Biogeosciences Discussions

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Comment on bg-2022-150

Anonymous Referee #3

Referee comment on "Concentrations of dissolved dimethyl sulfide (DMS), methanethiol and other trace gases in context of microbial communities from the temperate Atlantic to the Arctic Ocean" by Valérie Gros et al., Biogeosciences Discuss., https://doi.org/10.5194/bg-2022-150-RC3, 2022

Summary

The manuscript by Gros et al. reports on a large-scale survey of DMS, methanethiol and several other gases along the gradient between the eastern subpolar North Atlantic and the Arctic. Their main goal was to assess the concentrations and spatial distribution of these compounds and relate them to the environmental (physical, microbiological) conditions via statistical correlations. Based on their results, the authors aim to improve our current understanding on trace gas cycling in the context of a changing Arctic.

General assessment

The topic of this manuscript is certainly relevant for a wide biogeochemistry community and contributes to amend the large gaps of data coverage for marine trace gases; in particular for compounds which are understudied in comparison with other climate-relevant gases such as CO2 or CH4. The paper is very well written, its structure is clear and the methodological approaches are both sound and explained with enough detail. Although the manuscript is quite descriptive, the authors state clearly that they aim to report on the results of their survey. Hence, from that perspective, they were successful in achieving that. In my opinion a significant drawback of the study is the absence of air-sea fluxes of the different compounds measured in surface waters. Based on the content of the methods presented by the authors I cannot judge whether they have information at hand to do so, but it would be worth making an effort to provide estimates of air-sea fluxes for at least some of the compounds (I do think this can be done for DMS and CO). Other than that, most of my comments to the paper are minor (see below).

Specific comments

- Title: The current title is rather long and can be misleading. The word "Variability" is should not be used generally here since the authors are addressing the spatial variability of several trace gases, not their temporal variability.
- 29-30: Revise syntax, in particular after "understanding of". Also, I recommend stating how specifically the paper contributes to that understanding, since right now this could mean anything.
- 80: Please replace the word "levels" for "concentrations" here and it all instances where you refer to that quantity.
- 81: "numbers" is too unspecific. Please refer precisely to which measurable quantity you are referring to here.
- 82: Bacterial diversity and water masses were addressed. No sea ice data cover was presented and therefore it should not be presented as a factor in your experimental design.
- 88: The citation to Peeken (2016) is unnecessary. I know such reports have a doi number, but they do not constitute a source of peer-reviewed information and should only used when absolutely needed (see journal's regulations)
- 92: Stating that "usually" there is not sensor drift is not enough, even if the sentence is supported by a publication. The authors need to show that this was indeed the case during their survey in order to keep the credibility of their observations.
- 106-107: Here the citation is also unnecessary and should be removed. If the authors want to refer to the data used, there are better ways such a data set doi from Pangaea.
- 117: Delete point
- 123: delete "l" after Chl a
- 123: The R2 value in S1 is different than the one shown here. Revise.
- 153-154: Revise wording. I would suggest "The measurement principle of PTRMS is (…)" or similar.
- 214-217: The details on how this statistical analysis was setup should be explained in the "Material and Methods" section (i.e. independent and dependent variables, etc.). Otherwise the statement seems arbitrary (i.e. coming from nowhere).
- 234-235: Explain the details on how the system was adjusted. This reads as if the authors used the continuous system for profiling. Was that the case? If so, a detailed description is needed.
- 259-266: I am not convinced of the approach here. Why was station 19 removed from the analysis? It appears that although stations 19 and 32 have high productivity, both isoprene and CO behave completely different. Also, if one compares CO concentration at stations 19 and 39 (having contrasting chl a concentrations), it becomes evident that CO is not affected by the same processes as other gases. The reasons for this are unfortunately not discussed at all. In order to explain the variability of some of the CO concentrations at depth (e.g. at stations 32 and 43), the authors claim that differences in the profiles are due to "decreased photochemical production following lower light penetration". However, this is the case for all stations and therefore it is not a compelling reason to explain the decrease with depth. Perhaps the authors rather refer to the effect of different sea ice coverage percentages in light penetration (?). If so, they can easily explore this possibility by using such data which is widely available.
- 303: Is the mean value for surface waters or does it include the water column measurements? Please clarify.
- 308: Personal communications are not appropriate. Even less in this case since there are already two citations supporting the statement.
- 353: Same comment to personal communications. The authors already used Dybwad et al. (2021) as a defining criterion for the bloom stages in the study area at the time of sampling.
- 385-387: This statement is contradictory with the results presented by the authors for CO. Based on the data presented it is only clear that CO production is not necessarily tied to a biological component and that photochemistry might have had a more significant role at the time of sampling. The authors argue (L.313-315) that low CO

production by diatoms might be the explanation for the low concentrations at e.g. 19. However. this is speculative and cannot be substantiated with their observations. I recommend revising this aspect of the discussion.

- Supplementary information: there are inconsistencies in the naming of Figs. S6-S8. For instance, in L.235 S7 is mentioned although it does not match what is actually shown. In Fig. S8 no CO is shown (although announced in the main text) and the caption does not match the figure.