Second review of Cook et al. for TCD, August 2019, by Stephen Warren
“Glacier algae accelerate melt rates on the south-western Greenland Ice Sheet”

Recommendation: Major revision required.

This paper is still not ready for publication. Concerning the melting ice surface of the West- Greenland ablation zone in summer, the authors argue that addition of dust causes the ice albedo to increase, so that any reduction of ice albedo by light-absorbing impurities would be due to algae. This denial of a role for dust in reducing albedo is based on several questionable arguments, which I will point out in this review. Most of these questionable arguments can be classified into one of three classes of disconnects: (a) The authors’ response to my review, saying how the paper was changed, does not correspond with what the revised paper actually shows. (b) The way figures are described in the text does not correspond to what the figures actually show. (c) References cited in support of a claim do not actually provide the claimed evidence.

I do not doubt that algae absorb sunlight, but I do doubt the quantitative attribution of albedo change on Greenland to algal abundance. A lot of work went into this project, and I would like to see it come to more robust conclusions. It has the potential of becoming an important paper.

- Thank you for another round of review comments. In this document I will address each comment point by point. We have re-examined our paper in detail and overall we agree about the DISORT inversion issues. Thanks to the comments we ran a series of tests and found some unexpected behaviour in the model and have been unable to produce model outputs that we have sufficient confidence in to justify the model’s inclusion in this resubmission. For this reason, we have removed the DISORT inversion from the study. Instead, we have generated optical properties for three bulk mixtures of dusts that approximate the mineralogy and size distribution measured in the field and incorporated them into our model. In addition we have included a sensitivity study as supplementary information (Supp Info 8) that includes previously-published mineral dusts that have different mineralogies and size distributions. More details and responses to individual comments are provided below.

Major comments

(1) The major comment of my first review was to point out that the imaginary part of the dust refractive index should decrease across the visible spectrum from 300 to 700 nm, not increase as shown in the first submission. In their Response to Reviewers, the authors now thank me for “pointing out the unrealistic refractive index”, but then in their revised manuscript their dust imaginary index still increases across the visible (by a factor of 2.7 from 300 to 700 nm), as shown in the new Figure 3C. [An example of Disconnect Type “a”.]

- We appreciate the reviewer’s concerns regarding the DISORT inversions. We have reviewed the modelling in detail and we agree that the retrieval was not correct, and we have not been able to generate an imaginary refractive index that we have complete confidence in reporting in the revised manuscript. The mineral dusts did not look blue to the eye - they were very fine, near-white, dominated by quartz and feldspar minerals with very low abundance of red minerals. This is confirmed by mineralogical analysis undertaken by McCutcheon et al. (in preparation) and is consistent with previous literature (e.g. Wientjes et al. 2011). For these reasons, we have decided to remove the DISORT inversion from our paper and instead we have generated “synthetic” bulk refractive indices from our measured particle size distribution, measured relative mineral abundances and imaginary refrative indices for those minerals gathered from past literature. We have also persisted with our sensitivity study that includes typical Saharan dusts from SNICAR and low, medium and high-hematite dusts from Polashenski et al (2015), although we have moved this into Supplementary Information 8 to keep the main message in the manuscript clear. We consider this to be the most robust support for our conclusions regarding the albedo-lowering effects of mineral dusts and algae that we can feasibly produce with the available data. As you will see in the revised manuscript, this new methodology supports our original conclusion that mineral dusts have a very small direct albedo-reducing effect on the south-western Greenland Ice Sheet.

The relevant reworked sections of the manuscript are sections 2.5, 2.6 and 3.2, Table 1, Table 2, Fig 3B,C and Supp Info 8.

(2) I don't want this extraordinary claim of blue dust to enter the literature on the composition of Greenland ice without further evidence. What mineral composition gives it the blue color indicated by Figure 3C? In their response to my review, the authors indicate that the mineralogy of the dust is consistent with Figure 3C, and that a paper on this topic is soon to be submitted by one of the authors (McCutcheon). A brief summary of the mineralogy in that forthcoming paper should be included in this paper; it could be cited as "unpublished data" or "manuscript in preparation".

