Detailed responses to reviews

Saharan dust impacts on the surface mass balance of Argentière Glacier (French Alps)

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The authors thank the editor and the two reviewers for the time dedicated to reading the manuscript and for the constructive feedbacks, which we believe contributed to improving its quality. Answers to the two reviewers are listed in the coloured boxes and our responses in black below the boxes. The quotation marks are used when citing the manuscript in blue, and the modifications are highlighted in bold. We also provide a revised version of the manuscript, as well as a version with tracked changes.

1 Summary of the corrections

The main adjustments carried out in this revised version of the manuscript are the following:

- broader details on the ALADIN model (R1.2.)
- justifying our choice of attribution concerning the dust impact (R2.6.)
- addition of uncertainty tests regarding dust scavenging (R1.3.), and mass absorption efficiency of dust (R2.7.)
- adjustment of the mass absorption efficiency (MAE) of LAPs and its justification in a detailed analysis in section 3 of this response

2 Answers to Reviewers

2.1 Reviewer #2

R2.1. This study examines the impact of mineral dust and BC on the surface mass balance of a glacier in the French Alps. The study uses a variety of assimilated and reanalysis data to inform a multilayer snow model, CROCUS, which was adapted to account for LAP layer deposition and exposure within the snowpack structure. Results show that dust contributed significantly to water equivalent loss from the glacier over all years investigated, but especially in 2022 when compounding effects of dust deposition during the previous season resulted in exception melt loss. I think this study is very well structured and organized. I appreciate the wide array of data used and the thorough discussion of methods and limitations. The Figure and results are mostly well designed and can be clearly understood.

We thank reviewer #2 for his general comment and for the specific comments that helps clarifying the limitations of our study. We hope that our answers help to better apprehend our choices and results.

2.1.1 Primary concerns

Each of those primary concerns were detailed below by reviewer #2. So we gave an answer to each of those primary concerns directly under the linked comment below.

> **R2.2.** My primary concern lies within the discussion associated with BC scavenging and increased dust impact, detailed in the comments below.

We addressed this concern in the response to specific comment R2.6.

> **R2.3.** I would also request some clarity regarding the choice of both the LAP representation within the snow representation, [...]

We addressed this concern in the response to specific comment R2.7.

> **R2.4.** I would also request some clarity regarding [...] the choice of CROCUS itself.

We addressed this concern in the response to specific comment R2.9.

> **R2.5.** Overall, I think the approach in this study is great for assessing specific, well-observed glaciers. However, I question some broader claims of scalability and transferability to other regions.

We addressed this concern in the response to specific comment R2.13.

2.1.2 Specific comments

> **R2.6.** Line 530, Section 4.2, Figure 8, and elsewhere: There is mention of higher dust impacts when BC is low or highly scavenged within the snowpack. I agree that this increases the relative impact of dust on snowmelt (compared to BC), but as I understand it, this shouldn't directly affect the impact that dust has in generating melt water. In other words, 1g/m2 of exposed dust would generate X m w.e of melt, and 1 g/m2 of dust with 0.1 g/m2 black carbon would generate X m w.e. of melt (from dust) and Y m w.e. of melt (from BC). The point being that X m w.e. is the same in both scenarios. If this is not the case, then it is currently unclear what the mechanism is that would drive the same amount of dust to have a greater impact on melt in the absence of BC (because of higher scavenging). I would argue that having more BC retained in the snowpack with dust would enhance the impact of dust indirectly, by resulting in more rapid exposure of buried deeper dust layers from X+Y vs. just X melt. Please explain and/or clarify.

This is indeed an interesting remark and it is a question of attribution. To clarify the situation, we introduce Figure 1 of this document (not added in the revised manuscript) that illustrates the difference between the two possible approaches to quantify the impact of dust: the method suggested by reviewer #2 and the one adopted in this study.

In this study, the impact of dust was computed by comparing the simulation with [BC only] and the simulation with [BC+dust]. In other words, only the dust is removed in order to quantify its impact, by comparison with 'contaminated' snow, which we consider to be closer to reality. However, as mentioned in the manuscript in section 4.2, when using this reference, the impact of one LAP depends on the concentration of other LAP, because albedo is a non-linear function of LAP concentration (*e.g.*, Fig. 12 of Skiles, Painter (2017)). For instance, at a given timestep, less BC at the snow surface increase the dust radiative forcing.

In the second method suggested by reviewer #2, our understanding is that the impact of dust would be based on comparing the simulation [without LAP] and the simulation with [dust only]. However, this method, (i) is a comparison between 2 virtual scenarios ([no LAP] and [only dust]) and (ii) probably lead to overestimate the impact of dust (see Fig. 1).

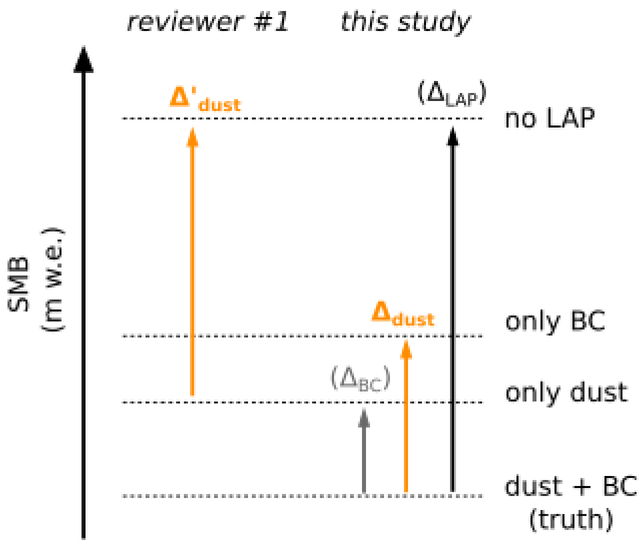

Figure 1: Choice on the attribution of the dust impact Δ_{dust} from different point-of-view. Note that we have intentionally (i) $\Delta_{\text{dust}} + \Delta_{\text{BC}} \leq \Delta_{\text{LAP}}$ and (ii) $\Delta_{\text{dust}} \leq \Delta'_{\text{dust}}$, this is due (among other factors) to the sub-linearity of the albedo decrease with respect to the LAP concentration.

