Cook et al.: “Glacier algae accelerate melt rates on the western Greenland Ice Sheet”.
Recommendation: major revision required.

We are very grateful for the reviewer’s critique of our paper. Several important points were raised that we have reflected upon and endeavoured to address in the revised manuscript. We have made substantial revisions that we hope have improved the manuscript. In particular, we have repeated our mineral imaginary refractive index retrieval and also added a sensitivity study showing the response of glacier ice-albedo to a range of Saharan dusts. We have also restructured the paper to better demonstrate the potential interannual feedback. Detailed responses to the major and minor comments are detailed below. Please note that we have uploaded one revised manuscript to satisfy both sets of review comments.

General statement

This is an interesting and important paper, attempting to quantify the role of algal growth in the energy budget of the Greenland Ice Sheet during the melting season. By comparing and contrasting two dramatically different summer seasons, the importance of interannual variability is shown. The paper should be published after the major comments are addressed.

Major comments

(1) My main concern, the one requiring “major revision”, is the inference of optical properties for dust. Figure 2B shows the imaginary part of the refractive index increasing by a factor of 4.4 from 0.4 to 0.7 µm wavelength. This contrasts with the usual finding for desert dust, whose imaginary index decreases by a factor of about 4 from 0.4 to 0.7 um, resulting in a red color in reflection (e.g. Patterson et al. 1977, Müller et al. 2009, Wagner et al. 2012). [These and other references are discussed by Dang et al. (2015, Section 6.4).] The dust of Figure 2B would have a blue color. Blue minerals do exist, but they are rare. I am therefore skeptical that Skiles’s method is obtaining the correct answer. If the absorption spectrum of Figure 2B is to be believed, the authors should present evidence for blue minerals in dust that deposits in this part of Greenland. Furthermore, the imaginary index shown for wavelength 0.5 um (4×10^{-6}) is a factor of 1000 smaller than what is typical of desert dust in the references cited above. Figure 2C shows the albedo effect of an arbitrary 300 ppm of dust. I didn’t find the actual dust concentration given in the paper for the ice that was measured; maybe I missed it. In any case, a plot or table of the albedo effect of the measured dust concentration should be added to the paper.

We thank the reviewer for pointing this out – it is correct to point out the unrealistic refractive index. Thanks to the comment, we revisited our reflectance measurements and found that the reflectance measured in the integrating sphere did indeed suggest higher reflectance in the red wavelengths, which led us to a bug in the inverted DISORT retrieval. This was fixed and the refractive index revised and incorporated into our radiative transfer model. The single scattering optical properties are comparable to dust of a similar size range presented by Flanner et al. (2007) and Skiles et al. (2017). The dusts found on the bare ice are not characteristically Saharan, have only trace amounts of red minerals and are much less strongly absorbing than those in Wagner et al., Polashenski et al., etc. This is a consistent finding between our mineralogical investigations (which will comprise an entire paper to be submitted imminently by co-author McCutcheon) and past studies such as Wientjes et al. (2011) and Tedesco et al. (2013) on the K-transect and Sanna and Romeo (2018) slightly further north at Equip Sermia.

To protect against possible underestimation of the mineral albedo-lowering effect, we also gathered optical properties for other dusts relevant to our study and included those in the model too, enabling some sensitivity testing. Those other dusts are the “global average” dusts from Flanner et al. (2007) and those from Polashenski et al. (2015) who recreated GrIS dusts from past literature studies and simulated low, median and high hematite scenarios. Those dusts are “characteristically Saharan” so as well as providing a sensitivity test, they also demonstrate the effects of possible future Saharan dust deposits by atmospheric transport. In the revised manuscript we compared the albedo reducing effects of algae against our real mineral dusts as well as those from the literature.

With our new k values and sensitivity study, our conclusion that algae are the dominant light absorbing particle remains unchanged. Please see the revised manuscript for full details of the analysis and findings.

