#Reply to editor

Dear McKenzie,

Thank you very much for your valuable comments. I wrote reply to your comments in blue.

Your comments: ‘The community has largely moved away from ’LAI’ in recent years, both because of the conflict with leaf area index and because ’impurity’ inaccurately implies contamination for particles (dust, algae, microbes) that are frequently natural or common (i.e. weathering from adjacent landscapes) and can even be intrinsic to snow formation. Light absorbing particles (LAPs) or constituents (LACs) are more neutral and accurate.’

> I have replaced all occurrences of ”impurity” with ”particles” and ”LAI” with ”LAP” throughout the manuscript including title. Only in Line 635, I retained the original wording because replacing it with ”particles” might make the meaning unclear (”in future high-impurity Asian mountain glaciers, …”).

Your comments: Also please do not use the term bioalbedo - biological growth enhances light absorption (i.e. the more accurate term is bio-co-albedo), the recommendation for the term is ’biological albedo reduction’ (BAR)

> I have replaced all occurrences of ”bioalbedo” in the Introduction with ”biological albedo reduction.” Line 69 in the manuscript with tracked changes

Our data are saved in Zenodo, and are already published (not restricted).

Reply letter to reviewer #1

Thank you for reviewing our manuscript and your helpful comment. We write our reply in blue to your specific comment.

After revising our manuscript, we write our reply in red to your specific comment. The revised text used in the main manuscript is written in Times New Roman. The line numbers indicate the line numbers in the manuscript with revision history.

This study primarily focuses on Ice albedo and its relationship with light-absorbing impurities and weathering crust. However, only Section 3.5 is closely aligned with the research theme, which appears somewhat insufficient.

> Thank you for your comment. Following Reviewer #2's comment, we revised the multiple regression analysis so that the albedo can be estimated when OM and GIT are zero (MD was not statistically significant). As a result, the albedo of clean ice (i.e., in the absence of OM and GIT) was estimated to be 0.48. We will also add a discussion of this value in the revised manuscript.

> In the previous manuscript, the explanatory variables in the multiple regression analysis were transformed as log 10 (X); however, since log 10 (X) is undefined when X=0, we instead used log 10 (X+A), where A is a nonzero constant. The ice albedo under zero impurity concentration and zero GIT (0.48) was then estimated from the regression equation obtained by the multiple regression analysis. And also we added following discussion at Line 601 – 605. 'This value is 0.14 higher than the commonly reported albedo of clean ice (0.34) (Oerlmans and Knap, 1998). It represents the albedo of ice in the absence of both a granular layer and organic matter; however, the underlying ice beneath the granular layer contains abundant air bubbles (Fig. 4). Therefore, we infer that enhanced scattering by these bubbles leads to the higher albedo.'

In contrast, Section 3.1 presents the characteristics of ice temperature and ice density of the glacial surface weathering crust. Although interesting, this content is only weakly related to the main theme of the study—unless it serves as a basis for determining the thickness of the weathering crust. If not, it is recommended to move this section to the Appendix.

> Several previous studies have focused on the ice density profiles of the weathering crust. In contrast to these studies, we propose a different physical parameter of the weathering crust, namely GIT (granular ice thickness). The surface GIT of the weathering crust is a key variable that characterizes its properties, particularly in subsequent analyses of albedo. Therefore, we consider that it should not be placed in the Appendix.

>I did not move the Section 3.1 to Appendix. The reason is written above in blue.

Furthermore, Section 3.6 discusses a large amount of content regarding the relationship between LAIs and GIT, which is also weakly connected to the main focus of the study and should be condensed.

>We agree that Section 3.6 does not directly discuss ice albedo. However, the relationships between each LAPs (replaced LAI to LAP according to editor's suggestion) and GIT (an indicator of the weathering crust) are closely linked to variations in ice albedo. Reviewer #2 suggested the possibility that OM and BC share a common source; however, we currently have no idea about their common source, and therefore we have not added further discussion on this point.

>We have not condensed Section 3.6. The reason is written above in

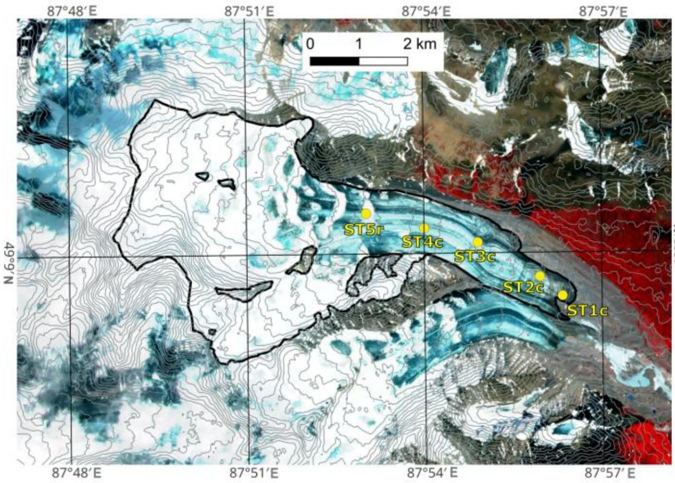
blue.

Alternatively, the title of this study could be revised to "Characteristics of the Glacier Surface Weathering Crust and Its Impact on Glacier Albedo."

