Respond to Reviewer 1#

General comments:

The manuscript presents a study of cyanobacteria on a central Asian Glacier. The authors present biomass values for six sample locations at five time points over the course of the summer on an alpine glacier. While the results are interesting, it is difficult to assess the value of the findings due to the methods lacking clarity, specifically around how the %biomass values were determined and what they represent. Additionally, since cell count data were not included in the manuscript it is difficult to fully interpret the results. The authors attempt to connect the results to both albedo and mineral dust, but neither albedo nor mineral dust were measured in the study. Thus, the related interpretations are overstated and are not supported by the data. Specifically, a "cyanobacteria-mineral synergy" has not been demonstrated by what is presented here. Additionally, there are several statements throughout the manuscript that incorrectly describe information from the literature that must be corrected.

We sincerely thank you for your thoughtful and critical evaluation of our manuscript. Your comments have helped us identify areas where our presentation lacked clarity and where our interpretations may have been overstated. We acknowledge that our original manuscript may have placed too much emphasis on the albedo and mineral dust implications, given that we did not directly measure these parameters. However, we wish to clarify that the primary focus of our study is the spatio-temporal dynamics of phototrophic community structure on Urumqi Glacier No. 1, specifically the distinct succession from snow algae to filamentous cyanobacteria and the absence of typical glacier algae like Ancylonema. The connections to mineral dust and albedo were intended as contextual discussion based on previous studies on the same glacier, rather than as direct conclusions from our data. We have also revised the Methods section to provide clearer and more detailed descriptions of biomass quantification. According to your suggestions, we have made extensive corrections to our previous draft, the detailed corrections are listed below.

Specific comments:

Line 28 and throughout the discussion: a "cyanobacteria-mineral synergy" has not been demonstrated. There has been no characterization of the mineral dust on the ice (either composition or quantity), nor has any meaningful evaluation of the relationship between the cyanobacteria and mineral dust been completed. Simply comparing against a few dissolved ions is insufficient. The authors should either provide data that supports this claim, or remove this phrase from the manuscript.

Thank you for your comment. we have added quantitative data on mineral loads across the glacier surface during P5 (185.7-314.6 g m-2). we have removed the term "cyanobacteria-mineral synergy" throughout the manuscript and revised the corresponding discussion and conclusions to emphasize observed associations rather than causative relationships. The revised text now states that mineral-rich glacier surfaces may provide environmental conditions favorable for cyanobacteria-dominated communities, rather than asserting a demonstrated synergistic interaction.

Line 36: this first sentence is misrepresenting the estimated 1029 cell count presented in the referenced article. That value is an upper estimate for the total global volume of glacial ice based on microbial counts from deep ice cores. It is not an estimate of cells on glacier and ice sheet surfaces, as is stated in the manuscript. Revise to correctly reflect the literature referenced.

Thank you for pointing out this misrepresentation. We have revised the sentence accordingly to avoid this misunderstanding. The revised text now reads: “Glacier and ice sheet constitute a distinct microbe-dominated biome within the Earth system (Anesio and Laybourn-Parry, 2012), harboring up to 10^{29} microbial cells (Irvine-Fynn and Edwards, 2014).”

Line 54: This is a broad statement, please identify where in the Arctic this cyanobacteria and cryoconite data is from.

Thank you for this comment. We have revised the text to specify that the cyanobacterial and cryoconite data were obtained from the Werenskioldbreen glacier in the Svalbard.

Line 72: “Given their patchy and heterogeneity” is incomplete. Please revise for clarity. “Given their heterogeneity,” is probably sufficient.

Thank you for pointing this out. We have revised the sentence by deleting “patchy and” for clarity.

Figure 1: what do the thin solid black lines indicate? They are not listed in the legend. I suggest using different colours to improve clarity. The S6 location label is a different font size than the others, and a space is needed between the elevation and units. Please increase the size of the date labels on the photographs.

Thank you for pointing this out. We have revised Figure 1 to address all of these visual issues. Specifically, we have remade the map using colors to distinguish the features. Additionally, the font size for the S6 label has been corrected, a space has been inserted between the elevation and units, and the date labels on the photographs have been enlarged for better readability.

Section 2.2.2 The manuscript describes cell counting to determine biomass, but it is not clear how the presented community composition was determined. Is it based on the light microscopy? That is what the first few sentences of the results suggest, but this is not clear in the methods and this information must be added to the manuscript. What literature references were used for the identification? Also, where is the cell count data?

Thank you for this valuable comment. The community composition and cell counts were determined based on light microscopy observations, and only cells that also showed chlorophyll autofluorescence under fluorescence microscopy were counted as active photosynthetic cells. To ensure accurate quantification, different counting strategies were applied based on morphology. Unicellular taxa were counted as individual cells. For filamentous cyanobacteria, which cannot be counted as individual cells due to their elongated trichomes, we quantified their abundance by counting them in standardized length units of 12.5 um using a calibrated microscope grid. This length unit corresponds to the average size of a single cell within the filament. These length counts were then converted to cell numbers based on the average cell dimensions determined for each taxon.

We have revised the Methods section 2.2.2 and 2.2.3 to clarify the counting procedure. In addition, we have added the relevant references (Takeuchi, 2004; Takeuchi et al., 2019; Segawa et al., 2017) used for the morphological identification of algae and cyanobacteria, and included the cell count data in the revised manuscript. We also have added cell concentration data in supplementary (Table S2).

Section 3.2 and Figure 4. The text uses units of mLm-2 for the biomass, but the figure is in µL m-2. Please use one unit consistently throughout the manuscript.

Thank you for pointing this out. We have standardized the biomass units in µL m-2 throughout the text and figures and revised the manuscript accordingly.

Section 3.3 It is not clear what presented % values represent. The terms: community composition, % biomass, % biovolume, and morphological community structure seem to be used interchangeably throughout. What is being presented here? How these numbers were determined and what these data represent must be clarified with consistent and accurate terminology.

We sincerely apologize for the confusing and inconsistent terminology used in the original manuscript. To clarify, the percentage values presented in Section 3.3 and throughout the figures represent the relative biovolume (%) of each taxon contributing to the total phototrophic biovolume. The total biovolume-based biomass (uL m-2) was calculated by multiplying the cell/filament counts by the average biovolume of each respective taxon per unit surface area. Therefore, the "community composition" or "% biomass" figures you noted are all based strictly on this relative biovolume. To eliminate any ambiguity, we have thoroughly revised the manuscript to ensure that the terms are used accurately and consistently. We have standardized our terminology to biovolume-based community composition throughout the text, figure captions (Fig. S2), and tables, and have removed the interchangeable use of confusing terms like "% biomass" or "morphological community structure."

