Dear Dr Skiles,

Thank you very much for a thorough and insightful review of our paper. We agree with the majority of points raised and have amended the manuscript accordingly, including adding a new figure. Clearly, someone with your level of expertise in spectroscopy and remote sensing would find some of the material in this paper unnecessary. However, we have received several pieces of positive feedback from the bioalbedo community about the usefulness of the material in the paper and the clarity of the structure - along with favorable comments from reviewer 1 - we feel strongly that the review of the existing practices/literature targeted at the biological and glaciological communities is needed and appropriate. Our intention is that those who wish to measure biological albedo reduction but lack specific expertise in spectroscopy and remote sensing will be guided towards the appropriate literature and experimental design by our paper, while the new model demonstrates why the proposed measurement standards are important. There is reciprocity between the new capacity to model biological albedo reduction and the methods that should be used to measure it empirically. Hopefully the amends detailed below along with those made to satisfy Reviewer 1 represent a sufficient compromise to proceed with publication.

Thank you for your comment on the inclusion of field spectra being a valuable addition to the manuscript. Field data were included in early drafts of the manuscript but were removed from the manuscript prior to submission as they were not deemed necessary in this theoretically-based paper. Sufficient description and interpretation of our field spectra significantly lengthened the manuscript and it was decided that a separate paper was required to properly present our empirical data. We now recognize that inclusion of at least example field data would be useful. We have now added a new figure with field measured albedo and nadir-view HCRF for a clean ice and algal ice site for reference and to demonstrate the variation in measurement between the two measurement techniques. We agree that this helps to justify the explanation of empirical methods that appears later on in the manuscript. We have refrained from a detailed interpretation of the field spectra or model validation as we simply believe there is too much to say and to do so would constitute a fundamentally different paper. These will be presented in additional papers in the near future.

We include below our responses to your specific comments.

Thanks again, your comments have greatly improved the paper.

Sincerely,

Joseph Cook

Detailed Response:

P1 L16: I would argue that this is well established. However, I have removed the word 'significant' as I agree it is ambiguous in terms of statistical significance.

P1 L41: Added citation to IPCC 2013 and Hansen and Nazarenko, 2004 as requested.

P2 L6: Changed "High Sierra" to "Sierra Nevada" as requested

P2 L7: Added "optically equivalent" as requested

P2 L37: Clarified on L37 that we are referring to ablating areas of the Greenland Ice Sheet and added the range of total bare ice extent for the past 15 years (citing Shimada et al., 2016) as follows: "The wide spatial coverage of algae on the ablating areas of the Greenland Ice Sheet (which, over the past 15 years have comprised between 5 – 16% of the entire ice sheet area: Shimada et al., 2016)"

P3 L9: Added "mineral" as requested

P3 L12-14: Reworded for clarity. Now reads: "The vertical position of light absorbing impurities in the shallow-subsurface is also important because in the visible wavelengths absorption by ice crystals is low (Warren, 1982), meaning light can penetrate the snowpack."

P3 18-20: Yes, fair enough. I appreciate that this could be misconstrued and is arguably tangential to our paper. Sentence removed.

P3 L41: Thanks for the suggestions, I have amended the sentence to read: "These models are well-validated for clean snow (Grenfell et al., 1994) and have been applied to snow contaminated with black carbon and dust (Painter et al., 2007, Flanner et al., 2007; Gardner and Sharp, 2010, Brandt et al., 2011; Kaspari et al., 2015), although their performance for impurity-laden snow has been questioned (Skiles et al., 2016)."

P4 L8: New citations added for black carbon in snow: Deems et al., 2013; Kaspari et al., 2014; Skiles et al., 2017)

P4 L12: Amended for clarity: "Both models characterized biological impurities simplistically by updating the absorption terms in existing inorganic impurity models rather than predicting the full optical properties of cells using Lorenz-Mie calculations."

P5 L5: Deleted. I originally mentioned this because UV absorbing pigments are known to be produced by ice algae, so there is some biological relevance.

P5 L4: Added the following sentence to the m/s: "Similar methodologies have previously been employed to infer the imaginary part of the refractive index of sediments (Light et al, 1998) and hydrated salt crystals (Carns et al., 2016) in sea ice and for dust in snow (Skiles et al., 2017)."

