Second review of Wang et al.: Quantifying spatiotemporal variability of glacier algal blooms and the impact on surface albedo in southwest Greenland.

I thank the authors for a thorough response to my comments on the previous manuscript. Overall, I consider the revised version to be a big improvement, and I am sincerely appreciative of the work that has gone into addressing my comments. Several of my concerns have been mitigated by their detailed response, and I consider the support for glacier algal distribution estimates using the 2DBA index to be sufficient. I think there is a lot of value in the tracking of the 2DBA and albedo over the MERIS record, and the authors have presented these analyses well. However, I still feel there are some uncertainties where cell concentrations are quantified using the index. My comments below relate specifically to cell quantification.

Quantifying cell numbers requires that the index is insensitive to spectral mixing and variations in ice albedo. However, there is insufficient evidence that this is really the case. In their response to my initial comments, the authors argue that ice grain size does not influence the 2DBA index on the basis that: a) Ryan et al (2018) found distributed impurities to explain the majority of the albedo variation on the SW Greenland Ice Sheet; b) “to their knowledge” the 2DBA index is not affected by variations in ice albedo; c) selected MERIS spectra show similarities with selected field spectra. I do not find these to be terribly convincing arguments, because:

a) Ryan et al. (2018) also did not account for the changing albedo of the underlying ice, so it is plausible that the reason why “distributed impurities” were found to be responsible for the majority of the albedo variation is that the combined effect of the impurities and their feedbacks to the underlying ice structure were actually measured. Ryan et al. (2018) made no attempt to quantify impurity concentrations, and I do not see how their work supports the insensitivity of the 2DBA index to ice grain size.

b) I find this argument illogical. The 2DBA index compares reflectance at 665 and 709 nm. The latter is in the NIR range, which is precisely the region most sensitive to changes in grain size. Even if the visible wavelengths are insensitive to grain-size changes, i.e. the denominator in the 2DBA equation is fixed, the numerator is in the NIR wavelengths, so the index must surely be sensitive to grain size. I do not see convincing counter-arguments from the authors on this issue, but it is important because it undermines cell quantification using the 2DBA index.

c) Selecting similar spectra is only slightly helpful, since there are likely very many spectra that are less similar than the ones chosen.

The authors suggest in the manuscript that they conducted a sensitivity analysis for grain sizes 500 microns and 1500 microns but they do not comment on the results, only mentioning that changing ice density had a negligible effect. I would like to see the results of varying grain size on the 2DBA index but also note that the simulations have presumably been performed with dust as the primary light absorbing particle, which the authors assert does not produce a positive 2DBA index anyway.

There is also an assumption in the manuscript that 1500 microns is the appropriate grain size for representing glacier ice across the bare-ice zone, but there will be huge variability in this in the real world, which will feed forward into uncertainty in the 2DBA where it is used for quantifying cell abundance. The relationship established in Wang et al. (2018) is also based upon observed relationships between 300 x 300 m OLCI spectra with a very few centimeter-scale field spectra, so this association is also likely subject to the same uncertainties listed above. While their new linear mixing analysis is helpful, and adds value to the manuscript, it only really supports detection of algae, not quantification.

To reiterate, I think the authors have done a good job providing evidence and arguments in favour of their 2DBA index probably being suitable for assessing algal coverage/distribution. I think the majority of the paper is therefore publishable in TC. In my opinion, removing the sections that present cell quantifications derived from the index and the associated discussion would leave a much more robust paper, unless the authors can provide robust validation and estimates of uncertainty. This would be a relatively small change overall, as the majority of the paper does focus on spatial distribution.