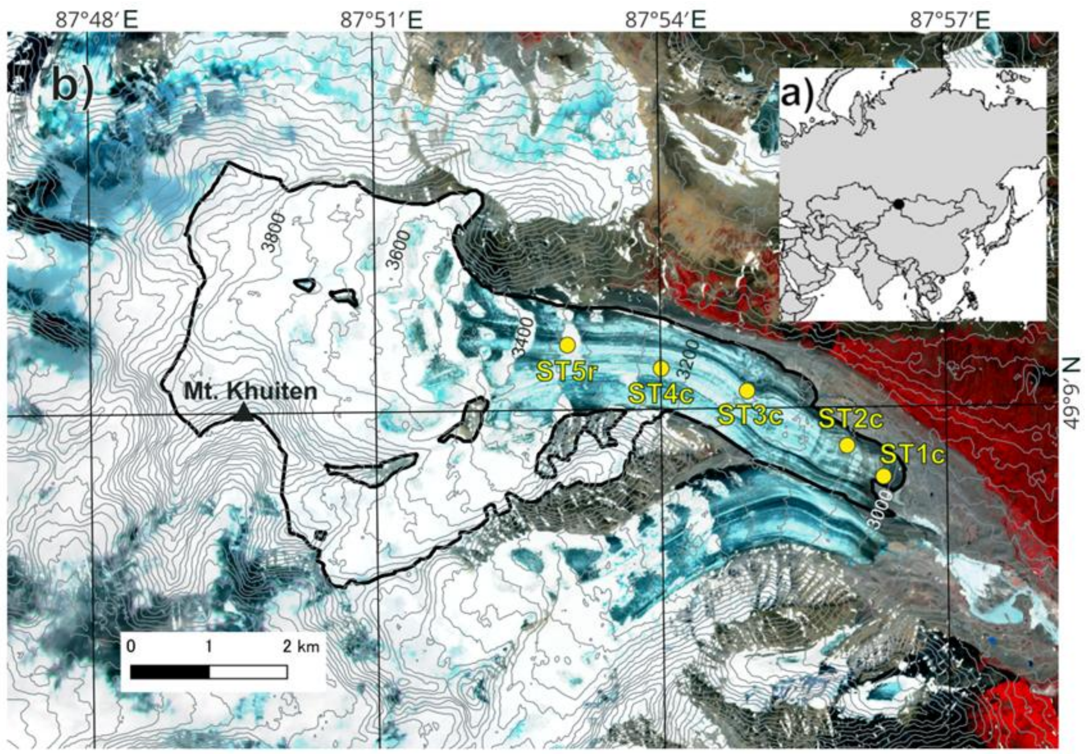
>Thank you for the suggestions about title. Our measurements of three 'light-absorbing impurities' (BC, mineral dust, organic matter) are also important for this manuscript as discussed in the section 3.6. And we thought location name are necessary in the tile. Therefore, we revise the title of our paper as "Characteristics of glacier surface weathering crust and light-absorbing impurities, and their combined impact on glacier albedo at the Potanin Glacier, Mongolia"

> We revised the title "Characteristics of glacier surface weathering crust and light-absorbing particles, and their combined impact on glacier albedo at the Potanin Glacier, Mongolia".

In Figure 1, I suggest that authors add essential geographic information, including contour lines and latitude/longitude coordinates.
>I will add contour and lat/lon coordinates as follows.



>I have revised the Fig. 1 and added lat/lon and contour line. And added contour line and explanation of stake location in the caption at Line 135.



Line 155-160: When measuring broadband albedo, what is the observation height of the pyranometer, and approximately what ground area does it cover?

> The height of the pyranometer is about 20(a) cm. The paired pyranometers receive shortwave radiations from the glacier surface hemisphere and the atmospheric hemisphere. Since the observation targets a uniform circle with a radius of 50(b) cm as much as possible, when the projected solid angle is considered geometrically, b2/(a2+b2) the target circle occupies approximately 0.86 to the projected solid angle of the hemisphere.

I will add the above description.

>I have added at Line 181-186, Page7 "During the albedo measurements, the height of the radiometer was about 20(: a) cm above the ice surface. The paired pyranometers receive shortwave radiation from the glacier surface hemisphere and the atmospheric hemisphere. Since the observation targets a uniform circle with a radius of 50(: b) cm as much as possible, when the projected solid angle is considered geometrically, $b^2/(a^2+b^2)$, the target circle occupies approximately 0.86 of the projected solid angle of the hemisphere."

Line 421-422: "In both dark and clean surfaces, the albedo at the granular ice surface was lower than at the ice surface below the granular ice layer at $\lambda > 850$ nm." This statement is inconsistent with Figure 6. Should it be $\lambda < 850$?

> This is my mistake. Thank you for your pointing out. I will revise it.

> Revised at L464 Page 17. '$\lambda < 850$ nm'

Line 485-486: "Table 1 summarizes the correlation coefficient between BB albedo and each LAI and WC thickness", should it be Table 2? Similarly, Line 510-511: "Multiple regression analysis results with BB albedo as the objective variable and impurity and

GIT as the explanatory variables are summarized in Table 2”, should it be Table 3?