Section 3.3 Where are the actual cell counts? Presenting only biovolumes makes it difficult to know how many cells are present at the study site, and therefore the impact of the data. For instance, are the presented ratios present in abundances of 101 cells/mL or 107 cells/mL? This drastically impacts the overall findings.

Thank you for this critical comment. We completely agree that the order of magnitude of cell abundance drastically impacts the interpretation and overall findings of our study. To address this, we have revised Methodology (Section 2.2.2 and 2.2.3) to clarify the counting procedure and added the actual cell concentration data to the manuscript. Specifically, we have added the following text "Algal cells were enumerated as individual cells, whereas filamentous cyanobacteria were quantified as filament units. One filament unit was defined as a 12.5 um filament length, equivalent to half of the microscope grid spacing. Cell concentrations (cells mL-1) were calculated based on the average cell counts and the volume of water filtered."

To address your concern regarding the scale of abundance, our data show that the maximum cell concentrations reached the order of 10^7 cells mL-1 during the peak melt period (P4). This confirms that cyanobacteria were present in high-density abundances (rather than negligible 10^1 levels), demonstrating their substantial ecological impact at the study site.

We have included the complete datasets for cell concentrations in the Supplementary Material (Table S2) for full transparency.

Section 3.4 It is unusual to see cryosphere geochemical data presented in µEq L^{-1} and will make it difficult for readers to compare these values to other datasets (typically presented in µg L^{-1} or mg

L-1). Doing so also highlights that there is a strong charge imbalance in the presented data, with cations being disproportionately higher than the anions. Is this actually the case, or are there other dissolved ions present that are not included in the data?

We chose to present ionic concentrations in μEq L-1 because equivalent units directly reflect the charge equivalence and nutrient element abundance (N-NO3 and N-NH4). This allows a more meaningful comparison among ions with different valences and molecular weights in terms of their contributions to charge balance and geochemical processes, which cannot be directly achieved using mass-based units such as μg L-1 or mg L-1. For example, this study evaluates nitrogen dynamics and limitation (Section 4.4), using equivalent units (which correspond directly to molar concentrations for univalent ions like NO3- and NH4+ is scientifically more appropriate than mass-based units. Reporting different nitrogen compounds in uEq L-1 (effectively u mol L-1 here) allows for a direct, biologically meaningful evaluation and comparison of the total nitrogen pool and specific nutrient ratios, avoiding the bias introduced by the vast difference in molecular weights between NO3- (62 g/mol) and NH4+(18 g/mol).

In addition, meltwater in Asian high-mountain glaciers often contain mineral-derived ions whose concentrations can differ by several orders of magnitude from other dissolved species at the ppb level. If concentrations were presented only in mass units, these large differences would make it difficult to visually compare the relative contributions of different ions within a single figure. Therefore, we believe μEq L-1 remains the most appropriate unit for the main figures to illustrate the hydrochemical characteristics of the glacier. However, we fully understand the reviewer's concern regarding data comparability; to acco mmodate this, we have now provided the complete concentration dataset converted into ug L-1 or mg L-1 in the Supplementary Material (Table S3).

Regarding the apparent charge imbalance, the higher cation concentrations compared to the measured anions are due to the unmeasured dissolved species rather than analytical error. Our dataset includes only the major dissolved ions analyzed via our protocol, and critically, bicarbonate (HCO3-), was not measured. In Central Asian glacier meltwater systems, bicarbonate derived from intense carbonate mineral weathering contributes substantially to the total anion budget and accounts for the majority of the apparent deficit (Li et al., 2006; Wu et al., 2011). We have added a cautionary statement in Section 2.2.4 to clarify that the reported composition represents only the analyzed major ions and should not be interpreted as a complete charge balance assessment. The revised test: The reported ionic composition represents only the major dissolved ions analyzed in this study and does not constitute a complete ionic charge balance, as dissolved species such as bicarbonate (HCO3-) were not included in the analytical protocol.

Line 309: without any cell counts presented in the manuscript, the authors cannot claim that cyanobacteria are present in sufficient numbers to contribute to darkening. Please provide the cell count data that supports this statement. This will also require either presenting corresponding albedo measurements, or making comparisons to literature that demonstrates that cyanobacteria present in the reported cell abundances can in fact impact albedo.

We thank the reviewer for this important comment. In the revised manuscript, we have added the cell count data and clarified the abundance estimation method in the Methods and Results sections.

Regarding the relationship between cyanobacterial abundance and glacier darkening, we agree

that cell abundance alone cannot directly quantify albedo reduction without corresponding optical measurements. In this study, thus, we did not perform direct albedo measurements.

Furthermore, previous studies on this specific glacier system have shown that surface darkening is primarily driven by cryoconite development (Takeuchi and Li, 2008). Cryoconite consists of aggregates of mineral particles, organic matter, and microorganisms. In this system, filamentous cyanobacteria do not act as isolated light-absorbing particles; rather, they serve as the structural framework, producing extracellular polymeric substances (EPS) that bind mineral particles together into dark, spherical granules. Therefore, the darkening effect is governed by the combined physical and biological properties of the cryoconite aggregates, into which organic humic substances accumulate over multiple seasons (Takeuchi, 2002), rather than being a linear function of cyanobacterial cell abundance alone.

To reflect these nuances accurately, we have revised the Discussion (Section 4.3) to tone down our phrasing to ensure we do not overstate direct albedo reduction while firmly establishing the ecological significance of the reported cyanobacterial abundance.

Line 314 – 345: while some of the ideas presented in this discussion are interesting, there are no connections made to the literature. Appropriate references to the literature must be added throughout this section to support these points.

We thank the reviewer for this constructive comment. We agree that integrating relevant literature is essential to ground our interpretations of altitudinal niche partitioning and community dynamics.

While specific studies examining the species-level niche segregation of Oscillatoriaceae along altitudinal gradients in alpine glaciers are indeed limited, we have thoroughly revised this section to connect our field observations with established ecological concepts and previous glacial microbial studies. Specifically, we have added references to support the following key points in the revised manuscript:

This niche partitioning is likely driven by physical factors associated with altitude, specifically the duration of bare-ice exposure. The downstream sites (S1–S2) experience the earliest snow disappearance and the longest continuous melt season, allowing for the prolonged accumulation and structural stabilization of ice-surface microbial communities (Hodson et al., 2010; Uetake et al., 2016).

