Answers to the reviewers

Dear Editor and reviewers, we first want to thank you for carefully evaluating the manuscript and giving us the opportunity to revise it accordingly. We carefully addressed each comment made by both reviewers and made substantial efforts to improve the general quality of the manuscript. You will find below our point-by-point responses to each of these comments. Please find attached a clean and also a tracking changes versions of the manuscript. Note that for some unknown reason related to the editing tool we are using, the tracked version does not contain the changes made in the abstract. We hope that this version will be satisfactory and thank you for your time in this matter.

Yours sincerely,

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Reviewer #1

This manuscript explores in depth the effects of different sampling strategies for the under ice irradiance and transmittance used to calculate primary productivity using photosynthetic parameters. The use of profiling platforms provide a clear advantage to irradiance point measurements and should be further used for upscaling primary productivity estimates. The manuscript in general reads well, the figures are clear and the authors did a good job analysing a large dataset.

Comment C1

However, the paper lacks a proper discussion about the role of the variability of the photosynthetic parameters themselves, which are influenced by other environmental parameters besides available irradiance. Detailed comments about how to improve the manuscript are available in the annotated version of the manuscript.

Answer A1

Our goal was to assess the impact of incident light variability on primary production estimates rather than the impact of variability in the photosynthetic parameters (which is a different question). For this, we used measured P vs. E curves, which in turn reflect the physiology arising from nutritional status, light history and species composition. We could have used parameters taken elsewhere to apply them to the Arctic (because the question is not about the variability in the parameters but in the light). This is why we decided to use the P vs. E measured in situ on the given day which in turn reflect all the different impacts that can influence them. The impact of parameter variability would only change a bit the distribution of errors (second order effect on changing the response to light) without altering profoundly the results within realistic ranges of values. Again because we are using the measured parameters, we are accounting for at least a part of this variability as measured during that cruise. We, however, added a new paragraph at the beginning of the

introduction to better present our objectives and briefly to discuss factors driving primary productivity and also to put our result in perspective with results from the literature.

Comment C2

Line 14: It should read:"spatially highly variable"

Answer A2

This has been corrected.

Comment C3

Line 21: None of these features is new to the Arctic icescape, so why is it increasingly complex? If this is supposed to be related to global warming and the increase of certain features vs others, these needs to be clarified. The first part of the introduction is written in a less misleading way for example.

Answer A3

This sentence has been reworked accordingly so it connects better to the first sentence of the introduction.

Comment C4

Using single point measurements of light or of carbon uptake or photosynthetic parameters? The photosynthetic parameters used in this study also come from single point measurements. Please clarify.

Answer A4

The measured photosynthetic parameters do not reflect local sea ice conditions (while light measurements do). Phytoplankton drift continuously relative to sea ice, so that what we measure reflects the light history of phytoplankton, not the light conditions right under the ice floe from which we collect seawater. We are now

precise that it was in reference to single location light measurements. We also reviewed the entire manuscript to make sure there was no confusion about the fact we are referring to light measurements and not carbon uptake. See also A1.

Comment C5

References to other studies using this kind of approach to calculate and upscale PP should be included here. (eg Fernandez-Mandez et al 2015)

Answer A5

The suggested reference has been added.

Comment C6

This study focuses on the impact of transmittance variability when calculating primary productivity, but it fails to point out the importance of the variability in point-measurements derived photosynthetic parameters, which are the key to obtain "more representative" primary production estimates. Photosynthetic parameters are not only governed by light but also by nutrients and there is no mention about this in this study. This limitations of the study to provide "better" or "more representative PP estimates" should at least be acknowledged and discussed.

Answer A6

This study is about how best measuring primary production of phytoplankton under sea ice. As explained in the introduction, PP derived from in situ incubation under some ice floe does not account for the variations in irradiance experienced by phytoplankton that drift under sea ice over a given period, e.g. 24 hours. The use of photosynthetic parameters to determine primary production is not a new idea (see for instance Rao and Platt et al. 1984; see also Morel et al. 1996 for a comparison with traditional in situ incubations). We think, however, that it is the best approach for phytoplankton under sea ice. Finally, note that the

photosynthetic parameters used in our study are those measured in situ. They do account for any effect of nutrient, light history and community composition. See also A1.

