EGUsphere, referee comment RC1
https://doi.org/10.5194/egusphere-2022-1086-RC1, 2022

Comment on egusphere-2022-1086

Andrew Tedstone (Referee)

Referee 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-RC1, 2022

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.

Minor comments:

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.

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).

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.

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.

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.