Anonymous Referee #2

Overview

This manuscript uses data from the MERIS satellite sensor to seek to quantify glacier algae bloom dynamics over the south west Greenland ablation zone. They justify their use of this sensor for detecting algal blooms by reference to their previous work using the very similar Sentinel-3 OLCI on the same topic (Wang et al, 2018), by selected references to some field observations, and by wider reference to remote sensing of ocean-borne algal blooms.

Response: We greatly appreciate the reviewer's careful reading and useful suggestions, which have improved our manuscript. In this revision, we believe that we have solved most of the concerns raised by the reviewer, please see our responses below.

Major comments

The manuscript tests several remote sensing ratio indices and shows that, to some extent, the 2BDA approach retrieves a different signal to that obtained by the 'bulk' Impurity Index or simple red band threshold approaches. This is a useful exercise in seeking to understand what signals can be retrieved from the MERIS/OLCI sensors.

As the manuscript is presented currently, I have some major concerns which prevent me from recommending publication in The Cryosphere.

There are known problems with seeking to apply band ratios/indices designed for chlorophyll-a retrieval from water bodies but which this manuscript does not engage with. I appreciate that the main studies which highlight these problems, by Cook et al. (TCD) and Tedstone et al. (TCD), are currently undergoing review and so were unlikely to have been available at the time when this study was started. But nevertheless, there is a lack of discussion of the wider literature on this issue; instead, Wang et al (2018) is cited as proof that chlorophyll-a-focused band ratios are appropriate for detecting glacier algae blooms, but I find that discussion of these problems is lacking there too, and so their 2018 paper is not an especially strong foundation on which to base the present study.

Response: In this revision, we added a section (5.1, shown below) to analyze and discuss the sensitivity of 2BDA index to dust presence using SNICAR simulations with variant dust sizes and concentrations (more details are in the response to reviewer#1). Our results indicate that the 2BDA index is much less sensitive to dust presence than the impurity index, and in our context (with impurity index mostly less than 1.0), the high 2BDA index (greater than 0.99) is unlikely caused by dust. Given that the 2BDA index is specifically designed for chlorophyll-a retrieval and the narrow bandwidths of MERIS, the 2BDA index (especially at values greater than 0.99) is uniquely biological due to glacier algae.

Since the discussion paper is not referenceable, we removed all discussion papers from our citations. The paper by Cook et al. (2020) was just published, and we added this citation to our introduction and discussion. Cook et al. (2020) also discussed the 'red-edge' feature present in their field spectra and argued that this feature is a unique chlorophyll-a feature.

5.1 Sensitivity analysis of 2BDA index to dusts

In this study, we utilized the chlorophyll-a spectral signature generated by glacier algae in the red-NIR region (Fig. 2d) to quantify the spatiotemporal variability of glacier algae over bare ice in southwest Greenland. The narrow bandwidths and wavelengths of MERIS designed for chlorophyll-a detection make the MERIS archive data a powerful tool to study supraglacial algal communities. The chlorophyll-a signal present in the MERIS spectra is consistent with (nearly) coincident WorldView-2 data and field hyperspectral measurements collected over dark ice with high algal abundance. Similar to the Sentinel-3 OLCI ratio index R_{709nm}/R_{673nm}, the MERIS 2BDA index R_{709nm}/R_{665nm} can effectively quantify the algal growth pattern through July to August. However, given that dusts may complicate the spectra and affect the 2BDA index, we performed radiative transfer modelling experiments using the Snow, Ice, and Aerosol Radiation (SNICAR) model (Flanner et al., 2007; Flanner et al., 2009) to test the sensitivity of 2BDA index to dust presence. Using SNICAR-Online (Flanner et al., 2007), we simulated the spectra for varying sizes and concentrations of dusts, and then calculated the corresponding 2BDA and impurity indices. The SNICAR input parameters include incident radiation (direct), solar zenith angle (60 degrees), clear- vs. cloudy sky (Summit Greenland), snow grain effective radius (1500 microns, considering the ice surface), snowpack thickness (100 m), snowpack density (400 kg/m^3), and dust concentration (0.1, 0.3, 0.5, 0.8, 1, 1.5, 2, 2.5, 3, 5, 8, 10, 30, 50, 80, 100, 300, 500, 800, 1000, 1500, 2000, 2500, and 3000 ppm) for four dust sizes (dust 1: 0.1–1.0μm; dust 2: 1.0–2.5μm; dust 3: 2.5–5.0μm; dust 4: 5.0–10.0 μm). We should note here that there has been some discussion in past literature of hematite-rich dust (e.g. Tedesco et al., 2013; Cook et al., 2020), which could produce a different spectral response. However, the study of Cook et al. (2020) finds very low concentrations of such dust, and therefore we consider its impact to be negligible. Figure 10 shows the scatterplots of impurity index vs. 2BDA index calculated for the SNICAR simulations (with circle diameters representing the magnitude of dust concentrations for four different dust sizes), and the density scatterplots from the MERIS data (impurity vs. 2BDA indices over bare ice). Figure 10 indicates that the impurity index is more sensitive to dust presence than the 2BDA index. In contrast, the upper bound of the impurity index we calculated from the MERIS data is around 1.0. According to the SNICAR simulations, the impurity index of 1.0 corresponds to a maximum 2BDA value of 0.99 (~500 ppm concentration for dust 4). This indicates that for our study area, the glacier algae identified with 2BDA index greater than 0.99 are unlikely to be false positives caused by dusts.

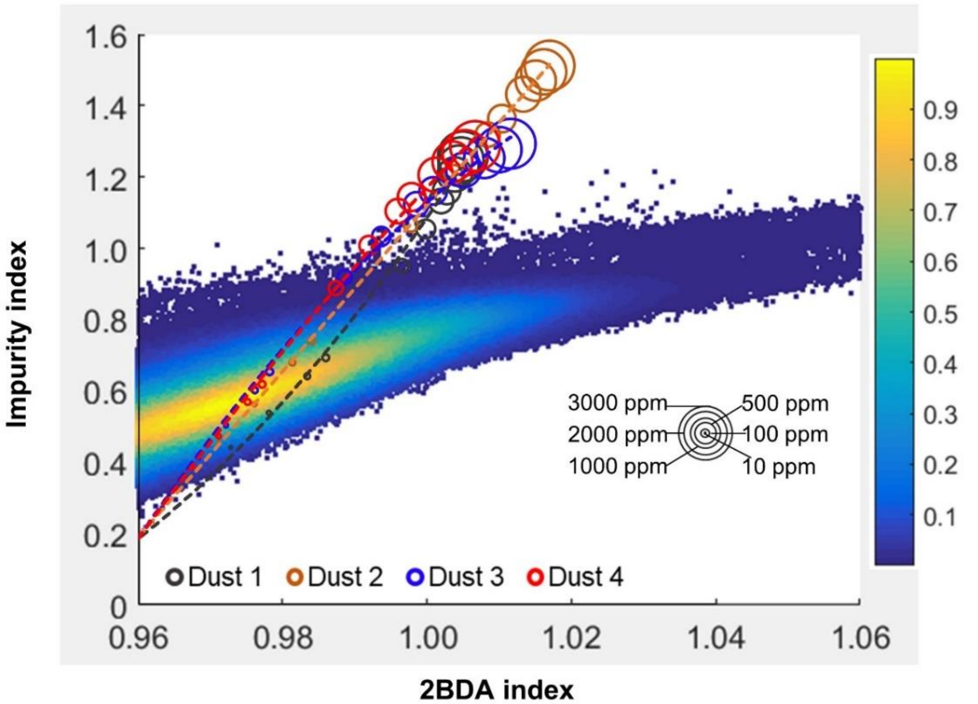


Figure 10: Impurity index vs. 2BDA index for MERIS pixels, excluding missing data in our study area between 2004 and 2011 (density scatter plot). Circles show impurity vs. 2BDA index from SNICAR simulations with varying concentrations of surface dust (with 4 different dust sizes). The circle size corresponds to the dust concentration, and dashed lines show the polynomial regression for each of the different dust sizes.

