Interactive comment on os-2017-21, Rev#2

We thank the anonymous referee for his/her useful and constructive comments and feedback, which have been helpful in improving our manuscript. In particular, we have toned down the phytoplankton identification and characterization. However, as far as we can gather from his/her comments, the reviewer is not fully familiar with some of the geochemical methods presented in the paper. This has resulted in criticisms and we have elaborated those sections to better articulate the scope and results of our study.

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.

General comments

The objective of this study was to investigate the composition of the suspended particulate organic matter in ice-covered and ice-free waters over the Laptev and East Siberian shelves. The main problem of this study is to assume that these samples are plankton-dominated, as indicated by the title. There are no data to support the fact that phytoplankton dominated the suspended particulate matter and such a dominance would actually be quite surprising over the shallow Siberian shelves (a lot of particulate material is resuspended and/or transported with the ice). An effort should be made to quantify the phytoplankton contribution and composition before resubmitting this manuscript. If the authors somehow collected ice samples during this expedition, it would be relevant to compare the composition of the particulate matter in the ice with the composition of the suspended matter.

Another important problem is that too much of the current manuscript is based on another paper submitted elsewhere by many of the same authors that seems to be very similar to the current manuscript. This problem must be addressed. Overall, while the study had the potential to provide interesting results from a very rarely sampled region, the current results do not bring very interesting or new information. It is well-known that ice covered regions are productive and display high concentrations of particulate matter. The interpretation of the results must be reevaluated in this context. Also, please keep in mind and specify throughout the manuscript that these are late summer observations and that conditions may be quite different during the productive spring period. Finally, the manuscript is too long, often repetitive, and the text needs to be revised by a native English speaker.

To the best of our acknowledge, this is the first study that have characterized the dual-carbon isotope fingerprint of plankton-rich samples in this region. Knowing the isotopic composition of plankton is relevant for several applications which were listed in the text. For instance, source apportionment models in the study area have historically relayed on data collected in other Arctic regions. This is why we think the results obtained are worth and important for the community working on the carbon cycling in this area. The timing of the phytoplankton blooming is well discussed in the text and results were interpreted accordingly

Specific comments

Title

I have never heard the terms supra-micron or supra-POM and I don't think there is a need for it. Please remove the term supra- throughout the manuscript.

As mentioned for the other reviewer, we have replaced the *"supra-micron POM"* term with POM (>10µm)

Abstract

Lines 50-51: Comments like these are not informative. Always be specific.

We followed the suggestion. The paragraph ends with specific details about changes in the study region (line 53-57)

Introduction

Lines 54-55: Provide more recent references.

We have updated the references with recent studies dealing with the sea-ice retreat in the Arctic Ocean (line 64)

Material and methods

Some information on the dates of sampling are required.

Lines 110-113: Several steps are unclear. When it is mentioned that particulate material was kept frozen, it means the filters? In which state were the samples transferred into the centrifuge? Were the samples thawed first?

This part was edited to make it clearer (line 137-141)

Lines 115-116: Such information belongs in figure captions.

Removed

Lines 150 and 154: IP25 is a highly branched isoprenoid mono-unsaturated alkene. Introduce it properly and only once.

Text changed accordingly (line 180-184).

Section 2.3 Microscopic images of plankton This is probably the biggest shortcoming of the study. It is baffling that the authors use microscopic images as a qualitative tool but did not include a quantification of the different phytoplankton groups. This would definitely improve the quality of the study. *Always precise if it is phytoplankton or zooplankton. Plankton is not a term precise enough.*

Of course, we agree that quantitative information would have been more informative. Ours is essentially a biogeochemical study and these snapshots obtained via SEM and transmitted light microscope, provide only complementary information. For this study, we took several SEM images (in the paper we show just one magnified example) about 10 per station randomly sampled in the freeze-dried material. This, combined with traditional microscope analyses, allowed us to see major trends within the dataset, for example we could hardly see any diatoms in the Laptev Sea while further east diatoms were dominant. Thus, despite the limitation of our approach (which we acknowledge) we still believe that this is relevant information. We decided to tone down this part in acknowledgement to the reviewer's comment but still keep it in the discussion. In the revised manuscript, we removed any comment on phytoplankton taxa in both abstract and conclusion. However, we would still like to keep this part in the discussion making sure that the reader understands the qualitative applications of our approach

The material and methods section is too long and often repetitive. Reduce.

