

## ***Interactive comment on “Quantifying spatiotemporal variability of ice algal blooms and the impact on surface albedo in southwest Greenland” by Shujie Wang et al.***

### **Anonymous Referee #2**

Received and published: 3 December 2019

#### 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.

#### Major comments

The manuscript tests several remote sensing ratio indices and shows that, to some  
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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.

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.

On justification of the 2BDA, Wang et al. (2018) point to Painter et al. (2001) as evidence that glacier algae can be 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 centimetre patch scales, which is not representative of OLCI or MERIS 300 m data. Some consideration of the scale mismatch is therefore required.

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.

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.

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.

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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.

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'.

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 focussed solely on glacier algae.

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

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.

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.

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.

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

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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.

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

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.

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.

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.

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

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.

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)

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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.

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.

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?

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

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.

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

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

L460: 'For each of the two variables'

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

Fig. 13a,b: why was a white mid-point of ~0.97 chosen? Aren't algae judged to be present at values < 1?

Fig. 13c,d,e: I am not sure what the relevance of providing these data are. At the very

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least it would be useful to add some kind of annual 2BDI and Impurity Index time series for comparison with the provided metrics.

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Interactive comment on The Cryosphere Discuss., <https://doi.org/10.5194/tc-2019-226>, 2019.