Comment C7

What snow data was used for the calculations at those stations instead?

Answer A7

The snow data was not used in statistical analysis per se. This data was only used to provide a general description of the sampling sites (Fig. 2). Hence, the missing data has no influence on the outcome of our analyses.

Comment C8

6 mL of headspace seems like a lot for this method since the 14C sodium bicarbonate can easily go into gaseous form. Also, did you estimate the number of algal cells per ml at the low biomass concentrations typical for Arctic waters..is this enough to have a reliable measurement?

Answer A8

Because of the carbonate system, there is about 2.4 micromol CO_2 per ml of seawater while there is about 0.018 micromol of CO_2 per ml of air at sea level. Therefore in the experiment with 1 ml of water and 6 ml of air the ratio of the volume of CO_2 in the air to that in the water is about 0.018*6/2.4 = 0.045%. Therefore assuming that the whole system became equilibrated within the 2 hours the underestimate of the photosynthesis would be at most ~5%. It is probably much less than this since the equilibrium takes time by diffusion across the interface.

The best way to assess whether the concentration of ^{14}C added is sufficient is to examine whether the PvsE curves are noisy (most easily observed particularly in the

Pmax region of the curve). The first curves we ran indeed had noisy measurements and were rejected, and we added more ^{14}C in the subsequent experiments to avoid this issue. There was no sign in the data that the latter curves with higher amounts of ^{14}C were more noisy than usual.

Comment C9

Are 2 hours enough for the algae to recover from the lag phase after sampling? It should also be specified that what is being measured with these short incubations is probably gross community production.

Answer A9

We are not aware of a 'post-sampling' lag phase. Lag phase generally refers to growth characteristics of microorganisms after the start of a new culture. As pointed out by the reviewer, our method likely provides values close to gross production given the relatively short incubation time. These short time incubation are common practice see for example Bouman et al. (2018). We have added a precision in the method section. See also A10.

Comment C10

gross or net?

Answer A10

Given the short incubation time, our method for deriving primary production most probably provides values close to gross production (Lewis et al. 1983). This precision has been added to the method section.

Comment C11

Usually photosynthetic parameters are biomass (Chla) normalised. How was biomass in the incubations taken into account in this study?

Answer A11

Indeed, photosynthetic parameters need to be normalized when they are used to assess the status of the photosynthetic apparatus in terms of photoacclimation, nutrient limitation, or when using them in models in which biomass is determined independently. In our case where we want to determine primary production only at the stations and at depths where the respective photosynthetic parameters were measured, there is no need to normalize the photosynthetic parameters.

Comment C12

Is this word repeated here?

Answer A12

This was deleted.

Comment C13

Since you are calculating rates per hour it should be productivity. Production is usually used for annual estimates.

Answer A13

We believe that there is no general agreement in the scientific community about such terminology distinction between primary production and primary productivity. See for instance Cullen (2001; Primary production methods, Encyclopedia of Ocean Sciences, volume 4, pp 2277–2284, Elsevier Ltd). The description of our method, and the units we used should make things clear enough. We are open to reconsider it if the editor judges it so.

Comment C14

Line 241: Please explain this better.

Answer A14

The text has been modified accordingly.

Comment C15

Which photosynthetic parameters were used for each profile? If you sampled 7 depths and you did 7 PE curves per profile, what was the variability of the PE parameters with depth? At which discrete depths did you use each parameter?

Answer A15

Primary productivity has been calculated over the first 40 m of the water column. Furthermore, photosynthetic parameters have been linearly interpolated between 0 and 40 m by 1 m increment. We are now providing this information in the text. See also A22 where we show vertical profiles of PAR. Additionally, our goal was to assess the impact of incident light variability on primary production estimates rather than the impact of variability in the photosynthetic parameters. We have made it clearer in the introduction.

