

Biogeosciences Discuss., author comment AC2
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Reply on RC2

Muhammed Fatih Sert et al.

Author comment on "Compositions of dissolved organic matter in the ice-covered waters above the Aurora hydrothermal vent system, Gakkel Ridge, Arctic Ocean" by Muhammed Fatih Sert et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-350-AC2>, 2022

We are grateful for the comments and edits of the anonymous reviewer who invest time for the revision of our manuscript. Our responses were given in the next column for each comment/edit and corresponding changes were applied in the revised manuscript accordingly. Since the line numbers are updated in the revised manuscript, new line numbers are given in the responses.

Anonymous reviewer 2

Authors response

Line 117: Is the GF/F filter pre-combusted?

Line 120: Yes, filters were pre-combusted. This was forgotten to mention in the text and is now fixed in reviewed version.

Line 118: Are the HDPE bottles acid-washed before use?

Line 122: No, HPDE bottles are not acid-washed but rinsed a couple of times with the sample and a brand-new bottle used every occasion. We have previously observed that there is no contamination added from the HPDE flasks for DOC and nutrient measurements

Line 124: 'the cartridges were then eluted into pre-combusted, amber glass vials with 2 ml methanol', previous studies have

Line 127: Volume for the elution was updated from Dittmar et al. in our experiments. We observed that using < 2 mL

generally used >6ml of methanol to elute DOM from the PPL-cartridges (e.g., Dittmar et al., 2008). So, I wonder if the DOM on the PPL could be completely eluted by 2 ml of methanol? MeOH does not elevate the extraction efficiency. Yet, using 2 mL volume enables us to use the eluted samples without additional evaporation and resuspension steps and decrease contamination potential.

Line 367: 'the $\delta^{13}\text{CH}_4$ value of the hydrothermal fluid source to be about -36 ‰', the estimate of fluid endmember should include the error derived from the curve fitting. And, the error needs to be considered in the discussion. Line 371: The text is updated as follows to include intercept uncertainty and R^2 values: "...we estimate the $\delta^{13}\text{CH}_4$ value of the hydrothermal fluid source to be -36 ‰ using a least squares linear regression of the entire plume methane dataset ($R^2 = 0.59$), with an intercept uncertainty of $\pm 3\text{‰}$ at the Aurora hydrothermal field...".

Line 398 and Figure 6b: What does 'average abundance' mean? The signal intensity of each formula? Please define it where it first appears in the text. Line 406: Average abundance term is modified to average relative intensity throughout the text and was defined in Section 2.3, Line 185 as follows: "Relative intensities were calculated by normalization with the most abundant ions in each mass spectrum (Kujawinski et al., 2009)."

Line 421 (Fig. 6a-d, Fig.S2) and Line 424 (Fig. 6a-d, Fig.S2): Refer to wrong figures? Line 434: Corrected as (Fig 6a-d, Fig. 3a-e) and Line 438: (Fig. 6b-d).

Line 420-421: Changes in DOM composition seem inconsistent with the changes in nutrient and DOC. For example, the average MW, diversity index, and UHC percentage of UL-1000 are similar to that of surface water (UL-5), while DOC/nutrients (except ammonium) of UL-1000 are much lower/higher than surface water. Line 432: This was addressed in the first paragraph of Section 3.2, Line 298-302, by stating that compositional changes are not necessarily coupled to bulk concentrations changes. To provide a link between sections a reference tag is added in the text as "This seems contradictory to molecular changes in DOM compositions (as detailed further in Section 3.3.3)...".

Line 460: 'H:C \leq 1.0' should be 'O:C \leq 1.0'. Line 472: Corrected.

Line 469-471: Low molecular diversity and relative abundances (average abundances?) are also observed in BG-samples at similar depths (Figure 6b,d). So, it is not sufficiency to demonstrate the influence of hydrothermal intrusion or the plume based on these data (i.e., molecular diversity, average abundance) alone.

Line 482: We agree with the referee that the diversity index and average relative intensities may not be sufficient to demonstrate the influence of hydrothermal intrusion in the lateral transect. This was the reason why we also indicate LPD percentages to emphasize compositional differences.

Line 529-532: This explanation seems more plausible to me, as the difference in DOM compositions between NP2-samples and BG-samples is greater than that between PL-samples and BG-samples (Figure 8).

Line 541: This was added to the end of the section just for the flow of the text.

Table 1: According to the text, DOM compositions of PL-3400 and PL-3500 have been measured but they are not labeled with superscript '3' in the table.

Suggested edits are corrected in the table.

Figure 7: The letter numbers of panels are inconsistent with the caption. In addition, the captions are corrected. Percentages of contributions of the PCoA1 and PCoA2 to the total variance need to be presented.

Letter numbers of the panels and the explained variances for both axes are added.

Figure 8: According to the Krevelen diagram, there are differences in DOM composition among different BG-samples, yet, the authors did not discuss the possible reasons for these differences in the current manuscript. Do the differences in DOM composition of different BG-samples indicate effects of non-hydrothermal plume processes? This information may help to distinguish the effects of hydrothermal and other processes.

This was partly mentioned in the first paragraph of section 3.3.3 stating that there were some exceptions in clustering of the samples by stations. But we, unfortunately, do not have further evidence for the reasons of compositional differences between samples from the same stations.

Supplementary: Figure 3, 4 and 5 have wrong numbers.

Corrected.