Both methods have been used in the literature to quantify this impact (*e.g.*, Flanner et al. (2007, 2012) used the first approach, and Réveillet et al. (2022) the second approach). As mentioned above, each has advantages and limitations. Nevertheless, based on the work of Mark Flanner and others (cited below) and considering that the potential overestimation of the impact inherent to the second approach represents a limitation, we prefer to retain the approach described in this study.

Citations from Flanner's work: Flanner et al. (2007): "We calculate instantaneous radiative forcing of carbon aerosols in both snow and the atmosphere at each radiative transfer time step as the difference in absorbed radiation with all aerosols and all aerosols except carbon aerosols." Flanner et al. (2012): "We calculate radiative forcing [of BC] each timestep as the instantaneous difference in absorbed surface energy with and without BC." In this case, adding other LAPs to the simulation decreases the RF of the initial LAPs: "These forcings are smaller than previous estimates (Flanner et al., 2007, 2009), demonstrating the importance of other factors. Reasons why forcing is different in this study are [...] (4) light absorption by dust in snow, which decreases BC forcing, was not considered by Flanner et al. (2007)" (Flanner et al., 2012).

To clarify and better justify our approach, we added the following clarifications in section 2.5: "The forcing ensemble was used as input to the simulation model to get the simulations considering the impact of mineral dust (simulations named "dust"), and the simulations without the impact of mineral dust (simulations named "no-dust"). Simulations without the impact of dust were obtained without the radiative effect of **dust** ~~LAP~~ as explained in Section 2.4.2. As a result, we obtained 40 paired simulations (dust and no-dust). **An important point is that BC remained present in all simulations. An alternative could have been to use the difference between (i) the simulation with dust and without BC, and (ii) the simulation without LAP at all as in Réveillet et al. (2022). This would have likely lead to larger quantified impacts (Flanner et al., 2012). However, we chose the simulation with both BC and dust as the reference as it is closer to the observed state of the snowpack, hence avoiding an overestimation of the dust impact and staying in line with other studies, *e.g.*, Flanner et al. (2007, 2012)).**"

R2.7. Section 2.4.1: As I understand it, the LAP implementation and resulting modeled albedo depends on optical properties derived from dust during previous years at the same glacier. Is there ample evidence that the optical properties do not vary that much? Explain.

We thank the reviewer to point out this limitation of the study.

Indeed, depending on the region of origin or its size distribution, the mineral dust deposited over the European Alps can have different optical properties (*e.g.*, Dumont et al. (2023); Caponi et al. (2017)). After deposition the optical properties of dust can also evolve inside the snowpack with (i) the location of the LAP with respect to the snow grains, *i.e.*, inside or outside of the snow grains, *e.g.*, He et al. (2018), or with (ii) the aggregation of the dust particles, *e.g.*, Bond, Bergstrom (2006).

To assess the impact of different absorption properties of dust, we did 2 additional sensitivity tests (added in Fig. 8 of the manuscript) using a low and a high value of mass absorption efficiency (MAE) of dust, respectively [$\mathrm{MAE_{dust}}(\lambda_0) = 27 \times 10^{-3}$ m^2 g^{-1}, $\mathrm{AAE_{dust}} = 3.3$] and [$\mathrm{MAE_{dust}}(\lambda_0) = 630 \times 10^{-3}$ m^2 g^{-1}, $\mathrm{AAE_{dust}} = 3.4$]. Those values are the extrema of MAE listed in Caponi et al. (2017) when considering the regions of Sahel and Sahara, the main sources of mineral dust for European Alps (Collaud Coen et al. (2004), or more recently Collaud Coen et al. (2025)).

The sensitivity tests show that increasing the dust MAE results in a higher dust impact, leading to enhanced melt and consequently a decrease in glacier surface mass balance. We noted that there was a significant difference in dust impact in 2022 between these extreme values of MAE ($\Delta_{\mathrm{dust}} < 0.4$ m w.e. for the low MAE and $\Delta_{\mathrm{dust}} > 1.4$ m w.e. for a high MAE). However, these extreme dust impacts are likely unrealistic since the simulations using extreme dust MAE (low and high) had lower evaluation rates than the reference scenario used throughout the study (see Table. 3).

To address your comment and the additional sensitivity tests mentioned below, the Methods and Results sections, as well as Figure 8, have been updated in the revised version as follows:
Method section of section 2.4.1: "**For the two uncertainty tests on the dust MAE (Fig. 2), we used the highest and the lowest values of dust MAE considering the regions of Sahel and Sahara of Caponi et al. (2017), respectively [$\mathrm{MAE_{dust}}(\lambda_0) = 27 \times 10^{-3}$ m^2 g^{-1}, $\mathrm{AAE_{dust}} = 3.3$] and [$\mathrm{MAE_{dust}}(\lambda_0) = 630 \times 10^{-3}$ m^2 g^{-1}, $\mathrm{AAE_{dust}} = 3.4$].**"
We added the following sentences in a new paragraph of section 4.1: "**In addition, the temporal variability of the absorption efficiency of dust was not represented in the model: the MAE used in this study was constant in time. However, the absorption efficiency of mineral dust can vary for each deposition event depending on the region of origin or the size distribution (*e.g.*, Dumont et al. (2023); Caponi et al. (2017)), as well as by its location within the snowpack either inside or outside the snow grains (He et al., 2018) and by the aggregation state of the dust particles (Bond, Bergstrom, 2006). As shown in Fig. 2, a low and a high value of dust MAE (Section 2.4.1) strongly affects the resulting dust impact ($\Delta_{\mathrm{dust}} < 0.4$ m w.e. for the low MAE and $\Delta_{\mathrm{dust}} > 1.4$ m w.e. for a high MAE). However, these extreme dust impacts are unlikely since the simulations using extreme dust MAE (low and high) results in degraded accuracy metrics with respect to in situ measurements compared to the reference scenario used throughout the study (not shown).**" Since this MAE remains poorly constrained and represent an interesting research perspective, we also propose to mention this limitation in the conclusion section: "Furthermore, large uncertainties linked to the percolation of BC within the snowpack might lead to significantly higher impacts of dust. **Using different mass absorption efficiency for the dust particles in snow also significantly modulated the results.** Further research is needed to understand these processes and to quantify their impacts."