We went through this section but couldn't find any redundant information. However, methods on CO2 measurements were moved to the supplementary materials

Results/discussion

Section 3.1. Surface water conditions Most of this section does not belong in the paper. All the results (salinity, temperature, nutrients...) for which material and methods were not presented in the precedent section must be removed from the manuscript and the figures/tables as well. This is even more crucial considering that the same results are part of another submitted paper from the same authors. It is not appropriate to submit the same results twice and all the results that were submitted in Humborg et al. must be removed. Instead the authors should refer to these results in the discussion, which would be much stronger if or once the other paper is published. It would be more appropriate to start the discussion with section 3.2.

Humborg et al has been accepted in *Global Biogeochemical Cycles (and updated in our ref list)*. Following the reviewer's suggestion, the discussion now starts with section 4.1 and the former 3.1 section dealing with the surface water properties has become section 3 ("*Surface water conditions during the SWERUS-C3 expedition*)

Lines 220-222: You should never write sentences in this form: Figure 1 displays...

This sentence was changed according to the reviewer comment (line 262)

"Table 1 reports..." This is the type of mistakes made at the undergraduate level.

Specifically for the above comment, please refer to point 3 of the "General obligations for referees" document on the *Ocean Science* website. Thanks

Line 225 and others: All maps (figures 1, 2 and 4) should be switched with North towards the top to help with the description of the results. This is the usual and correct way to place a map and it is less confusing when looking for the westernmost stations.

We must disagree on this point. In this special issue, there are at least 8 manuscripts (Miller et al., Anderson et al., o'Regan et al, Björn et al, etc) with the exact same polar projection. In fact, this is the default ESRI Arcgis projection consistent with the IBCAO format (Jakobsson et al., 2012.GRL). We will keep this format

Lines 230-232: DOC concentrations mirrored... 'Mirrored' does not mean the opposite, it means similar. Get an English speaker to review your paper. And please limit your use of the word 'thus'.

Of course, we did not mean similar in terms of absolute values. We were just commenting on the general spatial distribution as DOC is clearly affected by the river plume: low salinities are associated with high DOC concentrations. Sentence reformulated (line 273)

Line 252: It is late and unnecessary to introduce the term TerrOC at this point. Either you introduce it earlier or you use other terms for consistency.

TerrOC was removed and replaced with "terrestrial organic carbon"

Lines 284-287: These phytoplankton species are typically observed late in the season. This should be specified. Chaetoceros and Thalassiosira are pelagic species growing in water only while Fragilariopsis cylindrus and oceanica grow both in ice and water (they are not sea ice species necessarily). More information could be obtained through extensive and quantitative taxonomic analyses of the existing samples.

Again, we agree with the reviewer that this minor part of our ms has severe limitations but we believe it still provides some useful general information on the dominant trends within the study region (see previous comments). For example, we can clearly see a marked difference between the Laptev Sea (no diatoms with abundant dinoflagellates) and the East Siberian Sea (dominated by diatoms) regardless of the method used (SEM vs traditional microscope). Again, our analysis is based on several SEM images coupled with optical microscope slides

Lines 304-306: ... captured the signal of the sea-ice retreat that occurred shortly before... Sea ice retreat actually took place weeks and months before so it is not appropriate to say shortly before. The fact that IP25 was still detectable would be more likely the result of advection or resuspension.