- We fully appreciate and agree. As explained above, we have eliminated the DISORT inversion that was generating this unusual refractive index. New data from McCutcheon et al. (in preparation) is now used to generate a "synthetic" bulk refractive index for our local mineral dusts and that paper is cited as "in preparation" as suggested. The mineralogy of these simulated local dusts is presented in the new Table 1. This is detailed in sections 2.5, 2.6 and 3.2.

(3) The authors have ignored the request in my Major Comment #1, in which I asked the authors to show the computed albedo effect of the measured dust concentration; they continue to show just the computed albedo effect of arbitrary amounts of dust (100, 300, 500 ppm), and similarly for arbitrary concentrations of algae. At least a table is needed, giving measured dust and algal concentrations (ppm by mass). The numbers of cells are shown in Figure 2C, but these need to be combined with algal-cell size distributions to get the mass.

- We have now incorporated new dust concentration data from McCutcheon et al (in preparation) into our study to address this comment – specifically the mean (342 $\mu g_{dust}/g_{ice}$) and maximum (519 $\mu g_{dust}/g_{ice}$) measured dust concentrations from H_{bio} sites. We also estimated the algal cell mass-mixing ratio from our microscope images. The mean cell abundance was 2.9×10^4 cells/mL and the maximum was 4.91×10^4 cells/mL. We assumed a cell density of 0.87 g cm^{-3} (Hu, 2014) and an ice density of 0.917 g cm^{-3} to calculate the mean and maximum mass mixing ratios of 349 and 646 $\mu g_{algae}/g_{ice}$.

We have also continued to include additional hypothetical mass mixing ratios in our sensitivity study in Supp Info 6 to demonstrate that our conclusions are robust to a range of mass-mixing ratios.

(4) Lines 272-273. Tedesco et al. (2013) is cited as indicating "a lack of red mineral phases". In fact, Tedesco's Figure 6a shows that both dust and algae are "red", and that dust is redder than algae; Tedesco speculated that the goethite they found in their samples had dehydrated to hematite in the drying and heating process. Goethite is the hydrated form of hematite; it is not as absorptive as hematite but its absorption coefficient likewise decreases across the visible. It is true that goethite has a yellow appearance rather than red, but it is misleading to cite Tedesco as finding "a lack of red mineral phases", since the present authors are using the adjective "red" here to characterize the spectral slope of reflectance, which increases toward the red for both goethite and hematite. [Disconnect type "c"]

- We respectfully disagree with the reviewer on this point, for the following reasons:

1) Tedesco et al. (2013) identified an average of only 0.3 % goethite in the Greenland cryoconite samples, which, even if present as hematite prior to transformation during sample processing, would likely not be sufficient to account for the "red" colour in question.

2) Figure 6a in Tedesco et al. (2013) does not show that Greenland surface dust is redder than algae. The samples studied in Tedesco et al. (2013) were from cryoconite rather than surface ice, and therefore are unlikely to have contained glacier algae in abundances comparable to those we have measured in dark surface ice. In fact, the organic matter contained in the cryoconite is largely bacterial, cyanobacterial, humic substances, extracellular polymers and necromass – optically very different from surface algae.

3) Furthermore, the "red" colour measured in the mineral dust by Tedesco et al. (2013) is not representative of the natural material due to the manner in which the samples were processed. The samples were heated to 500 and 1000 degrees C, which is entering hornfelsic grade metamorphic conditions. This will have altered Fe-containing hornblende and pyroxene mineral phases thereby generating the reported "red" colour, which cannot be used to draw conclusions about the true reflectivity of the mineral dust in situ. In contrast, we *used a suitable chemical treatment rather than heat to remove organic matter from the samples and have data likely to represent more realistic in situ mineral optical properties.*

We have added text to the revised manuscript to make these points clear (line 556 - 564).

(5) Lines 518-521. "The imaginary refractive index of the mineral dust sample (Fig 3C) . . . indicating . . . scarcity of red minerals in the bare ice." This comment is not forthright; there is no mention of the factor-of-2.7 increase of imaginary index from 300 to 700 nm, which indicates not merely a scarcity of red minerals but actually a dominance of blue minerals. Don't be so timid! You must highlight this strange imaginary index, and point out how it contradicts the behavior reported by Tedesco et al. 2013. [Disconnect Type "b"]

We have reworked this section after removing the inverse modelling from our study.