As I understand it, the core of this problem is two-fold: (1) the optics of bare ice are insufficiently well understood to be able to guarantee that the reduction in reflectance around 667 nm compared to 710 nm is uniquely biological; and (2) other light absorbing impurities may interfere or present the same signal. Thus, based on published field evidence, there is little evidence that the band ratio approach is uniquely biological. Cook et al. (TCD) and Tedstone et al. (TCD) have more information on this and note that phenolic compounds for in the dominant glacier algae species can obscure potentially diagnostic spectral features. This being the case, NDCI etc may simply be measuring some combination of slightly different surface characteristics to the Impurity Index approach, rather than yielding information specifically on glacier algae growth. Thus, regarding inter-annual mapping of 'dark ice' vs glacier algae, there may be little advance on Shimada et al. (2016) or Tedstone et al. (2017), both of whom considered inter-annual variability in 'dark ice' dynamics over the timescales addressed here.

Response: We agree that the optics of bare ice and light absorbing impurities can complicate the spectral signal, but we respectfully disagree that "based on published field evidence, there is little evidence that the band ratio approach is uniquely biological". Current field studies (Stibal et al., 2017; Cook et al., 2020) presented the field hyperspectral data of dark ice with abundant glacier algae, and their data show the chlorophyll-a signature at the red-NIR region. However, they did not apply the band ratio approach, which doesn't necessarily mean that 'there is little evidence that the band ratio approach is uniquely biological'. As we discuss below, the ice/snow optics have little impact on the 2BDA index, and based on radiative transfer modeling experiments (response to reviewer #1, and revised discussion section 5.1), the upper limit of the dust impact on the 2BDA index is around 0.99. In contrast, the impurity index is more sensitive to dust presence. In the revised text, we have added more discussion and figures to show the difference between impurity index and 2BDA index. We also respectfully disagree with the statement 'Thus, regarding inter-annual mapping of 'dark ice' vs glacier algae, there may be little advance on Shimada et al. (2016) or Tedstone et al. (2017), both of whom considered inter-annual variability in 'dark ice' dynamics over the timescales addressed here', since according to our results (Figure 4), there are differences between 2BDA index, impurity index, and the R620nm reflectance that Shimada et al. (2016) and Tedstone et al. (2017) used for dark ice delineation. We would like to argue that their method doesn't account for any biological signal specific to glacier algae, and is more likely to be influenced by meltwater presence, ice optics and other impurities.

On justification of the 2BDA, Wang et al. (2018) point to Painter et al. (2001) as evidence that glacier algae can detected using chlorophyll-a indices. However, Painter et al refers to the specific case of snow algae growing on snow surfaces, which is not relevant here as this study engages only with bare ice surfaces. Thus, retrievals in this study can in fact be based only on paired cell counts and field spectra acquired by Stibal et al. (2017), a study which also indicates that chlorophyll-a-based approaches could be useful for remote sensing. However, the spectra that Stibal et al (2017, Fig. 3) present refers only to high algal abundance ice, over centimetres patch scales, which is not representative of OLCI or MERIS 300 m data. Some consideration of the scale mismatch is therefore required.

Response: We respectfully disagree with the reviewer on the point that the specific case of snow algae growing on snow surfaces is not relevant with the glacier algae growing on bare ice surface. There are differences between snow and ice spectra, but both of them are characterized by the decreased reflectance at 709 nm as compared with 665 nm. The spectral signature of ice and snow themselves exhibit a slope opposite to that of the chlorophyll-a spectra at this region. Although snow algae and glacier algae are distinct species, they both generate chlorophyll-a for photosynthesis activity and chlorophyll-a is their major photosynthetic pigment. The colours of snow algae and glacier algae are different mainly because snow algae generate secondary carotenoids which have reflectance peak at red band. However, according to Painter et al. (2001), this carotenoid feature does not block the chlorophyll-a absorption signal around 680 nm, so they detected snow algae based on the chlorophyll-a signature between 630 nm and 700 nm using the absorption at 680 nm and the reflectance feature at 630 nm and 700 nm. Glacier algae have brownish-grey colour because they generate purpurogallin pigments, and at the same time, they also generate chlorophyll-a for photosynthesis (similar to snow algae). However, as we responded to the first reviewer, compared with the purpurogallin pigment, Chlorophyll-a is more appropriate for mapping glacier algae for the following reasons:

1) Chlorophyll-a is the primary photosynthetic pigment of glacier algae (Williamson et al., 2018). The ocean color satellite sensors like Envisat MERIS and Sentinel-3 OLCI are designed to capture the Chlorophyll-a signal from highly-absorptive and optically complex water bodies,

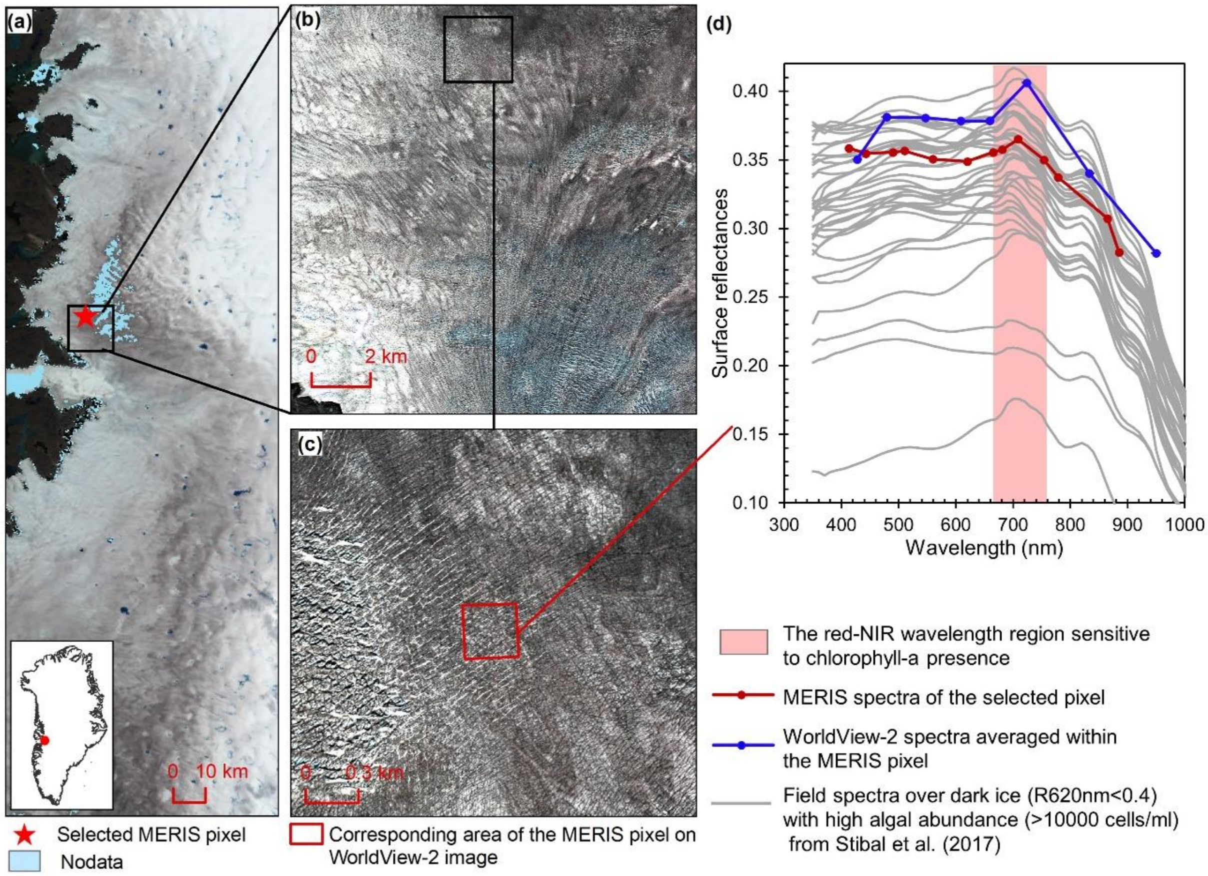
which means that the ocean color sensors are highly sensitive to the chlorophyll-a presence, making them very useful tools for glacier algae detection based on the biological signatures.

