris supply.*

And later used it to strengthen the end of our Discussion (Discussion – L543-548): This interpretation frames albedo decay as the interplay of roughness and impurities, but our data cannot isolate the two contributions to confirm it. Bair et al. (2022) identify why this is challenging even with spectral information: in snow composed of mixed pristine and dirty fractions, the darkening from roughness and from impurities remains difficult to separate, and our webcam imagery (Fig. ??) shows the two are coupled from the earliest stages of ablation. Under these conditions, z_0 serves as a robust proxy for the combined albedo decay due to surface microtopography and impurity loading, integrating both regardless of their relative contributions at any ablation stage.

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

AR: *In the reviewed manuscript, all typos have been corrected.*

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

AR: *This was an editorial mistake on our side, which we removed.*

3. Additional relevant changes to the original manuscript, not addressed in the replies to Reviewers

(All Figures and Table numbers refer to the new version of the manuscript.)

- **Fig. 1** – Which we use to show that errors in SNOWPACK's representation of optical snow properties are not directly transferred to systematic biases in TARTES.
- **Fig. 2(m–o)** – We added persistent above-zero wet-bulb temperatures in our set of suncup roughness onset diagnostic variables.
- **Data and Methods – L232-239** – The solar spectra, initially taken from Dadic et al. (2013), are now modeled for the neighboring Totalp site, using libRadtran.
- **Fig. 4(r)** – We added the fraction of irradiance per nm for the libRadtran-modelled solar spectra and used them to revise our original incorrect interpretation, according to which the suncup roughness signature was stronger under diffuse illumination conditions. This concept is now explained in detail in our **Results (L379-391)** and extensively addressed in our **Discussion (L494-507)**. **We also removed this wrong interpretation from our Abstract and Conclusions.**

References

- Amory, C., Gallée, H., Naaïm-Bouvet, F., Favier, V., Vignon, E., Picard, G., Trouvilliez, A., Piard, L., Genthon, C., and Bellot, H. (2017). Seasonal variations in drag coefficient over a sastrugi-covered snowfield in coastal east antarctica. *Boundary-Layer Meteorology*, 164(1):107–133.
- Bair, E. H., Dozier, J., Davis, R. E., Colee, M. T., and Claffey, K. J. (2015). Cues—a study site for measuring snowpack energy balance in the sierra nevada. *Frontiers in Earth Science*, Volume 3 - 2015.
- Bair, E. H., Dozier, J., Stern, C., LeWinter, A., Rittger, K., Savagian, A., Stillinger, T., and Davis, R. E. (2022). Divergence of apparent and intrinsic snow albedo over a season at a sub-alpine site with implications for remote sensing. *The Cryosphere*, 16(5):1765–1778.
- Bair, E. H., Rittger, K., Davis, R. E., Painter, T. H., and Dozier, J. (2016). Validating reconstruction of snow water equivalent in california's sierra nevada using measurements from the nasa airborne snow observatory. *Water Resources Research*, 52(11):8437–8460.
- Carletti, F., Bavay, M., Ghielmini, C., Bonardi, M., Lebersperger, P., Philippe, V. M., Bozzoli, M., Marin, C., Steijn, C., Geiser, M., Bertoldi, G., Grünenfelder, L. N., Premier, V., and Barella, R. (2025a). SnowTinel high-temporal-resolution ground truth dataset for SAR remote sensing of snow. [data set].
- Carletti, F., Marin, C., Ghielmini, C., Bavay, M., and Lehning, M. (2025b). Multitemporal analysis of sentinel-1 backscattering during snow melt using high-resolution field measurements and radiative transfer modeling. *EGUsphere*, 2025:1–41.
- Conway, H., Gades, A., and Raymond, C. (1996). Albedo of dirty snow during conditions of melt. *Water resources research*, 32(6):1713–1718.