(6) Lines 539-541, discussing Figure 3B. Albedo spectra for algae "downsloping with increasing wavelength between 0.35 and 0.45 microns . . . and a gentle increase to 0.70 microns. These spectral features are consistent with our field spectra for algal ice" This is not true. The field spectrum for algal ice (Figure 2B) starts at 0.40 not 0.35, and shows albedo increasing, not downsloping, from 0.40 to 0.45 microns. And Figure 2B (field) shows a steep increase from 0.6 to 0.7 microns, whereas the dashed line in Figure 3B (model) is flat from 0.6 to 0.7. [Disconnect Type "b"]

We have reworked this section and made more accurate descriptions of the spectral albedo.

(7) Lines 574-575. ". . . algal cells had a greater albedo-reducing effect than mineral dusts in north-west Greenland (Aoki et al. 2013)." This summary of the Aoki paper is misleading. Aoki et al. did conclude that the imaginary index of algae was larger than that of mineral dust, but did not conclude that most of the albedo reduction was due to algae. Their total impurity mass in the ice was 1127 ppm, of which 29 ppm (2.6%) was organic carbon (algae), so ~1100 ppm was dust. Their Figure 4b shows that they could explain most of the albedo reduction by 1000 ppm dust; the remainder (which looks like about 5% to me) is then attributed to algae. [Disconnect Type "c"]

We revisited Aoki et al. (2013) and have removed the citation from our manuscript for the following reasons. There is conflation between surface impurities, cryoconite and surface dust. Their figure 2B shows what those authors consider to be a "cryoconite" surface, which is what we would now consider to be a mixture of surface dust and glacier-algal biomass similar to that included in our study. Cryoconite is properly defined as: discrete granules of biological and mineral material that typically have a very dark brown-black colour and reside on the floor of cryoconite holes unless the local energy balance conditons favour them being evacuated onto the bare ice surface. For this evacuation to happen, turbulent heat fluxes must exceed radiant heat fluxes for a period of several days (i.e. persistent cloudy conditions) so that the weathered surface ablates downwards to expose smooth solid ice and the cryoconite holes that occupy the weathered crust "melt out". The morphology of the ice shown in their Figure 2B is not consistent with this process having occurred, and this is further confirmed by the sentence "there were also cryoconite holes (water-filled cylindrical melt-holes with cryoconite on the bottom)". If there are cryoconite holes present, any dispersed cryoconite must be spatially discrete, and the distributed impurities they refer to as cryoconite must actually be a dust and algae mixture comparable to that observed at our field site. Furthermore, they have not generated optical properties for the actual dust found at their field site, but simply imported dust optical properties from a pre-existing library where the mineralogy is assumed to be mostly illite, calcite, feldspar and chlorite derived from Asian dust samples (see Aoki et al, 2005) that are very different to our local mineralogy (and perhaps to the true mineralogy at their field site too). For these reasons, it is not helpful to compare Aoki et al.'s (2013) interpretation of their albedo data – certainly not their separation of mineral and biological effects - to ours.

Another important point is that where Aoki et al. (2013) refer to algae, they are making reference to the bright red snow algae that grows on melting snowpacks. This is taxonomically, morphologically and optically very different to our glacier algae.

Aoki et al. (2005): Sensitivity Experiments of Direct Radiative Forcing Caused by MineralDust Simulated with a Chemical Transport Model Journal of the Meteorological Society of Japan, Vol. 83A, pp. 315--331, 2005315

(8) Lines 544-546, and Figure 2C. The authors point out that mineral dust particles can "act as substrates for the formation of low-albedo microbial-mineral aggregates". This suggests an alternative explanation of the correlation shown in Figure 2C: The algae may be concentrated in patches of ice that have high mineral content, so the cell count then would be correlated with the albedo reduction caused by dust.

We agree, this is a central question that we have attempted to address using our radiative transfer modelling.