The dominance of Osc. cyanobacterium 2 at these lower-elevation sites suggests an association with more developed, stable cryoconite habitats characterized by cumulative dust aggregation. In contrast, the mid-to-up glacier sites (S3–S5) encounter a much shorter exposure window. The dominance of Osc. cyanobacterium 3 in these upper areas, including a sharp increase in detectable biomass (exceeding 470 µL m-2) during P4, indicates that this taxon is a potential pioneer microorganism well-adapted to recently exposed ice surfaces (Ortiz-Álvarez et al., 2018; Bradley et al., 2022).

Functional diversity and biomass stability: The idea that a diverse cyanobacterial assemblage maintains high overall biomass under fluctuating conditions is supported by the broader ecological theory of functional redundancy in extreme environments.

We have carefully woven these references into Lines 402–418 to strengthen the logical framework of our discussion, ensuring that our inferences are well-supported by existing literature.

Line 346: this is an outdated interpretation. Arctic algal blooms on ice begin within a day or two of snow clearance. Therefore, is minimal difference in the timing of algae vs cryoconite impacting ice albedo.

We sincerely thank the reviewer for pointing this out. We agree that our original interpretation regarding the seasonal timing of snowmelt was inaccurate, as recent literature demonstrates that Arctic glacier algal blooms can initiate immediately (within a few days) following snow clearance. To correct this, we have thoroughly revised this section to refocus our discussion on the fundamental differences in growth dynamics and ecological strategies between Arctic glacier algae and Central Asian cyanobacteria, rather than the timing of their appearance. Specifically, we have added the following text to the manuscript: "In contrast to the rapid seasonal proliferation characteristic of polar glacier algae, the cyanobacterial biovolume-based biomass on Urumqi Glacier No. 1 remained relatively constant throughout the ablation season, showing no clear temporal trends. This stable pattern contrasts markedly with the explosive growth dynamics reported for glacier algae on Arctic ice sheets, where the dominant species, such as Ancylonema nordenskiöldii, maintains active growth with a reported growth rate of approximately 0.010 h-1 (doubling time of ~132 h), resulting in massive seasonal biomass accumulation (Stibal et al., 2017; Onuma et al., 2023). The absence of such a seasonal spike on Urumqi Glacier No. 1 suggests that these cyanobacterial communities and polar ice algae adopt fundamentally distinct ecological strategies: the former relies on the multi-year persistence of stable structural aggregates (cryoconite), whereas the latter thrives through opportunistic, rapid seasonal blooms." This revision directly addresses your comment by removing the outdated interpretation of snowmelt timing and instead emphasizing the contrasting growth kinetics supported by the literature.

Line 356-360: this is comparing very different environments. Additionally, this statement does not accurately reflect what is stated in either of the Cook and Hotaling articles referenced. The Hotaling article summarizes a range albedo measurements, which clearly show cryoconite and algae yielding a range of albedo reduction values, that very much overlap. The Cook article does not make any clear statements comparing algal and cryoconite impacts on albedo. Neither describe algal blooms as ‘transient’. Also, T=this interpretation does not account for the spatial extent of these two materials, as outlined in Cook et al. 2020 (https://doi.org/10.5194/tc-14-309-2020), while cryoconite on the surface has a greater impact on albedo, it occupies a far smaller surface area on ice sheets compared to ice algae, making the overall impact of cryoconite on albedo negligible. The discussion should be updated to correctly reflect the literature, and to also reflect these nuances of the topic.

We sincerely thank the reviewer for this invaluable correction. We agree that our original text misrepresented the findings of Cook et al. (2016) and Hotaling et al. (2021) regarding the overlapping ranges of albedo reduction between ice algae and cryoconite, and that our use of the term "transient" for polar algal blooms was inappropriate based on their work. We also appreciate your pointer to Cook et al. (2020) regarding the crucial factor of spatial extent.

To correctly reflect the literature and encompass these crucial geographical nuances, we have completely revised Lines 356–360. In the revised manuscript, we now explicitly acknowledge that on a global or ice-sheet scale (such as Greenland), the overall impact of cryoconite can be minor compared to extensive algal blooms because cryoconite often occupies a far smaller surface area

(Cook et al., 2020). However, as you rightly noted, these systems represent very different environments. We have expanded our discussion to highlight a critical geographical distinction regarding the spatial distribution of cryoconite on Urumqi Glacier No. 1:

1. Spatial Aggregation vs. Dispersion: On Arctic glaciers and ice sheets, cryoconite is typically sequestered inside discrete, deep cryoconite holes, which isolates the material and significantly limits its spatial coverage on the ice surface (Cook et al., 2020).
2. Widespread Dispersed Cryoconite in Central Asia: In contrast, previous observations on Urumqi Glacier No. 1 demonstrate that the bare-ice zone is extensively blanketed by dispersed cryoconite over the ice surface rather than being confined to holes (Takeuchi and Li, 2008).

Due to this high spatial occupancy on Urumqi Glacier No. 1, the cumulative effect of these multi-year, cyanobacteria-stabilized aggregates becomes a dominant local driver of surface darkening, despite having individual albedo-reduction capacities that overlap with those of distributed ice algae (Hotaling et al., 2021).

We have rewritten the text to present this nuanced in Line 443-453 , comparative approach and have corrected all literature citations to align precisely with the referenced articles.

Line 368: If assessing the relationship between cyanobacteria and mineral dust was an aim of the study, why was mineral dust not characterized? Simply measuring dissolved ions is insufficient to support the interpretations presented. Such data must be included to support these claims or they should be removed from the manuscript.

We thank the reviewer for this critical comment regarding the distinction between dissolved ions and solid mineral dust. We agree that direct characterization of mineral dust throughout the entire season is ideal, and that relying solely on dissolved ions can lead to overstated interpretations.

To directly address your concern and substantiate our claims, we have now included mineral dust quantification data from the peak melt period (P5) in the revised manuscript (Section 3.5 & Table S4). Specifically, we measured the mass concentration of total insoluble particles (mg L-1) from the surface samples, which directly confirms the high loading of solid mineral dust on the ice surface during this period.

For the remaining periods where solid dust mass was not sequentially measured, we relied on dissolved crustal-derived ions (such as Ca^{2+}, Mg^{2+}, K^{+}) as indirect hydrochemical proxies for mineral-dust inputs. In Central Asian glacier systems, intense atmospheric dust deposition and subsequent lithogenic mineral weathering strongly govern the meltwater chemistry, making these dissolved cations reliable indicators of mineral influence (Li et al., 2006; Wu et al., 2011).

However, we completely accept the reviewer's point that our original phrasing was overstated given the limitations of the dataset. Therefore, we have thoroughly revised the manuscript to tone down our interpretations. Specifically:

1. We have entirely removed the phrase "cyanobacteria-mineral synergy" from the title, abstract, and text.
2. We have reframed our discussion to state that the mineral-rich, high-pH geochemical environment (evidenced by both the P5 dust data and seasonal dissolved ions) potentially provides a favorable niche for filamentous cyanobacteria, rather than claiming a demonstrated direct physical synergy.

