

Observations of late-winter marine productivity in an ice-covered fjord, West Greenland

Response to reviewers' comments

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Reviewer 1: As the authors have measured temperature and salinity for each of the experiments (if not samples), I wonder, why this data (though not being crucial) is not included in the data set. I also wondered, why the data file archived at PANGAEA did not include the full data set, i.e., DO results for each of the individual samples. Instead, it only included the results listed in Table 2 of the manuscript, i.e. mean values and uncertainties/ranges for the different experiments carried out in the fjord center and at its edge. I would like to ask the authors to comment on this.

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Response: There was little variation in ambient temperature and salinity (-1.5 to -1.7°C, and 32.6 to 32.8 psu). This is noted in Section 3, first paragraph. We originally only archived the summary of results as this was considered the most useful format; however, we will add results for individual bottles to the archive along with temperature and salinity measurements.

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Reviewer 1: page 5, second paragraph: Could you please add an explanation why there aren't any "dark" samples for February 22? Furthermore, what was the reason for changing the incubation time (1 vs 2 vs 4 days), and was there any notable effect?

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Response: We had some instrument/equipment failures while installing the bottles, so sometimes we were unable to use the full set of 10. We have explained this in the methods [approx. Line 55]. While annoying, this left us with spares with which to try longer incubation times, which was useful because we were finding that variability of the 1-day incubation results was high compared with the magnitudes of any changes. Longer experiments allowed potential for a stronger signal, but have the disadvantage that any methodological artefacts arising from the system being closed rather than open are also likely to increase during longer incubations. We do not have enough data to confidently assess whether or not the longer incubation times were helpful. An extra paragraph explaining the incubation times has been added to the methods.

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Reviewer 1: page 5, line 183: Figure 2a presents "ambient DO", not temperature and salinity as mentioned in the text. It would be useful to have this data included in the data file as well as in Figure 2.

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Response: The reference to Fig. 2a has been moved to the end of the paragraph (following the description of ambient DO). Ambient T and salinity data are now included in the archived data set.

Reviewer 1: page 6, line 191: please, add a reference to Figure 2c at the end of the sentence.

Response: This figure reference has been added.

- 40 **Reviewer 1: page 6, line 204: This should be Figure 2c (“dark”), not 2b, which shows results for the light incubations.**
Response: We have changed 2b to 2c.
- Reviewer 1: page 7, line 261: please, add some references at the end of the sentence “. . . in the Arctic.”**
Response: We have added a reference to Table 1 which summarises some previous studies in the Arctic.
- 45 **Reviewer 1: Table 1: Please, add the incubation time (or a respective range) for the results originating from the present study. They are provided for the other studies listed in the table.**
Response: Incubation times have been added in Table 1.
- 50 **Reviewer 1: Figure 1: The figure would clearly benefit from having some map coordinates. I can only guess that the black circle shown in Fig. 1a indicates the polar circle. What topography data set is used ?**
Response: We have added co-ordinates in Fig. 1b and revised the caption of Fig 1a to include the polar circle. The map does not show topography, but the source of the land outline has now been noted in the caption.
- 55 **Reviewer 2: I think this is a unique and essential dataset contributing immensely to the body of evidence on climate change issues. Extreme environmental conditions at the experiment sites were carefully mitigated to collect bias-free data. The only concern that I would have is that as a data paper, this manuscript should be free of any interpretation of the data. Such comments tend to suggest the author’s opinions and views to the readers. The authors should avoid such an explanation leaving just a general assessment as to the ways the data can be used, e.g., as a validation or calibration data set of remotely sensed observations.**
60 Response: Similar points were raised by Reviewer 2 and Reviewer 3. While the paper describes the data and results, with little interpretation because we do not have any supporting data with which to make any detailed interpretations, we have removed some interpretation from the Discussion section. Following these edits the Discussion section is now restricted to: (1) estimates of photosynthesis rates using differences between light and dark incubations, as this calculation is a standard
65 part of the light/dark incubation method; (2) a brief comparison between the incubation results and ambient dissolved oxygen results, as these two approaches show some important differences that highlight the advantage of using the two methods simultaneously; and (3) a short note on the potential influence of changes in under-ice irradiance. Where relevant we have retained a limited comparison of our results with previous studies, as this puts our results in the context of this previous work, but we have not suggested any reasons for the similarities/differences.
- 70 Finally, to reflect the above edits, we have changed the last part of the Abstract to: “Averaged over the full study period, dark incubations showed statistically significant decreases in DO of -0.36 ± 0.24 (near shore) and -0.09 ± 0.07 $\text{gO}_2 \text{ m}^{-3} \text{ d}^{-1}$ (fjord centre), indicating respiration rates that were 2-20 times greater than rates previously reported under sea ice in the Arctic. Meanwhile, a lack of significant evidence for photosynthesis suggests that the rate of photosynthesis – if it was occurring – was much lower than that of respiration. The data also show no significant evidence of a temporal trend in
75 metabolism rates over the study period; however, ambient sea water DO increased significantly at the fjord centre (0.023 ± 0.013 $\text{gO}_2 \text{ m}^{-3} \text{ d}^{-1}$), possibly attributable to processes not occurring in the incubations (such as sea ice algal photosynthesis). These data may improve our understanding of microbial activity in the fjord during winter, and its contribution to Arctic ecosystems under present and future conditions.”
- 80 **Reviewer 2: Below are some minor things I noticed while reading the manuscript.**

L13 and through the text: ‘±.’ I would not use this symbol in this context because it indicates a range. What you are trying to say is: ‘one standard deviation.’ And as you know, the SD is a positive root square of the variance. This note is just a cosmetic one. In some sciences, this is still acceptable.

