

Biogeosciences Discuss., referee comment RC1  
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## Comment on bg-2021-252

Miming Zhang (Referee)

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Referee comment on "Sea ice concentration impacts dissolved organic gases in the Canadian Arctic" by Charel Wohl et al., Biogeosciences Discuss.,  
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In the manuscript "**Sea ice concentration impacts dissolved organic gases in the Canadian Arctic**", the authors presented the measurements of dissolved methanol, acetone, acetaldehyde, dimethyl sulfide and isoprene in the sea ice zone of the Canadian Arctic from the surface down to 60 m in July/August. Up to now, understanding about the OVOCs (including seawater methanol, acetone, acetaldehyde) and isoprene were limited due to the lack of measurements in the Arctic waters. The authors tried to analysis how the sea ice cover impacts the dissolved gases. The data included shipboard underway measurements and vertical stations profiles. They found the gases indicated a different distributions characteristic associated with different sea ice cover conditions, such as in the ice free, 20%-50% SIC and 75%- 90% SIC areas. They suggested that the sea ice cover would impact the gases production and then distributions. Generally, in my opinion, there is no doubt that this data sets were deserved to be published. However, I also have some questions and comments for the author in order to improve this manuscript if they want to take my suggestions.

General comments 1/4 □

As we know, the sea ice paly 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.

Some minor points:

- Line 63, There is no Zhang et al., 2019 in the references. Please also check the whole manuscript.
- Figure 1, if you can present the real sea ice cover data (from <https://seaice.uni-bremen.de/data/amsr2/>), it would be clear.
- Line 111, Chl a measured by the sensor might not precise. Is there any biologist do the measurement Chl a through filtering the water? You can use this data if you have choice.
- Line 125, why not use the AMSR2 data in figure 1?
- Line 147-149, what is the clear number of the difference. Is it significant?
- 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?
- 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?
- 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.

10) Figure 10, the caption is unclear.

11) 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.