2) According to the studies by Remias et al. (2012) and Williamson et al. (2018), the spectral signatures (absorption peaks) of the purpurogallin pigment are concentrated in the UV region (278 nm, 304 nm, and 389 nm, Remias et al.,2012). To our knowledge, no satellite sensor can detect these spectral signatures. Although the purpurogallin pigment is very likely to account for the brownish-grey colour of glacier algae, its absorption over the entire visible spectrum is quite uniform, making it difficult to differentiate from other dark impurities. In contrast, chlorophyll-a can generate very strong spectral signatures in the red and NIR region, which is supported by field hyperspectral measurements for both snow algae and glacier algae. (e.g. Ganey et al., 2017; Painter et al., 2001; Stibal et al., 2017; Cook et al., 2020).

As we clarified in the text, we used the field measurements by Stibal et al. (2017) for qualitative evidence to show that the MERIS spectra, WorldView-2 spectra, and field hyperspectral data are consistent in terms of the spectral shape over algae-abundant ice. In this revision, we revised Fig. 2d to include more field spectra data from Stibal et al. (2017) to illustrate that the chlorophyll-a spectral signature at the red-NIR region is present across multiple measurement samples and dates. Additionally, the recently published paper by Cook et al. (2020) also discussed the 'red-edge' feature present in their field data, which is attributed to the chlorophyll-a generated by glacier algae. In regard to the scale issues, the MERIS (300 meter) spectra and WorldView-2 (2 meter) spectra are quite similar, and previous studies (e.g. Ryan et al., 2018) show that the areal percentage of the distributed impurities is up to >90% within individual MODIS pixels (500-meter resolution). Therefore, MERIS data can capture well the glacier algae signal over southwest Greenland; nevertheless, we agree with the reviewer that more investigations on the scale and spectral mixing issues are needed in future studies. We have revised the discussion to acknowledge those issues. Besides, as we responded to reviewer #1, we excluded the possibility of false positives to detect glacier algae caused by dusts when the 2BDA index is greater than 0.99.

Possibly a more minor concern: the cell counts used as field validation in this manuscript are very high, at 105 cells ml (Figure 2d), but I'm not sure that we would expect to see such high counts over these larger spatial scales (e.g. Williamson et al.,2018, FEMS). Furthermore, the field spectra seem to have quite high reflectance for the quoted cell counts compared to other field spectra in the literature, e.g. Figure A1 in Tedstone et al. (2017, TC). The field spectra shown here seems to be that in Stibal et al. (2017, GRL, Figure 3), but a cell count is not quoted there and so I raise this question here in case there has been an error in transforming Stibal et al's data for this study.

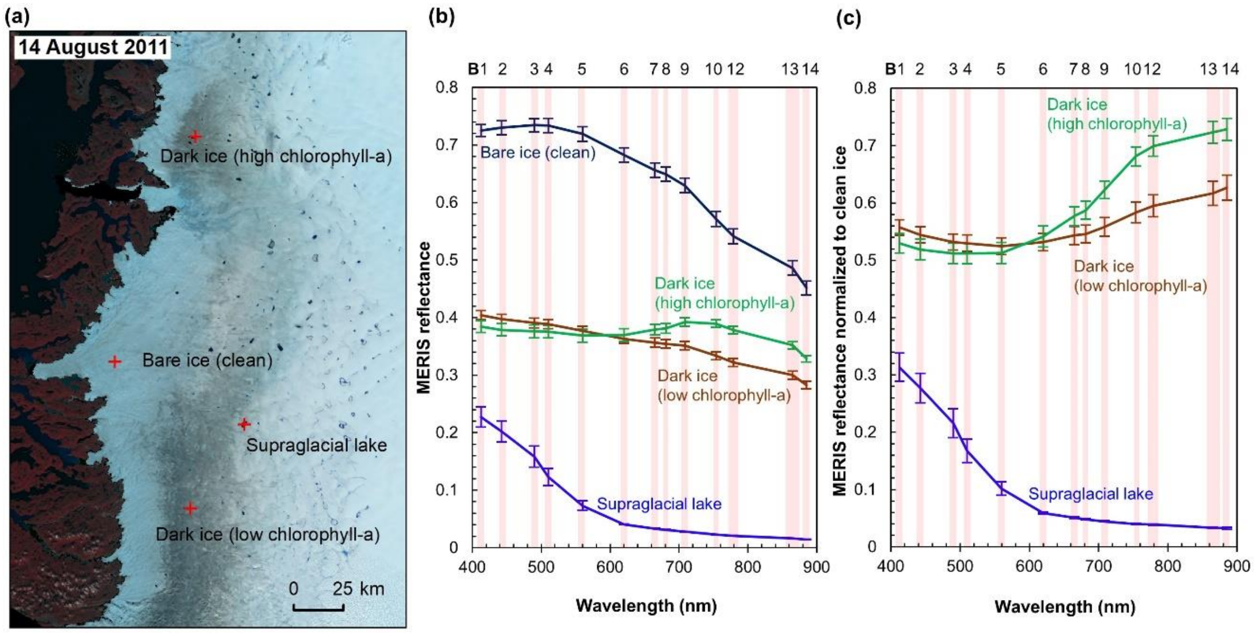
Response: In this revision, we clarified in the text on how we used the field data by Stibal et al. (2017). The field hyperspectral measurements collected by Stibal et al. (2017) were used for qualitative purposes for comparison with the MERIS spectra over dark ice to validate the chlorophyll-a spectral signature at the red-NIR region, specifically the bands of 709 nm and 665 nm used for 2BDA index calculation. We have revised Fig.2d by adding multiple in situ spectra collected over the algae-abundant dark ice (R620nm<0.4, and algal concentration >=10000 cells/ml) to illustrate that the chlorophyll-a spectral signature is present across multiple measurement samples and dates. We have double checked the original data published by Stibal et al. (2017) and ensured the correctness of our plotted spectra.



