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Interactive comment on "Observations and modelling of algal growth on a snowpack in northwest Greenland" *by* Yukihiko Onuma et al.

Anonymous Referee #1

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Dear Authors,

The submitted manuscript aims to address a worthwhile gap in our knowledge of snow algae – namely the ability to model the growth of algal blooms over a season. The paper combines empirical observations with a simple growth model, building upon previous work by the same group that applied a Malthusian model to explain the population dynamics of the snow algal bloom by adding a carrying capacity. I enjoyed reading the paper and appreciated the careful field measurements; however, I have two queries for the authors, along with some minor typographical corrections.

1. The model assumes no input or removal of cells, indicating that the population dynamics are dominated by in situ cell proliferation. However, no mention is made of the effects of scavenging by melting snow. The algae are likely to become concentrated

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onto the snow surface as the snowpack melts, and if this is the case then the authors would have measured an increase in surface biomass simply due to the concentrating effects of snow melt. Can the authors show that this was not the case? Similarly, it is hard to envisage zero cell export from the system. I suspect the snowpack algal population dynamics to be the result of in situ growth, scavenging by snow melt, export in meltwater and a small contribution by wind-delivery. Can the authors provide any data or observations to support all these factors being negligible apart from in situ growth?

2. I think the authors overstate the utility of the model. As it stands the manuscript shows that a logistic modelling approach is appropriate for predicting the population dynamics of snow algae for specific field sites; however, insufficient information is provided to enable the application of the model elsewhere. The fact that between these two sites the coefficient of determination for the model applied to observations varies between 0.64 and 0.96 suggests to me that additional site specific factors influence the growth rate. K is, as the authors discuss, dependent upon the nutrient availability and available space, but no quantitative link is drawn between either variable and the value of K. The slope of the model is assumed to be entirely dependent upon time, but it sees unlikely that this assumption would often be satisfied. Presumably, the rate of growth is in reality affected by many more variables. For example the study by Onuma et al. (2016) found algal blooms to initiate just 24 hours after melting began which the authors attribute to either a different algal species (can you confirm this with observations?) or 'weather conditions'. This suggests that the growth rate varies between sites and cannot be assumed constant. As I understand it, weather conditions are precisely what you are trying to analyse as potential drivers of algal growth in this paper, so it seems contradictory to invoke them as an additional source of uncertainty. It seems likely that the initiation and growth rate of snow algal blooms are the result of a combination of meteorological factors plus site specific variations such as nutrient availability in the snowpack that may well be difficult to unpick, limiting the predictive capability of the model on other snowpacks. Can the authors provide any more data or discussion

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to support the applicability of the model?

Specific Comments:

pg 1 line 23: change ‘bloom’ to ‘blooms’, change ‘changes’ to ‘change’ pg 1 line 24: change ‘bloom’ to ‘blooms’ pg 2 line 20: delete ‘works’ pg 2 line 26: these references are a mixture of ice algae and snow algae. Ice algae has been suggested to enhance ablative losses to the GrIS by reducing albedo (see more recent papers by Tedstone et al. 2017 and Stibal et al. 2017). To my knowledge, evidence for accelerated snow line retreat due to snow algal blooms has not yet been presented for Greenland. I suggest rewording this sentence accordingly or providing specific references for accelerated snow line retreat. Pg2 line 28: The Lutz studies were limited to the visible wavelengths and were not really measuring albedo, but a proxy. Better to say that they showed algae can modulate snow reflectance in the visible wavelengths. Pg3 line 5: superscript kmˆ2 pg4 line 4: normalising to area presumably requires that the algae only inhabit an extremely thin layer on the upper surface of the snow – other studies (e.g. Thomas et al., 1979; Hodson et al., 2017) indicate that subsurface red algae can exist. Are you confident that the algae were confined to the upper surface? Is this supported by your observations? Pg6 line 21: change ‘active radiation’ to ‘photosynthetically active radiation’ pg 7 line 24: delete ‘can’ pg 8 line 33: statistical significance should be supported by test name and values. pg8 line 28: change ‘snowfileds’ to ‘snowfields’

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