(2) The size distribution for dust is shown in a supplementary figure 1E, but the interesting data are crowded up to the far left end of the figure, so the location and shape of the peak are hard to see. I would like to see an expansion of the 0-3 um region of this figure. This is an important finding, so the new figure should be put in the main paper rather than in the supplement.

We have provided an updated PSD plot with an inset frame showing the range 0-4 um. This is presented along with the mineral dust single scattering optical properties in Figure 4 in the revised manuscript.

(3) Color of algae. The algae in Figure 1B are bright red. But the dark zone in Figure 1A is gray, not red. Please explain.

We agree - the microscope image is too red and does not represent the true colour of the algae – it was selected because it clearly shows the cell morphology, but on reflection the colour is critical to portray accurately. A new image has been added that more accurately shows the true colour of the algae. Sometimes the algae really do have a red-purple tinge to them that can even be discerned by eye in localised areas of very heavy biomass loading.

(4) The ice surface is modelled to consist of large hexagonal columns. That is unlikely to be realistic; the shapes are probably irregular. The main thing you need to consider is what is responsible for scattering of light in glacier ice, namely bubbles and cracks, which together determine the most important quantity, namely the specific surface area (SSA). Then a simpler radiative-transfer method could be used, as was done for glacier ice by Dadic et al. (2013, Section 6.1).

The near surface is composed of large, irregularly shaped grains as the reviewer suggests, and the SSA is the primary factor governing scattering in these ice media. To my knowledge, the assumption of hexagonally shaped grains shouldn't invalidate our approach as long as the SSA of the upper layers of ice is realistic. Since we do not have a good method for determining the SSA of the weathered layer of bare-ice empirically, we can only judge this by how closely we can recreate our field spectra. The new model does a much better job of this than the original SNICAR model based on Mie scattering. I believe that the new model is the best available for ice algae on weathered ice and a useful addition to the literature, but I do also appreciate its limitations and would be very interested to follow up by more closely examining the Dadic et al. (2013) model. We have added a sentence to acknowledge this in section 2.5.

(5) The authors collected data from a "dark" year (2016) and a "bright" year (2017), providing an interesting contrast. But the abstract and the conclusion, which should summarize the results, instead give the results only for the dark year, thus exaggerating the average effect of the algae. The reader will thus conclude that the authors are claiming more importance for their research topic than is justified, which is a shame. The authors have missed the opportunity to use the 2016-2017 contrast to highlight the effect of future climatic expansion of the snow-free season.

We agree fully and have reorganised and refined the manuscript to a) avoid exaggerating the effect of the algae, and b) use the interannual variability to demonstrate the potential growth-melt feedback.

(6) It would be good to extend this analysis to cover all of the GrIS. Is that possible, or do the Sentinel satellites not survey the entire ice sheet?

The classifier was deliberately not applied over the entire ice sheet because our lack of field samples from outside of the Kangerlussuaq region was prohibitive – before upscaling to the ice sheet scale it would be better to have ground spectra and samples from further north that indicate the same processes are in operation across the whole ablation zone. This is especially the case after addressing the comments from Reviewer 1, who had concerns about upscaling over the south-western region. However, scaling our classifier over the entire GrIS western margin for the entire Sentinel-2 record is underway, to be worked up into a paper once our field samples from near Upernavik (NW Greenland) are fully analysed.

(7) The author-list may be too long. The Author Contribution Statement concludes with the statement: "Other authors commented on the style and content of the final manuscript." This contribution alone does not qualify one to be an author, according to established principles of authorship, as given for example at http://www.icmje.org/recommendations/browse/roles-and-responsibilities/defining-the-role-of-authors-and-contributors.html Dr. Cook should carefully consider whether some of his 23 coauthors should be moved to the Acknowledgments. By crowding into the author-list, they deprive the real workers of the credit they deserve.

This paper represents the culmination of almost 3 years of dedicated work, drawing upon collaborations across a wide range of disciplines - many people have made important contributions along the way. The author statement has been amended to better reflect the contribution of these authors to the study.

