Wang et al. (2019): Quantifying spatiotemporal variability of ice algal blooms and the impact on surface albedo in southwest Greenland

Overall:
The authors have presented a paper that attempts to quantify biomass over the western Greenland Ice Sheet using the well-known "red-edge" technique that they refer to as "2BDA", which is often used for detecting chlorophyll-containing biota such as photosynthetic algal blooms in oceanic and lacustrine environments, vegetation and crop mapping. Biomass quantification over the Greenland Ice Sheet is a worthy research goal because Greenland Ice Sheet glacier algae very likely play an important role in controlling the ablation zone albedo that is not yet accounted for in energy balance models. This is well within the scope of The Cryosphere and the scientific question is worthy of consideration in this journal. There are some very useful aspects to the paper, including demonstration that there are ablation zone albedo processes that SMB models currently do not account for, the albedo time series over the western ablation zone, and the comparison between different band ratios. However, there are some major issues that need to be addressed before I can recommend publication. More detail is provided below.

Major Comments:
1) There is past literature that emphasises the importance of discounting abiotically-generated red-edge signals before assuming them to be diagnostic of photosynthetic life (see Sparks et al. 2009; Seager et al. 2005). The vulnerability of the red-edge to false positives is demonstrable using a simple radiative transfer model (easily replicated in-browser via http://snow.engin.umich.edu/). Fig 1A shows the results from SNICAR runs where all variables were held constant except the mass concentrations of completely inorganic impurities (Flanner et al.'s (2009) "global average dusts, type 4"). The lowest 2 spectral albedo curves returned a false positive 2BDA result (1.002 and 1.005). In Figure 1B, the model is run again identically but with a hematite-rich mineral dust taken from Polashenski et al. (2015), giving eight 2BDA false positives (1.004, 1.015, 1.026, 1.031, 1.039, 1.043 1.046). Tedesco et al. (2013) suggested that hematite-rich red dusts are present in the GrIS ablation zone (we note that Cook et al. (*in review*) disagreed about that but their paper remains unpublished). Taking Tedesco et al. (2013) to be correct about the prevalence of red dusts on the Greenland ablation zone therefore invalidates the red-edge as a biomarker due to the demonstrable potential for false positives. Convincing empirical data is required to demonstrate that the 2BDA signal is exclusively biological and robust to these types of false positive results.

A) B)

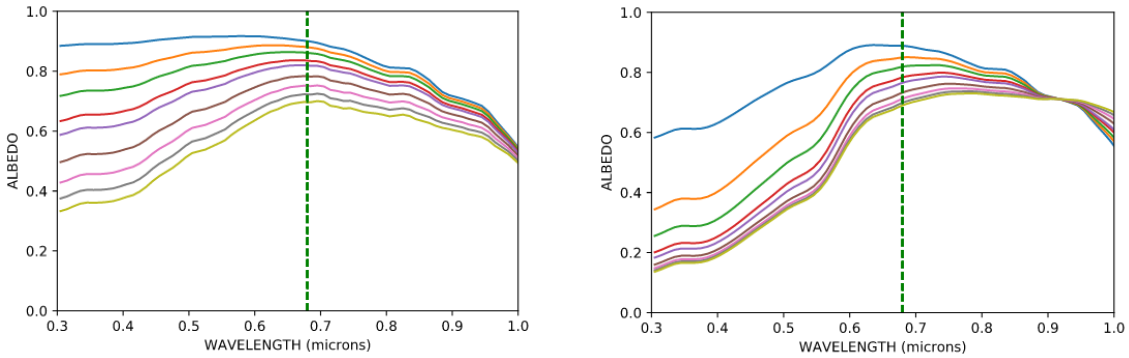

Fig 1: A) SNICAR runs with diffuse irradiance, homogenous snow with grain diameter 500 micron, density 400 kg m^{-3} and Flanner et al. (2009)'s "dust 4" in the upper 1 mm, in mass concentrations of 0.1, 0.3, 0.5, 0.8, 1.0, 1.5, 2.0, 2.5, 3.0 ug_{dust}/g_{ice}. B) Identical SNICAR runs but with Polashenski et al. (2015)'s high hematite dust.

2) The authors do not account for the spectral albedo of the ice itself. Ice albedo can vary dramatically independently of light absorbing particles and cause the 2BDA value to change,

undermining the biomass quantification. Figure 2 shows identical simulations to Fig 1, except the grain size is increased to 1500 microns. False positive results are returned as before, but the value of the 2BDA indexes - and therefore the retrieved biomass – change (Flanner et al. (2009) dust = 1.0009 and 1.004; Polashenski et al. (2015) dust = 1.0018, 1.014, 1.026, 1.032, 1.040, 1.043, 1.045, 1.046). The retrieved biomass therefore changed without any change in impurity loading. On real glacier ice where the ice albedo can vary by tens of percent independently of impurity concentration due to weathering crust development, meltwater accumulation and drainage, topography, impurity mixing and glaciological structure, the potential for highly error-prone retrievals is likely very high. The authors need to demonstrate that their band ratio is not vulnerable to this uncertainty, or that they can quantify and correct for it.