> These are my mistakes. Thank you for your pointing out. I will revise them.

>I have revised Line 537 Page 20 Table 2, Line 564 Page 21 Table3

Line 527: “BB albedo was obtained using equation (10), and RMSE was 0.08.” should it be equation (1)?

> This is my mistake. Thank you for your pointing out. I will revise it.

>I have revised Line 600 Page 22 equation (1).

Line 515-516: the t-value is positive for MD in 2022 and the whole year from 2022 to 2024, please explain the possible reasons.

> Thank you for your comment. We acknowledge that this point was not sufficiently discussed in the manuscript.

In both 2023 and 2024, the *t*-values for MD were negative, indicating that higher MD was associated with lower albedo, which is consistent with the general relationship expected for light-absorbing impurities. However, the corresponding *p*-values were large, indicating that these relationships were not statistically significant. In contrast, the *t*-value for MD in 2022 was positive, and the corresponding *p*-value was marginally significant ($p = 0.047$). Overall, these results suggest that MD does not make a clear or consistent contribution to albedo reduction.

Two possible explanations can be considered. First, mineral dust contains not only dark minerals but also relatively bright minerals such as quartz, which are expected to have a weaker effect on reducing albedo than other light-absorbing impurities. Second, the surfaces of mineral particles may be coated with organic matter, such as humic substances, thereby masking the direct influence of

the minerals on albedo.
We also plan to include this discussion in the revised manuscript.

＞We revised the multiple regression method in response to Reviewer #2’s comments. As a result, the year-by-year regression results changed slightly, and MD was not statistically significant in any year. Overall, the results still indicate that MD does not contribute to albedo reduction, which is consistent with the original manuscript. We also added two possible explanations for why MD may not affect albedo reduction as follows at Line 581-587 Page 22.

‘Overall, these results suggest that MD does not make a clear or consistent contribution to albedo reduction. Two possible explanations can be considered. First, mineral dust contains not only dark minerals but also relatively bright minerals such as quartz, which are expected to have a weaker effect on reducing albedo than other light-absorbing particles. Second, the surfaces of mineral particles may be coated with organic matter, such as humic substances, thereby masking the minerals' direct influence on albedo.’

Reply letter to reviewer #2

Thank you for reviewing our manuscript and your helpful comment. We write our reply in blue to your specific comment.

After revising our manuscript, we write our reply in red to your specific comment. The revised text used in the main manuscript is written in Times New Roman. The line numbers indicate the line numbers in the manuscript with revision history.

Specific comments:

1. Abstract: The current abstract is too qualitative. I suggest including some quantitative descriptions of the key results.

   ＞We will add to the abstract the correlation coefficients between BB albedo and log10-transformed GIT ($r = 0.41$) and OM ($r = -0.75$).

   >I have revised the part of the abstract Line 33-35 "In situ measurements of BB albedo showed a significant positive correlation with the thickness of the granular ice layer (log10-transformed; $r = 0.41$, $p < 0.001$), and a relatively strong negative correlation with organic matter (log10-transformed; $r = -0.75$, $p < 0.001$)." The number of words of the abstract is 230 (<250).

2. Page 2, Line 49: There are many other key factors (e.g., snow grain shape, snowpack thickness, snow microstructure, and snowpack density) that can affect snow albedo, which are not included here. I would suggest replacing the "are" in "Important factors … are the size of …" with "include" or mention the other factors as well.

   ＞Thank you for the comments. I will add those factors.

   >I added those factors as follows at Line 58, page 2 "Important

factors that determine the albedo of a snow surface are the size of the snow particles, snow grain shape, snowpack thickness, snow microstructure, snowpack density and the concentration of light-absorbing particles (LAPs)—….".

I replaced LAI to LAP and impurities to particles in all manuscript according to editor's suggestion.

3. Page 2, Line 60: "bioalbedo is more effective" This part is not clear. How is the effectiveness defined? Did the authors mean causing larger albedo changes per unit mass of particles? Please be accurate.

   > I plan to add below description.

   "The authors presented a new model, "BioSNICAR," which combines cell-specific optical properties with radiative transfer theory to quantify "bioalbedo." Using this model, they simulated how cell size and pigment composition influence reflectance spectra and established a method to isolate biological effects from the intrinsic properties of ice and inorganic impurities (e.g., dust and black carbon). Then, they demonstrated that bioalbedo significantly contributes to albedo reduction."

   > I have replaced following sentences at Line 68-73 Page 2
   "The authors presented a new model, "BioSNICAR," which combines cell-specific optical properties with radiative transfer theory to quantify biological albedo reduction. Using this model, they simulated how cell size and pigment composition influence reflectance spectra and established a method to isolate biological effects from the intrinsic properties of ice and inorganic impurities (e.g., dust and black carbon). Then, they demonstrated that biological particles significantly contribute to albedo reduction."

   # I replaced bioalbedo to biological albedo reduction in all manuscript according to editor's suggestion.

4. Page 2, Line 71: "… LAI are important factors increasing albedo …" This is a bit confusing to me. I thought LAI decreases snow and ice albedo rather than increasing it.