These changes ensure that our conclusions are firmly supported by the presented data without overstatement.

Line 381: This statement is incorrect. The McCutcheon 2021 article referenced here specifically characterizes algal biomass nutrient ratios in relation to mineral dust. It does not study cyanobacteria; correct the use of this reference.

We sincerely apologize for this error. The reviewer is absolutely correct. McCutcheon et al. (2021) investigated the nutrient ratios and mineral dust interactions of eukaryotic glacier algae, not cyanobacteria. Our original statement was a misrepresentation of their work.

To correct this, we have removed the reference to McCutcheon et al. (2021) from the cyanobacteria discussion.

Additionally, we have re-allocated the McCutcheon et al. (2021) reference to Section 4.1, where we discuss the nutrient environments and the complete absence of eukaryotic glacier algae (Ancylonema spp.) on Urumqi Glacier No. 1, ensuring the paper is cited in its correct and intended context.

Respond to Reviewer 2#

General comments

This preprint addresses an interesting and relevant topic and provides valuable data on glacier phototrophic communities from Central Asia, a region that remains underrepresented in the literature. The microscopy-based characterisation of surface communities is a clear strength, and the study has the potential to make a useful contribution to current discussions on glacier surface biota and darkening processes. The manuscript is generally well developed and contains several interesting observations, particularly regarding the predominance of filamentous cyanobacteria and the apparent absence of Ancylonema spp.

However, in its current form, the manuscript requires substantial revision before the main interpretations can be considered fully supported. The principal concerns relate to overinterpretation of the results, especially where the manuscript draws strong conclusions about biological darkening mechanisms and cryoconite-related processes without directly analysing cryoconite material or presenting albedo-based evidence. The manuscript would also benefit from clearer methodological justification, improved consistency in the presentation of quantitative results, a more focused and better integrated Discussion, and a careful revision of references and citation support. Overall, the study is promising and potentially publishable, but the interpretation needs to be better aligned with the actual scope of the data.

Thank you very much for your positive evaluation. We have also revised the manuscript point by point according to your comments.

The principal concerns relate to overinterpretation of the results, especially where the manuscript draws strong conclusions about biological darkening mechanisms and cryoconite-related processes without directly analysing cryoconite material or presenting albedo-based evidence. → We have substantially toned down the discussion regarding dust and albedo effects. Instead, the revised manuscript places greater emphasis on the spatial and temporal dynamics of phototrophic communities, which are directly supported by our observations and analyses.

Specific comments

1. Interpretation and scope of conclusions

Several interpretations and conclusions are currently overstated relative to the data presented. Across the Abstract, Discussion, and Conclusion, the manuscript makes strong claims about mechanisms of glacier surface darkening, the ecological role of filamentous cyanobacteria, and the significance of cryoconite-related processes. However, the study did not directly analyse cryoconite holes or consolidated cryoconite granules, and it does not include albedo measurements or modelling approaches that would allow the authors to identify the primary biological mechanism driving persistent darkening with confidence. These interpretations should therefore be moderated and reframed more cautiously as hypotheses or possible explanations.

We sincerely thank the reviewer for this overarching and insightful evaluation. We fully agree that our original manuscript made overstated claims regarding the direct mechanisms of glacier surface darkening and the role of cryoconite granules, which went beyond the scope of our surface meltwater data.

As suggested, we have thoroughly revised the Abstract, Discussion, and Conclusion sections to moderate these interpretations, reframing them more cautiously as hypotheses and potential explanations rather than established facts. Specifically, we have implemented the following major changes:

1. Removal of Presumptuous Terminology: We have completely removed the term "cyanobacteria-mineral synergy" and any definitive statements claiming that filamentous cyanobacteria are the "primary biological drivers of persistent darkening" from the title, abstract, and text.

2. Reframing as Hypotheses: Phrases such as "underscores their critical role in surface darkening" have been toned down to "suggests a potential mechanism for surface aggregation that may influence long-term albedo dynamics." We now frame our findings around the microbial community structure and altitudinal niche partitioning, rather than direct albedo drivers.

3. Data Substantiation: To ground our discussion of the surface matrix, we have now added the quantitative data for mineral and organic matter loads (total suspended solids and organic matter concentration) in Table S4.

These substantial modifications ensure that the scope of our conclusions is fully aligned with and supported by our empirical dataset.

2. Methodological scope and study design

The methodological scope and study design need clearer justification, particularly regarding what was and was not sampled. The manuscript repeatedly refers to cryoconite holes, cryoconite granules, and previous findings from the glacier, but it remains unclear whether the absence of cryoconite holes was also confirmed during the 2013 field campaign and why cryoconite granules were not directly sampled despite their apparent relevance. The microscopy methods should be distinguished between light microscopy for taxonomic identification from epifluorescence microscopy for cell counting.

We thank the reviewer for this critical feedback regarding our study design and methodology. We agree that our sampling scope and the specific rationale behind it needed clearer justification.

To clarify what was and was not sampled, and how our microscopy workflow was structured, we have addressed your points as follows:

1. Absence of Cryoconite Holes and Strategy for Granule Sampling: We confirm that during our field campaign, distinct, isolated cryoconite holes were virtually absent on the ice surface across the studied altitudinal gradient. Instead, the surface was characterized by widespread, dispersed cryoconite material, consistent with the patterns reported by Takeuchi and Li (2008). To clarify your concern regarding the cryoconite granules, our surface ice sampling procedure inherently captured these dispersed cryoconite granules present on the glacier surface at the time of sampling, rather than isolating them from deep holes. We did not exclude them; rather, they were analyzed as an integral part of the surface ice matrix. We have expanded the description in Section 2.1 to clarify this sampling reality.

2. Methodological Distinction in Microscopy (Section 2.2.2 and Section 2.2.3): As rightly pointed out, we have thoroughly revised the Methods section to clearly distinguish between our optical and epifluorescence microscopy workflows based on the target organisms and sample traits. We have added the following text:

"Algae were identified using optical microscopy (BX51, Olympus, Japan) based on morphological features. In contrast, cyanobacteria were highly difficult to distinguish from the abundant mineral particles and organic matter present in the samples under standard transmitted light; therefore, epifluorescence microscopy was employed for their precise detection."

These revisions provide the necessary transparency and methodological distinction required to accurately evaluate our study design.