Comment C16

Line 269: when measuring light or transmittance at a single point. The PE parameters come, also in this case from single point sampling.

Answer A16

These are two very different things. The light regime under a given ice floe reflects the local properties of this ice floe. The photosynthetic parameters we measure on phytoplankton we collect in the water column reflect (in terms of photoacclimation) the light history of the cells while drifting under the ice-pack.

Comment C17

Line 275: spot light measurements

Answer A17

We replaced it with single-location light measurements.

Comment C18

Line 278: It needs to be stated more clearly that representative values of PP are not only dependant on a good representation of the light field experienced by phytoplankton. *Nutrients, temperature and grazing are also important for representative values of PP. The approach of this paper is neat but only addresses one parameter influencing PP.*

Answer A18

We disagree with the last sentence of this comment because nutrient and temperature effects are implicit in the photosynthetic parameters we use, as we only use them for PP determination at the same station where they were determined. So, for instance, if low temperature depressed PP, this effect will be capture in photosynthetic parameters (mostly in Pmax). The reviewer is, however, correct that there are other factors that are important for primary production. We are now mentioning them in the first part paragraph of the discussion. These have been discussed at length in previous papers and many parameterizations have been proposed, the scope of our paper is to focus on the impact of incident irradiance in an ice region with variable transmittance. See also A1.

Comment C19

Line 288: again, spot measurements of light, not of 14C uptake

Answer A19

This was corrected.

Comment C20

Line 293: According to latest publication standards, scripts used for data analysis should be made available in a public repository. Please add the corresponding link.

Answer A20

The reviewer is right and it was the plan to make the Github repository public upon article acceptance. We have added a link to the repository in the article. The code will be public under the GPLv3 licence.

Comment C21

Line 313: was the sampling depth below the ice the same for both devices? Could this also be part of the explanation for the differences?

Answer A21

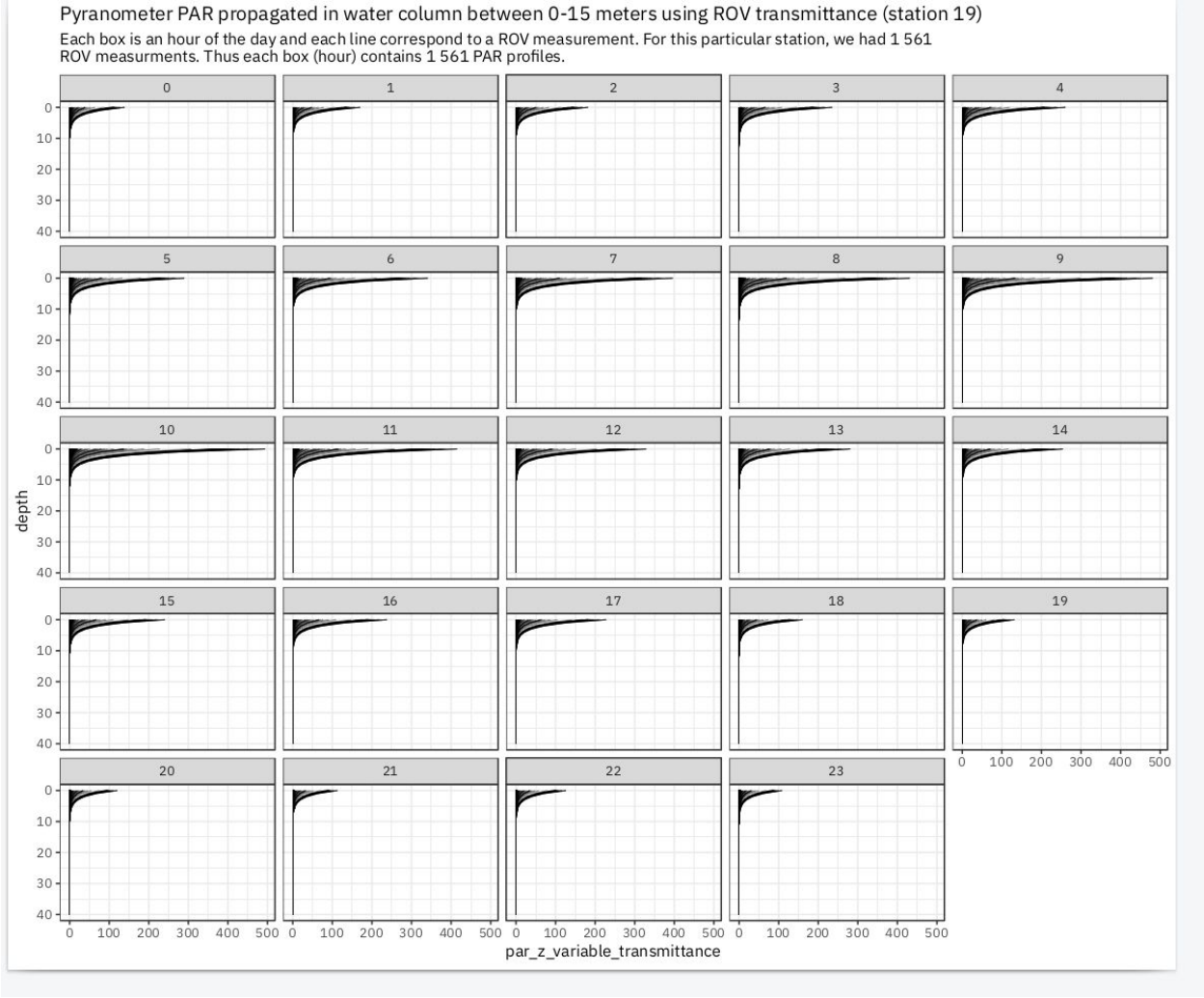
As indicated already in the text, the devices are operating both directly below the ice, or data are corrected respectively. Different sampling depth would result in a smoothing of the histograms (because irradiance contrasts get more diffused with increasing depth), as well as in a small shift of the modes. This is not the case.