Revised Figure 2: Comparison between the MERIS, WorldView-2, and field spectra over algae-abundant dark ice. (a) MERIS Level-2 image (true colour composite) acquired on 5 July 2010. Pixels with missing data are shown in blue. (b) WorldView-2 surface reflectance image acquired on 9 July 2010 over the square area in (a). (c) Zoomed-in WorldView-2 image, with the area (red square) corresponding to the selected MERIS pixel in (a). (d) Reflectance spectra for MERIS and WorldView-2 (2010), and field hyperspectral measurements collected over the algae-abundant dark ice at S6 by Stibal et al. (2017) in 2013.

The study also presents data that undermines its application of a Chlorophyll-a based band ratio approach. Figure 3b shows some averaged MERIS surface reflectance curves. Dark Site (Less Chlorophyll) has higher reflectance at 665 than 709 nm and so with 2BDA this site would presumably diagnose as 'clean ice' by comparison to the Clean Ice spectrum plotted above it. I do not see any comment upon this issue elsewhere in the text.

Response: We have revised the text to discuss this issue and added a subplot (Fig.3c) to show the normalized surface reflectance relative to the clean ice spectrum. We respectfully disagree with the reviewer that our presented data in Fig.3 undermines its application of a Chlorophyll-a based band ratio approach. As we responded to reviewer#1, "we have corrected the figure to refer to "high chlorophyll-a" and "low chlorophyll-a". To illustrate the chlorophyll-a signal better, we also plotted the relative surface reflectances (MERIS) for different surface types normalized to the clean ice spectra since the primary background spectral signal is from ice. For both water and ice, the spectrum shows a decrease in reflectance from 665 nm to 710 nm, which is opposite that of the chlorophyll-a spectrum. A 2BDA signal of less than one therefore does not imply that there is no chlorophyll-a present. A smaller rate of decrease could still be produced by low amounts of chlorophyll-a. Using the 2BDA index, we do not intend to classify the ice surface into 'algae' vs. 'no algae'. We use the 2BDA index to show the magnitude of glacier algal blooms varying over space and time. We think it is more appropriate to use 'high chlorophyll-a' and 'low chlorophyll-a' to describe those two sites. We agree with the reviewer that more discussions and investigations are needed to quantify the impacts of other darkening processes on 2BDA index. In this revision, we added the analysis of dust impacts on 2BDA index based on SNICAR simulations in the discussion section. We found that by combining the 2BDA index with the Impurity Index, we can exclude the possibility of false positives when the 2BDA index is greater than 0.99.



Revised Figure 3: MERIS spectra of different surface types. (a) MERIS Level-2 image (false colour composite) acquired on 14 August 2011 and locations of the four different sample sites. Each site has an area of 1.2 km by 1.2 km, composed of 16 MERIS pixels. (b) MERIS surface reflectance in 13 spectral bands over the four sites, illustrated by the mean and standard deviation values for each band over each site. (c) Normalized surface reflectance relative to the clean ice spectra.

I'm very confused about how the algal population doubling times were calculated. This is a critical part of the manuscript as it underpins the assertion that there is a 0.02-0.04 reduction rate in albedo for each algal population doubling.

Response: The methods for computing algal population doubling time were described in Section 4.3 (Lines 363-376 in the original manuscript). However, this section may have been somewhat unclear. In this revision, we have clarified how the population doubling time was estimated based on the fitted coefficients between 2BDA and time.

Overall, I would urge nuanced engagement with the question of how confident can we be that the differences between 2BDA, Impurity Index and Dark Ice metrics are due solely to algae and not *to other processes that might affect this band ratio? I suggest that this needs much clearer explanation in the methods about how Stibal et al's field data were used in this manuscript, and some nuanced discussion of the uncertainties surrounding Chlorophyll-a indices on ice surfaces. If these issues are addressed then the revised manuscript may be suitable for publication.*

Response: As we mentioned above, we have analyzed the sensitivity of 2BDA index and impurity index to dust presence by using the SNICAR simulations with variant dust sizes and concentrations (Section 5.1, Figure 10 in the manuscript). We have excluded the possibility of false positives for glacier algae detection caused by dusts using the 2BDA index (particularly greater than 0.99).

We have clarified in the text on how we used the field data from Stibal et al. (2017). The match between the MERIS spectra, WorldView-2 spectra, and field spectra (Figure 2 in the manuscript) indicates the chlorophyll-a signal can be effectively captured by MERIS.

Minor comments

I agree with the short comment by Daniel Remias that this manuscript should use the terminology 'glacier algae' in preference to 'ice algae'.

Response: As suggested, we have changed 'ice algae' to 'glacier algae' through the text.

The introduction includes wide-ranging references to both glacier algae and snow algae. Detailed discussion of the snow algae literature is not relevant here as this study focuses only on bare ice surfaces, so the introduction would benefit from being focused solely on glacier algae.

Response: We respectfully disagree with the reviewer on this point. We think it is important to discuss the differences between snow algae and glacier algae, as the differences may not be clear to a broader audience, as well as the similarities and techniques that can help inform our current study.

P3 L71: define what is meant by 'dirt'.

Response: We used 'dirt' according to the studies by Painter et al. (2001) and Takeuchi et al. (2006). However, since glacier algae do not generate secondary carotenoids like snow algae, the spectral characteristics of carotenoids are not related to our study. To avoid ambiguity, we have removed the statement 'the spectral characteristics of dirt may resemble those of carotenoids' from the manuscript.

P8 L209. Cook et al. (2019, Cryosphere Discussions) are cited for the first time here. If it is being cited then it should be introduced earlier during the lit. review section of the Introduction. Alternatively, if taking the view that Cook et al is under discussion and that it isn't 'referenceable', then all references to it should be removed.

Response: This paper is currently published online (Cook et al., 2020). We have included it in the introduction and updated the reference.

P9 L226: please quantify how the 'best' means of quantifying ice algae was obtained. This is not clear, either here or in the subsequent text.

Response: We have rewritten the paragraph to improve the clarity.

P9 L229: Dumont et al. (2014) focussed on impurity loading upon snow surfaces. Please comment further on the suitability of the Impurity Index for ice surfaces.

Response: The Impurity Index (Dumont et al., 2014) is the ratio between the natural logarithms of the spectral albedos at 545–565 nm (visible green band) and at 841–876 nm (NIR band). This index was built upon the hypothesis that the surface reflectances at visible wavelengths are more sensitive to impurity content than the NIR wavelengths, and Dumont et al. (2014) found that the Impurity Index is almost insensitive to grain size based on their field measurements and radiative transfer modeling results. Given the similar spectral shapes of snow and ice and the general assumption in radiative transfer modeling that ice has larger grain size than snow, we think it is suitable to apply the Impurity Index to ice surface. Besides, Dumont et al. (2014) quantified the impurity content using this index over the Greenland Ice Sheet for the May–July period from 2003 to 2013, including the bare ice zone in southwest Greenland. We have added more details in the text to describe the impurity index.

Results, section 4.1: I find this section very difficult to read. It would benefit from re-writing and introduction of paragraph breaks.

Response: This section has been rewritten as suggested and additional paragraph breaks have been added for clarity.

Fig. 3a: typo, August spelt 'Agust' Fig. 3b: provide MERIS band numbers at top of plot to aid cross-comparison back to Table 1. The colours of the two dark ice spectra lines are too similar to be able to tell them apart in print.

