Biogeosciences Discussions

Biogeosciences Discuss., referee comment RC2
https://doi.org/10.5194/bg-2021-58-RC2, 2021

Comment on bg-2021-58

Anonymous Referee #2

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-RC2, 2021

The work presented by Argentino et al described the concentrations of porewater sulfate, DIC, methane, and carbon isotopic signatures of DIC from eight sediment cores recovered from SW Barents Sea. By applying a two-component box model, they estimated the fractions of organic matter-dependent and methane-dependent sulfate reduction (OSR and AOM, respectively). Though none of their core fully recovered the entire sulfate reduction zone, they showed that AOM only accounts for ca. 2/3 of the total sulfate reduction in the two sediment cores they have a more complete dataset. They further correlated the geochemical findings with a seismic profile and suggested that the locations with higher AOM rates, and thus methane fluxes, may be associated with a shallow gas reservoir.

Overall, many of the conclusions are not supported by the data presented. This is mostly due to the recovery of only the upper sulfate reduction zone from the sediment cores. There is one core which may recover the SMTZ; however, there is no sulfate/DIC data from the second half of the core that can be used to verify their calculation. Before addressing the methane and sulfate dynamics with the porewater profiles, it is crucial to have the porewater composition from the entire sulfate reduction zone (and ideally part of the methanogenesis zone) determined. The downcore gradient of sulfate concentrations may change abruptly with depth, a phenomenon that has been commonly addressed in many locations (Zabel and Schulz 2001; Hensen et al. 2003; Haeckel et al. 2007; Holstein and Wirtz 2010; Hong et al. 2014). The entire flux/rate/fraction calculation done by the authors can be invalid if this is the case.

Similarly, the authors´ conclusion of methane source is based on one single methane carbon isotope analyses (without the hydrogen isotope analysed). They claim that the source of methane, similar to what has been found from a well that is ca. 10 km away, is a mixture of biogenic and thermogenic methane. While I understand it is difficult to provide more data when the methane concentrations are low in all the cores sampled, the authors then shouldn't make any inference on the source of methane as such an inference could be incorrect when more data become available. At least, the authors can show the detailed composition of headspace gas, which may (or may not) support their suggestion.

In the last figure of the paper (and last sentence in the conclusion), the authors illustrated a fanaticized scenario under future ocean warming. While I understand the authors intended to make the paper more appealing by linking the work to some future scenarios, such a suggestion is entirely irrelevant and not supported by the data at all. I suggest remove all these hand-waving sections from the paper.

Some more detailed comments of mine are included in the pdf file attached.

Please also note the supplement to this comment:
https://bg.copernicus.org/preprints/bg-2021-58/bg-2021-58-RC2-supplement.pdf