Minor comments

line 34. Change "albedo-reducing" to "albedo-enhancing" (since addition of dust raised the albedo).

This has been addressed by default since we have updated our whole mineralogy section

line 35. "western sector of the GrIS" Define, by adding the span of latitudes (65-70 N).

This has been amended

line 187. Give a reference for photosynthesis using 5% of absorbed sunlight.

Citations added

line 191 (also 425). Latent heat of fusion is 334 J/g, not 334 J/cm 3 . This is important since on line 75 the ice density is varied from 0.3 to 0.7 g/cm 3 .

This has been amended

line 288. Give a reference for these roughness-heights.

Citations added

line 313. “SN=0”. I am surprised that there was no algae in the snow.

We also expected that a snow algal bloom might form on the melting snow, and this was part of the motivation for establishing a field camp above the snow line in 2017. However, we never observed snow algae, even in light patches, forming on the snow at any point during the transition from dry snow to slush to bare-ice exposure, and this is corroborated by our field samples. We sometimes found them in samples of cryoconite, and we know from previous molecular work (Lutz et al. 2018) they are present in the environment, but we have not yet witnessed snow algae taxa in microscope samples or by visual assessment of the ice surface at any time across three field seasons between 2016 – 18 period, apart from one instance of a red algal bloom seen on snow inside a crevasse near the ice margin in August 2017, which we saw from the helicopter window as we departed!

line 400. Define what you mean by “area” of an absorption feature.

This has now been defined in the manuscript

line 440-441. “Directional reflectance . . . approximates the measurements made by orbital remote sensing platforms”. This is true if the satellite is nadir-viewing, since your surface measurement was a nadir view. Otherwise, your surface measurement is biased low because you are looking directly down into cryoconite holes, whereas the cryoconite material is hidden from the view of an obliquely-viewing satellite.

Yes, there may be some uncertainty due to off-nadir satellite viewing angles; however it is still a more appropriate measurement than albedo measured using a cosine collector.

line 550-551. “We demonstrate that the growth of algae occurs over a large proportion of the ablating area of the GrIS”. For this statement to be true, you would need to give estimates for the entire ablation zone, not just latitudes 65-70 of West Greenland.

This has been amended

Figure 3 caption. On line 1, “Albedo map” is for what wavelength? On line 3, change “D” to “E”, change “E” to “F”. On line 4, change “F” to “G”.

Thank you for pointing out these errors, we have amended them. “Albedo” has been changed to “Broadband albedo” - the values were for the whole solar spectrum calculated from multispectral reflectance using the Liang et al. (2000) or Knap et al (1999) narrowband to broadband conversion equations.

Figure 4C,D. The tick marks for months are for the beginning of the month or the middle of the month?

The tick marks are for the beginning of the month – this is now stated in the figure caption.

Figure 4D, vertical axis label. What does the adjective “cumulative” mean? Normally “cumulative” implies an integral.

Cumulative refers to the running total, i.e. the cumulative sum of pixels that have gone “dark” over the albation period.

Table 2. The percentages for the UAV add to 94%. What is the remaining 6%? For the Sentinel columns, the percentages add to 1%; probably they should all be multiplied by 100.

The remainder was classified as snow. We did not report the snow because we are interested in the bare ice zone, and the snow detected in S2 imagery was at or above the snow line, skewing our proportional coverage calculations for the bare ice classes. On reflection, it is probably better to report the snow coverage for the UAV images, as there are some small rotten ice patches. These are not detected in the S2 imagery because of their very small spatial extent compared to the ground resolution of the S2 sensor. We have added the UAV snow coverage to the table and clarified in the table caption.

Figure S1A, vertical axis label. Define IRF.

This has been amended

Figure S1B. Change the color coding to agree with Figure 1C.

This has been amended

Figure S1 caption, line 1-2. Delete "per unit wavelength", because the values are just unitless.

This has been amended

Very minor comments

line 64. Ryan et al 2018. Do you mean 2018a or 2018b?

amended

line 76. Define IMAU.