- Dadic, R., Mullen, P. C., Schneebeli, M., Brandt, R. E., and Warren, S. G. (2013). Effects of bubbles, cracks, and volcanic tephra on the spectral albedo of bare ice near the transantarctic mountains: Implications for sea glaciers on snowball earth. *Journal of Geophysical Research: Earth Surface*, 118(3):1658–1676.
- Doherty, S. J., Grenfell, T. C., Forsström, S., Hegg, D. L., Brandt, R. E., and Warren, S. G. (2013). Observed vertical redistribution of black carbon and other insoluble light-absorbing particles in melting snow. *Journal of Geophysical Research: Atmospheres*, 118(11):5553–5569.
- Dumont, M., Arnaud, L., Picard, G., Libois, Q., Lejeune, Y., Nabat, P., Voisin, D., and Morin, S. (2017). In situ continuous visible and near-infrared spectroscopy of an alpine snowpack. *The Cryosphere*, 11(3):1091–1110.
- Fassnacht, S., Williams, M., and Corrao, M. (2009). Changes in the surface roughness of snow from millimetre to metre scales. *Ecological Complexity*, 6(3):221–229. Special Section: Fractal Modeling and Scaling in Natural Systems.
- Fassnacht, S. R., Toro Velasco, M., Meiman, P. J., and Whitt, Z. C. (2010). The effect of aeolian deposition on the surface roughness of melting snow, byers peninsula, antarctica. *Hydrological Processes*, 24(14):2007–2013.
- Flanner, M. G., Zender, C. S., Randerson, J. T., and Rasch, P. J. (2007). Present-day climate forcing and response from black carbon in snow. *Journal of Geophysical Research: Atmospheres*, 112(D11).
- Gabbi, J., Huss, M., Bauder, A., Cao, F., and Schwikowski, M. (2015). The impact of saharan dust and black carbon on albedo and long-term mass balance of an alpine glacier. *The Cryosphere*, 9(4):1385–1400.
- Huang, C. (1998). Quantification of soil microtopography and surface roughness. In Baveye, P., Parlange, J.-Y., and Stewart, B. A., editors, *Fractals in Soil Science*, chapter 5, pages 153–168. CRC Press, Boca Raton, FL, USA.
- Manninen, A. (1997). Surface roughness of baltic sea ice. *Journal of Geophysical Research: Oceans*, 102(C1):1119–1139.
- Manninen, A. (2003). Multiscale surface roughness description for scattering modelling of bare soil. *Physica A: Statistical Mechanics and its Applications*, 319:535–551.
- Révillet, M., Dumont, M., Gascoin, S., Lafaysse, M., Nabat, P., Ribes, A., Nheili, R., Tuzet, F., Ménégot, M., Morin, S., Picard, G., and Ginoux, P. (2022). Black carbon and dust alter the response of mountain snow cover under climate change. *Nature Communications*, 13(1):5279.
- Rhodes, J. J., Armstrong, R. L., and Warren, S. G. (1987). Mode of formation of “ablation hollows” controlled by dirt content of snow. *Journal of Glaciology*, 33(114):135–139.
- Sanow, J. E., Fassnacht, S. R., and Suzuki, K. (2024). How does a dynamic surface roughness affect snowpack modeling? *Polar Science*, 41:101110. Special Issue on "The Seventh International Symposium on Arctic Research (ISAR-7)".
- Sterle, K. M., McConnell, J. R., Dozier, J., Edwards, R., and Flanner, M. G. (2013). Retention and radiative forcing of black carbon in eastern sierra nevada snow. *The Cryosphere*, 7(1):365–374.

- Tuzet, F., Dumont, M., Lafaysse, M., Picard, G., Arnaud, L., Voisin, D., Lejeune, Y., Charrois, L., Nabat, P., and Morin, S. (2017). A multilayer physically based snowpack model simulating direct and indirect radiative impacts of light-absorbing impurities in snow. *The Cryosphere*, 11(6):2633–2653.
- Tuzet, F., Dumont, M., Picard, G., Lamare, M., Voisin, D., Nabat, P., Lafaysse, M., Larue, F., Revuelto, J., and Arnaud, L. (2020). Quantification of the radiative impact of light-absorbing particles during two contrasted snow seasons at col du lautaret (2058 m a.s.l., french alps). *The Cryosphere*, 14(12):4553–4579.
- Walter, B. et al. (2024). Automated LiDAR remote sensing for measuring the spatial and temporal evolution of surface hoar formation. In *Proceedings of the International Snow Science Workshop (ISSW) 2023, Bend, Oregon*. ISSW 2023 proceedings.
- Yang, S., Xu, B., Cao, J., Zender, C. S., and Wang, M. (2015). Climate effect of black carbon aerosol in a Tibetan Plateau glacier. *Atmospheric Environment*, 111:71–78.