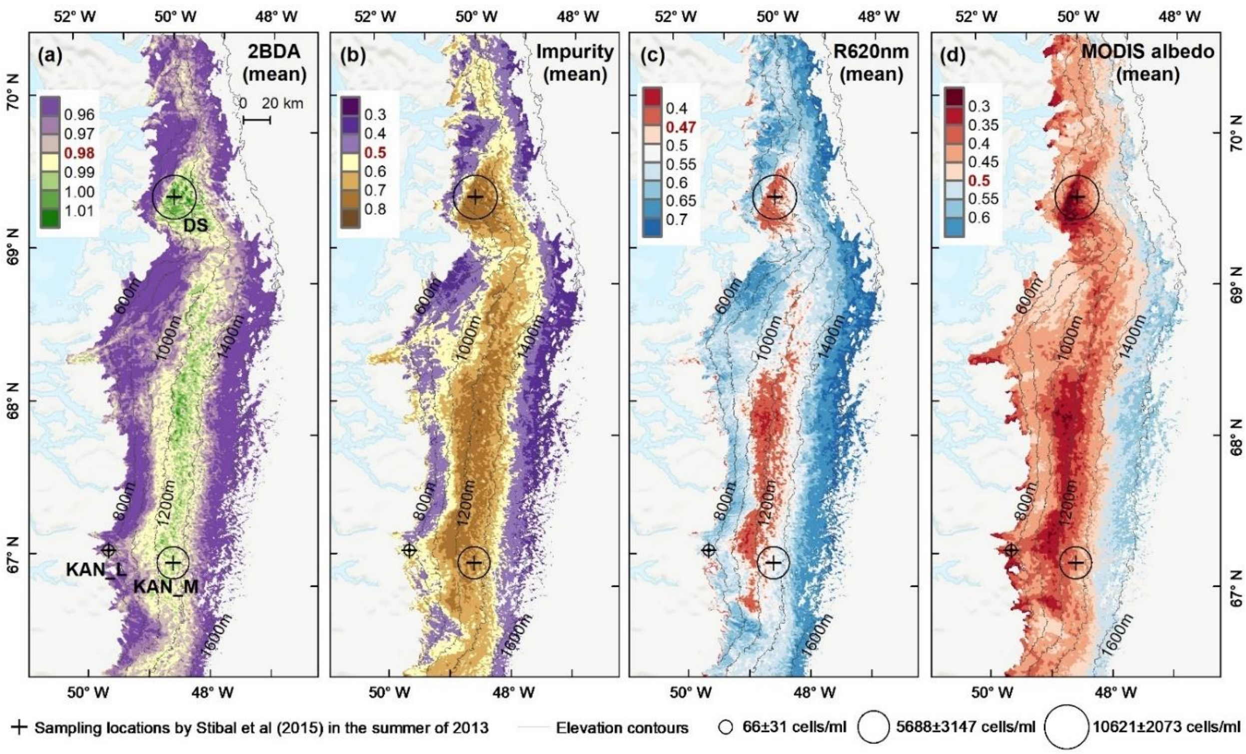
Response: We have revised the figure as suggested. We also added a subplot to show the normalized MERIS reflectances to the clean ice spectrum, showing the spectral signature of chlorophyll-a better.

P11 L275: full stop missing after '1400 m'.

Response: We have revised the text accordingly.

P12 L278-290: I do not follow the arguments being made in this section. Further, I disagree with the statement made in reference to Fig. 4, that 'Similar to the Impurity Index, the dark ice area is not only limited to the algae-abundant areas'. My examination of Fig 4 suggests that this is cherry-picking as conversely I saw plenty of evidence of a very good match between the two indices. As the authors central premise is that the 2BDA is 'uniquely biological' and so therefore yielding details not provided by the Impurity index or Dark ice index I propose that quantification beyond eye-balling the associated plots is required – ideally some statistical approach.

Response: In this revision, we changed the color scheme for each variable (2BDA, Impurity Index, R620nm, and MODIS bare ice albedo). Each color is specified based on the value range. The revised figure shows better the differences between those variables. Fig.4a clearly shows that along the central dark zone, 2BDA is highest at the elevation level of 1200-1400m, and comparatively the Impurity index is highest at the elevation level of 1000-1200m. The R620nm used for dark ice delineation in previous studies (Shimada et al. 2016; Tedstone et al. 2017) has the lowest value at the elevation level of 1000-1200m, more consistent with the Impurity Index. To show the spatial variation in more detail, we added a supplementary figure (Fig. A1) to illustrate the variations of different indices with elevation (at a 20-meter interval), which also shows the differences clearly. We have rewritten the text accordingly.



Revised Figure 4: Spatial patterns of the mean 2BDA index (a), impurity index (b), surface reflectance at 620 nm (c), and MODIS broadband albedo (d) over the bare ice zone during July and August from 2004 to 2011. The elevation contours illustrate the spatial variations of each variable with altitude. The cross labels show the spatial locations of the field sites DS, KAN_L, and KAN_M measured by Stibal et al. (2015) in 2013, along with the magnitude (circle labels) of glacier algal abundance for each site.

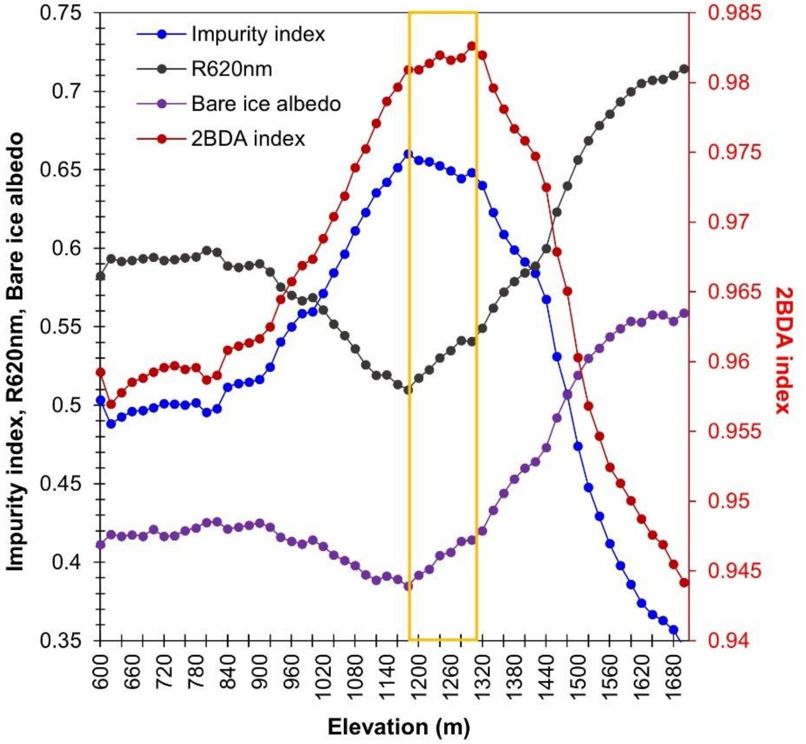


Figure A1. Spatial variations of the average 2BDA index, impurity index, 620 nm reflectance, and MODIS bare ice albedo at different elevations (20-meter elevation interval).

P12 L288-290: this study has no field data for the wavy patterns caused by ancient ice outcropping and does not provide any zoomed satellite imagery which shows them, so the reference to Wientjes and Oerlemans (2010) strikes me as somewhat speculative.

