Review Tommaso et al. os-2017-21

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 ^{13}C and ^{14}C 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 ^{13}C and ^{14}C 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

Title: Please revise the title replacing the “supra1-micron samples” term with “POM”.

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

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.

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

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

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.

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

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

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

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

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.

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

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

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

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

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

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?

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

r. 184 Please add the accuracy and precision of the measurements of CO_2 and $\delta^{13} C_{CO2}$.

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.

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.

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

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

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.

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

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)?

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.

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

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.

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.

r. 309-310 Please provide a statistical test showing a significant trend of CO_2 and $\delta^{13} C_{CO2}$.

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.

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….).

Fig. 6 Pleas consider different symbols for the LS and ESS data for the observed POM (be consistent, POC used here?) $\Delta^{14}C$, instead of dashed circles. The presentation of sources and references can be moved to the methods section.

r. 341 Pleas provide a reference for opposite directions of $\delta^{13} C_{CO2}$ or make clear that this is a hypothesis.

Fig. 7 Be consistent with the use of POM (legend text) and POC (x-axis title).

r. 360-361 As stated above the presented SEM data does not properly account for phytoplankton diversity. Convincing tests and reference values showing that diversity is low is lacking.

r. 381-383 As you have not presented measures of heterotrophic or autotrophic (e.g. primary production) the sentence should be rephrased. Your $\delta^{13} C_{CO2}$ and other data rather show that trophic balance vary between the Sea area studied indicating larger terrestrial influence and importance of heterotrophic activity closer to river discharge, than under ice-cover (i.e. ESS)? I suggest to not directly refer to primary production (first time mentioned in the MS?) as no such direct analyses has been done.

r. 390-391 Something wrong with the end of the sentence. Please correct.

r. 393-394 As motivated above I don´t think it´s demonstrated convincingly that dinoflagellates dominate the phytoplankton community. Provide better quantitative date on diversity or remove the conclusion.

r.396 Please delete the parenthesis. It´s not correct to use the “microbial loop” concept as this primarily was used to imply a sink for carbon. In the same sentence you rather state that it’s a link to higher trophic levels. The microorganisms is an integrated part of the marine food web globally, and in fact the original part of the food web in an evolutionary perspective. I have since long avoided the use of the term “microbial loop”.

r. 397-404 As commented above you should provide statistical test for these conclusions. Either show that two sea areas are different from each other using stations as replicates, or show a significant trend (systematic pattern) in the whole transect supporting your conclusions.

Technical comments

To simplify for the reviewer and save time I prefer not to make further categorization of the comments, which may anyway be a matter of subjectivity. They are found above.