R2.8. The LAP representation could be assessed indirectly by comparison to remotely sensed snow albedo. Why was a snow albedo comparison from Sentinel or another sensor omitted? Remotely sensed snow albedo could also inform modeling directly and may be more scalable. Explain why this approach was not used in this paper. If Sentinel has a pixel saturation issue that prevents good snow albedo detection, mention this.

We thank reviewer #2 to mention this idea. Indeed, broadband albedo derived from remote measured reflectances has been increasingly used for glaciological studies. For instance, Naegeli et al.

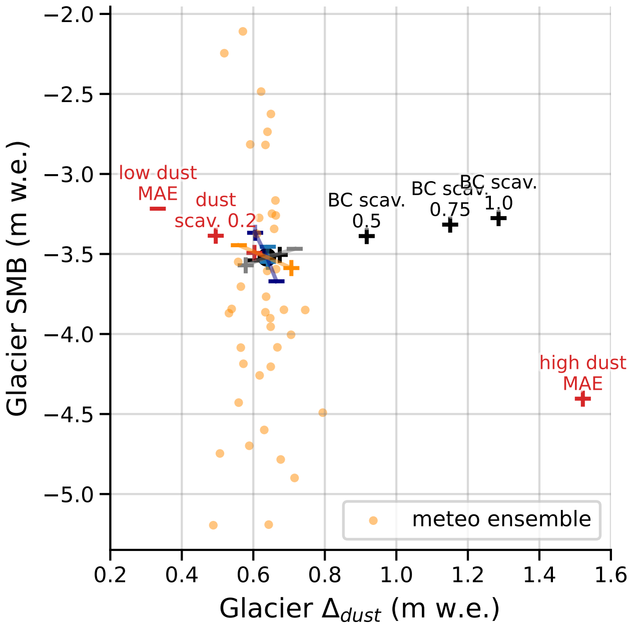

Figure 2: Figure 8: Impact of uncertainties relative to LAP scavenging efficiencies and deposition fluxes on the glacier-wide annual SMB (y-axis) and dust impact Δ_{dust} (x-axis) for hydrological year 2022. Reference simulation is represented by the central black dot. The 40 members of the ensemble are represented with the orange dots.

(2019) derived ice albedo trends using remote-sensing data using the method of Liang et al. (2003). However, we should remain cautious when comparing values of simulated broadband albedo and broadband albedo retrieved from remote-sensed reflectances, here Sentinel-2. First, Sentinel-2 measures the hemispherical-conical reflectance at given wavelengths. Since the reflection of snow is not isotropic (*e.g.*, Dumont et al. (2010)) (*i.e.*, depends on the observation angle, and also varies with the solar zenith angle), a reflectance of snow on a given surface may differs from the albedo (bi-hemispherical reflectance) of this surface. Exploiting correctly the relationship between snow reflectance observed from S2 and albedo would require a more complex processing, *e.g.*, Lamare et al. (2020). The method of Liang et al. (2003), initially developed for other land surfaces than snow, does not account for the anisotropy of snow. Second, the uncertainties of the reflectance retrievals in complex terrain may be too high to estimate the accuracy of the simulated albedo, as shown in Cluzet et al. (2020) for Sentinel-2 (S2) images. For instance, the strong dependency to the slope is illustrated for S2 derived albedo in Fig. 3a. That is why we initially based our evaluation on surface type (snow, ice), instead of broadband albedo values derived from S2 images.

Considering your comment, and the limitations mentioned above, we added a comparison between the simulated albedo and the albedo derived from the observed S2 reflectances in the Appendix of the revised version of the manuscript (Figure 3 of this review). The method is explained in the new section 2.2.6: "**Broadband snow albedo derived from Sentinel-2 reflectances**" of the revised manuscript: "**Using all images of Table B2, we compared the simulated broadband albedo at noon, and the observed broadband albedo derived from Liang et al. (2003), *i.e.*, S2 albedo** $= 0.356 * R_{B2} + 0.130 * R_{B4} + 0.373 * R_{B8} + 0.085 * R_{B11} + 0.072 * R_{B12} - 0.018$ **where** R_{Bi} **is the reflectance of Sentinel-2 band** i. **Only the points where the surface type (Section 2.2.3) was snow for both the model and S2 were used. Appendix Fig. 3 shows this comparison.**" The precautions to be taken regarding this comparison are mentioned in the caption of Appendix Fig.3.

It should also be noted that these results highlighted a number of limitations, particularly concerning the choice of MAE, and led to additional modifications in the revised manuscript, detailed in section 3 of this document.