Response: The study by Wientjes and Oerlemans (2010) indeed has no field data for the wavy patterns caused by ancient ice outcropping, but they do show the zoomed in ASTER satellite image (15-meter resolution) in their Fig.8 to illustrate the observed wavy patterns which are typical for outcropping tilted layers of ice. In our context, the 2BDA index indicates that along the central dark zone, there were more glacier algae distributed at the elevation level of 1200-1400m as compared with the 1000-1200m elevation level. However, the darkening index (R620nm, e.g. Shimada et al., 2016 and Tedstone et al., 2017) and the Impurity Index (Dumont et al., 2014) indicate that the 1000-1200m elevation zone also contains high impurity content, suggesting that other darkening processes potentially played an important role in this area. Therefore, we discussed the possibility of ancient ice outcropping in this area based on Wientjes and Oerlemans (2010). In this revision, we provided additional evidence (Fig.A4 in appendix) to support this observation, using 2-meter resolution WorldView-2 images to show the differences between those two elevation zones. The WorldView-2 image (Fig.A4b) clearly shows the wavy patterns mentioned by Wientjes and Oerlemans (2010) at 1000-1200m, and very different textures are visible at 1200-1400m (Fig.A4c) where high algae content was identified by 2BDA index.

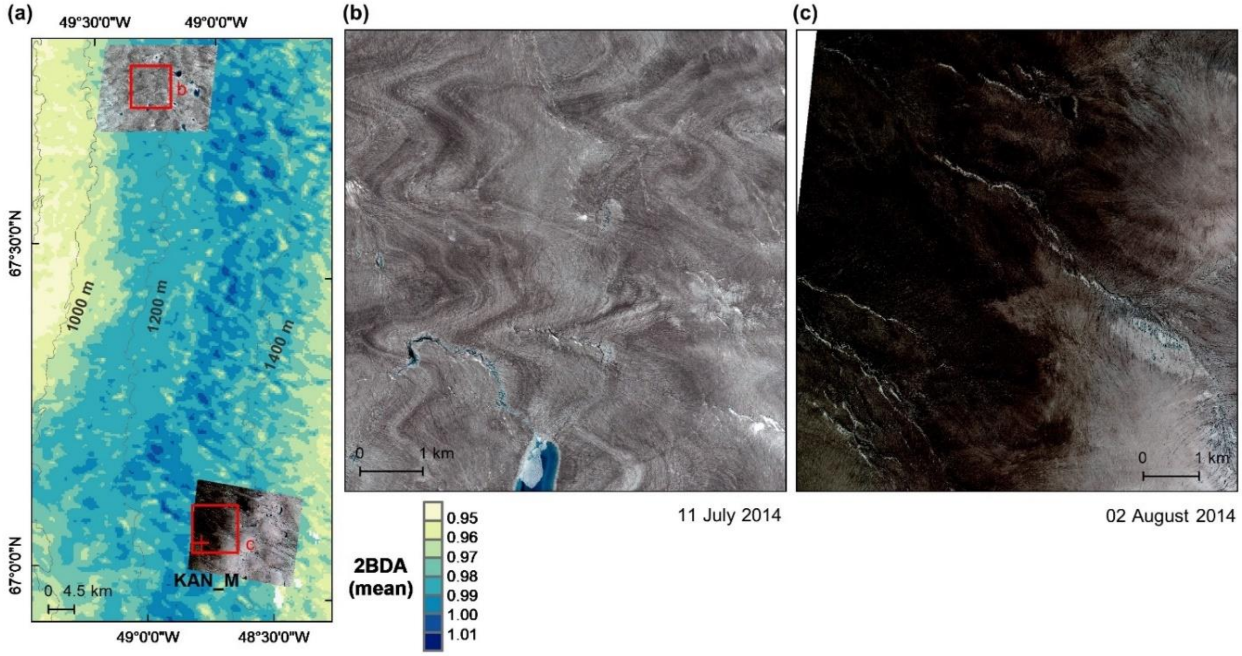


Figure A4. Comparison between WorldView-2 imagery over the dark ice site (b) with low 2BDA index (at 1000-1200m elevation) and the dark ice site (c) with high 2BDA index (at 1200-1400m elevation). The WorldView-2 image (b) illustrates the 'wavy' pattern that Wientjes and Oerlemans (2010) suggested to be caused by ancient ice outcropping.

P13 L302: 'exhibits different spatiotemporal variations'. Are these differences statistically significant? They are almost impossible to identify by eye, apart from in one or two years of record 2BDI. Consider doing some elevational binning to support your case.

Response: In this revision, we grouped the annual 2BDA and Impurity Index time series into different elevation bins (600-800m, 800-1000m, 1000-1200m, and 1200-1400m), to better illustrate the differences, and calculated the average values for each elevation bin. We added a supplementary figure (Fig.A2) in the appendix to show the annual time series of 2BDA and Impurity indices for different elevation levels, and revised the text accordingly.

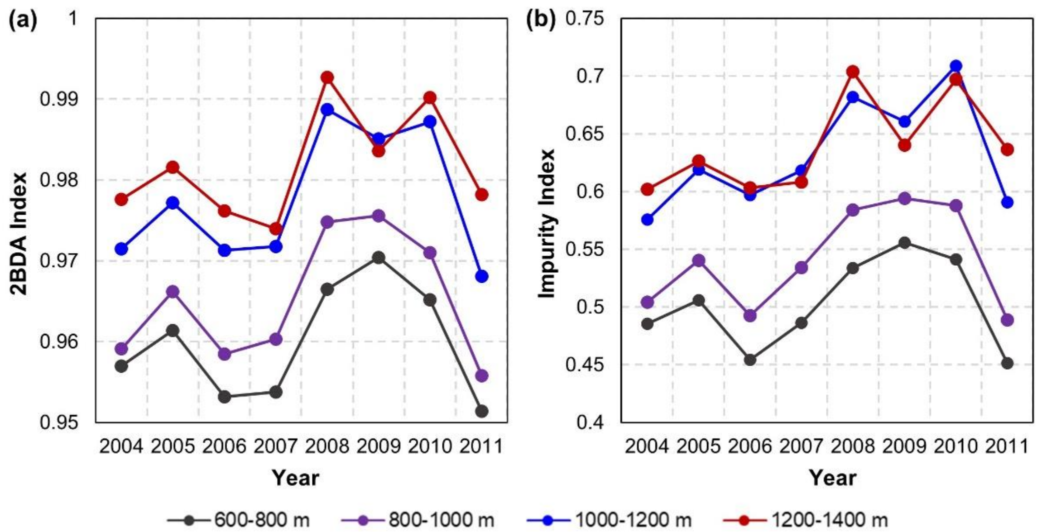


Figure A2. Interannual variability of 2BDA index and impurity index at the elevation levels of 600-800m, 800-1000m, 1000-1200m, and 1200-1400m.

Fig. 5: add 2BDA and Impurity index labels to each row of subplots.

Response: We have revised the figure as suggested, and changed the color schemes for 2BDA and Impurity maps respectively to illustrate the differences.

Fig. 6: What p-value where these trends culled at, if at all? I also note that the R2 values in the referenced appendix plot are very small.

Response: We added the p-value maps to Fig.A3 to show the spatial extent where the annual 2BDA index, Impurity Index, and MODIS broadband albedo have statistically significant annual variation trends from 2004 to 2011 which are limited to a few areas. Although the trends in most areas are not statistically significant at the 95% confidence level, we still think it is useful to examine the patterns of interannual trends for the different indices. We have revised the text to make note of the locations where Fig. A3 shows statistically significant trends.