Corrected according to this suggestion. Resuspension would bring lignin and other wax lipids (cutins) which, however, were not detected in the POM samples. Advection from surface waters might be a reasonable hypothesis though. Text changed accordingly (line 355-356)

Lines 378-380: However, it would then remain elusive why such an aged land-derived influence was not visible in the river-dominated LS waters while it affected the sea-ice dominated region. Is it that elusive? It is puzzling that the authors did not consider that the presence of this land-derived material is likely the result of the release of material that was trapped in the ice during its formation on the shallow shelf. The trapped material is transported towards the outer shelves and released during ice melt, which was occurring at the time of sampling. This is an important and well-known process on the Siberian shelves. The interpretation must be improved to consider these ice- released particles. *How old? Be more specific.*

It's not puzzling at all as we specifically used organic biomarkers to trace the land-derived material. Figure 2 shows what type of CuO oxidation fingerprint you would get in a case of land-derived influence. For example, lignin phenols are clearly dominant in terrestrial soil samples while they are close to detection limit in our POM samples. Cutin derived products (waxes on plant leaves) were not even detected in the POM samples. As stated in the text, lignin phenols and cutins are exclusively produced on land and their negligible concentration in POM samples implies insignificant terrestrial influence. In other words, the samples are dominated by marine material. In fact, POM samples are consistent with the CuO oxidation fingerprint of algal batch cultures which mainly yield fatty acids, dicarboxylic acids, p-hydroxy phenols and benzoic acids (in this order of abundance) (Goni and Hedges, 1995)

Through this comment, we have realized that we did not mention the particulate transport by fast ice in the text. To provide a better picture regarding the sediment transport in the study region, we added a sentence about this mechanism (line 303)

Lines 394-396: Hence, results suggest a heterotrophic environment in the outer LS open waters where the river-derived DOC is transferred to relatively higher trophic levels via microbial incorporation (i.e, microbial loop). This sentence reflects a poor comprehension of the food web. Energy is not transferred to higher trophic levels through the microbial loop.

Here we refer to the terrestrial carbon being transferred from the DOC to the heterotrophic community via bacteria present in the Lena river plume. According to the radiocarbon signature (Fig. 6) of the Laptev samples, we infer that the DOC from the Lena is taken up by the microbial communities on which other heterotrophic communities (e.g. *Protoperidinium* spp;) feed on. Despite the fact that the samples have been collected in a region affected by the Lena plume (see salinity and DOC data, Fig 2), our modern 14C signature of POM largely differs from the particulate material supplied by the Lena river characterized by a 14C depleted signature (Fig. 6). In contrast, the POM signature seems to be more consistent with the Lena DOC fingerprint (Fig. 6). This would also explain the depleted stable carbon isotope composition despite the negligible terrestrial influence (biomarker results). It's also worth mentioning that DOC in the outer Laptev Sea is over one/two order of magnitude higher than the POC (Humborg et al., 2017; Salvado et al 2016)

Table 1 What is TN? Mean sea ice percentage is over which area?

TN is total nitrogen. Data are not discussed so they were removed in the new version

Table 2 This table does not belong in this manuscript.

As previously mentioned, these data are presented only with the intention of contextualizing our results. The discussion has been rearranged following the reviewer's suggestion. We now start the discussion from the new organic geochemistry data. Showing complementary data is a common procedure when studies are part of a multidisciplinary expedition during which research teams measured different parameters. Humborg et al has been recently accepted and properly cited in the manuscript

Table 3 This qualitative analysis is nearly useless. The authors should definitely invest in quantitative taxonomic analyses to support their results.

See comments above

Fig. 1 Switch North up.

See comments above

Fig. 2 Should be removed, presented in other submitted paper.

See comments above

Fig. 4 Patterns are often not as clear as described by the authors in the results/discussion.

Be careful when interpreting.

As just mentioned above, we have divided the study area into two sub-regions (Laptev Sea_open waters and East Siberian Sea-ice-dominated) and carried out a T-test as suggested by reviewer#1 to show whether or not the differences are statistically significant

Fig. 6 The new results should also be presented as whisker boxes for consistency. In the caption: East Siberian Sea, not Eastern Siberian Sea.

Caption corrected. The whisker plots presented here are used to summarize large dataset. For example the ICD end-member is made of 301 radiocarbon data. However, we don't think we can do the same for the POM samples in each considering the limited number of radiocarbon values

Fig. 7 Why only for East Siberian Sea?

Because only the East Siberian Sea is an autotrophic system where CO2 is actually consumed by biological activity (i.e., depletion compared to the atmospheric value) (Humborg et al., 2017)