General comments

It was a joy to read the manuscript by Dubnick and colleagues. The paper describes in detail the geochemical transformation (evolution) of late-season glacial meltwater as it undergoes freezing subglacially over winter. To my knowledge this is the first study of the type that can provide empirical data on these processes in a very interesting field site that can act as a natural laboratory. Despite the particular nature of the study site, the results presented here are of broader relevance and can inform on a wider array of subglacial systems. They exemplify what microbial and biogeochemical processes can occur subglacially in the context of subglacial environments as being active microbial habitats. They do so in a quantitative way by combining geochemical models, geochemical and microbial observations and go beyond simple data reporting often the case in similar studies. The manuscript is extremely well written, with great context and explanation for both expert and non-expert readers, well within the relevant scopes of the Cryosphere journal.

However, before recommending the article for publication, there are some minor comments that I believe would need addressing or clarifying:

Specific comments

1. Fig. 4: Is it possible to add the uncertainty on measurements here? (e.g. Root sum square of precision and accuracy for geochem measurements and 1 or 2 standard dev on cell counts for each sample).

2. I would also add the uncertainty when reporting cell numbers in the text (e.g. section 3.7). I suspect uncertainty in cell numbers might have a greater impact on the enrichment models than for geochem species.

3. Ln 378: Why higher relabund of Gammaproteo in channel ice than rules out other subglacial sources for late-season runoff? The results presented here suggest that incremental freezing might enrich for Gammaproteo. Couldn't the same apply for distributed system subglacial waters (i.e. subglacial sources that might slightly differ from basal ice composition) that would also contribute to late-season runoff? Or are the authors suggesting that these organisms in late-season runoff originate from the supraglacial environment?

4. Ln 433: Can you cite other studies that describe % contribution of basal sources to late-season subglacial residual waters? I would assume late-season runoff to be normally relatively depleted in basal elements (e.g. glacial flour/distributed system input) compared to earlier/peak melt. Not entirely convinced that the pond waters described here are "unlike the waters contained beneath many other polythermal glaciers"; at least late-season residual waters in subglacial channels that is. Or do the authors mean "basal waters from distributed drainage systems"?

5. Ln 434: Perhaps I missed the obvious results (very possible) but why exactly do the geochemical models suggest a maximum basal ice contribution to the pond of 15%? Wouldn't increasing the % contribution of basal ice to the pond better decrease the

enrichment/depletion discrepancy of the model on Fig. 4a? At least for geochemically-relevant species.

6. Ln470: Are you excluding basal waters/melt from other potential sources to late-season runoff? I again think several microorganisms detected in the pond waters might have originated and selected from subglacial sources too (as mentioned on the the first sentence of 4.4 Ln457 and the follow-up paragrap Ln 476-488). I suggest adding "subglacial waters" too to the sentence as potential source. But I agree with the overall interpretation of niche selection.

7. The above comment also makes me caution the conclusion statement that the detected bacterial populations most likely are generalists from supraglacial environments. To me they seem like typical subglacial populations; perhaps generalists yes and ultimately originated from extraglacial systems prior to glacial inception but I'm not sure this is what the authors are claiming (I still think the results are pretty cool though!).

8. There seems to be no discussion on why the cell abundance in the ponded water appears to be depleted relative to model. This seems a bit strange to me. Again, perhaps a higher contribution of basal ice to the model might help explain this (can see technical comments below)?

Data availability:

1. I believe a bit more information is needed regarding the geochemical models used (even if relatively simple). I.e. where/how were they run? Custom scripts? What program (Matlab, R, Exce?). If code was used, it should ideally be uploaded in a repository or included as supp material.
2. Similar comment regarding the bioinformatics: e.g. might it be possible to upload the mothur logfile or command-line summary (e.g. batch file) to a repository or as part of Supp Mat.
3. I'm not sure the Zeonodo link for geochem data is correct and could not access the data (maybe my bad though).

Technical comments

Ln 55: should also include Gill-Olivas 2023: https://bg.copernicus.org/articles/20/929/2023/

Methods: consider changing the subscript acronyms in equations to capital letter to avoid confusion with lowercase roman numerals used for "steps" (e.g. $X_{ii(i)}$ for "incremental ice" could become $X_{II(i)}$ etc)

Methods: consider specifying that the geochem model was also applied to cell concentrations legend of Fig. 3a: isn't inversed? Here shows that red = 100% water and dark-blue = 100% frozen seems opposite to data and theoretical values? (also consider changing rainbow scale to alternative more colour-blind friendly palette (e.g. Viridis))

Fig. 5b: Should x-axis be "relative abundance to channel ice"?

Ln 529: not sure the Zenodo link is correct?

Fig. S3a : I think bed and ice-surface are swapped in the legend. Also what are the capital "A" and "B" above both axes?