

Biogeosciences Discuss., referee comment RC1  
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## Comment on bg-2021-58

Anonymous Referee #1

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Referee comment on "Sulfate reduction and anaerobic oxidation of methane in sediments of the South-Western Barents Sea" by Claudio Argentino et al., Biogeosciences Discuss., <https://doi.org/10.5194/bg-2021-58-RC1>, 2021

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Using "linear" porewater sulfate gradients to estimate methane fluxes has been frequently reported from areas with high methane fluxes especially in the past few decades. The approach has raised uncertainties due to sulfate consumption by OSR and AOM reactions. Therefore, some studies have expanded the diffusive flux calculations on depth profiles of DIC concentrations and stable carbon isotopes of DIC (e.g., Wehrmann et al. 2011, Chemical Geology; Burdige and Komada, 2011, L&O) or have applied a reaction-transport model to simulate sulfate and methane depth profiles (e.g., Wallmann et al., 2006, GCA; Dale et al., 2019, GCA) in order to get better estimations on OSR and AOM processes. The controls on  $\delta^{13}\text{C-DIC}$  and  $\delta^{13}\text{C-CH}_4$  depth profiles with special focuses on the SMTZ were also examined in recent years (e.g., Burdige et al., 2016, JMR; Chuang et al., 2019, GCA; Meister et al., 2019 JMS). The relevant literature reviews are largely lack in this study, hence, which results in the lack of novel aspects. Despite the authors have been aware of some references in the supplement for their  $\delta^{13}\text{C-DIC}$  and DIC calculations, they don't have data to provide direct constraints on their flux estimations. The study presented by Argentino et al. needs more consolidated works on the introduction, methods, results and discussion, so that I couldn't recommend the publication in the journal of Biogeosciences.

Comments:

- None of the sediment cores collected in this study reached the SMTZ. The limited data (only sulfate) can't support the estimation of the depth of SMTZ. Many studies have shown that sulfate might stay constant in the deeper depths. The authors should provide other valid data to support their arguments (e.g., sediment cores reaching SMTZ and showing varying depth of the SMTZ and data to constrain OSR and AOM such as depth profiles of  $\text{NH}_4^+$ , TA,  $\text{H}_2\text{S}$ ,  $\text{Ca}^{2+}$ , DIC, TOC,  $\delta^{13}\text{C-CH}_4$ ,  $\delta^{13}\text{C-DIC}$  etc.).
- Eq. 3 used in other studies such as by Martin et al. (2000, GCA) and Hu and Burdige

(2007, GCA) is to estimate the amount of calcite dissolution adding to the porewater. The fact is that  $\delta^{13}\text{C-DIC}$  in the porewater is not only affected by SOC source and AOM reaction but also SOC degradation, carbonate mineral dissolution and precipitation etc. Therefore, the estimated OSR and AOM rates are not valid, despite the authors have additional information on  $\delta^{13}\text{C-DIC}$  calculations in the supplement which have no data reaching SMTZ to support their arguments.

- The scale of seismic profile is different from the length of sediment cores. Gas migrations can also be controlled by the tectonic structure beneath the coring sites. Therefore, gas accumulation shown in the seismic profile doesn't mean gas exist in the cored sediments.
- Line 95: Sulfate analysis needs some more detailed information on analysis details and analytical quality. Why was sulfate measured by ICP-OES? How did authors separate other sulfur species? If the authors assume that all the sulfur species in the porewater measured by ICP-OES is only sulfate. This may overestimate sulfate fluxes. Or This may imply no  $\text{H}_2\text{S}$  production through OSR and AOM which is in contradiction to their flux calculations.
- Line 110: What kinds of gas standard were used?
- Line 137: Hu et al., 2017 and Hu et al., 2010 are led by different authors.