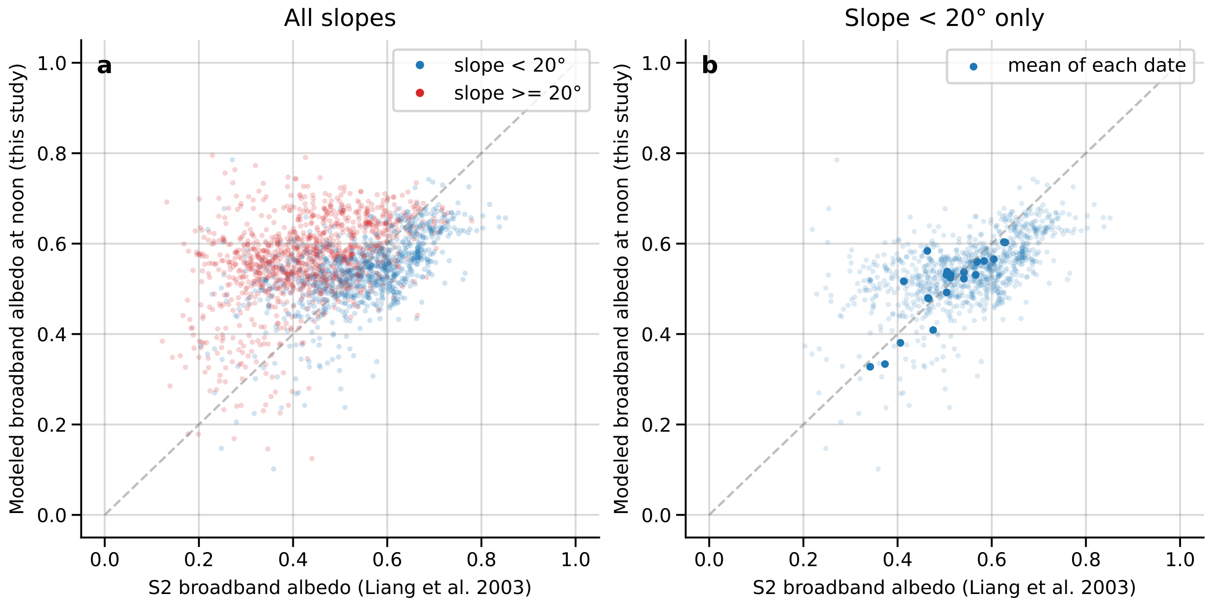

Figure 3: **Figure B3. Comparison between simulated broadband albedo and broadband albedo derived from S2. See section 2.2.6 for methods and limitations. (a) Influence of the slope. (b) Comparison for slopes < 20°. The mean of all dates, except 2, shows less than 0.1 of difference between the modelled albedo and the albedo derived from S2. This comparison of broadband albedo has to be taken with cautious. Indeed, there are several limitations. First, the broadband albedo derived from S2 (Liang et al., 2003) was not developed for snow, an anisotropic surface (*e.g.*, Dumont et al. (2010)), *i.e.*, the reflectance depends on the observation angle. Second, the uncertainties of the reflectance retrievals in complex terrain may be too high to estimate the accuracy of the simulated albedo, *e.g.*, Cluzet et al. (2020), or panel (a) of this figure where the broadband albedo derived from S2 strongly depends on the slope.**

> **R2.9.** Section 4.3: I appreciate the thorough discussion of uncertainties regarding LAPs, but think there also needs to be more discussion of broader model uncertainties. What are the limits of CROCUS? How would using different [...] physically based snow models vary the results? If word count is an issue, I would recommend condensing some lines from the detailed discussion of SMB and LAPs.

This comment was initially combined with the next comment (R2.10.). Please refer to the next comment for the uncertainties concerning the choice of the model providing the meteorological forcing.

In the initial version of the manuscript, we did not discussed the snow model choice. We used here the SURFEX-ISBA/Crocus model because (i) it has already been widely used in the European Alps to model glacier SMB (*e.g.*, Gerbaux et al. (2005); Dumont et al. (2012); Réveillet et al. (2018)), (ii) it has a detailed representation of the physical processes governing the snowpack evolution (which is important for our study assessing impacts in snow), and (iii) this model is one of the only models that combined a detailed layered description of snow microstructure with an explicit representation of the radiative effects of LAPs. Indeed, Crocus is coupled with the radiative transfer model TARTES (G. Picard, Q. Libois, 2024) and this coupling has already been used in multiple studies (*e.g.*, Tuzet et al. (2020); Dumont et al. (2020); Réveillet et al. (2022)). For such reasons, we think that Crocus is our best option to quantify the impact of LAP in the Alps.

Nevertheless, Crocus, as every model, is subject to modelling uncertainties related to the physical processes driving snow evolution, or from the radiative transfer model. This is now discussed in section 4.3 of the revised version of the manuscript: **"The uncertainties in the results can also be related to the choice of the model and the parameters used in the model (*e.g.*, Lafaysse et al. (2017)). In this study, this primarily concerns the physical processes driving snow**

evolution and the radiative transfer model. Concerning the radiative transfer model, here TARTES coupled with the snow model, comparisons between radiative models (SNICAR and TARTES in G. Picard, Q. Libois (2024)) indicate that the choice of the radiative transfer model has a significantly smaller impact on the results than variations in the optical properties of the LAP, here represented using the MAE of each LAP. Therefore, the uncertainty related to the radiative model is accounted for in this study by varying the dust MAE factor (Section 4.1). Concerning the snow model, an ensemble approach can provide estimates of snow model uncertainties. Réveillet et al. (2022) applied this method to Crocus and reported results on the impact of the LAPs comparable to deterministic simulations (Supplementary Fig. S10 of Réveillet et al. (2022))."

R2.10. Section 4.3: [...] How would using different NWPs [...] vary the results?

For the meteorological forcing, we used the SAFRAN reanalysis. It is indeed NWP-based, however SAFRAN also "combines information from numerical weather prediction models (ERA-40 reanalysis from 1958 to 2002, ARPEGE from 2002 to 2021) and the best possible set of available in situ meteorological observations" (Vernay et al., 2022), *i.e.*, SAFRAN is a reanalysis and not just a numerical weather prediction. In addition, it was the most performant meteorological reanalysis over the French Alps given our usage. Thus, using a different NWP without any assimilated observation would very likely lead to higher uncertainties.

Nevertheless, this dataset has identified errors, *e.g.*, incoming longwave bias in Réveillet et al. (2018). So, we used in-situ data using 2 AWS to improve the accuracy of this forcing (section 2.3.2). And to account for the remaining errors and to avoid using meteorological forcings from different sources, we applied stochastic perturbations on the SAFRAN data (section 2.3.6), taking into account the errors of SAFRAN w.r.t. the AWS moraine. This way, we propagated the forcing uncertainties and we quantified their impact on the results. The median-Q10-Q90 of the results using the meteorological ensemble are often given in the manuscript in the form of XXX [XXX, XXX]. At each point, this led to meteorological uncertainties of around ± 1 m w.e. on the annual simulated SMB, with meteorological uncertainties on the fraction of melt due to dust varying between a few percent for the lowest impact (*e.g.*, at point P1) to $\pm 10\%$ for the highest impacts (*e.g.*, at point P2).

Finally, our study is mostly based on relative differences between simulations, thus lowering the impact of uncertainties related to the meteorological variables (except the LAP deposition fluxes) and to the surface model. That is actually also a reason why the different result in term of dust impact do not diverge a lot (often small standard deviation compared to the median).

Continuing the paragraph of the previous comment R2.9. on the model uncertainties, we propose to add the following sentences: "**Furthermore, uncertainties related to the snow model choice are generally smaller than those associated with the forcing (*e.g.*, Etchevers et al. (2004); Günther et al. (2019)). To explore the uncertainties related to the meteorological variables, we did the choice to use an ensemble of forcing produced using stochastic perturbations on the adjusted SAFRAN data (section 2.3.6). Ultimately, the dust impact presented in this study is expressed as relative differences between simulations rather than as absolute values, which limits the influence of uncertainties related to the forcing and to the snow model choice.**"

R2.11. What if there is no information about the optical properties of snow? Is CROCUS best suited for very large glaciers? This also relates to my comment on line 623.