   >This is my mistake. I will revise as 'Weathering crust and LAI are important factors changing albedo in glacier ablation areas'

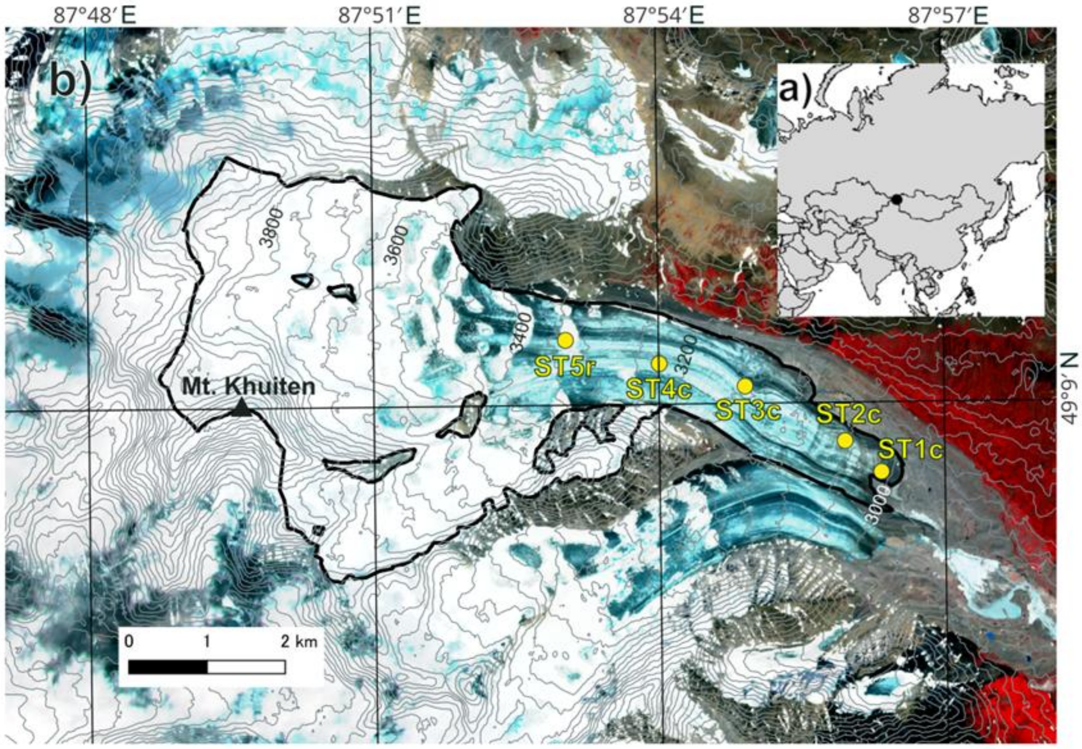
   >We have revised the Line 86, Page 3 as "Weathering crust and LAP are important factors changing albedo in glacier ablation areas".

5. Figure 1: What do different colors represent? Please clarify them in the figure caption.

   >Since meteorological data are not used in this study, the AWS sites shown in blue have been removed. Only the locations of the stakes relevant to this study, indicated by yellow labels, are retained. In addition, following Reviewer 1's comment, contour lines and latitude/longitude have been added to the map. On the caption of Fig. 1 I will revise as follows. "Location map of the Potanin Glacier a) and observed stake location at the Potanin Glacier b). The edges of the Potanin glacier are outlined in black. Yellow numbers indicate the location of stake, sampling site. The back satellite imagery is Sentinel 2 (False color) acquired on 15 July 2023."

   >We added some sentences at caption of Fig. 1 (Line 134-135 Page 5) as follows. 'Contour lines are drawn at 50 m intervals. Yellow numbers indicate the location of stake, sampling site.'

   # I have added contour lines according to reviewer #1.



6. Section 2.2.2: (1) What is the measurement accuracy/uncertainty for BB albedo? What times of each day were those BB measurements conducted? Were they around local noon? Albedo measurements under high solar zenith angle may introduce large uncertainties.

   >The uncertainty of the broadband (BB) albedo observations is defined as the standard deviation of five repeated measurements at the same site. The average standard deviation across all observations was 0.019. As this is derived from the observational results, we plan to add this information to Section 3.2.

   >We have added 'The standard deviation was used as the uncertainty of BB albedo.' at Line 179 in Section2.2.2, Page 7. And in section 3.2 we have added the value of uncertainty ±0.019 in addition to average value at Line 395 Page 14.

Albedo measurements were carried out between 11:00 and 15:30 local time, during which the solar zenith angle ranged from 26° to 45°. This description will be added to Section 2.2.2.

> We added 'Albedo measurements were carried out between 11:00 and 15:30 local time, during which the solar zenith angle ranged from 26° to 45°.' at Line 180-181 in section 2.2.2 Page 7.

(2) What is the measurement accuracy/uncertainty for those LAI species?

>For MD and OM, there is some uncertainty associated with the mass measurements themselves; however, this is considered negligible compared to the uncertainty arising from the estimation of the sampling area. Sampling was conducted over an approximately 150 × 150 mm area on the ice surface. Assuming an uncertainty of ±2.5 mm in each side length, the sampled area ranges from 21025 (145 × 145) mm^2 to 24025 (155 × 155) mm^2, resulting in an uncertainty of approximately ±7.0% in the mass per unit area. The same applies to BC. Measurements BC using the SP2 (Single Particle Soot Photometer) provide concentrations in water; these are converted to areal units ($\mu g\ m^{-2}$) by multiplying by the sampled water weight and dividing by the sampling area. As the sampling area is the dominant source of uncertainty in this conversion, the resulting uncertainty is likewise estimated to be approximately ±7.0%.

