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.

We would appreciate very much a number of constructive comments. We also glad in being able to hear that you enjoyed reading our paper. Our responses (blue text) to each the reviewer's comment (in black text) were described as follows. We also uploaded manuscript, which was revised with yellow marker as suggested, on the discussion board.

Major Comments:

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 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?

As the reviewer pointed out, algal cell abundance is possibly increased by the effects of scavenging or wind-delivery and reduced by the effect of melt water. We conducted additional analysis to discuss the effect of scavenging on algal growth in snowpacks (please see the Figures S1 and S2 in this response letter). The results show vertical profiles in algal cell concentration, snow density and snow temperature in snowpit of the study sites before the first appearance of snow algae on the surface. The vertical profiles showed that all of the snow layers in the study sites didn't include any algal cells, indicating that a scavenging by snow melt don't affects a temporal change in algal abundance. The effects of wind-delivery appeared to be small enough to calculate algal growth because the initial concentration of algae on the surface, which was likely due to wind in the study sites, was substantially smaller than the final concentration (Site-A: 3.1×10^3 cells m^{-2} vs. 3.5×10^7 cells m^{-2}, Site-B: 7.4

cells m^{-2} vs. 5.0 × 10^{5} cells m^{-2}). The movement of algal cells by melt water also appeared to be small enough to calculate algal growth because algal cell concentrations in the study sites gradually increased with snow melting except for the period from day 201 to 214 at Site-A. Therefore, we simulated the temporal changes in algal cell concentration using logistic model based on the assumption that there is no inflow or outflow of algal cells on the snow surface. We have revised the manuscript to discuss the effects of scavenging, wind-delivery and melt water on a temporal variation in algal abundance (pg 4 lines from 4 to 6, pg 6 lines from 10 to 11 and pg 8 lines from 17 to 26). And, we have added the snowpit result to the discussion about the origination of algal cell (pg 7 lines from 6 to 7).

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 to support the applicability of the model?

Generally, the Michaelis-Menten equation has been used to control a growth rate of microbial cell using a nutrient concentration and coupled with the Malthusian model for utility reproduction of algal growth in lakes or oceans (e.g. Lavoie et al., 2005). However, we consider that it is difficult to use Michaelis-Menten equation, which requires a temporal change in nutrient concentration related to algal growth, for simulation of algal growth rate on a glacier now because it is hard to analyze a temporal change in nutrient concentration in various glaciers. On the other hand, modeling using a carrying capacity of a logistic model has the advantage of that data of nutrient concentration in snow aren't needed to simulate algal growth. Carrying capacities in various snowfields is likely to be obtained by approximation between observational maximum algal abundance and mineral dust weight because nutrients, which are required for algal growth, are supplied on glacier with mineral dust as described in the manuscript (pg 10 line 13). For these reasons, we used a logistic model to simulate algal growth in this study. As the reviewer pointed out, there probably is an insufficient information for applying of logistic model to various fields. In future, a logistic model should be validated or improved on the basis of biological process in order to accurately reproduce a temporal variation in algal cell abundance and apply the model to various snowfields. We have revised the manuscript about the utility of a logistic model to apply to snowfields worldwide and the discussion of carrying capacity as suggested (pg 10 lines from 18 to 20 and pg 11 lines from 1 to 6).

Specific Comments:

1. pg 1 line 23: change 'bloom' to 'blooms', change 'changes' to 'change'

The words have been corrected (pg 1 line 23).

2. pg 1 line 24: change 'bloom' to 'blooms'

The word has been corrected (pg 1 line 24).

3. pg 2 line 20: delete 'works'

The word has been deleted (pg 2 line 20).

4. 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.

The sentence has been revised as suggested (pg 2 lines from 26 to 29). We have added the explanation about reduction of surface ice albedo due to ice algal bloom and revised the explanation about accelerated of snow melting due to snow algal bloom as suggested. The following reference has been added at pg2 line 27.

Stibal, M., Box, J. E., Cameron, K. A., Langen, P. L., Yallop, M. L., Mottram, R. H., … Ahlstrøm, A. P.: Algae drive enhanced darkening of bare ice on the Greenland ice sheet, Geophysical Research Letters, 44, doi: 10.1002/2017GL075958, 2017.

Tedstone, A. J., Bamber, J. L., Cook, J. M., Williamson, C. J., Fettweis, X., Hodson, A. J., and Tranter, M.: Dark ice dynamics of the south-west Greenland Ice Sheet, The Cryosphere, 11, 2491-2506, doi: 10.5194/tc-11-2491-2017, 2017.