Concerning the optical properties of snow itself in Crocus, they are calculated from Crocus prognostic variables (*e.g.*, snow grain size, density). Thus there is no need for any information other than the meteorological forcing to compute them. We added the following sentence in section 2.4 to clarify this point: "Crocus simulates the evolution of the snow cover with a 15 minutes time-step representing different processes including thermal diffusion, phase change, water flow, snow metamorphism and compaction. **Snow optical properties (single scattering albedo and absorption cross section for each snow layer) are calculated from the prognostic variables of the model (grain size, density, layer thickness).** In Crocus, the snowpack is discretized as a one dimensional column with up to 50 layers, [...]."

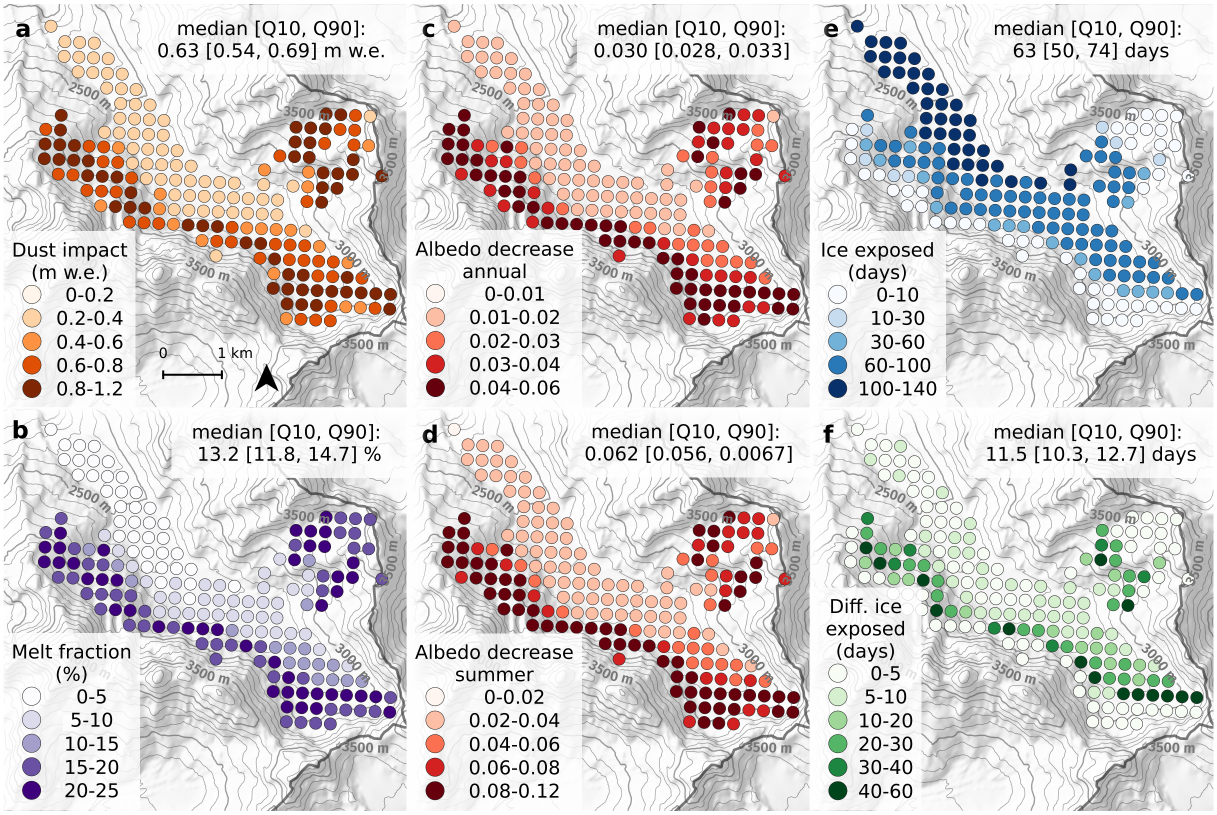

Figure 4: **Revised Figure 6.**

For "very large glaciers", if it refers to large glaciers in the European Alps, Crocus has already been used widely and demonstrated its ability to perform well regardless of the glacier size (*e.g.*, Gerbaux et al. (2005); Dumont et al. (2012); Réveillet et al. (2018)). We propose the following modification in section 2.4: "which was already used to simulate **alpine glacier SMB regardless of the glacier size, *e.g.*, Gerbaux et al. (2005); Dumont et al. (2012); Réveillet et al. (2018).**". If it refers to glaciers outside of the Alps, we answered below under the comment R2.13.

R2.12. Figure 6: Basic cartography issues. Please add a scale bar and a north arrow to the maps. Some indication of elevations (maybe very faint topo lines) would help the reader understand the distribution of these changes over terrain. I would recommend including the same basemap as Figure 1 (d) in these panels. This is the take-home figure of this paper, and it would be helpful to orient readers, especially those just "skimming" and those not familiar with the glacier.

These element were indeed missing in Figure 6. We thank the reviewer for pointing this out. A scale bar and a north arrow have need added to the first plot. Grey isolines have been added to every plot of this figure. We did not added the glacier outlines, to keep a light and readable figure. The resulting revised figure is Fig. 4 of this document.

R2.13. Line 623: Is this truly "easily" scalable if this modeling work relies heavily on bias correction and adjustments from automated in situ measurements (as discussed in section 2.3.3)? Are these relationships scalable to other glaciers at other latitudes? Most glaciers do not have automated measurements, let alone a glacier observatory, and modeled forcings have much higher uncertainties in more remote regions (Himalayas, Andes, Rockies). Would this approach to such glaciers truly be reasonable? Please clarify how such an approach would be transferable with ease. *Combined with the general comment:* Overall, I think the approach in this study is great for assessing specific, well-observed glaciers. However, I question some broader claims of scalability and transferability to other regions.