(9) Section 3.3, lines 577-610. This section analyzes the albedo trough centered at 1.02 microns, following Nolin and Dozier (2000). Nolin and Dozier found the 1.02-micron trough to be deeper for lower albedo (coarse-grained snow), whereas Supp Info 5B here shows the opposite, namely deeper trough for higher albedo. The 1.02-micron feature is therefore not useful for discussing "indirect effects of algae". The entire section 3.3 should therefore be shortened to just the last four lines 607-610, making a single sentence starting "Algal growth is stimulated . . . " [Disconnect Type "c"]

Yes, the relationship does show a deeper trough for higher albedo. We consider this to be an artefact of an overall lower albedo across the entire spectrum effectively "dampening" all of the reflectance features. However, the point we were making was simply that there is albedo reduction in the NIR associated with increased LAP loading as well as in the visible wavelengths, indicative of secondary or "indirect" albedo reducing processes related to grain size and shape, melt water accumulation, etc. We have dramatically shortened this section as suggested, but kept two additional introductory sentences to make the point of the paragraph clear to a wide readership, and hope that this is acceptable.

2(10). Figure 4ABC. Half of the solar energy is at wavelengths <0.7 microns, and 80-90% (depending on cloud thickness) is at wavelengths <1.0 microns. In these figures, the peculiar wiggles in the visible region, the most energetically important part of the spectrum, are squeezed into a tiny region on the far left of the figures. These wiggles need to be discussed and explained in the text, and the figure should be redrawn, for a domain 0.3-1.5 microns instead of 0-5 microns.

These figures related to our DISORT inversion and have therefore been discarded.

Minor comments.

line 206. ". . . they are large, far outside the domain of Mie scattering". Mie theory is not restricted to small size-parameters. Admittedly Mie calculations do become expensive for large size-parameters.

We appreciate that technically Mie calculations can be solved for any size of particle, but we also acknowledge that there is a huge computational cost associated with solving them for particles in the size range of our glacier algae. The Mie scattering domain is commonly described in the literature as 2 > size parameter < 2000. We acknowledge the point and have changed the text accordingly, to:

"they are large, making Mie calculations impractically computationally expensive" (line 202 in revised manuscript)

line 247. "real refractive index". I think you mean imaginary refractive index, or complex refractive index.

This has been corrected.

line 262. "four global average dusts from Flanner et al. (2007)". The Flanner 2007 paper concerns black carbon; it contains no information about dust. Give the correct reference. [Disconnect Type "c"]

The citation as been updated to Flanner et al. (2009) where the dust aerosols in SNICAR were discussed for the first time (citation now moved to Supp Info 8): Flanner, M.G., Zender, C.S., Hessm P.G., Mahowald,

N.M., Painter, T.H., Ramanathan, V., Rasch, P.J., Springtime warming and reduced snow cover from carbonaceous particles. Atmospheric Chemistry and Physics, 9: 2481-2497, 2009.

line 320. “CI” has not yet been defined.

This has been corrected.

Line 478.Change 2B to 2D.

This has been corrected

Line 484. Change 2C to 2B.

This has been corrected

Line 488. Change 2C to 2D.

This has been corrected – we think the reviewer meant 2D → 2C.

Figure 2B. The labels Hbio, Lbio, CI, SN should be defined in the figure caption.

This has been updated

Figure 6. This is a nice figure showing the contrast between 2016 and 2017.

Thank you

Table 2 should be rearranged. Seeing the numbers from different methods side-by-side would help the reader compare them. For example, the first line (water albedo) would read “0.31, 0.08, 0.08”.

This has been updated as requested (now presented as Table 3 in revised manuscript)

Spelling and punctuation.

line 181. Change gluteraldehyde to glutaraldehyde.

This has been corrected

line 290. “the the”

This has been corrected

line 983. distributed

This has been corrected

line 1240-1245 (Table 1). “ug” is not the correct symbol for micrograms. Use the Greek lower-case mu.

This has been corrected

Additional Comments

We also acknowledge receipt of four comments raised separately by email directly to the lead author. Since these were all related to a figure that is no longer included in the revised manuscript (being associated with the DISORT mdoel inversion) we consider them addressed.