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Biogeosciences Discussions

Interactive comment on "Sulfate deprivation triggers high methane production in a disturbed and rewetted coastal peatland" *by* Franziska Koebsch et al.

Anonymous Referee #1

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In the paper the effect of rewetting of agricultural peat field on methane formation has been studied by way of pore water and sediment chemistry, isotopic analysis as well as by studying the prevailing microbial community. The authors found evidence that rewetting by fresh water may increase methane emissions due to lack of sulfate and its reduction. In conclusion they suggested using marine water instead of fresh water in waterlogging.

1. The paper is in the scope of BG while it deals with interactions between biological and chemical processes in former cultivated field being subsequently wetland.

2. The authors have used versatile and state-of-art methods and the results are novel.

3. Substantial conclusions have been made. The supression of methanogenesis by sulfate reduction has been known for a long time. However, in this kind of practical context such substantial conclusions have not been made earlier.

4. The scientific methods are mainly clearly presented, with the exception of pH. Line 123: It was not stated that pH was measured even though the pH device was presented in the same sentence. In addition, the pH was presented in the principal analysis but not discussed. It is suggested that pH will be discussed.

5. The results are clearly presented and mainly in line with the text and the figures. However, there are discrepancies between the results in Table A1 and Figure 4.

Line 268: The authors write that "H2S concentrations were below detection limit ($\sim$1 μM, Fig. 4)". However, according to Table A1 there are higher concentrations (at 10 cm 3 μM, and at 40 cm 2 μm). In addition, the sulfate concentration is suggested to be reported with the same accuracy in Table A1 and Fig. 4. Otherwise the readers of the journal might get confused.

Line 274: The same comment as above regarding AVS, and similarly with the other spots.

6. The results support the interpretations and conclusions partly but I feel that the equivocal vertical methane concentrations do not clearly support the interpretations and conclusions.

For example, along the studied transect the sum of methane concentration till the depth of 40 cm is the highest in spot 2 and the lowest in spot 1. I feel that the topsoil of spot 1 might be aerated occasionally and therefore methanogen formation was the lowest there. The authors do not present any data for water levels in the spots although water saturation is crucial in determining whether the soil is aerobic or anaerobic.

In addition, in spot 2 the highest methane concentration is at the depth of 30 cm and there is still some sulfate left in pore water but not any methanogens. On the contrary,

at the depth of 30 cm and 20 cm methane concentrations are the lowest in the profile but in these layers there is not any sulfate left. This is contrary to the hypothesis and should be discussed in the text.

7. The description of experiments, the result table and calculations are sufficiently complete and precise to allow their reproduction by fellow scientists.

8. The authors refer to related work and clearly indicate their own new contribution. The title is clear and reflects the contents of the paper.

9. The abstract provides a concise and complete summary.

10. The overall presentation is well structured and clear.

11. The language is fluent and precise.

12. Mathematical formulae, symbols, abbreviations, and units are correctly defined and used.

13. The number and quality of references are appropriate.

14. The quality of supplementary material is appropriate, but the water table depths in the spots should be presented in a cross-section or in a table.

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