We agree with this comment, there are some limits. It should be noted that the Crocus model has already been used in other regions than the European Alps to model the snow cover, *e.g.*, Gaillard et al. (2025); Luijting et al. (2018); Fréville et al. (2014); Lejeune et al. (2007). When claiming the scalability and transferability, we were indeed implicitly referring to monitored areas (*e.g.*, the European Alps) where the meteorological forcing (including solid precipitations) have less uncertainty than in remote areas.

We modified the manuscript accordingly: "The simulation setup of this study is easily transferable in time and space **for glaciers where measurements of winter solid precipitation and summer melt exist. An automatic weather station is also useful to ensure the accuracy of the meteorological forcings used in the model. As a result, this work paves** the way towards a comprehensive regional assessment of the impacts of LAPs on glaciers **in monitored regions**."

R2.14. Line 447: Do you mean the sensible heat flux? Latent heat fluxes are generally negative during melt.

Thank you for this remark on the following sentence of the initial version: "Hence, the impact of dust alone at the glacier-wide scale in summer is likely higher than the impact of the latent heat flux, which represents a few percents of the total melt." We wanted here to focus on the mass loss due to the latent heat flux, *i.e.*, negative latent heat flux, *i.e.*, sublimation. However, this sentence is poorly link with the scope of the study. We removed that sentence in the revised version of the manuscript.

2.1.3 Technical corrections

R2.15. Line 1: color → turn (Just a recommendation for slightly smoother wording. Also consider "...deposits frequently darken alpine glaciers")

We changed from 'color' to 'turn', and mentioned the darkening afterwards: "Saharan dust deposits frequently **turn** alpine glaciers orange **and darken their surface**."

R2.16. Line 15: to account → accounting

We modified accordingly "Hence, we recommend **accounting** for impurities to simulate the distributed surface mass balance of glaciers."

R2.17. Line 588: depositions → deposition, radiations → radiation

We modified accordingly "... because of: (i) higher dust **deposition**; and (ii) higher shortwave **radiation**."

R2.18. Figure 5c: Change month abbreviations to English (Jan., Jul.) or spell out the month. Also, decrease the size of x-axis labels (or rotate slightly) on red/purple plots on the right. Currently, it looks like "19202122" instead of "19 20 21 22". Please specify that this is year.

Thanks. We switched to English. An indeed the years were not readable, we tuned the x-axis labels and added this information in the legend: "**Double digits refers to the years.**"

R2.19. Line 620: their → its

We modified accordingly "... for the period 2019-2022, **its** impact can not be totally neglected...".

3 Authors additional corrections

During the review process, we identified some inaccuracies that have been corrected, and some perspectives of evolution that have been investigated in this revised version. They are listed and detailed below, together with the corresponding modifications implemented in the revised version of the manuscript. While these corrections (especially the point 1) slightly change some of the numbers related to the quantification of dust impact, they do not alter the conclusions of the paper.

1. In the initial version of the paper, we did not redistribute the ALADIN wet and dry deposition fluxes with respect to the SAFRAN precipitations, hence deposited LAP were lost (*e.g.*, when ALADIN wet depositions without SAFRAN precipitations). We added this redistribution in this revised version following the method of Réveillet et al. (2022). Such a modification increased the simulated concentration of dust and BC at the snow surface, hence lowering albedo and advancing snow melt-out date compared to the initial version. So we adjusted and lowered (i) the mass absorption efficiency (MAE) of LAP of dust to [$\mathrm{MAE_{dust}}(400\ \mathrm{nm}) = 71 \cdot 10^{-3}\ \mathrm{m^2/g}$; $\mathrm{AAE_{dust}} = 3.2$] and the MAE of BC at 550 nm to 7.5 $\mathrm{m^2/g}$, (ii) the roughness length of snow $z_{0,\mathrm{snow}}$ from $5 \cdot 10^{-4}$ m to 10^{-4} m, now better reproducing the ratio $z_{0,\mathrm{snow}}/z_{0,\mathrm{ice}}$ of 10 in Réveillet et al. (2018), and of ≈ 7 in Gerbaux et al. (2005).

To assess the accuracy of this revised parametrization, we tested different values of MAE (presented in Fig. 5), and we evaluated them using the following evaluation metrics:

- comparison to the summer melt measured at the stakes *(in the initial version)*
- comparison to the surface type evaluated from S2 *(in the initial version)*
- comparison to the broadband albedo derived from S2 (Fig. 6 b) recommended by reviewer #2 *(new in this review)*
- comparison to the broadband albedo measured at the AWS (Fig. 7) *(new in this review)*

Table 3 summarizes the evaluation results. The new parametrization used in this revised version showed accurate results for all the evaluation metrics. The summer melts of the initial parametrization of MAE were too large compared to the stake measurements, and the simulated albedo are lower than the albedo retrieved from S2. Using low values of dust MAE (low parametrization), melt metrics were the best, however the comparison against S2 albedo and the AWS SMOD showed worse results than for the new parametrization. So, the new parametrization appeared as the best trade-off. The new MAE values were therefore used as reference in this revised version.

The MAE values of this revised version were lower than that of the initial version (Fig. 5). However, the initial MAE values (from Tuzet et al. (2020); Réveillet et al. (2022)) were used in in the framework of a seasonal snowpack at intermediate elevations (1'500 to 3'000 m a.s.l.). On glaciers, the LAP surface concentration may reach higher values due to longer melt seasons. This could likely lead to more aggregation of LAPs in the snowpack, which decrease their MAE (*e.g.*, Bond, Bergstrom (2006); Schwarz et al. (2013)). Therefore, it is reasonable to find lower optimal MAE on glaciers.

As a result, those revised MAE values did not affect the conclusions of this article.

