Interactive comment on os-2017-21, Rev#1

We thank the anonymous reviewer of his/her comment on our manuscript. We have carefully considered all suggestions. In particular, we have toned down the phytoplankton characterization which was criticized by both reviewers. Where requested, we have further elaborated our arguments with statistics to reinforce our interpretations.

We also apologize to the reviewer as some sentences looked odd due to the poor proofreading after that several people have worked on the same document with track change. These broken sentences were fixed and the manuscript has this time been proofread by a native English speaker.

REV#1

Carbon geochemistry of plankton-dominated supra-1 micron samples in the Laptev and East Siberian shelves: contrasts in suspended particle composition.

General comments

This study want to improve the understanding of the chemical composition of plankton that dominates regions of the Arctic Ocean characterized by different sea-ice coverages and and in the ice-free Laptev Sea. The authors conclude that terrestrial carbon influence the POM in the Laptev Sea with higher influence of the River Lena. In the East Siberian Sea with ice-cover the influence of land is smaller. This is a valuable study in an important part of the ocean with a paucity of observations.

The methodology for 13C and 14C analyses seem adequate. However, this is not the case for the estimates of plankton diversity based on qualitative data from scanning electron microscopy. Either the authors have to convince us that this method is adequate and provide quantitative data analyzed by proper statistical tests. Otherwise markedly constrain conclusions regarding phytoplankton diversity or remove this entirely, perhaps using references to other studies instead.

Also the 13C and 14C analyses consistently lack objective statistical tests to support the conclusions made. Even if this data in general looks convincing this needs to be added.

The text is quite OK but there are some sentences which are not currently understandable. It does not appear that the last version has been checked by an English speaking person, which is recommended.

Provided that these remarks and important specific comments below are remedied I recommend that the manuscript is published after a major revision.

Specific comments

1) Title: Please revise the title replacing the "supra1-micron samples" term with "POM".

We have replaced the term *supra micron* with "particulate organic matter (>10μm)" throughout the text

2) r.28-36 I suggest to make the introductory paragraph shorter motivating the addressed question and the overall design of the study.

We have shortened the abstract as requested.

r.31-32 Unclear why "supra-" is used at this stage. Not a common term in my mind. Please just state "..the larger than 10 µm particulate organic matter (POM) fraction…" in the abstract. It´s clear from your definition what fraction that is covering.

Changed throughout the text consistent with the title (see above)

r. 37-41 These conclusions need to be better supported by statistical tests as specified in the result and discussion section.

We ran a two-tailed T-test (homoscedasticity) and a Welch T-test (heteroskedasticity) to assess whether the differences between Laptev Sea and East Siberian Sea were statistically significant (see Methods). The text was changed accordingly when discussing differences between regions (e.g. line 324, 326, 353, 360).

r. 42 "..communities via microorganisms". There are not reason to indicate several "loops". Please write concise and avoid unnecessary terms for clarity.

The term "loops" was removed according to the reviewer's suggestion

r. 44 The methodology does not seem adequate to assess the changes in diversity. Se comment in the result and discussion section. Also unclear what you mean by "…which is confirmed". Please rephrase.

We agree with this comment. We toned down the part that deals with the characterization of algal assemblages. This is no longer part of the abstract and conclusions. However, we would like to leave Fig. 5 and some brief discussion in the text just to provide qualitative information.

r. 45-46 Unclear what is meant by "…follows the general growth vs CO2aq supply model…". Please present in a clearer way.

Rephrased to make it clearer (line 49-52).

r-48-50. What basis is this prediction based on? Please add a specification.

The destabilization of the Arctic cryosphere encompasses several aspects including sea-ice, glaciers and permafrost soil. Specific references were provided in the Introduction (line 63-84) which now contains even more details about the study region. Here in the abstract we can only report a summary of the future trajectories in response to the climate change.

r. 65 What uptake are you referring to? Please rephrase and clarify.

It refers to the CO2 uptake. Sentence rephrased (line 74).

r. 67 "…also project to water-…" Does not seem like proper English. Not understandable. Please rephrase.