Comment C22

Line 324: How do you define depth of the euphotic zone for the depth integration of PP?

Answer A22

This was addressed in A15. The graphs show that light was negligible below 40 m (see below).



Comment C23

Line 328: productivity

Answer A23

See A13.

Comment C24

Line 330: If the P mixing calculations include open waters while the P underice does not, how could it be that the maximum PP is under ice and not in the mixed calculation?

Answer A24

This is a good question. After verification, the calculations are correct. This is because, as stated in the text, a 10% transmittance threshold was used to filter out SUIT transmittance used in the mixing models thus reducing the value of calculated PP.

Comment C25

Line 350: Again in this paragraph it needs to be clear that the number of measurements/samples that you are referring to are light measurements and not photosynthetic parameters/carbon uptake. Also I miss the PE parameters results and assessment of their variability and error.

Answer A25

We are now explicit about the fact that the numerical simulations were simulating the effect of averaging light measurements performed at a number of random locations. See also A1.

Comment C26

Line 370: Why some references show 2, 3 or 4 names and others the regular Name et al? please check.

Answer A26

We are using the LaTeX template given by JGR. The bibliography is automatically formatted and not under our control.

Comment C27

Line 391: references?

Answer A27

We replaced “suggestions” with “hypothesis”. See also A46.

Comment C28

Line 397: How do these relative errors compared to the errors derived from the 14C method to measure carbon uptake and the curve fitting to obtain the photosynthetic parameters? What about the variability of photosynthetic parameters?

Answer A28

Addressed in A1.

Comment C29

Line 410: Is there no data from this study on the photosynthetic parameters that could be discussed here?

Answer A29

We have added a few sentences to discuss E_k from our result and how it compares with the literature.

Comment C30

Line 428: covering an area of how many meters?

Answer A30

This depends on the device. Basically, this analysis is giving the error that one would make when performing *n* random measurements distributed all over the area. As stated in the introduction, this is in the order of a few hundreds of meters for the ROV and a few kilometres for the SUIT.