I will add the above description in the section 2.2.2.

> We have added at Line 204-209, Page 7-8 as follows. 'For MD and OM, there is some uncertainty associated with the mass measurements themselves; however, it is considered negligible compared to the uncertainty arising from estimating the sampling area. Sampling was conducted over an area of approximately 150 × 150

mm on the ice surface. Assuming an uncertainty of ±2.5 mm in each side length, the sampled area ranges from 21,025 (145 × 145) mm^2 to 24,025 (155 × 155) mm^2, resulting in an uncertainty of approximately ±7.0% in the mass per unit area.'.

And added at Line 236-238, Page 8 'Regarding the uncertainty of the BC amount per unit area, the sampling area is the dominant source of uncertainty in this conversion. Therefore, the uncertainty is estimated at ±7.0% for MD and OM. '

7. Section 2.2.3: What is the accuracy/uncertainty for ablation rate measurements?

   >Stake mass balance measurements were carried out at every 5 days. If The error of scale reading and surface roughness was assumed to be 1cm, the ablation measurement error was 20mm/5days, = 2 mm/day. I will add the uncertainty in the Section 2.2.3.

   >In our previous response, we considered only the uncertainty in ablation height and did not convert it into water equivalent. We have added the text as follows at Line 251-253, Page 9. "If the errors in scale reading and surface roughness are assumed to be 10 mm, the ablation measurement error is 20mm×900 kg m^{-3}/1000 kg m^{-3}/5days, = 3.6 mm w.e./day, taking into account that the measurements were conducted every five days."

8. Section 2.2.4: What is the accuracy/uncertainty for spectral albedo measurements?

   >The uncertainty of the spectral albedo is defined as the standard deviation of five repeated measurements at each site. We plan to show the standard deviation as a gray shaded area in Fig. 6, and to indicate in the caption that the gray region represents the standard deviation (STD).

   >The uncertainty of the spectral albedo measurements was

estimated as the standard deviation of five repeated measurements at the same location. We originally planned to illustrate this uncertainty as a gray shaded region in Fig. 6. However, the standard deviation was sufficiently small that the shaded region almost completely overlapped the mean spectral albedo curve and was therefore not visible. To make the uncertainty clearly visible, we plotted the standard deviation against a separate right-hand y-axis with an independent scale. We also added a description of the standard deviation to the figure caption at Line 479-480, Page 16.

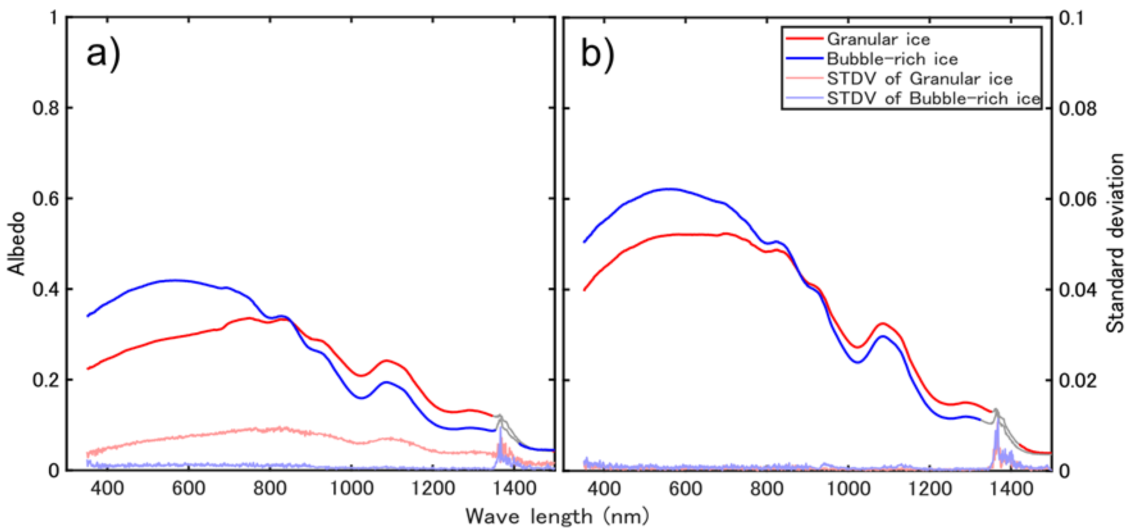


Figure 6. Spectral albedo observed on a) dark and b) clean surfaces at ST2c. Red lines indicate the spectral albedo at the surface of the granular ice. Blue lines indicate bubble-rich ice surface removed from surface granular ice. The standard deviations are plotted on the right y-axis using the corresponding light colors. Physical characteristics of granular ice and the amount of LAPs are summarised in Table S1.

9. Section 2.2.5: What is the accuracy/uncertainty for humic

substance measurements?

