The Cryosphere

Discussions

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Interactive comment on "Brief Communication: Capturing scales of spatial heterogeneity of Antarctic sea ice algae communities" *by* A. L. Forrest et al.

Anonymous Referee #1

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This is an interesting study and the authors have collected a unique dataset using cutting edge methodology. The paper is generally well written and structured. However, in my opinion the paper has some shortcomings in regards to some data analyses and text, and I feel this unique dataset has not been utilized to its full extent. Below I have provided numerous remarks on the text as it is often vague and long-winded. In several instances I also suggested to cite more relevant and recent literature. Furthermore I made additional suggestions for more in-depth analyses of the data. Key critical points are a) the development of the NDI to chlorophyll a relationship (20 versus 60 data points, presentation of Figure 3), b) a lack of a discussion of effects of the distance (6m) of the AUV sensor to the subsurface of the ice (is there any information

on the chlorophyll concentration in the under-ice water, how did this distance affect signal/noise ratios and NDI-based chlorophyll a estimates) , c) lack of information on AUV location and potential navigation errors, and c) the calculation of patch-sizes based from data with a large foot-print (of the radiometer), e.g how were data-points for Fig.4 calculated. A revised manuscript might not fit the TC "Brief Communication" format anymore, however a detailed "Supplementary Information" section might be useful and could help to keep the manuscript in a short format. Given these shortcomings the manuscript requires major revisions.

Abstract: Please focus the abstract on your study and your results. In particular the last two sentence are vague. I would prefer to see some data on algal biomass, ice thickness and snow thickness from this study in the abstract, rather than a description of "where to go next". More generally, I suggest to focus the manuscript on the scientific results rather than on the innovation in engineering.

P1, L 12: "to quantifying ecosystem responses" (quite a long shot to go from simple observations to predictions...)

P1, L14:.. to describe the spatial heterogeneity of ice algal distribution.

P1, L15: rather define the ice as "land-fast sea ice" rather than first-year.......

P1, L15-16: "These results....monitored" (Please clarify sentence)

P1, L 16 -17: This a very vague statement. The manuscript does not provide any information how ice algal communities could be categorized or how productivity could be measured. I suggest to delete and re-write abstract as per above comments.

Introduction: P1, L 23 -24: Please be more specific. Focus could be on the entire Southern Ocean (e.g. Saenz & Arrigo 2012, Arrigo 2014, Meiners et al. 2012) or Antarctic fast-ice ecosystems. Rysgaard is an older (and Arctic) reference. Maybe cite Mundy et al. 2007 and the recent papers of Campbell et al. 2014, 2015?

P1, 28 -29: This statement is true for Arctic sea ice and Antarctic land-fast sea ice, but

not necessarily true for "Antarctic sea ice" (see Horner et al. 1992, Arrigo 2014).

P1, 29-31: Please clarify sentence (Significance of what?)

P2, L1: Classic work on ice algal patchiness in Antarctic land-fast sea ice has been conducted by Swadling et al. (1997). This would be a good citation.

P2, L5: Maybe re-phrase? "Links between ice algal biomass and the under-ice light field are well established. . ..

P 2, L 9-10: As far as I understand NDI explain variability in the biomass (e.g. in a dataset used for calibration efforts), but a NDI does not explain spatial variability! Please correct this sentence.

I suggest major re-write of the introduction. It should provide an overview of a) the importance of Antarctic land-fast sea ice studies and b) recent advances in technologies to measure ice algal biomass non-invasively.

Methods: P2, L 13: This study did not measure transmittance! Transmittance is defined as the ratio between incoming solar radiation at the surface of a medium and the amount of radiation at the bottom of the medium. Rather than "spectral properties of light transmittance" the "multi-spectral under-ice irradiance" was measured. Please use proper terminology.

P2 L17-18: "patches a few meters across" and "very small (1s to 10s of meters" is duplicated information, I suggest to rewrite/combine these sentences

P2, L18: report snow thickness in "m" (SI unit) rather than "cm"

P2, Line 23: replace "Southern Ocean" with "land-fast sea ice in McMurdo Sound".

P 2, L25: Could you please provide a linear regression and an R2 for the ice thickness (measured from cores) versus ice thickness (measured from the sonar) relationship. This would greatly help to understand sonar-based ice thickness error.

P2, L30: "greater spatial precision", please provide information on the AUV positioning system and its accuracy, it might be useful to cite recent AUV (Katlein et al. 2015, JGR) here

P3, L4-5: Given the speed of the vehicle and the downsampling to 1Hz there will still be some overlap of the area measured. Was this accounted for (e.g. through calculating a running mean?)

P3, L 10: Are there any measurements of the Chlorophyll a concentration in the under-ice water available? How does integrated Chla in in the 6m of water-column compare to the integrated Chla concentration in the overlying sea-ice? This would be very useful information to understand the signal:noise ratio in the measurements.

P3, L 28: "two volumes"? , please be more specific and maybe add a reference for this methodology

P4, L1: please provide reference for method of determination of chlorophyll a

P4, L13-15: Much more detail is required how the calibration of the AUV data was "tuned". It would be preferable to 1) show the results from all (60?) sites where ice cores and radiometer measurements were taken simultaneously and to discuss the derived best NDI (e.g. to compare with results from previous NDI studies) 2) show how these results were affected from discarding "selected" cores 3) show/explain the AUV calibration tuning method in detail and discuss potential error propagations through these 3 steps Also please explain NDI "(Ratio)" as x-axis descriptor in Figure 3. Is this indicating a further normalization of the originally determined NDI results, or should this read just "NDI" Figure caption indicates: N=19, but it appears that it is N=20.? Text indicates 14 sampling sites with a total of 60 "replicate" measurements Are these results from the 14 or 20 sites or is this a random subset of the 60 individual measurements. It appears that using the full 60 individual measurements would result in a stronger statistical relationship of NDI versus integrated algal biomass. "Damaged" core could be plotted in a specific colour. This would make are stronger argument for the used

NDI to chla relationship (which has a relatively poor fit).