Defined

line 107. Define GNU.

This is a recursive acronym nested within a recursive acronym…! GIMP stands for GNU Image Manipulation Program, where GNU itself is a recursive acronym that stands for "GNU Not Unix" and describes a collection of free operating system distributions based on UNIX - it might not be that helpful to define in the paper as the definition is GNU's all the way down…!

line 109. Hildebrand. The reference list says instead Hillebrand.

This has been corrected

line 127. Define PSD.

PSD has now been defined on first use.

line 152. Cook et al 2017. Do you mean 2017a or 2017b?

This has been clarified

line 160. Dauchet et al. 2015 is missing from the reference list.

Reference added

line 169. Warren and Brandt 2008 is missing from the reference list.

Reference added

line 180. Define IRF.

Now included in revised manuscript

line 362. Lee and Pilon 2013 is missing from the reference list.

Reference added

lines 535-537. Say that this sentence applies to the year 2016.

This sentence was revised in the new manuscript

line 614. Update reference from TCD to TC.

Amended

line 620. Reference is out of order.

Amended

line 739. Reference is out of order.

Amended

line 743. Update reference from TCD to TC.

Amended

Spelling and punctuation
Hyphenate these adjectives:

albedo-reducing (lines 31, 61, 339, 343, 347): *amended throughout*
light-absorbing (lines 48, 69, 324, 531): *amended throughout*
bare-ice (line 63, 263, 269): *amended throughout*
long-term (line 65): *amended throughout*
ice-sheet (line 67, 231, 389): *amended throughout*
remote-sensing (line 72, 78, 441): *amended throughout*
high-algal-biomass (line 115): *removed during rewrite*
volume-weighted (line 152): *amended*
satellite-derived (line 222): *amended*
random-forest (line 232): *amended throughout*
cloud-free (line 235, 272): *Amended once, second instance removed in rewrite*
dark-ice (line 265, 266): *amended*
two-stream (line 346): *amended*
low-albedo (line 496): *amended*

line 87. Insert comma after “conditions”: *comma added*
line 191. Change “determine” to “determined”: *sentence removed in revised manuscript*
line 210. “oin”. You probably mean “in” or “on”: a*mended*
line 238. Insert comma after “columns”.: *comma added*
line 256. Change “model are” to “models are”.: *sentence removed in revised manuscript*
line 262. Insert comma after “Terra”: *sentence removed in revised manuscript*
line 275. Insert comma after “year”: *sentence removed in revised manuscript*
line 276. Insert comma after “available”: *sentence removed in revised manuscript*
line 441. Change “are” to “is”: *sentence removed in revised manuscript*
line 443. “The this” needs fixing: *sentence removed in revised manuscript*
line 645. Capitalise “smith”: *amended*
line 658. Fix the author-list: *fixed*
line 678. Change Fettweiss to Fettweis: *amended*
line 714. Fix “H.-G.rensen”: *fixed*
line 746. Fix “sheetL”: *fixed*
Supp Info 3. Change “Assymetry” to “Asymmetry” in three places: *amended*

References

Dadic, R., P.C. Mullen, M. Schneebeli, R.E. Brandt, and S.G. Warren, 2013: Effects of bubbles, cracks, and volcanic tephra on the spectral albedo of bare ice near the Trans-Antarctic Mountains: implications for sea-glaciers on Snowball Earth. J. Geophys. Res. (Earth Surfaces), 118, doi:10.1002/jgrf.20098.

Dang, C., R.E. Brandt, and S.G. Warren, 2015: Parameterizations for narrowband and broadband albedo of pure snow, and snow containing mineral dust and black carbon. J. Geophys. Res., 120, doi:10.1002/2014JD022646.

Müller, T., A. Schladitz, A. Massling, N. Kaaden, K. Kandler, and A. Wiedensohler (2009), Spectral absorption coefficients and imaginary parts of refractive indices of Saharan dust during SAMUM-1, Tellus B, 61, 79–95.

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