>As repeated measurements were not possible for this sample due to limited sample volume, the uncertainty is estimated based on values reported in the literature. We plan to add the following statement at the end of Section 2.2.5.

"The reproducibility of this determination methods of humic fractions based on coefficient of variation (CV; n = 5) is < 6%, while CV for log(A400/A600) and A600/C is 0.2% and 2.5%, respectively (Watanabe et al., 2007)."

And add the following paper in the reference list.

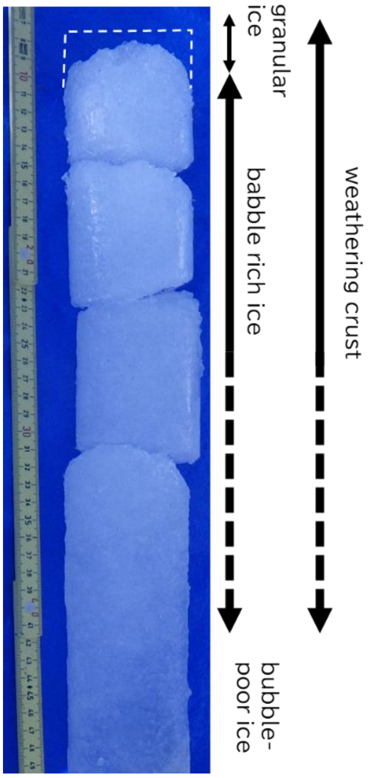
Watanabe, A., Kawasaki, S., Kitamura, S. and Yoshida, S.: Temporal changes in humic acids in cultivated soils with continuous manure application, Soil Sci. Plant Nutr., 53, 535–544, https://doi.10.1111/j.1747-0765.2007.00170.x., 2007.

>We have added following sentence at the end of section 2.2.5. (Line 305-308, Page 10) "The reproducibility of this determination method for humic fractions, based on the coefficient of variation (CV; n = 5), is < 6%, while the CVs for log(A400/A600) and A600/C are 0.2% and 2.5%, respectively (Watanabe et al., 2007)."

And added Watanabe et al. (2007) in the reference list in Line 961-963.

10. I would suggest the authors include one photo for the weathering crust and its structure as an additional panel in Figure 1.

> I will add the photo below as panel (d) in Fig. S1. I will also add the following caption. “A core of weathering crust collected near ST2c on 30 August 2024. Because the fragile the granular ice layer disintegrates during ice coring with an ice auger and is not preserved in the recovered core, its original position is indicated by a white dashed line in the photograph.”



>You suggest that photo for the weathering crust should be add in Fig. 1. But, Fig. 1 is map of glacier. Then, I thought that Reviewer would like to suggest the photo should be add in Fig. S1. I have inserted the above photo, which include core structure, in Fig. S1 and added the caption “d) A core of weathering crust collected near ST2c on 30 August 2024. Because the fragile the granular ice layer disintegrates during ice coring with an ice auger and is not preserved in the recovered core, its original position is indicated by a white dashed line in the photograph.”

11. Figure 3: How did the authors determine the bottom depth of weathering crust layer based on the ice density profiles? The current bottom interface seems to have same (or similar) ice density as the layer below.

>As we may not have fully understood the reviewer's intent, we are unsure whether this adequately addresses the question; however, the granular ice layer has a relatively low density, whereas the underlying ice, although rich in bubbles, has a density closer to that of solid ice, as shown in the figure. Therefore, the distinction between the granular ice layer and the underlying bubble-rich layer is clear.

> Above fragile granular ice layer is clearly different from the bubble rich ice layer under the granular ice layer, then we can identify the granular ice layer as written in the 'the boundary is clearly identified using a scoop.' at Line 350-351, Page 12.

12. Page 15, Line 422: Should it be "lambda < 850nm"?

>It's my mistake. Thank you for your pointing out.

> I have revised at Line 464 Page17.

13. Section 3.4: Could the authors provide some insights on how these different types of humic acids were formed?

>We will add following sentence in the last of section 3.4

"Such humic acids may have accumulated due to slow decomposition of organic matter from plant or microbial origins with slow progression of humification. Low log(A400/A600) could be attributed to a pigment, although it was not a green one typical of Type (data not shown). Chromophore of the green pigment, 4,9-dihydroxyperylene-3,10-quinone, is substituted with carboxy groups and/or hydroxyl groups and linked with aromatic or aliphatic moieties in humic acids and comprises the refractory fraction in humic acids (Ikeya et al., 2013). Although the pigment in the present humic acids is still unknown, it might also be enriched due to its recalcitrance."

And add the following reference

Ikeya, K., Sleighter, R.L., Hatcher, P.G., and Watanabe, A.: Fourier transform ion cyclotron resonance mass spectrometric analysis of the green fraction of soil humic acids, Rap. Commun. Mass Spectrom., 27, 2559–2568, https://doi.org/10.1002/rcm.6718, 2013.

>I have revised as above at Line 505-512, Page 19. "Such humic acids may have accumulated due to slow decomposition of organic matter from plant or microbial origins with slow progression of humification. Low log(A400/A600) could be attributed to a pigment, although it was not a green one typical of Type (data not shown). Chromophore of the green pigment, 4,9-dihydroxyperylene-3,10-quinone, is substituted with carboxy groups and/or hydroxyl groups and linked with aromatic or aliphatic moieties in humic acids and comprises the refractory fraction in humic acids (Ikeya et al., 2013). Although the pigment in the present humic acids is still unknown, it might also be enriched due to its recalcitrance."

