

Biogeosciences Discuss., author comment AC2
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Reply on RC1

Claudio Argentino et al.

Author 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-AC2>, 2021

We thank the reviewer for the useful comments and constructive criticism. In the Barents Sea we generally employ a 6 m-long gravity corer for investigating biogeochemical processes related to methane oxidation. Unfortunately, this length of sediment recovery does not allow us access to the deeper sediments and hindered the description of deeper sulfate profile terminations, and direct interception of the SMTZ in areas located away from the shallow seismic anomaly. The vertical sampling resolution was adequate to describe the sulfate gradient in the uppermost 2-3 m and we found good linearity in all the examined pore water profiles. Very low microbial activity was measured in the upper 2.4 m of sediment at NW Ingøydjupet trough (Nickel et al., 2012) so we assumed that sulfate diffusing in the sediment undergoes little sulfate reduction linked to organic matter oxidation in the upper sediment column and is mainly consumed deeper, within the ZMTZ. For this reason, we linearly extrapolated the sulfate profiles to deeper sediment to estimate the depth of the SMTZ. It is worth mentioning that sediment core 358-GC actually reached the predicted SMTZ (3.5 m) as shown by CH₄ concentration in the gas samples plotted in Fig.2. For the above reasons we considered the sulfate profiles in the deeper sediment not covered by pore water data to approximate a linear shape also in correspondence of cores 354-GC, 355-GC, 356-GC, 361-GC, 362-GC, 363-GC. This approach has been employed in other cases where the core penetration depth did not allow a direct observation of the SMTZ depth (Borowski et al., 1999; Fan et al., 2018; Graves et al., 2017; Mazumdar et al., 2007; Panieri et al., 2016). We do not have other constraints for the deeper sedimentary column to completely rule out the possibility of concave-down trends for the deepest sections. However, a concave-down termination would result in a deeper SMTZ compared to the one calculated from linear regression, further emphasizing the difference in SMTZ depth between cores located above the seismic anomaly and cores away from it, in agreement with our general interpretations. We implicitly excluded concave-up termination of the sulfate profiles as we assumed steady-state conditions. As described in AC1, DIC fluxes entering and leaving the SMTZ can be used to quantify DIC release by OSR and AOM in the SMTZ. We only have pore water samples above the SMTZ and the deep DIC flux component cannot be quantified, nor DIC sequestration by authigenic carbonates. The strongly depleted d¹³C of DIC can be discussed qualitatively but we also propose to enrich manuscript's discussion by including case scenarios of carbonate precipitation and deep DIC fluxes based on average global models (Akam et al., 2020) to give quantitative constraints (AC1 comment). We agree

that datasets including a wide range of parameters such as $\delta^{13}\text{C-CH}_4$ and $\delta^{13}\text{C-DIC}$, ammonium, POC, would allow complete reaction-transport modeling able to calculate depth-integrated reduction rates, methanogenesis rates and pathways, but the simple modeling approach used in our study explicitly focused on processes within the SMTZ. We acknowledge the reviewer for the recommendations and will take them into consideration for future studies.

The reviewer said: "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."

Seismic section in Fig. 4 shows high-resolution P-cable 3D seismic data with vertical resolution of ~ 4 m and horizontal resolution of 6.25 by 6.25 m. Considering these values, we can confidently argue that the top of the seismic anomaly reaches within range of the seafloor (considering the vertical resolution/dominant frequency of the near-seafloor sediment and their velocity). This is in agreement with the methane-rich gas sample collected at the base of core 358-GC and the shallow SMTZs in 358-GC and 359-GC.

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?

We thank the reviewer for pointing this out, sulfate analyses were conducted via ion chromatography, we will modify the text accordingly.

Line 110: What kinds of gas standard were used?

We used Messer® CANGas calibration gases (specialtygases.messergroup.com). This information will be included in the manuscript.

Line 137: Hu et al., 2017 and Hu et al., 2010 are led by different authors.

We thank the reviewer for this observation and corrected the reference.

Best regards,

Claudio Argentino

On behalf of the authors

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