Comment C31

Lines 442-444: There have been later studies (eg. Palmer et al 2011, Fernandez-Mendez et al 2015..) in which similar approaches to calculate PP have been used. I am missing a more thorough comparison with previous primary productivity data. Do your estimates fall in the range of previous published values for that area? What about the PE parameters? Despite the differences in 14C uptake methodologies, do they agree with previous published work? A part of the discussion dedicated to the variability of PE parameters is key and missing in this manuscript.

Answer A31

Addressed in A1.

Comment C32

Line 460: How can you be sure that your primary productivity estimates are more accurate? The light transmittance is definitely more accurate but there are many other parameters affecting primary productivity. As highlighted in Palmer et al 2011 and many other studies photosynthetic parameters can vary substantially spatially. If the temporal and spatial variability of photosynthetic parameters and phytoplankton biomass are not well resolved with more in situ measurements of carbon uptake and Chla, the primary productivity estimates can not be significantly improved, no matter how well resolved the light field below the ice is.

Answer A32

Your repeated comments in the line of this one show that we were not clear enough about the goal of our study. The question is how to best determine primary production by phytoplankton under sea ice, given that single-location incubations or deck incubations won't work because they do not account for realistic light conditions. As argued in previous studies, the use of PvsE curves combined with, at the exact same station, appropriate light measurements seem to be the best compromise. Using photosynthetic parameters measured elsewhere is not the approach we are describing here. With everything else being constant our paper show that accounting for variability in the light leads to more accurate estimates; they have the same limitations as all the other estimates of primary production with regards to the estimates of the photosynthetic parameters, but we avoided some of the pitfalls by measuring them in situ at the same time as the transmittance measurements. See also A1.

Reviewer #2

Review of the manuscript entitled "Sensitivity of phytoplankton primary production estimates to available irradiance under heterogeneous sea-ice conditions" submitted by Massicotte et al. for publication in a regular issue of JGR Oceans.

The manuscript is based on extensive fieldwork north of Svalbard, and comprises deployment of ROV and the SUIT for under ice optical measurements, and specifically PAR transmittance below a varying snow and sea ice cover. Primary production rates based on measured photosynthetic parameters and transmittance data from the ROV and the SUIT. Rates are then compared and evaluated, as of larger (SUIT) and smaller (ROV) spatial scales regarding the obtained transmittance data relative to the production rates. The manuscript is well organized, clearly written, and with good illustrations.

Comment C33

It's a specialized study requiring some in-depth knowledge about sea-ice, optics, and primary production and I'm not sure that the results of the study/recommendations will be applied in broader Arctic marine ecological studies.

Answer A33

More and more ecological studies conducted in the Arctic and Antarctic have work packages dedicated to better understand how the changing sea icescape is influencing ecosystem functioning. In the paragraph, just before the conclusion, we are now explicitly providing different steps that can be easily adopted during field camp campaigns to obtain the best possible estimates of primary production under spatially heterogeneous sea ice surface. We have also modified the last sections of both the introduction and the discussion to emphasis on the importance of the results of this study. It also provides clear guidance towards improving remote sensing estimates. See also A35.

Comment C34

I need a more clear purpose of the study and why this is important, and if the results of the study, as it appears, are important and significant.

Answer A34

We substantially modified the second paragraph of the introduction to describe why traditional methods for estimating primary production of phytoplankton under sea ice are not appropriate. We further added a section describing the general direction that should be implemented/adopted by new methods.

Comment C35

I would like to have a section on how to implement these results in future studies. The authors claim that "The results provide a new guidance on how to..." in line 97.

Answer A35

The reviewer is right. We are now explicit about the different strategies that should be adopted to obtain the best possible estimates of primary production under spatially heterogeneous sea ice surface. See the paragraph just before the conclusions. See also A33.

