

# Reply to Reviewer 1 comments for: “Sea ice concentration impacts dissolved organic gases in the Canadian Arctic” by Charel Wohl et al.

Many thanks to the reviewer for taking the time to thoroughly review the manuscript and provide constructive comments. The reviewer provided thought provoking comments, which has helped us to improve the manuscript. Please see our responses below. Reviewer comments are in normal font and author’s replies can be found in italic.

## General Comments

As we know, the sea ice play an important role in influence the dynamic of biological activity and photochemistry in polar oceans, and subsequently, indirectly impact the production and release of OVOCs and biogenic gases. The author clear presented the temporal and spatial distributions of gases associated with the sea ice concentrations. However, to make the conclusion that the sea ice concentration impacts the gases is not preciseness. They did not perform a timeseries observation at a stable station to investigate the influence of sea ice dynamic to seawater gases. Might be the title “characteristic of dissolved gases in marginal sea ice area in Canadian Arctic” is more suitable.

On the other hand, the authors should also make some description in the dynamic of nutrients and phytoplankton activities from previous reports. To my knowledge, in the July or August, the Arctic Ocean in Baffin Bay is possible flourishing with high phytoplankton biomass (Bloom). If the nutrients were enough to support the growth of phytoplankton, it would be easily to observed the bloom in the marginal sea ice area. However, along the cruise track, both biogenic gases and Chl a indicated low values except some period like July 27-28. Please also check the satellite Chl a data from ocean color website. Then, we can know how the phytoplankton growth in the whole Bay. The seawater DMS levels  $< 3 \text{ nmol L}^{-1}$  were not high compared with previous studies. If there is no problem with the measurement method, the phenomena should be noticed. Might be the phytoplankton bloom in early July or June consumed the surface nutrients. Thus, to know the profile nutrients information is very important to explain the data.

*Thank you for suggesting a different title. Indeed, to better understand and be more conclusive, future research should focus on making year-round observations at the same station. We added this idea to the end of the manuscript, under future research. Most of the analysis in the paper focusses on how VOC concentrations change with sea ice concentration in space, which we take to be a proxy for the seasonal influence of sea ice. While this sampling strategy wasn’t Lagrangian, we prefer to stay with the current title as the word ‘impacts’ does not necessarily imply direct causation.*

*Indeed, the DMS concentrations from this cruise are lower than previous measurements in this region at a similar time of year. In the manuscript, we discuss that our DMS concentrations are lower than previous measurements. We explain that peak DMS concentrations probably occurred before our sampling campaign, a hypothesis supported by satellite inferred DMS estimates by Galí et al. (2018). Also Collins et al. (2017) found high interannual variability in DMS concentrations, despite approximately sampling at the same time and same location over two years. DMS in the Arctic is also spatially very heterogenous, so it is also possible that our cruise track did not cover any DMS hotspots. Nutrient concentrations from this cruise have only been made available very recently and a brief discussion has been included in the revised manuscript.*

*The following changes have been made to the manuscript in regards to this comment made by the reviewer:*

*In Sec. 2.1, the following sentence has been added:*

*Inorganic nutrient measurements (nitrate) were carried out as described in Randelhoff et al. (2019).*

*Nitrate measurements have been added to the casts presented in Figure 7.*

*When discussing the underway auxiliary data (Sec. 3.2), we added the following sentence:*

*Nitrate concentrations at 5 m ranged between 0 and  $0.7 \mu\text{mol dm}^{-3}$  suggesting that the phytoplankton bloom sampled here is very advanced as nutrients near the surface are depleted.*

*DMS discussion (Sec. 4.5) added:*

*Low surface nitrate concentrations measured during this cruise also suggest that the sampling presented here has been carried out after peak phytoplankton growth.*

*Satellite Chl a data was overall not as conclusive due to poor satellite coverage and spatial heterogeneity of the spring bloom. But overall it shows higher Chl a concentrations at the beginning of July compared to the beginning of August, further supporting that most sampling presented here occurred after the peak bloom phase.*

#### **Some minor points**

Line 63, There is no Zhang et al., 2019 in the references. Please also check the whole manuscript.