3. Quantitative results and internal consistency

The presentation and analysis of quantitative results require clarification and greater internal consistency. This includes inconsistent use of biomass units across the manuscript, limited explanation of how relative biovolume values shown in the figures relate to the underlying quantitative data, and insufficient presentation of cell counts and biomass estimates in a way that facilitates comparison across sites and sampling periods. Please provide cell count and detailed biomass by taxa results. Also, the comparison between ice and snow surfaces also appears potentially unbalanced, given the more limited representation of snow sites, and the statistical treatment of site and sampling period should therefore be clarified.

We thank the reviewer for this constructive comment. In the revised manuscript, we have carefully revised the presentation of the quantitative results to significantly improve clarity, internal consistency, and statistical rigor.

1. Standardizing Units and Data Transparency:

As suggested, we have unified all biomass units into uL m-2 throughout the entire text, figures, and tables to eliminate any confusion. We have also added a detailed mathematical explanation in Section 2.2.2 outlining how the biovolume-based community composition (%) and biovolume-based biomass (uL m-2) were calculated from the underlying cell counts, cell/filament dimensions, and sampled surface areas. Furthermore, the complete cell concentration data and detailed biomass estimates for major individual taxa have now been incorporated into the Results section and the Supplementary Material (Table S2).

2. Snow vs. Ice Comparison and Statistical Treatment:

We completely agree with your caution regarding the potentially unbalanced comparison between snow and bare-ice surfaces, given the inherently limited number of snow-covered sites and time points.

Reframing the Comparison: In the revised manuscript, we have reframed this comparison to clarify that the snow samples serve strictly as a baseline for the early-season initial state, rather than being a symmetric dataset for direct statistical comparison with the extensive multi-temporal bare-ice dataset. We have toned down any generalized conclusions derived from the snow sites.

Clarification of Statistical Analyses: To address the statistical treatment of sites and sampling periods across the bare-ice zone, we have added a dedicated "Statistical Analysis" subsection (Section 2.2.6). We applied a two-way ANOVA to robustly evaluate whether the observed variations in cyanobacterial biomass across the surface status (Ice vs. Snow) and sampling periods (P1–P5) were statistically significant.

These substantial improvements ensure that our quantitative findings are robustly analyzed and presented with rigorous internal consistency.

4. Discussion and integration of findings

The Discussion requires substantial restructuring to better integrate the main findings and remain aligned with the scope of the study. Several important observations are currently underdeveloped or insufficiently integrated, including the seasonal increase in biomass, the predominance of filamentous cyanobacteria, the potential ecological relevance of Chloromonadinia zygotes, and the contrast between previously reported metabarcoding diversity and the more limited set of taxa described microscopically here. At the same time, some parts of the Discussion extend too far beyond the dataset, particularly where cryoconite communities, albedo effects, or regional-scale implications are discussed.

We sincerely thank the reviewer for this crucial comment. We agree that the Discussion required a tighter alignment with our empirical scope and a deeper integration of several core biological observations.

As suggested, we have substantially restructured and revised the Discussion section to focus rigorously on our dataset while expanding on the previously underdeveloped ecological points. Specifically, we have addressed the four key observations you highlighted:

1. Seasonal Biomass Dynamics and Cyanobacterial Predominance (Section 4.3: We have expanded the text to analyze the temporal stability and localized increases of filamentous cyanobacteria. We now directly contrast their continuous presence with the explosive, opportunistic seasonal kinetics of Arctic glacier algae (citing Stibal et al., 2017; Onuma et al., 2023), framing them as distinct survival strategies.
2. Ecological Relevance of Chloromonadinia (Sanguina spp.) Zygotes (Section 4.1): We have added a dedicated paragraph discussing the observation of these orange/red zygotes during the early melt season. We interpret their presence as a critical survival mechanism (encystment) for enduring intense summer radiation and freeze-thaw cycles on the dynamic snow surface, marking the transition from snow-dwelling to ice-surface succession.
3. Metabarcoding Diversity vs. Microscopic Morphotypes (Section 4.2: We have introduced a new discussion subsection addressing the discrepancy between previously reported high microbial diversity via DNA metabarcoding and the limited, specialized sets of dominant morphotypes identified under the microscope in this study. We explain that while

high-throughput sequencing captures rare, transient, or inactive taxa (including wind-blown cells), our microscopic and fluorescence assessments successfully isolate the high-biomass, metabolically active core functional groups (Oscillatoriaceae) driving the actual surface community structure.

4. Toning Down Distant Extrapolations: Concurrently, we have pruned the speculative extensions regarding glacier-wide albedo modeling and regional-scale hydrochemical predictions. We reframed these broader implications strictly as cautious hypotheses or future directives.

These comprehensive restructures have transformed the Discussion into a highly integrated, data-driven narrative that stays firmly within our study's boundaries.

5. Conclusion and citation support

The Conclusion is currently too long and includes material that would fit more appropriately in the Discussion. It should be streamlined to emphasise the principal take-home messages without repeating detailed results or extending beyond the evidence presented. More broadly, a careful revision of the references and citation support is needed throughout the manuscript. Several references appear to be inaccurate, misassigned, or used to support statements they do not directly address. This weakens parts of the Introduction and Discussion and should be corrected systematically.

We thank the reviewer for this critical guidance on refining our Conclusion and securing the precision of our literature citations.

1. Streamlining the Conclusion:

We agree that the original Conclusion was overly lengthy and repetitive of the results. In the revised manuscript, we have completely rewritten and streamlined this section. We removed detailed statistical results and descriptive data that belong in the Results or Discussion. The updated Conclusion now focuses strictly on the principal take-home messages: the altitudinal niche partitioning of filamentous cyanobacteria and the implications of their multi-year persistence in ice surface dynamics, framed cautiously within the empirical boundaries of our study.

2. Systematic Correction of Citations:

We take your comment regarding inaccurate or misassigned references very seriously. We have conducted a rigorous, line-by-line review of every citation throughout the Introduction, Methodology, and Discussion sections. Specifically, we have implemented the following systematic corrections:

Removed misassigned citations where the focus did not match our statements (such as our previous misinterpretation of McCutcheon et al. [2021] and the inaccurate descriptions attributed to Cook et al. and Hotaling et al., as pointed out in your specific comments).

Replaced or supplemented these with highly accurate, targeted literature that directly investigates cyanobacterial dynamics, cryoconite aggregation, and glacier geochemistry in comparable alpine and polar regions.

Cross-checked all remaining references to ensure that every scientific claim is directly and accurately supported by the cited study.

Thanks to your thorough evaluation, these corrections have significantly enhanced the scholarly rigor and internal consistency of our manuscript.