Comment C36

It also appears that the study is kind of "closed around itself" as without mentioning actual measured primary production rates from the area. I'm further not fully convinced that primary productions rates can be derived based on transmittance and measured photosynthetic parameters. Primary production also depends on biomass, and where these measured at stations? At least for the sake of the argument that there were no differences in biomass, i.e. Chl-a between stations. This also applies for nutrient concentrations.

Answer A36

To answer the first part of this comment, it is important to note that this approach was introduced in the 80's by people such as Trevor Platt, and has from thereafter been often used. In situ incubations remain the standard in open waters for PP, but are not appropriate for ice-covered areas. For the second portion of the comment, we are now comparing our results with other studies which also used the same method (i.e. from photosynthetic parameters). We found out that our estimations of primary production were falling within the range of observed values. We also now acknowledge that there are other factors such as biomass, temperature and nutrients that drive primary production and argue why we believe that combining available light and photosynthetic parameters represent an efficient approach to estimate primary productivity. Please note that our photosynthetic parameters are not normalized to biomass (chlorophyll) so when primary production is computed for the location of interest no biomass is necessary. Biomass-specific photosynthetic parameters are used when parameterizing their variability in view of future or larger spatial/temporal model, which was not the goal here. Please see the new paragraph at the beginning of the discussion and also A1.

Comment C37

Further, with all this equipment and instruments, I'm surprised that they did not install a simple PAR sensor with a logger on the ship to avoid the conversion from energy to photons?

Answer A37

We agree that the use of a PAR sensor would have been beneficial. However, no PAR sensor was available and the surface RAMSES measurements only covered the times of SUIT and ROV measurements. For this reason, we used the CM 11 pyranometer which provided a complete time series of incident irradiance data.

Comment C38

83. Is this relevant? I don't see the method applied in the present study?

Answer A38

We think that these three lines provide useful information for the reader because they can help in the planning of other fieldwork. The reason we did not apply the method of Massicotte 2018 is simply because upward radiance was not measured by either the ROV and the SUIT. Otherwise, we would have used it.

Comment C39

94. I miss some more clear hypothesis and or purposes.

Answer A39

The reviewer is right. We have added one sentence at the end of the last paragraph of the introduction to present the main objective of the paper.

Comment C40

182. Floats? At what distance from the ice?

Answer A40

The word “floats” has been replaced with “buoyancy blocks”. There was no distance to the ice as the net rides directly along the underside of the sea ice. The next sentence in the text states its vertical position precisely: “*that keep it at the surface in open water or in contact with the sea ice*”.

Comment C41

185-192. As mentioned already why not install a PAR sensor with logger on the ship??

Answer A41

This would have been indeed beneficial but was not available on site.

See answer A37.

Comment C42

200. What photosynthetic parameters? Be specific.

Answer A42

All photosynthetic parameters are described in the next section. We are now referring the reader to the appropriate section that contains all the details.

Comment C43

208. was

Answer A43

This was corrected.

Comment C44

310. 0.001%? With a surface PAR of 500 this gives an under ice PAR of 0.005. Is it realistic to measure such low PAR values with the RAMSES sensors?

Answer A44

Yes, this is consistent with the noise level specification of the manufacturer.

Comment C45

323. Table 1. I was not able to find it in the manuscript?

Answer A45

We are not sure what happened. We provided a single PDF file containing the three tables presented in the manuscript. We will double check when submitting the corrections.

Comment C46

391. What or whose suggestions? Be more specific.

Answer A46

We meant “hypothesis” rather than ”suggestions”. The sentence has been reworded to better reflect our thoughts.

Comment C47

407. sea surface? Must be sea ice surface?

Answer A47

The reviewer is right about the sentence not being clear. We have modified the sentence to specify that *the physiological state of the phytoplankton community* ***under the sea ice surface***...

Comment C48

418. Redundant information. Be more specific.

Answer A48

The redundant sentence has been removed.

Comment C49

440. wasoften was often

Answer A49

This was corrected.

Comment C50

451-452. sea surfaces Sea ice surfaces?

Answer A50

This was changed to sea *ice* surfaces.