5. 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.

The sentence has been revised as suggested (pg2 line 29).

6. Pg3 line 5: superscript km^2

The word has been corrected (pg 3 line 10).

7. 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?

As the response to major comment 1, the vertical profiles in algal cell concentration in snowpack of the study sites show that there were no algal cells in snowpack before snow algae first appear in surface snow (Figures S1 and S2). In addition, snow algal cells are likely to be supplied from the atmosphere on surface snow in the study sites. These results suggest that snow algal cells concentrated to surface snow although a part of the algal cells may be flowed to subsurface snow by the melt water. We have revised the manuscript to discuss the effect of algal cells in subsurface snow on algal growth in surface snow (pg 8 lines from 21 to 24).

8. Pg6 line 21: change 'active radiation' to 'photosynthetically active radiation'

The words have been changed (pg 7 line 1).

9. pg 7 line 24: delete 'can'

The word has been deleted (pg 8 line 11).

10. pg 8 line 33: statistical significance should be supported by test name and values.
We have added a result of statistical test (Student's *t*-test) to the sentence (pg 9 lines from 29 to 30).

11. pg8 line 28: change 'snowfileds' to 'snowfields'
The word has been corrected (pg 9 line 23).

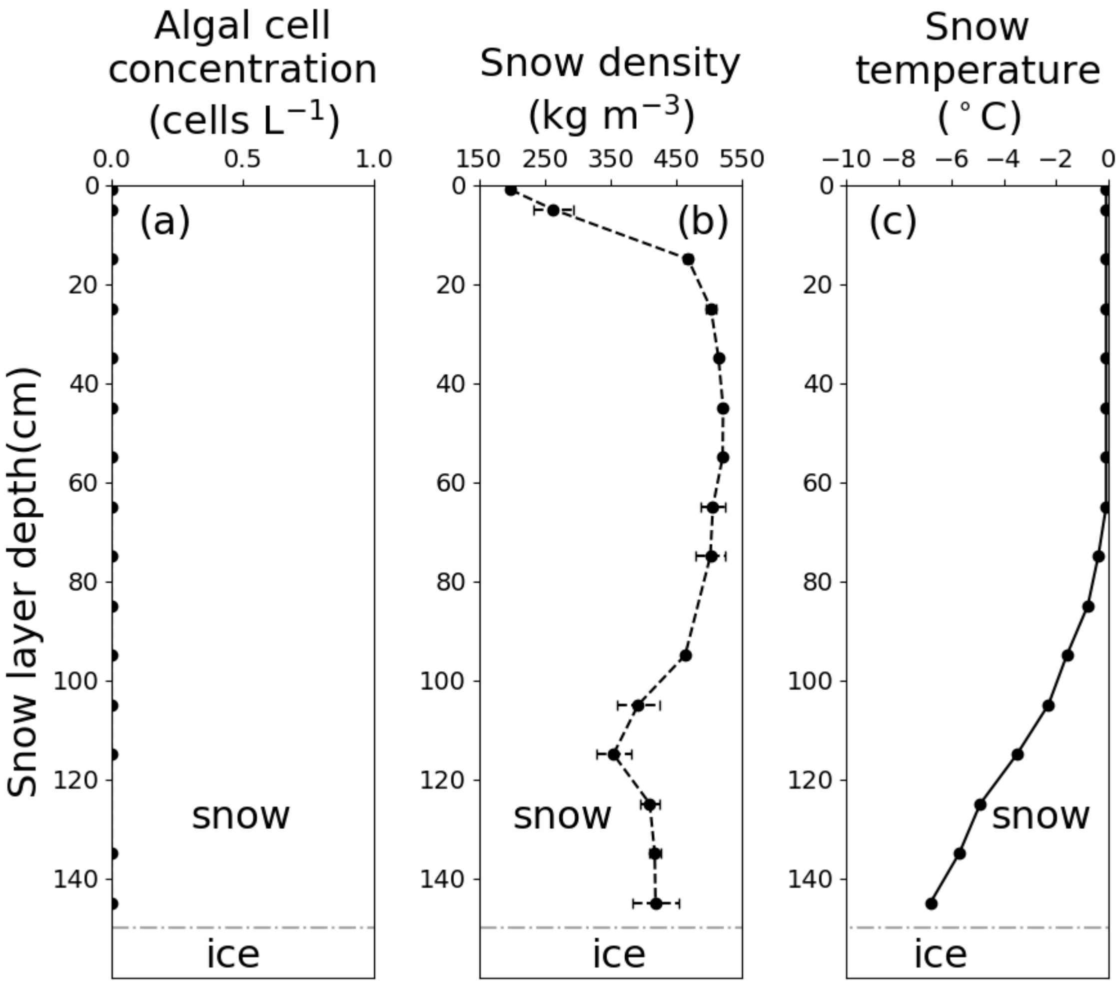

Figure S1. Vertical profiles of snow algal abundance and physical properties in a snow pit on day 162 at Site-A. (a) algal cell concentration, (b) snow density, (c) snow temperature in snow. Standard deviation shown by error bars.