P5 L21: Yes, I agree this reads ambiguously. Now updated for clarity.

P5 25: The SNICAR code I used did not have so wide a range of ice grain sizes as this, so the larger grains were produced using a Mie code and Warren and Wiscombe's data for ice optical properties.

P7 L27: Added the following text and citations to P8 L14: "Non-spherical grain shapes can influence the surface albedo (Dang et al., 2016; Libois et al., 2013) and can be simulated in the radiative transfer scheme TARTES (Libois et al., 2013)."

P7 L35: Thanks, I have added the following text to P8 L14: "It is also important to note that optical grain size and observable grain size are different properties and high uncertainty results from assuming that they are equivalent – this is because the observable grain size does not represent the absorbing path length."

P9 L28: These are model results not observations. We do not yet have sufficient cell count or pigmentation data to link biological growth with albedo decline (although this data is forthcoming and will comprise a separate paper) except to say that areas of higher biomass were associated with lower albedo and characteristic spectral shapes. We have acknowledged the uncertainty described in your papers on dust in snow: "Skiles et al., 2016 and Skiles and Painter (2016) found that modelled albedo decline due to dust and black carbon in snow was dissimilar to measurements, indicating the urgent need for radiative transfer modelling of biological impurities to be field validated. We have recently undertaken field work for this purpose and will publish the outcomes in a future paper."

P10 L33: added "Isolating the biological contribution to snow and ice albedo is crucial for modelling ice dynamics under future climate scenarios where the intensity and spatial coverage of algal blooms will likely vary differently to inorganic impurities."

P11 L15: Text added: "This was found to be true for three other classes of dust (size fractions 0.05-0.5 um, 0.5-1.25um, 1.25-2.5 um) although the data are not shown here. Real dust optical properties vary widely and SNICAR uses a global mean that is unlikely to be a good representation of real dusts in any specific location."

We have opted not to discuss BC in detail in this paper for two reasons: 1) it is documented thoroughly in many other recent papers and our focus is really on biological impurities; 2) one of the central questions in the bioalbedo community is "what darkens the Greenland Ice Sheet (and other ice masses)" - the two main hypotheses are algal blooms and melt-out of Holocene dusts. Therefore we have focused our discussion on these materials. Still, we agree that BC needs to be mentioned here, so we have added the following: "In our simulations black carbon was found to be a more effective albedo reducer than algal cells per unit mass (data not shown)."

Section 4.8 We would like to keep this section as we suspect it is one of the less well understood aspects of spectroscopy. While this is elementary to you and your peer group, this is not obvious information for those tackling bioalbedo from a biological or glaciological background and therefore bears repeating, with direction to appropriate supporting literature, in this paper.

P11, 38- I have deleted the text 'preferentially melt smaller grains with larger SSA's'

P11 L40: "visible spectrum" amended to "solar spectrum"

Section 4.10: Again, we do not want to remove this section because we feel it is important for future bioalbedo work. We have added further clarifying information about the relationship between hemispheric reflectance and albedo and cited your work in Casey et al (2017) as requested. I'm surprised that the inclusion of broadband albedo measurements is unclear to you given its importance for determining energy balance. Many studies will measure albedo of impurity laden ice only to estimate the effect of impurity loading on energy balance and will only want broadband measurements. I also expect that many of the end users of BioSNICAR will discard the spectral information in favour of broadband albedo. We don't really think we have equated reflectance with albedo, having explicitly stated that to estimate albedo directional/conical reflectance must be integrated over the viewing hemisphere taking into account the ARF in three places in the manuscript (pg 7 and section 4.8 and section 4.10); however, we appreciate that we could have been clearer so we have updated the discussion to include the following text and citation of your paper:

"… to determine the reflectance properties of a surface component with dimensions of the order of decimetres (e.g. an algal bloom), a hemispheric-conical reflectance measurements (HCR: Fig. 1A) made using a sensor with a limited field of view are likely more appropriate and can be used to estimate the albedo by integrating over the entire viewing hemisphere, accounting for anisotropic scattering."