The sentence was fixed (line 77).

r. 92-93 if the term supra- is not earlier defined and internationally agreed upon I see no reason to use it here. Just use POM and define the size fraction studies for simplicity.

We have replaced the term supra-micron fraction with POM throughout the text as suggested

r. 96. "..characterized by bulk…" If the MS has not been language checked by English speaking persons or dedicated companies please do so.

Again, this was one of those broken sentences resulting from poor text proof reading after several co-authors worked with track changes (line 107). All these odd sentences were corrected

r. 92-101. This paragraph should be moved to the methods section.

This is just a brief overview which we would like to keep in the Introduction. The Method section provides further and more specific details on the methods used.

r. 106 Please start with presenting the studied Sea areas and their characteristics. Please consider to use a map with sampling sites.

The map with cruise track and sampling site is already provided in the manuscript (Fig. 1). We added a section after the introduction to introduce the major features of the study region (section 2, line 114-129).

r. 112 Pleas add how the Falcon tubes were cleaned.

The method section was updated to include the cleaning procedure of the Falcon tubes (line 142-143).

r. 119 How long time after sampling was the analysis? Was samples frozen all the time to analysis?

The samples were kept frozen until lab analyses (line 141).

r. 166 How was samples taken and preserved? Were they concentrated in some way? Please add. Is there any reason why not other autotrophs like flagellates, picoplankton and cyanobacteria were included?

Samples were collected via large volume filtration of a nylon mesh. This was already well described in the text. We added further details about the samples collections (139-142). The 10um cut-off was chosen to avoid collecting fine terrigenous material in suspension.

r. 170 What was the number of cells counted and precision of counting per sample?

As we said this is a qualitative method which provides a snapshot of the dominant assemblages. It is not meant to be a statistical analysis. Thus, we agree with the previous comment and we have toned down this part.

r. 184 Please add the accuracy and precision of the measurements of CO2 and δ13 CCO2.

We added the precision for both CO2 and δ13C-CO2. However, this section was moved to the supplementary material.

r. 229 Please present (for all variables) some confidence intervals or test, validating what are statistically significant differences between stations (i.e. accounting for spatial and short term temporal variability). E.g. if you want to claim differences between sea areas show by a proper statistical methods that they are different from each other.

We used the T-test between ice-free and ice-dominated regions while Pearson correlation was used to investigate the relationship among variables (see Methods section 3.5). However, we mainly focused on the new data presented in this study (dual-carbon isotope and biomarker data). The rest of the data regarding the surface water properties were elaborated, discussed and interpreted in other studies (Humborg et al., 2017; Salvado et al., 2016).

r. 236 The data referred to in Humborg et al. need either to be published before accepting this paper, or data presented in this paper.

Humborg et al has been just accepted in *Global Biogeochemical Cycles DOI: 10.1002/2017GB005656*

r. 243 How do you define depletion? Please be more specific and refer to comparative data or references. Similar for "low" at r. 245.

This sentence was modified according to this comment (line 286-289).

r. 255 Please specify what "margin" that is referred to. This sentence is not possible to understand. Please rephrase.

We have replaced "margin" with "continental margin" for clarity (line 300).

r. 259 It´s not obvious how the concentration of lignin or hydroxyl fatty acids will say anything about effects on the POM fraction. Do you mean the conc. of these compounds in the POM? What about many other effects on living POM like species composition and functionality like growth or edibility? Do assume that most POM is non-living? Please motivate you analyses better relative the aim.

As stated in the text, lignin phenols and cutin acids are uniquely produced by terrestrial vegetation. Therefore, these analyses were carried out to assess whether or not the samples were affected by land-derived material (i) directly supplied by rivers, (ii) trapped in the sea-ice and (iii) resuspended from the sediment. The concentrations of lignin phenols are close to the detection limit while cutin were not detectable. This implies that the material collected is primarily autochthonous marine POM.

Fig.3 Please add what error bars are showing. Please specify what the values are relative against (carbon or mass?).

