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Comment on bg-2021-223

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Community comment on "Long-term incubations provide insight into the mechanisms of anaerobic oxidation of methane in methanogenic lake sediments" by Hanni Vigderovich et al., *Biogeosciences Discuss.*, <https://doi.org/10.5194/bg-2021-223-CC1>, 2021

I think this is a great paper so I want to congratulate all of the authors for this neat work.

- Regarding the AQDS amended incubations, the authors mention that no ^{13}C enrichment of the DIC was detected in the presence of this electron acceptor. In addition to the explanation given in lines 399 to 404, I would like the authors to consider two more plausible explanations:

1) AQDS has a very low electron-accepting capacity thus it may have been quickly reduced to AH₂QDS by heterotrophic microbes making it unavailable for methanotrophs. Also, the authors did not specify the concentration of AQDS employed in the study in the materials and methods section. How high was the electron-accepting capacity of the humic substances employed in the humic-amended treatments when compared with the electron-accepting capacity of AQDS?

2) Humic substances possess a greater range of redox-potential than AQDS (check Valenzuela et al 2021) thus depending on the AQDS and CH₄ concentrations the reaction with AQDS might have been thermodynamically unfavored when compared with the reaction of CH₄ oxidation coupled to humics reduction.

- Concerning the microorganisms driving the methanotrophic process the authors mention that the percentage of ANME related microbes was very low and speculate that type I methanotrophs (aerobic) might have been involved in the process. These results indeed agree with previous studies in which AOM occurs in the absence of ANME, additionally, studies have demonstrated that some putative aerobic methanotrophs can oxidize methane under strict oxygen limitation. The authors need to support these findings by citing the papers related to these very interesting findings.