Overall comments:

The manuscript by Forrest et al. applies AUV-based measurements of transmitted multispectral irradiance to estimate spatial variability in bottom ice algal pigment biomass using a normalized difference index of 470 and 565 nm. The manuscript presents a very neat dataset of NDI-derived chlorophyll concentrations over three 500-m transects. However, there is a lot more that could have been accomplished and thus this submission seems to be a presentation of a partial dataset and analysis, and does not really have the impact to warrant a short note style publication. There could have been a lot more in-depth analysis of the spectral response of transmitted irradiance to the different cover types (e.g., snow, ice, chlorophyll, water, etc.). For example, analysis of the TriOS dataset could have been used to examine the hyperspectral response, which would provide arguments to support/refute limitations/benefits of the AUV-based multispectral sensor. This suggestion leads to an aspect of the manuscript that was troubling, i.e., the choice of multispectral wavelengths for the Satlantic sensor used on the AUV. Knowing that previous research had shown NDIs using wavelengths closer together (on the order of 10-20 nm apart) better explain ice algal pigment biomass variability, why were wavelengths chosen to be spaced far apart? That is, was there no choice on the wavelengths used in the sensor when originally purchased?

Another aspect of the manuscript that is perhaps the most troubling was the fact that a calibration of the AUV-NDI method is attempted, but in the end not used. Instead a range of core-based measurements is used to confine the NDI-values. Such a standardization is not really appropriate as it suggests NDI-estimates of chlorophyll concentration were absolute. A more simple approach could be to present un-calibrated NDI values as really, only spatial variance is analyzed in the AUV dataset. To support such a data presentation and analysis, an additional examination on the influence of snow depth, ice thickness and chl concentration on NDI values would have been useful to establish the dominant role of chl on NDI variability. Such an analysis would place greater confidence on similar statements made in the manuscript.

To conclude, I believe the dataset warrants publications and the pros/cons of the application of AUVs to estimate large spatial scale estimates of ice algal biomass be discussed in the general scientific audience. However, the presented analysis is largely incomplete and requires a more in-depth analysis and data presentation. Therefore, I suggest the manuscript is rejected, but potentially invited back for a full-length paper after a more complete analysis and interpretation is attained.

Line-by-line comments:

Page 2:
Line 6 – Ehn and Mundy (2013) is not really an appropriate citation for the statement.
Line 20 – 500 m

Line 29 – 1-cm… 1-m
Line 30 – Ice thickness or more appropriately, draft?
Line 32 – is this draft or thickness – if thickness, how do you account for ridge sails and snow cover? That is, a description of the methods needs to be included.

Page 3
Line 2 – Fig. 1 c – From the figure, it is not clear how ice thickness sonar estimates agree with measurements.
Line 9 – A citation backing the statement on "more spatial precision" is needed as it is not clear why a statement would be true, provided the idea that you are seeking to cover a larger area with the AUV.
Line 16-18 – Why "therefore"? Does downsampled mean an average of 25 measurements? and what is the downsampled footprint being measured if the AUV is moving at 1.5 m s-1, with 75% of the measurement coming from a 4 m footprint at 25 Hz?
Line 19 – 10-nm
Line 22 – Depends on the algal pigment biomass in that algae can have a strong signal at 670 nm if enough biomass within the sea ice.
Line 23-28 – Why such a lengthy description here? I assume you took transmitted irradiance measurements over a depth profile to calculate water column attenuation coefficients for specific wavelengths. Or, did you use coefficients provided in Kirk (2011)? The more I read this paragraph, it was unclear.

Page 4
Line 7 – Attached platelets? Were there unattached platelets? Was there ice algae on the platelets?
Line 18-19 – "passed through a nozzle of 60 ml syringe"? I have never heard of this method before. Is it common? I have a little concern that this method could act to burst cells? Typically, gentle back and forth inversion of the sample re-suspends the cells, which can then be subsampled. How were aliquots taken from the melted sample? Was the sample homogenized in terms of suspending the cells equally in the sample before subsampling?
Line 32 to Line 2 on Page 5 - ? It is not clear what a damaged core is and why one would not include the samples in the analysis. These details should be flushed out in the results when showing **all** the data to start.

Page 5
Line 6-8 – Why even do the TriOS NDI calibration? Why not just compare the irradiance response of the two sensors together?
Line 17 – spelling - *Nitzschia frigida*
Fig. 2 – These are pretty incredible pictures. Visual inspection of the biomass shown in Fig. 2 looks to be very dense and growing up into the ice bottom beyond the saw in the picture. Is an average estimate of 26.7 mg m-2 realistic, or were these pictures from particularly high chl concentration sites?
Line 24 – R2 would be good to provide information on how much variability in chl concentration was described by the regression against the NDI values. It is included

in Fig. 3 caption, but why use an adjusted R2 value? The calculation for an adjusted R2 is to take into account the influence of more than one variable in a multiple regression, but only a linear simple regression is accomplished.
Line 25-31 – There appears to be a lot of discussion and citations in the results section. Is this appropriate for the journal?
Fig. 3 – Technically, the axes are plotted wrong. Chlorophyll should be the independent variable and NDI the dependent.

Page 6
Line 23-24 – There appears to be a lot of interpretation and discussion in the results section – suggestion to split this out.

Page 7
Line 4 – Why are there no comparisons with literature that already exists on ice algae patchiness? There were at least 5 studies in the 90s to early 2000s on this topic.
Line 30 – Is there a citation to support this statement? It makes sense, but has someone investigated this before?

Page 8
Line 22 – ROVs do the same task, so it is not clear why this point is made. Actually, one can argue that the need to keep an AUV further from the ice bottom than an ROV would be a detriment to the technique due to the error associated with water column attenuation. The main benefit of an AUV is distance covered by a single deployment, which is not demonstrated in the current manuscript. Is the inclusion of the statement on line 22 valid given the results presented?