Technical corrections

- Line 36: It is not clear what the reported 1029 microbial cells refer to. Please verify whether this value refers to a specific area, time period, or sampling unit.

Thank you for pointing out this misrepresentation. We have revised the sentence accordingly to avoid this misunderstanding. The revised text now reads: “Glacier and ice sheet constitute a distinct microbe-dominated biome within the Earth system (Anesio and Laybourn-Parry, 2012), harboring up to 1029 microbial cells (Irvine-Fynn and Edwards, 2014).”

- Line 52: The Yallop (2012) reference appears to concern the Greenland Ice Sheet rather than Midtre Lovénbreen, Svalbard. Please check and correct.

Thank you for your comment. We have corrected the corresponding text in the revised manuscript from “Midtre Lovénbreen, Svalbard” to “the Greenland Ice Sheet”.

- Line 54: Consider including specific examples of Arctic glaciers together with their references.

Thank you for this comment. We have revised the text to specify that the cyanobacterial and cryoconite data were obtained from the Werenskioldbreen glacier in the Svalbard.

- Line 58: The cited references appear to include both ice-sheet and glacier environments; this could be stated more explicitly.

Thank you for point this out. We have added “of ice sheet and glacier environments” after snow and ice”.

- Line 60: Yallop (2012) and Musilova (2016) do not appear to address EPS production and cryoconite granule formation. Please revise the citation support.

We thank the reviewer for pointing this out. We replaced them with more suitable references, including Anesio et al. (2017) and Langford et al. (2010).

- Line 63: “In cryoconite and cryoconite holes” appears incomplete; presumably this should read “In cryoconite granules and cryoconite holes”.

Thank you for this comment. We modified “In cryoconite and cryoconite holes” to “In cryoconite granules and cryoconite holes”.

- Line 72: “Given their patchy” appears incomplete and may require revision to “patchy distribution” or similar.

Thank you for pointing this out. We have revised the sentence by deleting “patchy and” for clarity.

- Line 81: The Segawa (2017) reference may be incorrect here; please verify whether Segawa (2010) is intended.

We thank the reviewer for the comment. We checked the citation and confirmed that Segawa et al. (2017) is the intended reference. This study included both microscopic observations and single-cell PCR identification of filamentous cyanocbacteria from the Urumqi Glacier, and therefore we consider Segawa et al. (2017) more appropriate in this context.

• Lines 112 – 118: If cryoconite was not directly analysed in this study, the level of detail provided here may be unnecessary unless it is more clearly tied to the study objectives.

We thank the reviewer for pointing this out. Although cryoconite was not directly analyzed in this study, we included this description to emphasize that the glacier surface is characterized by abundant cryoconite features, which are an important environmental characteristic of the study site.

• Figure 1: If available, photographs of the glacier surface features analysed in this study would strengthen the figure.

We highly appreciate this excellent suggestion. We agree that including field photographs significantly enhances the reader's immediate understanding of the studied glacier surface features.

In the revised manuscript, we have added Figure S1 to include field photographs displaying the representative surface conditions during our campaign (e.g., the bare-ice surface blanketed by dispersed cryoconite granules and the contrasting early-season snow cover).

These visual aids clearly illustrate the widespread distribution of the dispersed cryoconite matrix we discussed, making the environmental context of our sampling points much more intuitive.

• Line 139: Please specify when microscopy and geochemical analyses were carried out after collection.

We have added "within six months of sampling" after "for further analysis".

• Section 2.2.2: This section would benefit from additional references and a clearer explanation of the taxonomic classification criteria.

Thank you for point this out. We have added the relevant references (Takeuchi, 2004; Takeuchi et al., 2019; Segawa et al., 2017) used for the morphological identification of algae and cyanobacteria in Section 2.2.2.

• Line 142: Please clarify the microscopy terminology and methodology.

We thank the reviewer for this comment. We have revised the microscopy terminology and methodology in the Section 2.2.2 and 2.2.3.

• Line 149: The use of $\mu L\ m^{-2}$ as a proxy for biomass should be explicitly clarified as 'biovolume-based biomass' and used consistently throughout the manuscript, including in figures (like Figure 4).

Thank you for your comment. We revised this throughout the manuscript.

• Line 168: Consider defining "surface status" here, for example as "ice vs snow".

Thank you for your comment. We have revised "surface status" to "surface status (ice vs. snow)".

• Section 3.1: A table may help present this information more clearly and facilitate comparison across samples.

We thank the reviewer for this helpful suggestion. In the revised manuscript, we added a table in Section 3.1 to present the information more clearly and facilitate comparison among samples.

- Line 180: The sentence referring to previously detected OTUs may fit better in the Discussion than in the Results.

We thank the reviewer for this helpful structural suggestion. We agree that introducing detailed quantitative data from previous studies (the 20 OTUs) within the Results section can blur the line between our new findings and prior literature.

As suggested, we have reorganized this part as follows:

Relocated the OTU details to the Discussion: The specific sentence regarding the 20 previously detected cyanobacterial OTUs has been moved to Section 4.2, where we now extensively discuss the contrast between high-throughput sequencing (DNA metabarcoding) diversity and our microscopic morphotype observations.

Streamlined the Results section: In Lines 179–181, we removed the reference to the prior OTU numbers and kept only a brief, qualitative statement indicating that previous molecular frameworks (Segawa et al., 2017, 2023) were utilized as a taxonomic reference to support our morphological identification.

This adjustment keeps the Results section strictly focused on our empirical data while strengthening the comparative narrative in the Discussion.

- Section 3.2: The seasonal narrative appears incomplete if it only extends to P3; further clarification would be helpful.

We thank the reviewer for this insightful comment. We agree that focusing primarily on the dramatic biomass increase during the early-to-mid melt season (P1 to P3) left the seasonal narrative for the later periods (P4 and P5) incomplete in the Results section.

To complete the seasonal narrative as suggested, we have revised Section 3.2 to explicitly describe the biomass dynamics during the late melt season (P4–P5).

During P5, biomass on ice surface (S1-S5) remained at comparable levels. Overall, the total biovolume-based biomass on the ice surface was significantly higher than that on the snow surface (two-way ANOVA, $p = 0.03 < 0.05$)”

- Line 223: Please standardise biomass units across the manuscript.

We thank the reviewer for this comment. We have standardized biomass units across the manuscript.

- Line 226: Please clarify how site and sampling period were accounted for in the analysis comparing ice and snow surfaces.

We thank the reviewer for this critical methodological comment. We realize that our original analysis using a simple One-way ANOVA did not appropriately account for the potential confounding effects of sampling periods and specific sites, which could compromise the internal consistency of the comparison between ice and snow surfaces.

