Biogeosciences Discussions

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Comment on bg-2021-350

Anonymous Referee #1

Referee 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-RC1, 2022

The authors have collected a set of samples in the vicinity of hydrothermal vent plumes in the Arctic Ocean and analyzed the complexity of DOM in these samples. The manuscript requires some revision, but would be acceptable for this journal.

One item that would improve presentation of the data is to plot the variables in figure 6 against the measures of hydrothermal vent plumes shown in figure 4. This would allow the reader to easily link the parameters that show the presence of a plume with the data collected in those samples. Lacking that the reader is left to squint at multiple depth profiles.

The manuscript should have been checked for errors before sending it out – missing citations and a bibliography with no year information are careless errors.

Line 28 'less abundant compositions' – this sentence is too vague to interpret. Do you mean abundance in terms of number of elemental formulas/mass-to-charge values or abundance in terms of peak areas? (note that as I read later the methods make it clear you are talking about number of formulas).

The Hawkes and Rossel papers are only appropriate citations for the alteration of refractory DOM. The work on production of dissolved hydrocarbon gases requires other sources, likely papers from Seewald and colleagues.

Line 49 – what about the research of Arrieta et al. which does show it is available, but too dilute to be used?

Line 97 'Ocean Floor Observation and Bathymetry System' – I have no idea what this is, an ROV? Camera system? AUV? CTD/rosette system? Bottom lander?

Line 109 – there appear to be references to a figure or table missing.

Line 115 – what does '2s' mean ?

Line 145-how do you have a sensitivity for the fluorometer in ug/l without a description of how it was calibrated to convert volts to ug/l? Also, from the results section, I don't think you mean sensitivity here, but detection limit.

Line 184 – how did you determine which adduct to use when combining the positive and negative ion mode data? And, how did you handle *m/z* values that are multiple adducts from a single neutral mass?

Line 197 – can you describe the percentage calculation a different way as this is not clear. How are you grouping LPD/CAR/LGN/UHC? And why is it different from the sum of CHO/CHON/CHOS?

As I read later, I think the authors have done this:

LPD + CAR + LGN + UHC = 100%

And a separate calculation for the elemental formulas to be :

CHO + CHON + CHOS = 100%

However, what about elemental formulas that have multiple heteroatoms (CHONS?)

Line 268 'No substantial primary production occurred in the top 50 m layer of the water column, as shown by the nutrient minima and the Chl a maxima at ~40 m water depth. During the Arctic summer (from March to September), this layer gets depleted in inorganic nutrients and enriched in dissolved organic matter' – I am not certain what is data from the present project and what is speculation. Since the samples in this project were collected in Sept/Oct, where is the source for the March to September nutrients? And, if nutrients are low in the surface, that could be an indication they have already been consumed and hence that is not a good marker for low primary production. From the sample set they have, the authors cannot make statements about primary production, and only can provide statements about the end products of primary production.

Line 290: 'do not indicate any anomaly in relation to plume dispersion distant from the vent due to dilution with seawater' – I agree with this statement, but it would be easier to see on the figures if you mark the depth of the buoyant plume on figure 3.

Line 293 – 'This seems contradictory to molecular changes in DOM compositions' – at this point in the manuscript you have not discussed the DOM composition so the reader has no basis to understand this point.

Line 384 'precluding a proper assessment of the geochemical processes influencing methane there' – this phrasing is odd. The authors are clear about the caveats, but don't end with statement saying you cannot do this analysis (after spending a page doing it).

Line 392 'the features obtained in different modes, combining positive and negative ESI datasets provides a considerable advantage for differentiating samples.' How? The previous paragraph does not provide any information about different samples.

Line 424 ' DOM abundances' is too vague – please continue to be specific and refer to the number of elemental formulas since you also have DOC concentration data.

Line 428 'CHON and CHOS heteroatom contents were maximal at the surface' – this is a stretch. Looking at figure 6j, CHON % is higher at depth than the surface (which the authors note in the next sentence), while the range of CHOS is so wide at the surface and depth that you cannot make any such statement.

Line 436 – this paragraph is speculation and statements about global carbon use from six stations near a hydrothermal vent site only detracts from their messages about organic matter from hydrothermal vents.

Line 470 – figure 5 lacks subplots so this is an error.

Line 525 – reference to the wrong figure again.

Please add years to the references information.

Figure 1e - I like the schematic – it's a nice representation of the sampling area.

Figure 3 – why are there lines connecting some samples and not others? I would remove the lines entirely as they do not aid in interpretation of the data in the figure.

Figure 5: Mid-Atlantic Ridge. Also, I think I understand the plot the range of endmember values from the different systems, but the way this is plotted it appears that the other systems studied all have 1/methane concentrations of zero. I would put the comparisons to other end members to the left of the 0 value on the x-axis to avoid this interpretation. Why is Pedersen et al. 2010 discussed in the text but missing from the figure?

Figure 6b, what is average abundance? I suspect this comes from the confusing statement in the methods (line 28), but even seeing the plot I still do not understand what is calculated here.

Figure 7 – what is the variability for each of the PCoA axes? This information is needed to interpret the distribution of points in the multidimensional space.

Table 1: correct to parentheses. Also when you list 'DOM' in the table here, do you mean DOC concentrations or SPE-extracted DOM? This is not clear.