85 Response: Presenting results as a mean \pm SD or as a mean \pm error is a widely used convention in this field, and for consistency (and clarity) we have followed this convention. We also note that there is a difference between mean \pm SD and mean \pm error; except where noted, we have used the latter format. Errors were calculated as described in Section 2.1.

Reviewer 2: L94. ‘shortly after the transition from polar night to spring conditions.’ How many days since the end of the polar night?

90 Response: We have added that the end of the polar night was on 21 January.

Reviewer 2: L151. ‘...the of the last...’

Response: this typo has been fixed.

95 **Reviewer 2: L209 ‘variance’ change to ‘standard deviation.’**

Response: This has been changed.

Reviewer 2: Add coordinates and the north arrow to the maps. Add the year to the caption of the graphs and tables.

Response: These have been added.

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Reviewer 3: I would suggest changing a title. In my opinion ‘marine productivity’ should be exchanged with e.g. “marine microbial respiration”, as the second term more clearly indicates what is actually provided and was directly measured contrary to ‘productivity’, which can only be indirectly retrieved from oxygen measurements and which in practice was not observed at all.

105 Response: We agree the title needs changing but instead have changed ‘marine productivity’ to ‘marine microbial activity’ as our experiments were designed to measure net microbial productivity rather than just respiration.

Reviewer 3: Abstract: I think the main finding “rate of photosynthesis – if it was occurring – must have been much smaller than that of respiration” should be mentioned in the Abstract.

110 Response: We have noted this in the abstract. This part of the abstract now reads: “Averaged over the full study period, dark incubations showed statistically significant decreases in DO of -0.36 ± 0.24 (near shore) and -0.09 ± 0.07 gO₂ m⁻³ d⁻¹ (fjord centre), indicating respiration rates that were 2-20 times greater than rates previously reported under sea ice in the Arctic. Meanwhile, a lack of significant evidence for photosynthesis suggests that the rate of photosynthesis – if it was occurring – was much lower than that of respiration.”

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Reviewer 3: Introduction: generally, it is well written, informative and well explaining the importance of the study. I think it could also refer to the work by Assmy et al. 2017 (Scientific Reports): Leads in Arctic pack ice enable early phytoplankton blooms below snowcovered sea ice.

Response: We have incorporated this study into the introduction.

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Reviewer 3: Please provide a reference to the statement “While estimates of marine net primary productivity (NPP) based on satellite retrievals of chlorophyll a have shown a link between reductions in sea ice cover and increases in NPP across much of the Arctic during 1998-2009”

125 Response: Brown and Arrigo (2012) used remote sensing to study the link between increasing NPP and reduced sea ice cover; this reference is included at the end of the above sentence.

Reviewer 3: Dataset: it is stated that up to 10 samples per experiment were analysed. Also in Pangea it is written that datasheet should contain 167 data points, however the database I had an access contain only 13 records, which I assume are already the means of the particular measurements. However, as the errors of the means are substantial, I do think it would be great to have a possibility to work on all the data (measurements).

130 Response: We will add results from individual bottles to the data archive on Pangea.

Reviewer 3: If the effect of the glacier was taken into consideration in the sampling design, ideally if data could be supplemented by e.g. turbidity levels. The same applies to providing the data that were measured for sure (temperature, ice thickness).

135 Response: There will have been very little runoff from the glacier in February/March, and the sampling design just compared near-shore, shallow water with much deeper water in the fjord centre. The effect of large meltwater inputs in the summer would of course be a very interesting future study as the ice sheet has been found to be exporting nutrients to the surrounding coastal waters (e.g., Hawkings et al., 2014 and Lawson et al., 2014 which we have cited in the paper).

140 **Reviewer 3: Results: I cannot find those results : “Throughout the study period, the sea water temperature and salinity varied between -1.5 and -1.7o C, and between 32.6 and 32.8 psu, respectively (Fig. 2a).”, neither at the Figure nor in the dataset.**

145 Response: The reference to Fig. 2a has been deleted as those results are not plotted. The temperature/salinity measurements are now in the archived data.

Reviewer 3: Why there is no ‘rate of change’ on Fig. 2c & 2d for the 21.02.2013 in the Fjord Edge station ?

150 Response: no dark bottles were used in that incubation. Therefore, there is no respiration value in Fig 2c and no corresponding photosynthesis value in Fig. 2d for that date.

Reviewer 3: According to the results description, which is as follows: “DO calculated for the light and for the dark bottles is interpreted as GPP. None were found to be significantly different from zero (Fig. 2d)” there was no production occurring ! That’s why I proposed to change the title.

155 Response: The title has been changed as noted above.

Reviewer 3: How is the result described in the Abstract “Averaged over the full study period, dark incubations showed statistically significant decreases in DO of -0.36 ± 0.24 (near shore) and -0.09 ± 0.07 gO₂ m⁻³ d⁻¹ (fjord centre), indicating respiration rates that were 2-20 times greater than rates previously reported under sea ice in the Arctic.” obtained? What kind of calculation stays behind ? Is that a difference between first and last sampling ?

160 Response: These values are calculated using the bottles from all incubations at the respective hole.

Reviewer 3: Conclusions: I am afraid some data interpretations may be too far reaching and cannot be supported by the dataset provided: e.g., “at Lillefjord, then it is likely that ice algal photosynthesis commenced earlier than photosynthesis in the underlying water column” “in this study, the difference is most likely attributable to net production by sea ice algae (which would increase DO in the ambient sea water), contrasting with net respiration in the underlying water 270 (which would decrease the DO in the closed incubations). This suggests an earlier onset of photosynthesis at the underside of the sea ice than in the underlying water.”

Response: Reviewer #2 also commented on the interpretations being too detailed. Therefore, we have reduced the level of interpretation as described in the corresponding response to Reviewer #2 above.