*The reference list has been updated and this has been corrected.*

Figure 1, if you can present the real sea ice cover data (from <https://seaice.uni-bremen.de/data/amr2/>), it would be clear.

*The sea ice covered area is approximately indicated for illustration purposes as a shaded area due to the dynamic nature of sea ice cover and difficulties of conveying this information for a month-long deployment. The approximate location of the sea ice edge is based on the average sea ice concentration for the whole cruise duration using AMSR2 satellite data.*

*The figure description has been updated to make this clearer.*

Line 111, Chl a measured by the sensor might not be precise. Is there any biologist who does the measurement of Chl a through filtering the water? You can use this data if you have a choice.

*Unfortunately no direct HPLC measurements of the chlorophyll a pigment were made during this cruise. Ship technicians compared the fluorescence measured underway and the CTD mounted sensor. They find a linear correlation of 1:1 between both sensors and estimate that the accuracy of the fluorescence measurement is on the order of  $0.1 \mu\text{g mol}^{-1}$ . Please do also note that apart from the correlation between isoprene and Chl a, most discussion of Chl a relies on relative, rather than absolute concentrations.*

Line 125, why not use the AMSR2 data in figure 1?

*Briefly, the sea ice edge data is based on an AMSR2 average, but only shown here as an illustration and thus approximate location.*

*This has been updated in the figure description.*

Line 147-149, what is the clear number of the difference. Is it significant?

*The difference between CTD and underway due to this acetaldehyde contamination was highly variable. We observed on the order of 500 nmol dm<sup>-3</sup> (approximately a factor of 100) higher concentrations in the CTD. This led us to label these clearly as a contamination.*

Why you use the 20%-50% SIC in figures for discussion? You should explain that in Method. Is there any define for heavy sea ice area or ice-free area?

*The casts have been grouped in panels by SIC. The grouping is based on the following definitions; in remote sensing, an ice-free area is generally considered to display ice coverage of less than 15 % (Wang et al., 2020b) and ice breakup has been defined by Ahmed et al. (2019) as the time the sea ice concentration changes from above to below 90 %.*

*Based on these few definitions, sampled stations, and corresponding SIC, stations were grouped as (a) 75-90 % near full ice cover/during ice break up, (b) 50-20 % partial sea ice coverage and (c) 0-15 % ice-free. No stations were sampled with SIC 70-50 %.*

*This has been added to the manuscript.*

For me, it is difficult to read the figure 2-6, where is the station numbers? I do not know where the stations along the cruise track. Please mark it if it is possible. Or use the date to indicate it?

*We decided to use the date to indicate this and updated figure 2-6. This greatly improved the figure in our opinion. Thank you for your input.*

Line 482, as you presented that you did not measure the atmospheric gases, the flux calculation for those gases with high levels in atmosphere by using a constant value seems bring large uncertainty. The authors should pay attentions to make the conclusion of "source or sink". If there is any other published paper calculate by the same method, you can cite those papers to let the readers know that it is reasonable.

*Methanol and acetone fluxes were computed using published atmospheric measurements at a similar time of year and place, this is similar to methods used by Beale et al. (2015). Indeed, this brings about some uncertainty in the calculated flux and this is acknowledged in the text. However, methanol and acetone were highly undersaturated (mean cruise saturation 22 and 27 % respectively). Thus this should not hugely influence our conclusion that the sea ice zone is a sink to those gases. Additionally, we present the equilibrium gas phase mixing ratio, which should allow readers to assess the flux direction independently.*

*This additional explanation has been added to the manuscript.*

Figure 10, the caption is unclear.

*Thank you for pointing this out. The caption has been updated to read:*

*Figure 10 (a) Timeseries of underway sea ice concentration, wind speed at 10 m and calculated air sea exchange velocity for DMS. Short gaps in the timeseries of the air sea exchange velocity of DMS are due to gaps in the recording of the underway sea surface temperature. (b) Calculated underway fluxes of (b) DMS and (c) isoprene.*

Line 576, replace the "Greatest" with "higher" or ? Higher emissions of biogenic VOCs were observed in ice-free areas than those with heavy SIC. The value of flux is not significant with DMS or isoprene. Please also check the whole manuscript.

*We decided to update the manuscript and use the sentence suggested by the reviewer.*