

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.

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 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.

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.

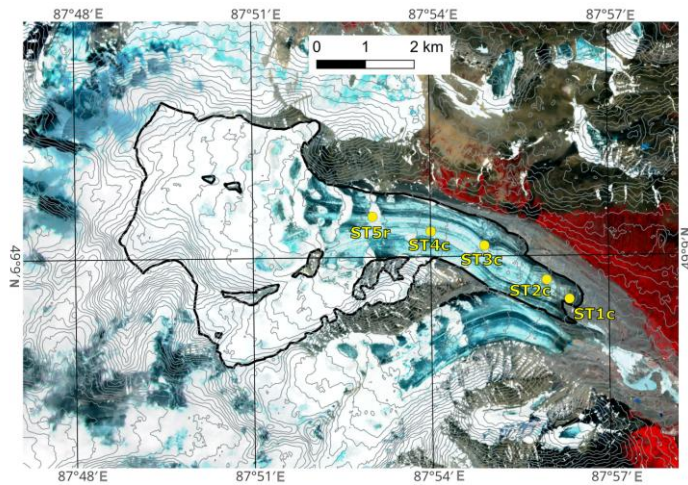
>We agree that Section 3.6 does not directly discuss ice albedo. However, the relationships between each LAI and GIT (an indicator of the weathering crust) are closely linked to variations in ice albedo. Reviewer #2 suggested the possibility that OM and BC share a common source; however, we currently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

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"

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.



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, $b^2/(a^2+b^2)$ the target circle occupies approximately 0.86 to the projected solid angle of the hemisphere.

I will add the above description.

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.

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.

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.

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.