A) B)

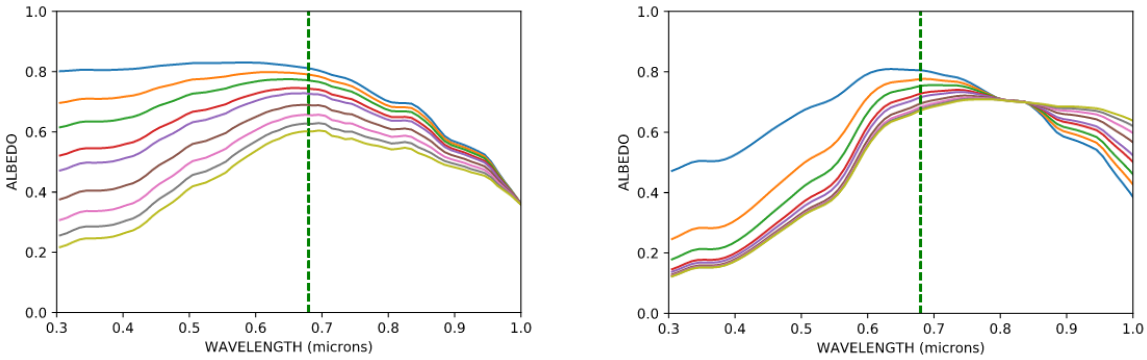

Fig 2: A) SNICAR runs with diffuse irradiance, homogenous 1500 micron, 400 kgm-3 snow with Flanner et al. (2009)'s "dust 4" in the upper 1 mm, in mass concentrations of 0.1, 0.3, 0.5, 0.8, 1.0, 1.5, 2.0, 2.5, 3.0 ug_{dust}/g_{ice}. B) Identical SNICAR runs but with Polashenski et al. (2015)'s high hematite dust.

3) The authors do not adequately address the problem of scale mismatches between the normal length scales of typical algal blooms (biomass varies dramatically over 1-10 m length scales) and the satellite used to gather their data (300 x 300m). Surface heterogeneity must surely introduce major uncertainties as the spectral reflectance of each pixel is the combined product of many highly variable surfaces. How would, for example, cryoconite, surface water, dust, crevasses and surface topography at the sub-pixel scale affect their biomass quantification? These unquantified factors must influence the predicted cell concentration independently of real-world changes to the mass concentration of algae, but they are not discussed in the paper.

4) There is insufficient detail regarding the use of field spectroscopic measurements as "ground truth". Only one single field spectrum is presented in the paper and the measurement conditions are not reported. Has it been picked because it matches well with the MERIS spectrum, is it a mean (in which case of how many samples, and what do the error bars look like) or is it the only available spectrum? How do other spectra in the field dataset compare? Can the authors provide evidence to suggest that centimeter scale field spectroscopy measurements are truly representative of the biomass over entire MERIS pixels?

5) The authors are selective with their citing of literature under review. If they wish to include papers still under discussion they should explain why the issues of spatial scale encountered in 20 m Sentinel-2 pixels discussed by Cook et al (in review) and Tedstone et al. (in review) do not also apply to their 300 m MERIS pixels. If they decide to stick to published literature they should explain how the presence of hematite-rich dusts on the ablating Greenland Ice Sheet as reported by Tedesco et al. (2013) does not invalidate their assumption that the red-edge is uniquely biological.

Specific Comments:

Title: As suggested by Daniel Remias in the open discussion forum, please adopt the generally accepted terminology “glacier algae” that distinguishes these algae from those found in sea ice.

General point about chlorophyll: Referring to “chlorophyll” is somewhat ambiguous as it could imply total chlorophyll or one of several chlorophyll variants. Please be specific that you mean chlorophyll-a.

L39: Any citation for yellow/orange snow algae? They are normally thought of as green or red.

L71: The authors rightly criticise carotenoid-based remote sensing methods because of possible false positives due to “dirt” but ignore the potential for equivalent red-edge false positives due to dust.

L75: The authors claim chlorophyll is the appropriate pigment to use to identify ice algae despite also stating that the coloration of the algae is primarily due to purpurogallin pigments. Why, then, is that not the appropriate pigment to use to identify glacier algae?

L75: “owing to its unique spectral signatures between 665-710 nm (Gitelson, 1992; Painter et al., 2001; Wang et al., 2018)”: Chlorophyll-a absorbs in narrow bands around 680 nm and 440 nm. Any effects extending up to 710 nm are due to interactions with the surrounding medium. This is why Painter et al. (2001) was able to use the narrow 680 nm absorption feature as a diagnostic tool for chlorophyll-a detection.