We propose the following modifications. For the method section 2.3.1: "**ALADIN wet and dry deposition were redistributed with respect to the the SAFRAN precipitation as in Réveillet et al. (2022), *i.e.*, wet depositions when SAFRAN precipitations are positive, dry depositions otherwise.**" The revised values of the dust MAE have been integrated in the manuscript as indicated under comment R1.14., and the values of low and high dust MAE were also integrated in the manuscript as indicated under comment R2.7. We removed words from section 2.4.1: To compute the albedo of snow, the refractive indices of ice~~, mineral dust and BC were~~ was set as **in** Réveillet et al. (2022)~~, which quantified the impact of LAPs in the same region as in this study (the~~

~~European Alps)~~. Particularly, the mass absorption efficiency of dust [...]" and to discuss the choice of MAE, we added the following discussion in section 4.1: "**The MAE of dust used in this study was slightly lower than initial values used in previous studies in the framework of a seasonal alpine snowpack at intermediate elevations (Tuzet et al., 2020; Réveillet et al., 2022). The reliability of this parametrization was assessed by the evaluations of section 3.1 and the evaluation of the modelled snow albedo in Appendix Fig. 3. Such lower MAE implicitly accounts for the probable aggregation of LAP (likely due their high concentrations during long melt seasons) that lowers the MAE (*e.g.*, Bond, Bergstrom (2006); Schwarz et al. (2013)).**"

The numeric values, concerning the results, of Sections 2.3.5, 3.1, 3.2, 3.3, 4, 4.1, 4.2, 4.3 have been modified (highlighted in the track change version attached to this response). Figures 3, 4, 5, 6, 7, 8 and Appendix Fig. D1, E1, E2, E3 have also been modified accordingly. In section 4.2, the word "almost" was removed. In section 4.3, CPU costs were adjusted to account for the addition of the sensitivity tests.

2. We modified the unit of the third row of Figure E1, from "m w.e." to "**%**".

3. The total melt was computed as the difference between the minimum and the maximum of SWE during the year, it is now computed accordingly to the formulation stated in section 2.5 of the manuscript, *i.e.*, as the sum of every snow or ice melt of the year.

4. We cited a recent article in the introduction (Menounos et al., 2025) dealing with glacier and LAP: "By lowering the albedo, LAPs contribute to significantly accelerate snow and glacier melt around the world, *e.g.*, [...] **Menounos et al. (2025)**."

5. All k_{BC} were adjusted to $k_{\mathbf{BC}}$.

6. In the Conclusion section, **"This year,"** was added to link to the previous sentence.

7. In the Acknowledgements section, **"Ghislain Picard"** was added.

8. Code and data availability have been revised, now providing access to the code repository, and forcing files in a Zenodo repository.

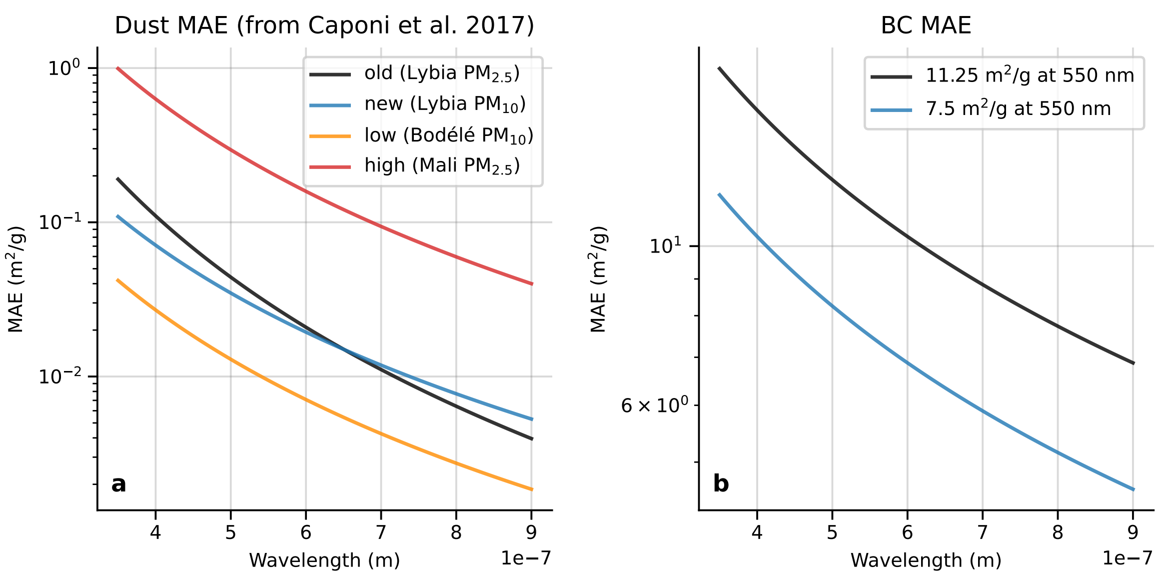

Figure 5: Numerical values used for mass absorption efficiencies (MAE) for both dust and BC. New and old values are respectively the ones of the revised and the initial version. The MAE of dust are from Caponi et al. (2017) with couples [$\text{MAE}_{\text{dust}}(\lambda_0)$; AAE_{dust}] being respectively equal to [$110 \cdot 10^{-3}$ m^2/g; 4.1], [$71 \cdot 10^{-3}$ m^2/g; 3.2], [$27 \cdot 10^{-3}$ m^2/g; 3.3] and [$630 \cdot 10^{-3}$ m^2/g; 3.4] for old, new, low and high parametrization. The old BC MAE was an enhanced MAE of 11.25 m^2/g at 550 nm previously used in Tuzet et al. (2020) to "implicitly account for the potential absorption enhancement due to internal particle mixing or particle coating". The new BC MAE of 7.5 m^2/g at 550 nm for BC is a non-enhanced MAE close to the external-mixing of BC used in SNICAR (Flanner et al., 2021).

