EGUsphere, author comment AC1
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Reply on RC1

Tilly Woods and Ian J. Hewitt

Author comment on "A model of the weathering crust and microbial activity on an ice-sheet surface" by Tilly Woods and Ian J. Hewitt, EGUsphere, https://doi.org/10.5194/egusphere-2022-1086-AC1, 2022

Thank you very much for the kind words and constructive comments which will help us to improve our manuscript. We really appreciate the time taken to write the review, and we are glad that you enjoyed reading the manuscript.

Below is a copy of the comments with our responses written below each one.

Reviewer comment: This article presents what is to my knowledge the first model to capture two/three bare-ice processes simultaneously: (i) the development and destruction of the porous near-surface weathering crust; (ii) the growth of microbes (e.g. ice algae) within that crust; and (iii) nutrient availability within the crust. In doing so it synthesises and 'formalises' several field-based studies from the last ~50 years. The model's behaviour corresponds well, at least at first order, to field observations, and it yields further process-based insight. These insights are especially strong in revealing the inter-play between (a) shortwave radiation versus other radiation sources for crust development and microbe growth; and (b) the impacts of radiation upon melt and in turn runoff of microbes and nutrients.

I commend the authors on this timely and thorough study of a complex topic. It's written concisely and with excellent figures. I enjoyed reading it. I caveat that as my knowledge lies rather more in field observations of these processes than mathematics, I cannot formally assess the suitability of the numerical methods employed, and so I have restricted my comments accordingly. Within this context, overall I find this study to be in excellent shape and I have only a few minor comments, mainly concerning the discussion/wider applicability of the model.

Reviewer comment: Thank you for your positive words. Coming from a mathematical background, we appreciate your insight into field observations.

Minor comments:

Reviewer comment: I would really like to see this model run for a full melt season at a location such at the SW GrIS 'dark zone' where these processes are known to be important. However, I appreciate that this is almost certainly too much work and content for the present study, so instead it would be very useful to at least comment on the feasibility of such a temporal model suite in the Discussion/Conclusions. I make this comment partly in the context that bare ice albedo schemes in the major regional climate

model surface schemes are very simple, often yielding quite poor comparison with in-situ observations (e.g. Fettweis et al., 2017, The Cryosphere) and so the search is on for more physical approaches that yield closer correspondence with observed albedo.

Response: We agree that it would be interesting to study evolution over a melt season, and also agree that this would be too much work for the current study. However, this is certainly something we hope to investigate in future. If allowed to revise the manuscript, we will ensure to mention temporal modelling in the Discussion/Conclusions. Thank you for the literature suggestion.

Reviewer comment: Shortwave radiation attenuation with depth: Cooper et al. (2020, The Cryosphere) present the first observations to my knowledge of light attenuation through a weathering crust. I did not see this study referenced in the present m/s. Please consider commenting on how their observations compare to the choices from Hoffman et al. (2014), Taylor and Feltham (2005).

Response: We had not come across the Cooper et al. paper before - thank you for the suggestion. We will compare the observations in this paper with the choices we have made based on Hoffman et al. (2014) and Taylor and Feltham (2005).

Reviewer comment: Microbial abundance and its interaction with runoff: Overall, I concur with the approach taken here. I agree that the parameter A_max is basically a reasonable choice. However, with surface ice algal abundances in excess of 10,000 cells ml-1 reported previously for south-west Greenland (e.g. Cook et al., 2020, The Cryosphere, Wang et al., 2018, Geophysical Research Letters), I think some consideration of how the surface can support such high abundances is still warranted. Specifically, I wonder if the instant microbial runoff here is realistic.

To my understanding, on the basis of the modelling in the present study, then we would expect the high growth rates in large melt years to be offset by widespread microbial runoff - yet we see that in large melt years then we get high persistent algal abundance, implying that the cells can persist at the surface. I'm not sure whether the mechanisms by which algal cells can persist at the surface have been identified by the microbiology community, so there is probably a knowledge gap here. Nonetheless, I am of the view that currently the study provides rather an estimate of the microbial abundance within the weathering crust, but not the total 'system' abundance including algae also 'stored' on the surface of the weathering crust as could be implied at lines 564-565.

Response: Our understanding is that there are some microbes that live on the surface (eg. in the top 2cm) of the weathering crust (ice algae) and others that live in the weathering crust aquifer below the surface, with the surface microbes (ice algae) being able to sit on the ice surface, and the aquifer microbes getting carried around in the meltwater. For simplicity, we chose to focus on the microbes being transported in the weathering crust aquifer, neglecting the surface ice algae for now. This is partly why we chose instant microbial runoff, since we are assuming the microbes in our study move with the meltwater, unable to attach themselves to the ice. Your comment suggests we ought to make this clearer in our manuscript, which we will do. We also acknowledge that neglecting ice algae makes our current model less useful for making significant claims about the albedo, since ice algae provide a major contribution to this. However, we wanted to demonstrate the kind of behaviour that can result from a coupling between microbes, melting and albedo, so that our study can be used as a starting point for further investigations that include surface ice algae too. To improve our study, we could write a separate model (which could be coupled to the current model) for the evolution of surface ice algae which can persist in the presence of runoff, but this is an area for future work.

Reviewer comment: Similar to my comment about behaviour through a full melt season,

I would welcome some brief discussion about how the model could capture (or not) the spatio-temporal dynamics of algal blooms and weathering crusts.

Response: The model definitely has the potential to be extended to consider spatio-temporal dynamics. This is something we are currently working on.

Reviewer comment: Literature suggestion: the authors might not be aware of Schuster's (2001) PhD thesis, 'Weathering crust processes on melting glacier ice (Alberta, Canada)'. This could be worth considering, in particular because it contains the only other significant attempt to model the weathering crust that I'm aware of.

Response: Thank you for the suggestion. This does seem of relevance and we will compare our model with Schuster's.