L79: “Quantification of ice algae biomass from satellite data based on the chlorophyll-a feature has received less attention since the chlorophyll-related satellite bands designed for land generally have coarse spectral resolutions.” This is just one of many reasons why remotely detecting algae over glacier ice is not simple. Other complexities include the complex pigmentation of the algae, the spatial resolution of the remote sensing instruments relative to the typical length scales of individual surface features (including algal blooms) and critically the optics of the underlying ice that vary dramatically over space and time and which are not yet well described. These issues are as important as the spectral resolution and must be acknowledged.

L100 – 120: Much more detail is required here. For example, how many actual field samples were used to validate your remote sensing retrievals? What were the biomasses measured at those sites? What were the measurement conditions? On which dates were spectra available at which sites? Were the measurement times conistent and how do they compare to the satellite overpass times? What was the sensor footprint size for the field measurements and how were these upscaled to the satellite pixel scale?

Figure 1: Please provide details of the field spectrum presented as the dashed line. Where/when was it collected and how does it compare to other field spectra presented in this paper?

L209: Chlorophyll-a is the primary photosynthetic pigment, but not the primary light-absorbing pigment. In both the studies you have cited the chlorophyll-a absorption feature is actually extremely subtle – in fact in Cook et al. (in review) it was only really discernable in the derivative spectra and indistinguishable in the raw reflectance. In Stibal et al. (2017) the spectrum are presented with a very truncated y-axis to make the pigment feature discernable.

L211: “pure ice” has lower reflectance at red wavelengths compared to shorter wavelengths.

L216-220: This line of reasoning borrows heavily from studies of chlorophyll-a dominated species in other environments and still requires the red-edge to be validated over glacier ice where LAP and meltwater mixing, complex pigmentation and ice optics are potential confounding variables.

Figure 2: How did the authors select the field spectrum to plot on this figure? Is this the average of all available? If so please provide error bars and number of observations. Also, 184 cells/mL reported in the legend is a tiny amount of algae, unlikely to explain the albedo reduction observed – is this a typo? What was the mineral dust type and concentration in the same area – could it also explain the red-edge? How much of the albedo reduction can be attributed to the algae and how much to melt water/dust? If the absorption is mostly due to chlorophyll-a as the authors suggest, why is the absorption maximum outside of the chlorophyll-absorption range shown in Fig 2c and why does it extend across the visible wavelengths? Why do the field spectra and remotely sensed spectra diverge below ~640 nm?

Figure 3: A) "Agust" → August; B) The authors present spectra for "dark ice (more chlorophyll)" and "dark ice (less chlorophyll)". However, there does not seem to be any positive 2BDA signal in the latter spectrum at all. Is it actually "dark ice (no chlorophyll)"? If so, there are additional darkening processes occurring on the ice. What processes are darkening the ice in those areas and to what extent do those ice darkening processes also influence the biomass retrievals in areas where there is a positive 2BDA result? What effect does this have on retrieved biomass? What is the detection limit for the 2BDA method?

L413: Cook et al. (in review) mention that a red-edge signal was present in most of their algal hyperspectral data but they do not mention false positive rates and they opted not to use that method for their spatial upscaling. It would therefore be useful to know the false positive rate in the present study and how it scales to 300m MERIS pixels.

L416: It is not clear to me from the manuscript precisely how you have inferred algal cell abundance. Please provide further methodological details.

L450-460: Another explanation for this is that the overall ice albedo is lower, there may be smoother ice and more water at the surface, and rather than there being less algae, the red-edge signal is simply erased by an overall dampening of the spectrum across all wavelengths (i.e. putting dark impurities on dark ice has a less detectable effect that putting the same impurities on otherwise bright ice). Can the authors demonstrate that this is not the case?

References:

Cook et al. (in review) Glacier algae accelerate melt rates on the south western Greenland Ice Sheet, The Cryopshere, https://www.the-cryosphere-discuss.net/tc-2019-58/#discussion

Painter, T. H., Duval, B., and Thomas, W. H.: Detection and quantification of snow algae with an airborne imaging spectrometer, Appl. Environ. Microbiol., 67, 5267–5272, https://doi.org/10.1128/AEM.67.11.5267-5272.2001, 2001.

Tedesco, M., Foreman, C., Anton, J., Steiner, N., Schwartzman, T.: Comparative analysis of morphological, mineralogical and spectral properties of cryoconite in Jakobshavn Isbræ, Greenland, and Canada Glacier, Antarctica. Annals of Glaciology, 54(63), 147-157. doi:10.3189/2013AoG63A417, 2013.

Tedstone, A. J., Cook, J. M., Williamson, C. J., Hofer, S., McCutcheon, J., Irvine-Fynn, T., Gribbin, T., and Tranter, M.: Algal growth and weathering crust structure drive variability in Greenland Ice Sheet ice albedo, The Cryosphere Discuss., https://doi.org/10.5194/tc-2019-131, in review, 2019.