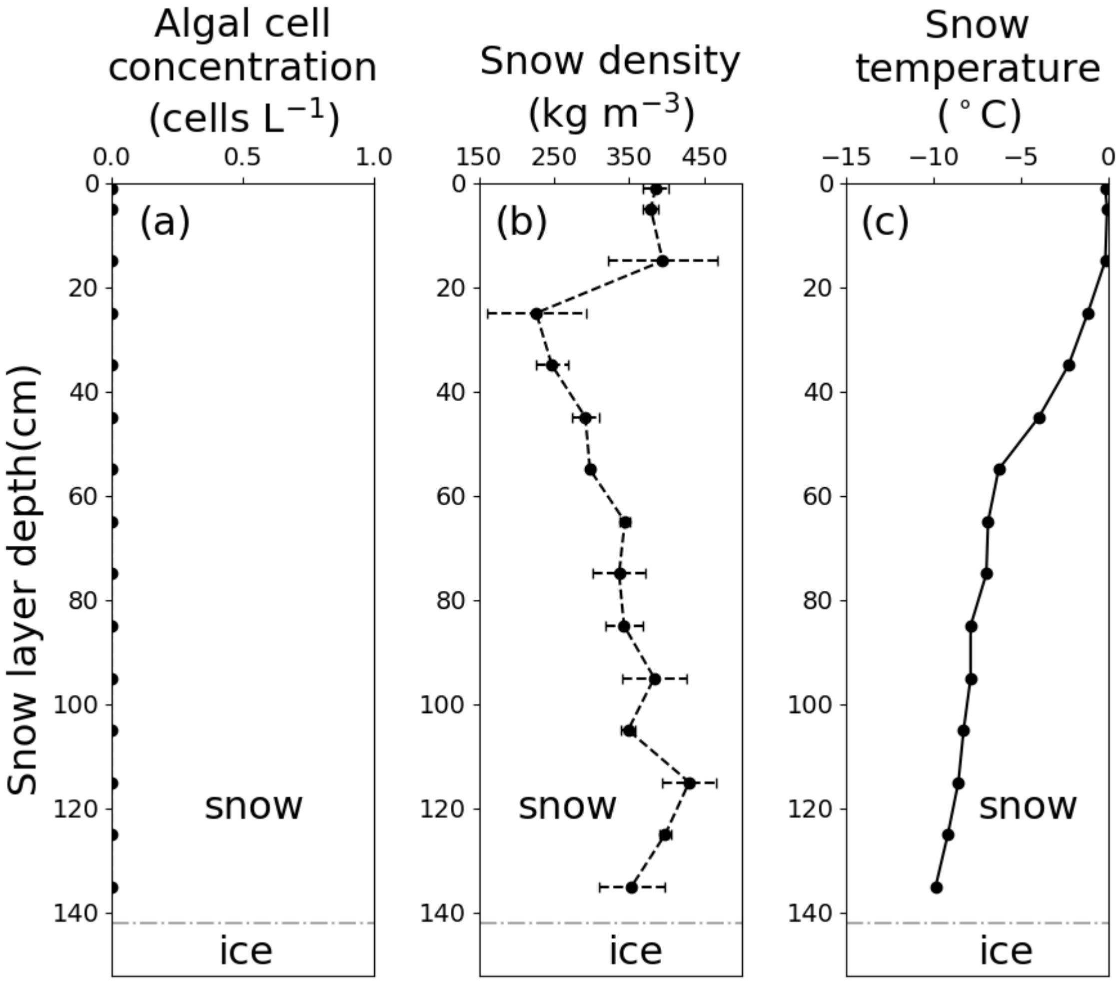

Figure S2. Vertical profiles of snow algal abundance and physical properties in a snow pit on day 168 at Site-B. (a) algal cell concentration, (b) snow density, (c) snow temperature in snow. Standard deviation shown by error bars.