Fig. 7: Please provide some indication of measurement spread at each point, e.g. +/- 1 s.d. I would also prefer to see just the 2014 MERIS data for comparison with the 2014 algal abundance time series, rather than the aggregate 2004-2011 time series which is shown currently. Previous work e.g. Shimada et al. (2016) and Tedstone et al. (2017) has shown that there is considerable inter-annual variability and so I think more value here could come from detailed analysis of how algal growth proceeds in each season.

Response: Since the Envisat MERIS was operational from March 2002 to April 2012, we are unable to provide the 2014 MERIS time series coincident with the 2014 field data. In this revision, we removed the figure from our manuscript. To consider the algal growth in different seasons, we extracted the seasonal growth functions (2BDA vs. time) for different seasons and compared with the growth function extracted from the 'climatological' averages over the two example sites KAN_M and DS (Fig. A5).

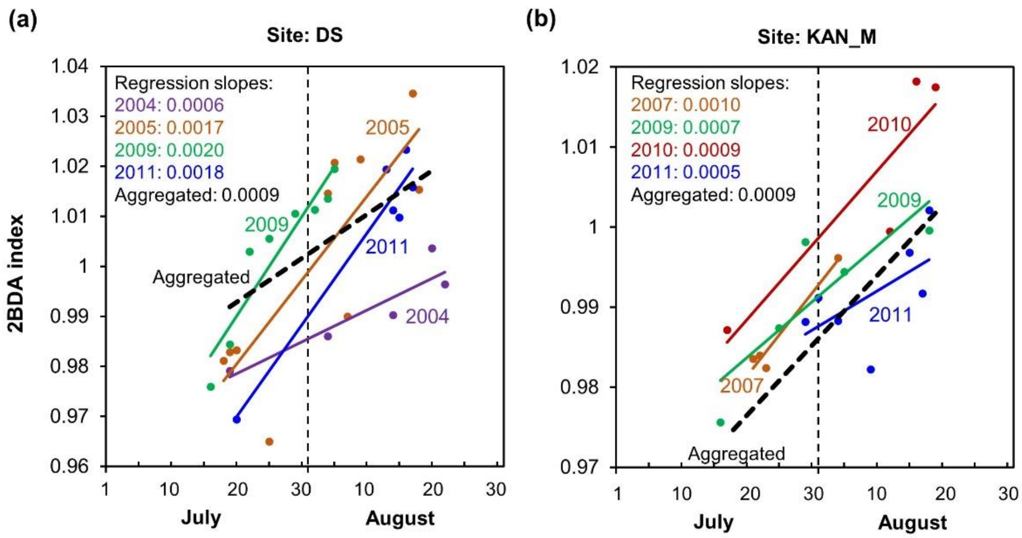


Figure A5. Temporal trends of 2BDA index through mid-July to Mid-August in different years at sites DS (a) and KAN_M (b).

P16 L342-359: very wordy. Requires paragraphing. Also consider in here which assertions can be retained once major review comments are addressed. It remains particularly difficult to follow the links with the field data despite close reading of the m/s.

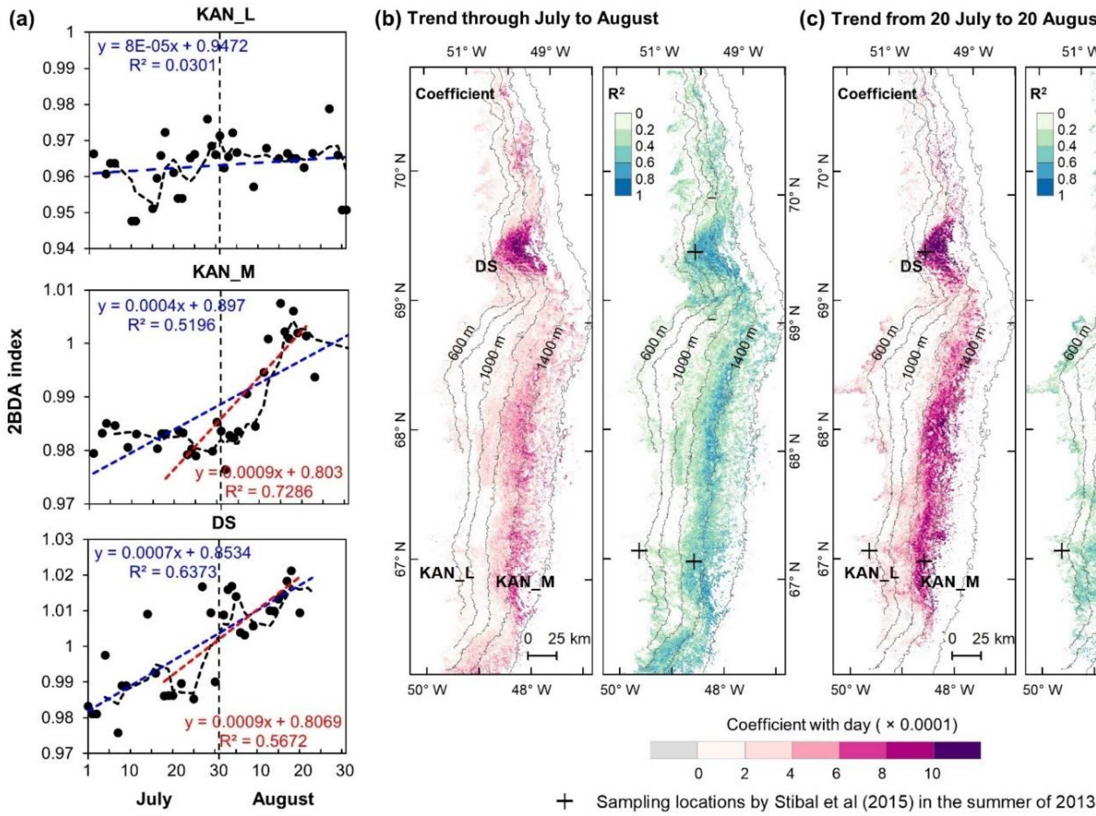
Response: We have rewritten and restructured this section, and have added more text describing how and why we compared the remote sensing data to previous field data.

Fig. 8: (a) panels use inter-annual averages of each day and are therefore not especially useful at the process-level: like any other process, algal growth is not actually dependent on time but on a range of processes. Examination of individual years with varying melt season characteristics would therefore be more useful. At the very least, it would be good to see faint lines for each year plotted into the background of these panels. Associated question: how much 'noise' is there in individual years relative to the 'climatological' averages being shown?

Response: We aggregated the daily data in different years to estimate the overall seasonal trend since the data for some years are not sufficient to obtain a reliable seasonal trend. We agree that algal growth is a complicated process, being affected by multiple factors like nutrients, meltwater, sunlight, temperature, and so on, which needs further investigation in the future by combining in situ and remote sensing observations with meteorological and regional climate modeling data. However, characterizing the seasonal trend of algal growth over time is still useful for understanding the average patterns of seasonal growth across multiple years. In this revision, we added a supplementary figure (Fig. A5) to show the temporal variations of 2BDA index in different years over the sites KAN_M and DS (algae-abundant area), showing that the regression slope of 2BDA vs. time is quite consistent between different years despite some variability at the DS site.

Fig. 8c and section 4.4: is the chosen breakpoint of 20 August statistically significant?

Response: We choose 20 August as the breakpoint because for most of the years we studied, snowfall happened after 20 August and covered the bare ice surface where glacier algae grow. In this revision, we removed the points with p-value greater than 0.05 from Fig.7b and Fig.7c.