And added reference Ikeya et al., 2013 at reference list in Line 851-853, Page 32.

14. Figure 8: It is interesting to see both the relationship between albedo and granular ice thickness as well as the dilution effect of dust concentration. Is the albedo increase more contributed by the dilution of dust concentration or the enhanced scattering from thicker granular ice?

>It is very interesting question. We expect that the use of a

radiative transfer model in future work will enable a quantitative assessment of whether the observed effect is due to concentration dilution or scattering by ice.

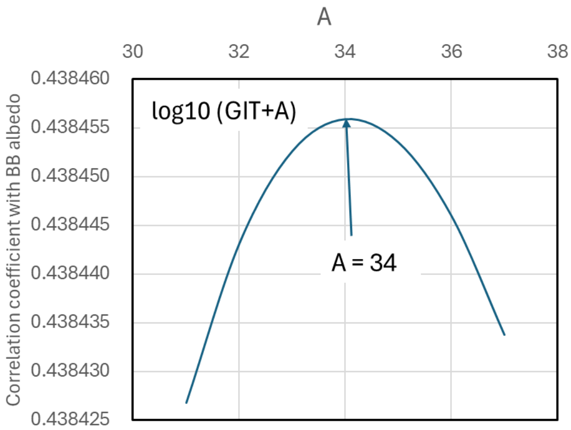
> This response differs from our previous one. Regarding the question, “Is the albedo increase more attributable to the dilution of dust concentration or to enhanced scattering from thicker granular ice?”, the results of the multiple regression analysis provide one answer. In the analysis, MD was not statistically significant, whereas GIT was statistically significant. This indicates that the increase in albedo is associated with GIT, rather than MD. The results of the multiple regression analysis are already described and discussed in the main text.

15. Equation (1): Why did the authors use logarithm form for all predictors, which does not work for zero concentration cases?

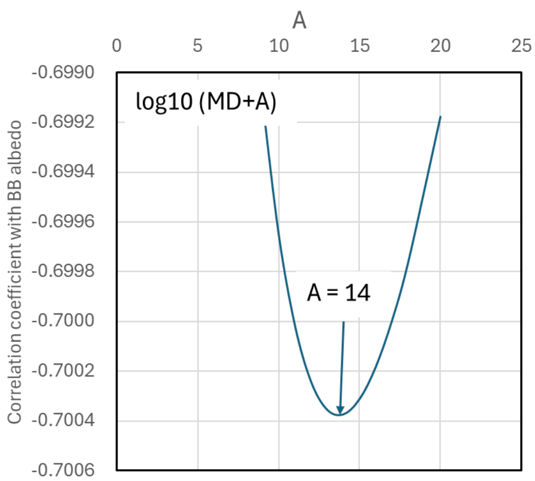
> Because multiple regression analysis assumes a linear relationship between the dependent and independent variables, we must applied a logarithmic transformation to the explanatory variables, namely impurity concentration and GIT.

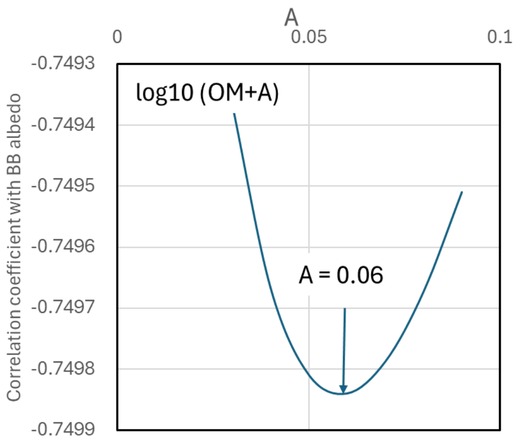
We agree that it is problematic, as you pointed out, that the multiple regression equation becomes invalid when the explanatory variables take a value of zero. Therefore, for the explanatory variables MD, OM, and GIT, we adopted a transformation of the form log(X+A) instead of log(X), where

X is MD, OM, or GIT, and A is a non-zero constant. The value of A was determined by examining the correlation between BBalbedo and log(X+A), and selecting the value that maximizes the absolute value of the correlation coefficient (below



figures). (Below figures will be add as supplement.)





The multiple regression analysis show that MD was not statistically significant, as in the previous analysis.

BBalbedo=0.215643×$\log_{10}$(GIT(mm)+34)-0.14461× $\log_{10}$(OM(g/m2)+0.06)-0.02629. eq.(1)

The RMSE was 0.08. If both GIT and OM were zero, the multiple regression equation predicts a BB albedo of 0.48. This value is 0.14 higher than the commonly reported albedo of clean ice (0.34) (Oerlmans and Knap, 1998). It represents the albedo of ice in the absence of both a granular layer and organic matter; however, the underlying ice beneath the granular layer contains abundant air bubbles (Fig. 4). Therefore, we infer that enhanced scattering by these bubbles leads to the higher albedo.

We conducted separate multiple regression analyses for each year, as presented in Table 3. We will also revise the analyses by adopting the log(X + A) transformation described above.