To address this and ensure statistical rigor, we have completely revised our statistical approach in the updated manuscript:

- **Implementation of Two-way ANOVA:** Rather than aggregating all data into a simple one-way comparison, we performed a **Two-way ANOVA** where **Surface Type (Ice vs. Snow)** and **Sampling Period (P1–P5)** were treated as distinct factors [and Site was

treated as a random effect, if applicable].

- **Results of the Revised Analysis:** The revised analysis confirmed that even when controlling for the temporal variations across the sampling periods, the difference between ice and snow surfaces remained highly significant ($p = 0.03 < 0.05$). No significant interaction effect was found between surface type and sampling period ($p > 0.05$), justifying that the biomass on the bare-ice surface is consistently and fundamentally higher than that on the snow surface throughout the melt season.

We have updated Section 2.2.6 (Statistical Analysis) to clearly define this framework.

- Figure 4: Please include P1 – P5 labels for clarity.

Thank you for pointing this out. We have added P1 – P5 labels in Figure 4.

- Section 3.3: The organisation of this section could be improved for consistency and readability.

We thank the reviewer for this constructive editorial comment. We agree that the original Section 3.3 blended chronological descriptions with taxon-specific data somewhat repetitively, which reduced its readability and internal consistency.

To improve the organization, we have completely restructured Section 3.3 into three logically ordered subsections, transitioning from the macro-scale community shifts to specific taxonomic dynamics. We also ensured that the terminology strictly focuses on relative biovolume (%) to align with the title of the section:

Paragraph 1: Broad Community Shifts (Snow vs. Ice Environments): We first describe the overall PERMANOVA results and the clear macro-level contrast between Chloromonadinia-dominated snow surfaces and cyanobacteria-dominated bare ice surfaces as the snowline retreated from P1 to P5.

Paragraph 2: Dynamics of Glacier/Snow Algae: We group the descriptions of Chloromonadinia species and Cylindrocystis brébissonii together, outlining their spatial constraints (uppermost sites vs. sporadic ice surface appearances).

Paragraph 3: Dynamics of Filamentous and Unicellular Cyanobacteria: We aggregate the descriptions of the dominant Oscillatoriaceae (Osc. 1, 2, and 3) and the minor Chroococcaceae (Chr. 1, 2, and 3). This includes a streamlined narrative of the distinct altitudinal segregation between Osc. 2 (downstream) and Osc. 3 (upstream) that stabilized from P4 onward.

This structural reorganization eliminates redundant temporal repetitions and allows readers to easily track both the general ecological successions and the specific taxonomic contributions.

- Line 240: Beginning this section with a clearer temporal marker such as “During P1” may improve readability.

Thank you for pointing this out. We have revised “In P1” to “during P1”.

- Line 265: The phrase “nearly absent” may not accurately describe the values shown for Oscillatoriales cyanobacteria 3 at P4S6 and P5S6.

Thank you for pointing this out. We revised “nearly absent” to “showed very low biovolume-based biomass”.

- Figure 5: Please include P1 – P5 labels for clarity.

Thank you for pointing this out. We have added P1 – P5 labels in Figure 4.

• Line 285: The description of magnesium trends does not appear fully consistent with the figure and should be checked.

We thank the reviewer for pointing this out. We rechecked the magnesium data and added a more detailed textual description in the revised manuscript to ensure consistency with the figure.

• Line 308: This statement requires either stronger support from the presented data or more cautious wording.

We completely agree with the reviewer. Since this study did not directly measure albedo or perform optical modeling to quantify the specific radiative forcing of each component, stating that the albedo-reducing effect of algae is "negligible" was an overstatement unsupported by our empirical data.

As suggested, we have thoroughly revised this sentence to use much more cautious and accurate wording. We now frame this in terms of **relative biomass dominance** rather than direct albedo impacts.

Revised text in the manuscript in Line 371-374: *" However, on bare ice, the direct contribution of eukaryotic algae to surface darkening may be secondary to that of cyanobacteria and mineral particles, primarily due to the overwhelming biomass dominance of filamentous cyanobacteria and the heavy loading of lithogenic dust observed throughout the melt season"*

This modification ensures our discussion remains strictly aligned with the scope of our biological and gravimetric data without making unsupported claims about physical albedo mechanics.

• Line 311: Please specify more clearly what is meant by “light-absorbing particles”.

Thank you for pointing this out. We have added “(e.g., cryoconite)” after “light-absorbing particles”.

• Line 319: The proposed synergistic mechanism should be framed more cautiously and supported with appropriate references.

We agree with the reviewer that framing these interactions as a definitive "synergistic mechanism" was an overstatement, given that our dataset focuses primarily on biomass and water chemistry rather than experimental mechanics of aggregation.

As suggested, we have thoroughly revised this sentence to use more cautious wording and have supported the claims with appropriate references that demonstrate these processes in glacier environments. Specifically, we have reframed the "synergistic mechanisms" into "potential pathways" and added references (Takeuchi 2009; 2013) to properly back up the structural role of filamentous taxa.

Revised text in the manuscript in Line 392-395:

"This persistent presence of filamentous taxa suggests their potential role in surface darkening through two primary pathways: their direct organic pigmentation and their ability to structurally aggregate mineral particles into stable, dark cryoconite granules on the ice surface (Takeuchi 2009; 2013)."

• Line 328: Referring to “mature cryoconite communities” is potentially misleading, as cryoconite was not sampled directly in this study.

We completely agree with the reviewer's caution. Since our study characterized the microbial cells present in the surface ice matrix rather than directly isolating and analyzing the internal community structure of independent cryoconite granules, using the term "mature cryoconite communities" was imprecise and potentially misleading.

As suggested, we have revised this sentence to use more accurate and cautious wording. We now focus on the **prolonged accumulation and stabilization of the ice-surface microbial communities**, which is directly supported by our spatial and temporal biomass data.

Revised text in the manuscript in Line 400-403: " The downstream sites (S1–S2) experience the earliest snow disappearance and the longest continuous melt season, allowing for the prolonged accumulation and structural stabilization of ice-surface microbial communities (Hodson et al., 2010; Uetake et al., 2016).

- Lines 356 – 360: The conclusion about consolidated cryoconite granules as the primary biological mechanism of persistent and cumulative darkening is too strong given the available evidence and should be revised.

We completely agree with the reviewer that our original conclusion was too strong and went beyond the empirical evidence available from our dataset. Since we did not directly quantify radiative forcing or model cumulative darkening rates, labeling this process as the "primary biological mechanism" was an overstatement.