Results: P4, L 21: do you mean "interstitial AND in the platelet ice", e.g. in figure 3 samples are distinguished between "interstitial" versus "platelets". Please clarify statement.

In addition algal communities in Figure 2b and 2d are looking different to those pictured in 2a and 2c. Algae in 2b and 2d closely resemble "stand" communities as commonly found in the Arctic. Were strands also evident in any of the core samples?

P4, Line 26: I understand that the NDI (470 nm : 565) was used for calibration of the multi-spectral AUV-based measurements but please also show the best NDI to Chla relationship that was derived from the 60 point measurements. This might help future studies in selecting the bands of AUV mounted radiometers.

P 4, L 26: Given that for NDI (470 nm : 565 nm) versus Chla the R2 was only 0.403, I doubt that this NDI explained the "highest proportion of the variability in algae biomass"

P5, L5: In this paragrapah I would just state that the poor predictive power of the algorithm was a result from the limited number wavelengths available from the AUV mounted radiometer.

Again it would be good to show the best NDI to Chla relationship as derived from the high-resolution/hyperspectral point measurements.

P 5, L 7: I suggest to delete reference to "multi-year sea ice was largely absent". Was there any multi-year sea ice in your sampling area of 500 x 500 ?

P4, Line 11: shorten "we consider it possible to assume" ?

Figure caption 1: ". . ..where spectral intensity measurements were made (CE1, CE2 and CE3) across 60 m swaths", please specify if this statement is correct. In the Methods sections it is explained that that the footprint of the radiometric measurements was about 4m in diameter. This is contradicting 60 m wide swaths.

Figure 3: I suggest to show data with a log scale y-axis starting at 1 and ending at 100 mg Chla m-2. The current y-axis starting at 5 and with a maximum of 100, and at the same time showing "linear" axis-descriptors is confusing. Why are only 20 data points shown, when you have measured 60? It would be preferable to see all 60 paired measurements of Chla versus NDI.

P5, L 17-18: Clarify sentence there seems to be an "and" missing.

Discussion: P 5, L 23-24: No data on the varying amplitude and length scales of patchiness for spectral irradiance are shown. Please be more specific in the terminology used to describe your data.

P5, L 23-24: "For each transect, chla estimates were interpolated to constant spacing between samples to allow a spectral analysis of the data using a standard Fast Fourier Transform (FFT) similar to time series analysis". This information needs to go in the Methods part of the manuscript. Define "constant spacing". Is this the length scale of the radiometric foot-print of approx. 4 m in diameter? Please detail you produced the data presented in Figure 4. They seems to have a higher resolution than the maximum resolution that can be achieved by the measurements, e.g. the 4 m sensor footprint.

P5, L 26-27: Please show these correlograms (maybe in a Supplementary Information section?) .Variograms have been calculated for light transmission in Arctic sea ice and this would allow for interesting comparisons (Katlein et al. 2015).

P6, Line 3: There are more relevant references available for the spatial variability of ice algal biomass, e.g. Rysgaard et al. 2001, Steffens et al. 2006: Søgaard et al. 2013). I suggest to cite these and discuss new data (this study) in relation to these previous studies.

P 5, Line 10: Here it would be good to cite and relate to some other studies that investigated snow – ice algal biomass relationships. One additional sentence about potential thresholds in the snow ice algal relationship and the seasonally changing

influence of snow on ice algae would be useful.

P5, Line 15: One would assume that most possibly platelet ice – (partly) consolidated into the ice sheet was driving the biomass distribution. From Figure 3 it appears that platelet ice was associated with higher algal biomass. Could the sonar data shed some light into this? E.g. could the sonar data be used to detect platelet ice patches – presumably associated with a higher surface roughness? This study provides a unique dataset for testing this. Rather than mentioning this as a hypothesis, I suggest to use the data to test this relationship.

P6, L 7: Ate there any ice draft data available from the other two transects? Why were analyses of the correlation between ice draft and NDI-derived algal distribution restricted to this single transect?

P 6, L 17-22: Please clarify both these sentences. Do you want to say: "The poor NDI to Chla relationship does not allow for more complex analyses of the relationships between physical sea ice properties and ice algal biomass. Nevertheless, our data provide proof-of-concept to use AUV technology to measure ice algal spatial variability"?

P6, Line 24: please define "infiltration ice" ?

P7, L3: "fast ice" rather than "first-year sea ice"

P7, L6: "well above" rather than "well below"? One could argue that most ice core studies work on areas of 10m by 10m .

P7, L 7: "ice coring" rather than "sample coring"

P7, 9: "this most cryptic of sea ice algae communities"? most cryptic compared to what other communities?

Figure 4: a) Why is sea ice draft only shown for one transect, when data for all 3 transects are available (see Fig.1) b) What is the bin-size for the chlorophyll a lines. It appears that they are shown at a higher resolution than 4 m, but in the text 4 m (radius)

is given as the radiometric foot-print of the sensor. Please explain and adjust (bin) chla values to 4 m length scale - if necessary.

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Discussion paper