Reference

Oerlemans J, Knap WH. A 1 year record of global radiation and albedo in the ablation zone of Morteratschgletscher, Switzerland. Journal of Glaciology. 1998;44(147):231-238. doi:10.3189/S0022143000002574.

> We modified the multiple regression method as described in the previous reply. Although the regression results for each year changed, the combined analysis using data from 2022–2024 yielded the same result as before, with GIT and OM remaining statistically significant. We have changed the description about multiple linear regression analysis. We have changed the description from Line 563 -605 Page 21-22.

"A multiple linear regression analysis was performed with BB albedo as the dependent variable and impurity concentration and GIT as explanatory variables (Table 3). Multiple linear regression assumes that, with the other explanatory variables held constant, the relationship between the dependent variable and each explanatory variable is linear. In this study, the relationships between BB albedo and both impurity concentration and GIT were non-linear; therefore, the explanatory variables were log-transformed to achieve linearization. Because log10(X) is undefined when the impurity concentration or GIT equals zero, the explanatory variables were transformed as log10(X+A), where X denotes the impurity concentration per unit area or GIT, and A is a constant set for each explanatory variable. The value of A was varied to maximise the absolute value of the correlation coefficient between log10(X+A) and BB albedo. Multiple regression analysis results with BBalbedo as the objective variable and impurity and GIT as the explanatory variables are summarised in Table 2. Here, all explanatory variables are in logarithmic form. In terms of p-values, only GIT was statistically significant in 2022, GIT and OM were statistically significant in 2023, and only OM was statistically significant in 2024. all explanatory variables (GIT, MD, OM) are statistically significant in 2022. Only OM remains statistically significant in 2023 and 2024. MD was not statistically significant in any year. In addition, if MD acted as a light-absorbing impurity, the t-value would be expected to be negative; however, it was positive in 2022. As for the t-value, which should be negative when the explanatory variables have an inverse correlation with the objective variable, it is positive for MD in 2022 and 2024. The multiple regression analysis for the whole year's data, MD, was not statistically significant. Overall, these results suggest that MD does not make a clear or consistent contribution to albedo reduction. Two possible explanations can be considered. First, mineral dust contains not only dark minerals but also relatively bright minerals such as quartz, which are expected to have a weaker effect on reducing albedo than other light-absorbing impurities. Second, the surfaces of mineral particles may be coated with organic matter, such as humic substances, thereby masking the minerals' direct influence on albedo.

The most closely related explanatory variable to BB albedo was OM, followed by GIT, based on the t-values. The following equation is the result of a multiple regression analysis using the data from 2022 to 2024.

$$\mathrm{BBalbedo}=0.2156\times\log_{10}(\mathrm{GIT(mm)}+34)-0.1446\times\log_{10}(\mathrm{OM(g/m2)}+0.06)-0.0263. \quad (1)$$

Each GIT and OM has a limit on the available range as follows.

$5 \leq GIT \leq 145$ (mm), $0.024 \leq OM \leq 23.92$ (g m^{-2})

BBalbedo was obtained using equation (1), and RMSE was 0.08. If both GIT and OM were zero, eq. (1) predicts a BB albedo of 0.48. This value is 0.14 higher than the commonly reported albedo of clean ice (0.34) (Oerlmans and Knap, 1998). It represents the albedo of ice in the absence of both a granular layer and organic matter; however, the underlying ice beneath the granular layer contains abundant air bubbles (Fig. 4). Therefore, we infer that enhanced scattering by these bubbles leads to the higher albedo."

And Table 3 was also revised as follows.

Table 3. Summary of multiple regression analysis when the objective variable is BB albedo and the explanatory variables are GIT, MD, OM and BC. All explanatory variables are 10logarithmic. R^2 denotes the coefficient of determination, and n indicates the number of samples. A_{GIT}=34, A_{MD}=14, A_{OM}=0.06, A_{BC}=1.0.

year	R^2 (n)		log(GIT+A_{GIT}) (mm)	log(MD+A_{MD}) (g m^{-2})	log(OM+A_{OM}) (g m^{-2})	log(BC+A_{BC}) (µg m^{-2})
2022	0.57 (39)	p	3.4×10^{-5}	0.65	0.22	-
		t	4.75	0.45	- 1.24	-
2023	0.85 (44)	p	9.4×10^{-3}	0.59	2.9×10^{-7}	-
		t	2.73	-0.55	- 6.15	-

2024	0.75 (42)	p	0.27	0.44	3.8×10^{-3}	0.50
		t	1.11	-0.77	- 3.09	-0.68
2022-2024	0.60 (125)	p	6.2×10^{-3}	0.23	1.2×10^{-7}	-
		t	2.8	-1.2	- 5.6	-

16. Lines 611-623: The strong OM-BC correlation may also reflect the common emission source of BC and OM, which lead to correlated BC and OM deposition onto snow/ice surfaces.

> The correlation between OM and BC may also reflect a common emission source. However, we currently do not have an idea to demonstrate that they originate from the same source, so we prefer to leave this discussion for future work. Thank you for this helpful comment.

> As I wrote in the previous reply, I have no idea for common emission source of BC and OM. So, I did not add the discussion.