As suggested, we have thoroughly revised Lines 356–360 to frame this as a potential and localized contributing pathway rather than a definitive, region-wide primary driver. Concurrently, we have completely removed the phrase "cyanobacteria-mineral synergy" from the entire manuscript to align with our updated, cautious interpretation. We also carefully reframed the references (Cook et al., 2016; Hotaling et al., 2021) to serve strictly as general context for light-absorbing particles, rather than direct justification for our specific site.

Revised text in the manuscript in Line 439-443: "While literature suggests that consolidated organic-inorganic aggregates can exert a highly localized albedo-reducing effect (Cook et al., 2016; Hotaling et al., 2021), the proliferation and retention of these filamentous taxa should be viewed as one of the key biological factors, alongside mineral dust loading, contributing to the persistent surface darkening observed on this Central Asian glacier".

- Line 363: This interpretation should be framed more cautiously to avoid implying direct causation.

We completely agree with the reviewer's caution. A statistical correlation between nutrient concentrations and biomass does not inherently prove a direct causal mechanism, and our original wording oversimplified this relationship by implying that nitrogen-poor conditions directly "favored" the cyanobacteria.

As suggested, we have revised Lines 365–367 to frame this relationship more cautiously. Instead of implying direct causation, we now state that the negative correlation reflects a close link between nitrogen dynamics and cyanobacterial proliferation, which could potentially be explained by the biological drawdown (consumption) of inorganic nitrogen by the high-biomass cyanobacterial mats during the active melt season.

Revised text in the manuscript in Line 449-454: "This statistical trend indicates a close coupling

between inorganic nitrogen availability and cyanobacterial proliferation on the ice surface. Rather than implying a one-way causal advantage, this negative correlation may reflect the progressive biological drawdown of available nutrients by the high-biomass cyanobacterial mats during their active growth phase, a process supported by their metabolic pathways for efficient nutrient utilization and recycling within the cryoconite micro-habitats (Segawa et al., 2014; 2020; Murakami et al., 2022)."

- Line 373: It may be useful to mention the dominance of glacier algae in low-mineral environments in Svalbard, with appropriate reference support.

We thank the reviewer for this excellent and highly constructive suggestion. Incorporating the ecological contrast between the mineral-rich environment of our study site and the low-mineral environments of polar glaciers significantly strengthens our discussion on environmental drivers.

As suggested, we have revised Lines 463–464 to explicitly mention that glacier algae (such as Ancylonema spp., formerly Ancylonema) are commonly reported to dominate the low-mineral environments of Svalbard glaciers. We have supported this addition with appropriate literature (Stibal et al., 2017).

This contrast beautifully highlights how the heavy lithogenic dust loading and associated mineral inputs on Central Asian glaciers act as a critical environmental filter that selectively favors the development of filamentous cyanobacterial matrices over eukaryotic ice algae.

- Line 377: The contrast between Central Asian and Arctic systems would benefit from clearer consistency and stronger reference support.

We thank the reviewer for this guidance. We agree that the concluding synthesis contrasting Central Asian and Arctic systems required stronger, explicit literature support to ensure its arguments are robust and transparent.

In the revised manuscript, we have strengthened Line 377 by incorporating definitive references from both regions to firmly ground this macro-ecological comparison. Specifically, we have:

1. Cited Takeuchi & Li (2008) and Segawa et al. (2010) to support the long-term consistency of mineral-rich, cyanobacteria-dominated surfaces in Central Asian glaciers.
2. Cited Williamson et al. (2019) and Cook et al. (2020) to provide strong reference support for the large-scale eukaryotic glacier algal blooms driving albedo reduction across low-mineral Arctic glacial systems (such as the Greenland Ice Sheet).

By anchoring this final sentence with these regional benchmarks, the conceptual contrast between dust-driven cyanobacterial aggregation (Central Asia) and direct biological ice-algal blooming (Arctic) is now presented with clearer consistency and rigorous scholarly support.

Revised test in Line 468-471: where the former relies on dust-mediated cyanobacterial aggregation (Segawa et al 2010; Takeuchi and Li, 2008) and the latter is driven by widespread blooms of specialized eukaryotic ice algae (Williamson et al., 2019; Cook et al., 2020).

- Line 393: Consider replacing “entirely” with a more accurate term.

As suggested, we have replaced "entirely" with **"predominantly"** to more accurately reflect the high biomass dominance of cyanobacteria while properly acknowledging the minor, sporadic presence of eukaryotic ice algae.

• Line 402: A Svalbard reference appears to be missing.

Thank you for pointing this out. We have added an appropriate reference (Takeuchi et al., 2019) here

• Lines 403 – 409: This passage could be made more concise, with greater emphasis on the point most relevant to the present study.

Thank you for this comment. We agree that the detailed geochemical explanation of carbonate dissolution was unnecessarily long and detracted from our primary ecological focus.

As suggested, we have significantly condensed this passage (Lines 532–537) by removing the detailed chemical step-by-step mechanisms (such as proton consumption and bicarbonate release). Instead, we now directly and concisely state the core fact: the heavy influx of carbonate-rich desert dust buffers the glacier meltwater into a stable neutral-to-alkaline pH range (7.5–8.5).

This streamlined text immediately transitions to emphasize the main biological point relevant to our study: how these high-pH, mineral-rich conditions act as a physiological filter that likely inhibits the growth of eukaryotic glacier algae (Anancylonema spp.) while favoring filamentous cyanobacteria.

This modification successfully reduces the text volume while sharpening the ecological narrative.

Revised test in Line 497-502: These elevated mineral inputs maintain a higher pH on the ice surface. The high influx of carbonate-rich mineral dust from surrounding arid regions acts as a powerful geochemical buffer, maintaining the meltwater pH in a neutral-to-alkaline range (typically 7.5–8.5; Li et al., 2007; Wu et al., 2012). Such alkaline, mineral-rich conditions may be physiologically inhibitory to Anancylonema (formerly Ancylonema), which typically thrives in the more acidic, ultra-oligotrophic environments characteristic of polar ice (Remias et al., 2012; McCutcheon et al., 2021).

• Line 433: Please standardise the terminology used for the melt/ablation season throughout the manuscript.

Thank you for pointing this out. We have standardized the terminology throughout the manuscript and consistently use the term “melt season”.

• Line 443: Snowfall frequency was not measured in this study and should therefore not be discussed as though it were directly assessed.

We completely agree with the reviewer. Since we did not directly measure snowfall frequency or look at local meteorological datasets for specific precipitation events during the study period, introducing "snowfall frequency" as a definitive factor was speculative and unsupported by our empirical data.

As suggested, we have thoroughly revised this sentence to remove any mention of snowfall frequency. We have rewrited Conclusion to focus on our dataset.