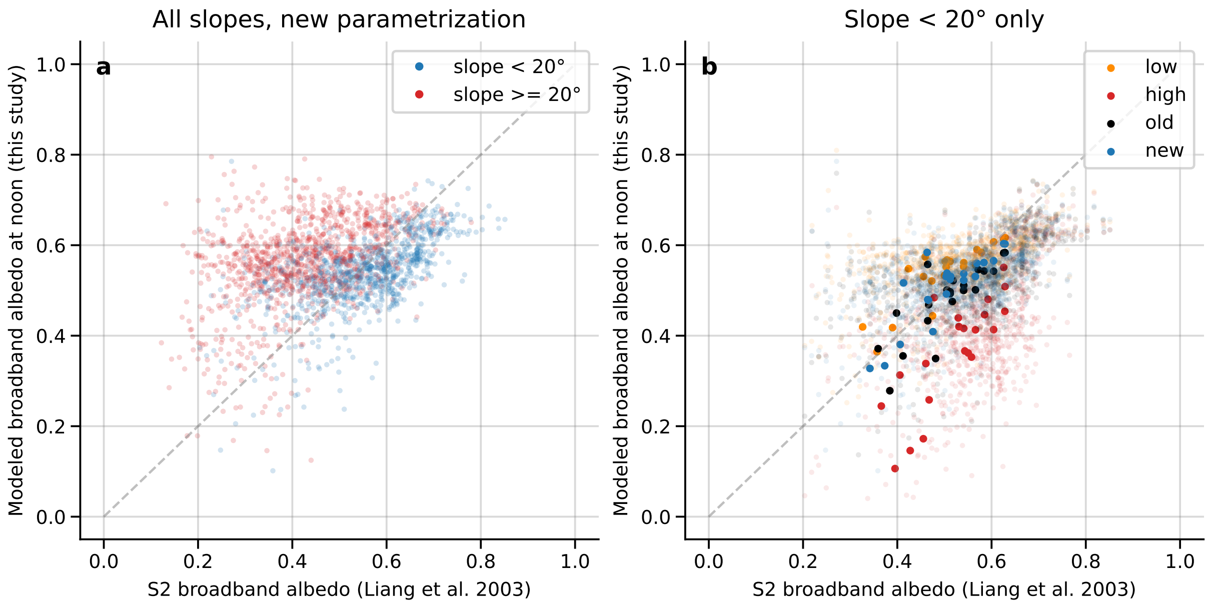

Figure 6: Comparison between modelled broadband albedo and observed broadband S2 albedo using the method of Liang et al. (2003) (S2 albedo $= 0.356 * R_2 + 0.130 * R_4 + 0.373 * R_5 + 0.085 * R_{11} + 0.072 * R_{12} - 0.018$ where R_i is the reflectance of Sentinel-2 band i). All S2 dates mentioned in Table B2 were used. Only the points where the surface type was snow for both the model and S2 were used. a) Using the new MAE parametrization of the model, blue point are for slopes $\leq 20°$, and red points for slopes $> 20°$. b) Using only slopes ≤ 20 ° for the different model MAE parametrizations found plotted in Fig. 7. Large opaque dots are the means for each date.

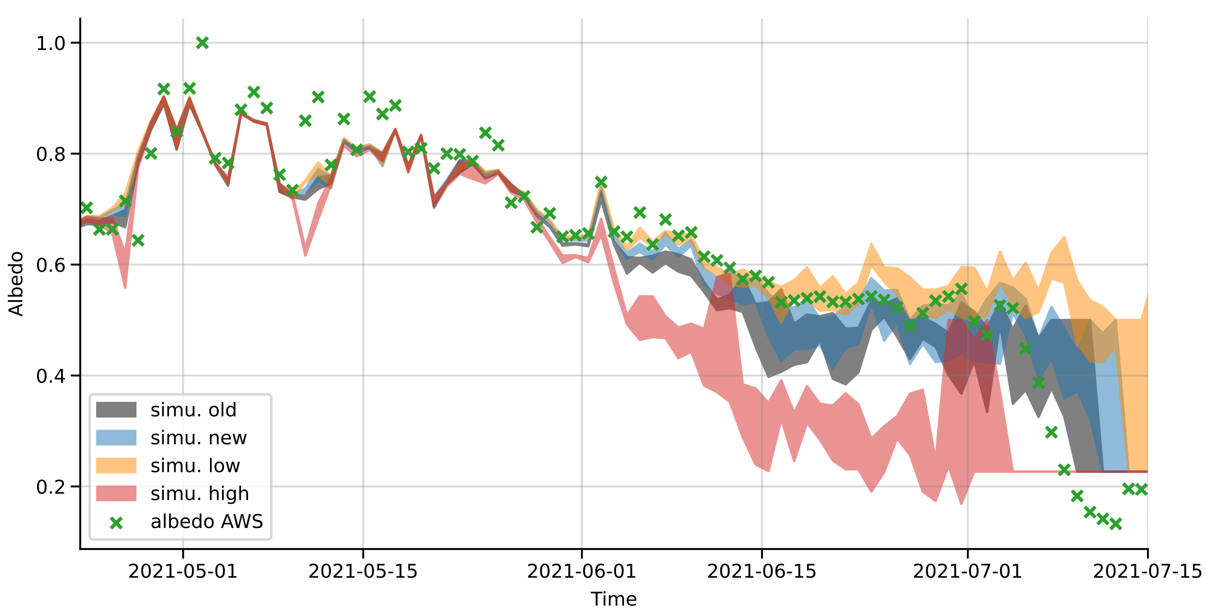

Figure 7: Albedo at the AWS glacier location. Albedo AWS (green crosses) is the measured albedo at the AWS computed as the ratio of reflected and incoming shortwave radiation between 9:00AM and 15:00PM. The simulated albedo (coloured contour envelopes) are the albedo values at noon plotted for the 4 simulation points near the AWS glacier.

		stakes summer mean error				stakes summer RMSE				vs. Sentinel-2		vs. AWS	
$z_{0,\mathbf{snow}}$ **(mm)**	**MAE dust, MAE BC**	2019	2020	2021	2022	2019	2020	2021	2022	snow albedo	surface type	snow albedo	SMOD
0.1	high, new	1.12	0.94	0.97	1.08	1.30	1.08	1.09	1.38	too low	too much ice	too low	5–6 d early
0.1	old, old	0.67	0.28	0.40	0.60	0.89	0.52	0.56	0.97	too low	OK	too low	2 d late
0.1	new, new	0.49	0.10	0.25	0.46	0.74	0.43	0.45	0.86	OK	more snow	OK	4 d late
0.2	new, new	0.57	0.19	0.31	0.57	0.78	0.44	0.48	0.90	OK	OK	?	?
0.05	new, new	0.33	-0.08	0.12	0.24	0.69	0.47	0.38	0.84	OK	more snow	?	?
0.1	low, new	0.33	-0.16	0.02	0.28	0.63	0.44	0.31	0.76	too high	too much snow	OK	6–9 d late

Table 1: Summary of the evaluations for different parametrizations. See Fig. 5 for the numeric MAE values. Melt evaluation against the stakes measurements (in m w.e.) and surface type evaluation against S2 are the metrics initially used in Figure 3 of the manuscript. SMOD stands for snow melt-out date. Evaluations, except evaluation with stakes, are qualitative. Question marks stand for uncalculated metrics.

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