This figure shows the CuO oxidation fingerprint of the samples. Upon Cuo oxidation organic matter releases different biomarkers whose composition provide information about the source. The percentage refers to the total CuO oxidation yields and the error bar reflects that natural variability observed in different carbon pools. For example, soils are rich in lignin phenols and cutin acids while phytoplankton batch cultures upon CuO oxidation don't yield these terrestrial biomarkers. By contrast, they produce a large amount of low molecular weight fatty acids consistent with the POM collected in this study. We added further details in the caption of Fig.3.

Samples

r. 276 Support you statement with a tests showing that these are different. What do a base the "high" and "low" assessments on (relative what)?

As previously mentioned, we used a T-test/Welch T-test to show that these differences are statistically significant ($p<0.01$) (line 324)

r. 275-287 SEM is not a proper method to asses diversity of phytoplankton (or present quantitative SEM data from sufficient number of samples?). That the diversity of phytoplankton is different between sea areas is therefore not sufficiently well demonstrated. Concentrations of at least major taxonomic groups is requested based on microscopy counting with adequate methodology (e.g. sediment chambers and reverse light microscopy) and statistical precision presented. Preferably also including flagellates, picoeucaryotes and cyanobacteria. Established diversity index should be used and tested for difference.

As previously mentioned, we have used this method to provide a general overview of the dominant taxa. We agree with the reviewer on this comment and we are aware that this procedure has only qualitative applications. We would still like to keep this part but make clear in the text that this is only qualitative information. Any reference in the abstract and conclusions about the phytoplankton taxa were removed from the revised text.

r- 288-289 Unclear how a line can detect a bloom. Please be more specific.

The sentence was corrected (line 336).

r.293-294 I don´t agree that the presented data convincingly show that dinoflagellates were dominating. How is the SEM preparation influencing different phytoplankton species? Is there a selections for robust dinoflagellate shells? Provide a reference or control experiment clarifying this.

There is no reason to think of selective preservation for different taxa during sampling and microscope analysis as all the samples have been treated in the same way. Despite the fact that a taxa quantification would provide much better statistical evidence, these results would be consistent with the qualitative investigation done here, in particular the large difference between LS (rare diatoms) and ESS (diatom dominated). Again, we took several SEM images for each sample which are absolutely consistent with what observed with the optical microscope.

r. 297-301 As presented isn´t IP25 then specifically indicating presence of sea-ice diatoms, not "..sampling of different plankton taxa…? Also can other sources of IP25 have contributed to the variability. Consider a re-interpretation. I suggest to calculate if the found conc. of IP25 could come from an expected conc. of diatoms in the sea ice and present that.

IP25 detects specific sea ice diatoms (Pleurosigma stuxbergii var. rhomboide, Haslea crucigeroides (and/or Haslea spicula) and Haslea kjellmanii, Brown et al., 2014b) which account for only a minor fraction of the sea-ice taxa. No other sources (e.g. from land) supply IP25 expect from these sea-ice taxa. To the best of our knowledge, the actual end-member doesn't exist and it likely varies depending on the concentration of these aforementioned species in the sea-ice

r. 309-310 Please provide a statistical test showing a significant trend of CO2 and δ13 CCO2.

p values were added in both linear correlations (Fig.7).

r. 316-317. On what basis is it assumed that the present dinoflagellates are hetero- or mixotrophic? That some dinoflagellates can eat bacteria is well shown in the literature. However, not that they are significant consumers of diatoms? Please provide a reference for this if so.

Our hypothesis is based on the dual-carbon isotope fingerprint. Specifically, out of the different carbon source known in the region (Fig.6), the dual-carbon isotope signature of the POM is consistent only with the Lena river DOC (and rather different from the Lena river POM). Thus, in our hypothesis, dinoflagellates feed on bacteria that develop in the terrestrial DOC-rich plume of the Lena river.

r. 324-325 I suggest to rephrase to "…, supporting the importance of terrestrial DOC as a carbon source for the food web in the river plume….).

Text changed according to this suggestion (line 399-400).