"With knowledge of ARFs for the sample surface and reflectance panel, these measurements can be used in conjunction with an angular integration model to approximate albedo (Grenfell, 2011; Casey et al., 2016)"

We have also provided a new figure (Figure 6) which demonstrates the difference between albedo and HCRF measurements for the same two sites, and discussed it in the following text:

"The variation between albedo measured using a cosine collector and reflectance measured using an 8° collimating lens at nadir view for the same surfaces (clean ice and algal ice near the IMAU Weather station 'S6' on the southwest Greenland Ice Sheet) is demonstrated in Figure 6. The measurements were all made within 15 minutes during constant clear sky conditions on 14th July 2017 using the same spectral radiometer, fibre optic and tripod arrangement that did not move between measurements. The variation is most likely due to spatial integration of a wider range of surfaces for the cosine collector and the lack of angular integration for the collimating lens."

Additional amends:

The term 'critique' has been changed to 'discussion' throughout the manuscript to better represent the aims and scope of the paper in response to reviewer comment. This includes the paper's title.

We added an explicit mention of the target audience to make clear the intended use of the paper and justify the review sections: "specifically aimed a broad audience likely comprising glaciologists and biologists rather than spectroscopists or remote sensors"

New Figure:

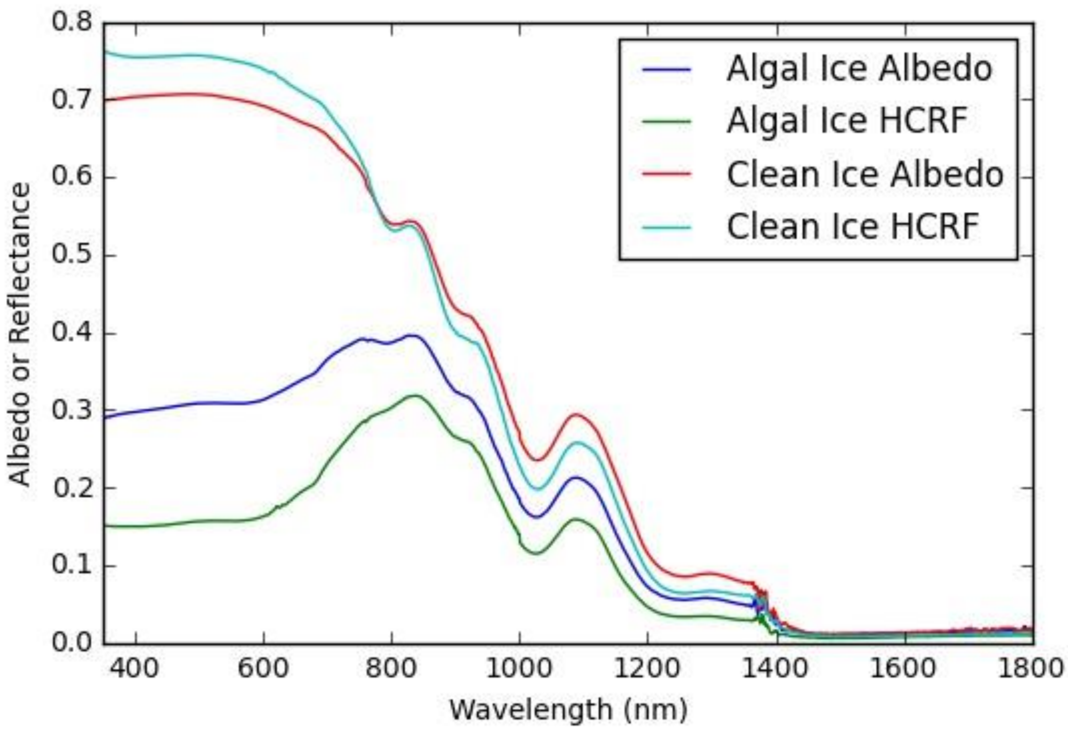

Figure 6: Field measured albedo (cosine collector) and nadir-view HCRF (8° collimating lens) for one clean ice and one algal ice surface. The spectra were collected using an ASD FieldSpec Pro 3 spectral radiometer following the methods described in Figure 5 and section 4.10 on 14th July 2017 during constant clear sky conditions near the IMAU Automatic Weather Station 'S6' on the Greenland Ice Sheet.