Revised Figure 7: Temporal trends of the 2BDA index over July and August. (a) 2BDA time series and temporal trend analysis over KAN_L, KAN_M, and DS. (b) Regression slope and R^2 estimates of the temporal trend analysis for the period of July–August (for areas where the p value <=0.05). (c) Regression slope and R^2 estimates of the temporal trend analysis for the period of 20 July–20 August (for areas where the p value <=0.05).

Discussion: excessively wordy in places, can be shortened without loss of meaning. Fig. 11: provide colorbar to interpret density colors. Consider providing R2 values instead of just R.

Response: We have revised the discussion section to improve clarity. We added the colorbar to Fig.11 as suggested. We use R instead of R^2 as our focus is to discuss the correlations between MAR albedo and MODIS albedo, and between the albedo bias and 2BDA index. The metric R describes the strength of linear association between two variables, while R^2 is generally used in regression models to represent the amount of variability in y (dependent variable) that can be explained by the regression model, which is not the focus in our context.

Text of page 22: this paragraph is overly long. It requires a re-structure.

Response: We have restructured this portion into three paragraphs and have revised the text.

P22 L456-459: might be worth noting here that this is opposite to the results of Tedstone et al. (2017).

Response: We have added discussion of the Tedstone et al. (2017) study as suggested.

L460: 'For each of the two variables'

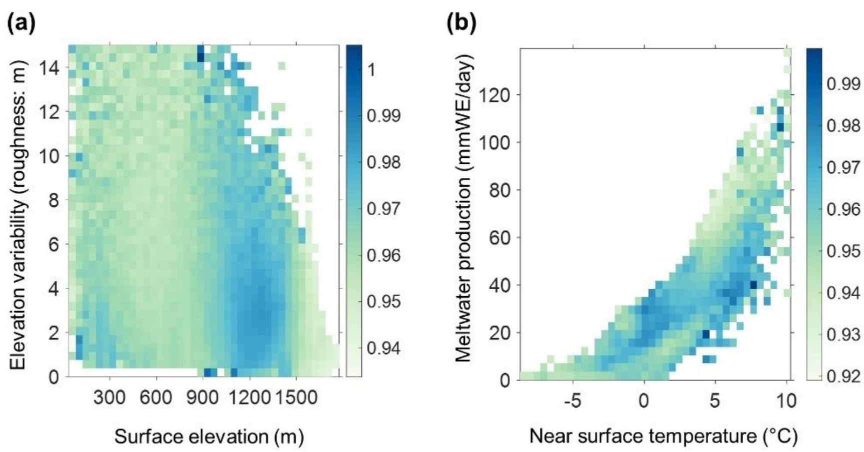
Response: We have changed the text accordingly.

P23 L474-481: reads hugely speculatively, especially given the relative lack of process-level understanding about ice algae available in the literature.

Response: This statement is indeed somewhat speculative, but we have provided these suggestions based on our analysis as a point of discussion for future study. We restructured and rewrote this section to clarify that these statements are somewhat speculative and more work is necessary to better understand these relationships.

Fig. 13a,b: why was a white mid-point of _0.97 chosen? Aren't algae judged to be present at values < 1?

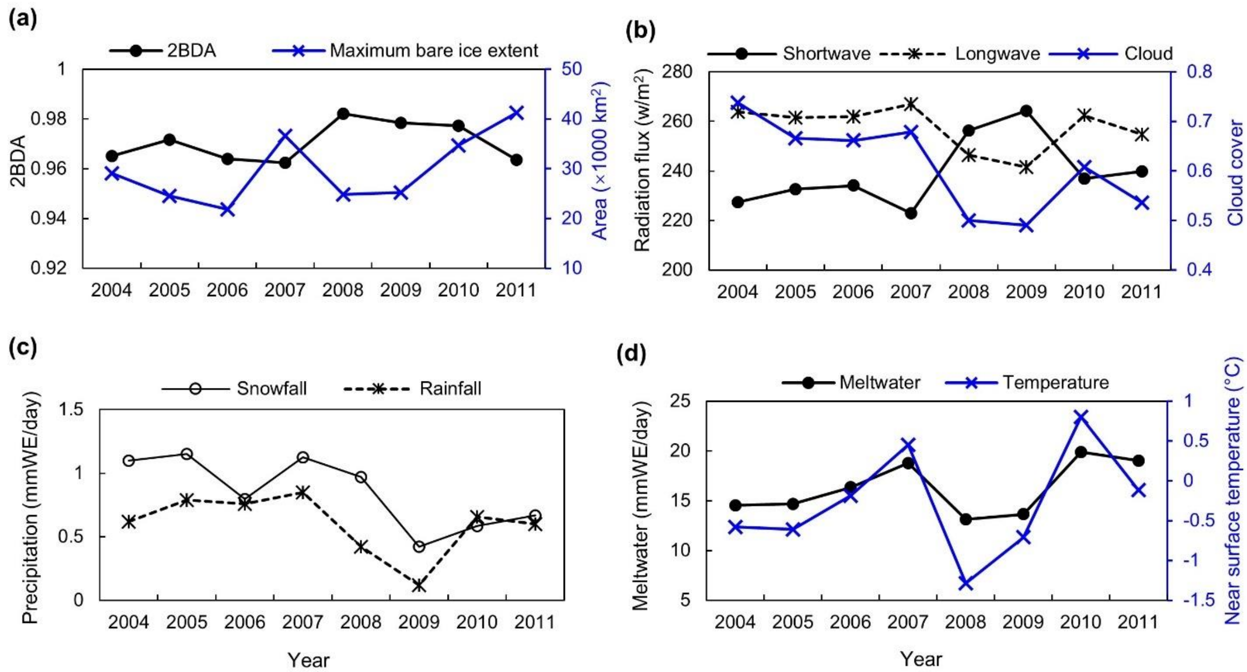
Response: We didn't intend to use 0.97 as a thresholding point. We changed the color scheme to avoid the confusion caused by color scheme. We also moved the subplots (a) and (b) to the appendix (Fig.B1). As noted in the response to reviewer #1, there is no particular threshold for which algae are deemed to be present or not present. Algae may still be present at 2BDA values less than 1, as the background bare ice spectrum shows a decrease from 665 to 710 nm. However, as we note in response to reviewer #1, there is a higher likelihood of dust impacting the 2BDA index below values of 0.99.



Revised Figure B1. (a) 2BDA index versus surface elevation and roughness (elevation variability within each MERIS pixel). (b) 2BDA index versus near surface temperature and meltwater production simulated by MAR. The colour bars in (a) and (b) indicate the average 2BDA index for each two-dimensional bin defined by the two variables on horizontal and vertical axis.

Fig. 13c,d,e: I am not sure what the relevance of providing these data are. At the very least it would be useful to add some kind of annual 2BDI and Impurity Index time series for comparison with the provided metrics.

Response: We added the annual 2BDA time series to the figure as suggested. This analysis is meant to be a preliminary investigation of possible relationships between algae and climate forcing, and is provided as a discussion point for future research.



Revised Figure 13: (a) Average 2BDA index and maximum bare ice area from 2004 to 2011. (b) July-August mean of downward shortwave and longwave radiation fluxes and cloud cover over the study area from 2004 to 2011. (c) July-August mean of rainfall and snowfall. (d) July-August mean of meltwater production and near surface temperature.

Reference for the response

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