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Interactive comment on "Carbon degradation in agricultural soils flooded with seawater after managed coastal realignment" *by* Kamilla S. Sjøgaard et al.

Anonymous Referee #1

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General comments:

authors: Sjogaard et al. This manuscript evaluates the effect of flooding soils with seawater on the carbon mineralisation pathways and rates in these soils. This is clearly a relevant topic with respected to planned managed coastal realignement projects to improve coastal defences against sea level rise. The experiment tackles an environmental issue and seems to be well designed and executed. The manuscript is well written and to the point. However, the major hypothesis (hypothesis 3: does the flooding of soils promote organic carbon preservation?), which is the core and carries the impact of this paper is not well supported (see section below). Furthermore, there are a few more issues and some technical corrections that need revision before this

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Discussion paper

manuscript is ready to be accepted in BGS. These issues need to be addressed before the manuscript is ready for publication. I recommend major revisions.

Major specific comments:

- Paragraph 4.3: This paragraph is, according to me, the most important conclusion of this manuscript. If coastal soils are re-exposed to marine conditions, will they promote carbon burial and this form a negative feedback on atmospheric CO2 concentrations? Unfortunately, this is also the least documented paragraph, and it does not provide enough evidence to valid such a strong conclusion as posed on P13L11-12 (this study suggests that the majority of soil OC will be permanently preserved . . .).

There is not data or values of pre-flooding mineralisation rates, nor a comparison to normal marine conditions. Furthermore, the TCO2 flux of 67 mmol m-2 d-1 in the uncultivated soil measured by the end of the experiment (and the value of 239 mmol m-2 d-1 on day 13) (see section 3.2) are indications for an extremely high mineralisation rate. The effluxes in the cultivated soil (29 mmol m-2 d-1) indicate normal rates for marine sediments. It is highly likely these rates are transient, and are driven by the DOC production, but this would mean that the standard soil conditions do not produce this DOC, and thus that reinstating marine conditions actually inhibits carbon burial.

Hence, there seems to be no direct evidence that newly flooded coastal habitats will be hotspots for carbon burial. I propose that the authors give a stronger foundation for this paragraph, and show that re-exposure to marine conditions actually decreases the carbon mineralisation (e.g. by providing an estimate of pre-flooding mineralisation rates, or by comparing the carbon burial to pre-flooding carbon burial, and normal marine carbon burial rates).

- Paragraph 4.4: FeIII is indeed efficient as being a sulphide buffer in flooded soils, however, figure 6 shows that virtually all FeIII is converted to FeII by the end of the experiment. This indicates that the FeIII sulphide-buffer was exhausted and sulphide will start accumulating after $\sim$1 year. This should be mentioned in this paragraph, and

I would also reconsider the term ‘efficient buffer’ when this would only be active for the time span of 1 year.

- Paragraph 2.3: I have a few remarks/questions for the data analysis that was applied:

When calculating the slopes of the rates in the jar experiments, did you apply any outlier check?

I don’t agree with the linear data interpolation that you used to correct for missing data points. In my experience, reactions rates tend to follow exponential trends rather than linear ones. If you want to use this linear interpolation, I would advise to include a small section on the possible errors you make while doing this interpolation.

The correlation you used to convert organic matter to OC units is based on only two points? How did you estimate the significance? Can you show a plot that shows the OM vs the OC, and what model you used?

Minor specific comments:

- P1L11, P3L18: I seem to get a bit confused with the sentence structure. Was station C not in the area that was reflooded? And is the sampling area reflooded, planned to be reflooded or not planned to be reflooded?

- P7L14-21: you mention that the experimental period was not long enough to achieve full saturation of SO_4 at 20 cm depth. However, in the C cores, sulphate reaches that depth after the first week, and the concentration at depth decreases over time. This shows that sulphate consumption increases over time (most likely when the FeIII inventory decreases). The UC cores show an increase of sulphate over time and have indeed not achieved saturation at depth.

- P8L11-14: You say that TCO_2 production could not be determined below 5 cm depth. You then estimate this TCO_2 production by assuming that SR was the dominating pathway at depth. However, when I look at Table 3, you show that the contribution of other anaerobic pathways was 19% for UC and 54% in C, so SR was clearly not the dominant pathway. Also, considering the high FeIII concentrations in the sediment, I would assume that dissimilatory iron reduction is also an important pathway. Considering this, I have some problems with Figure 5, where you show that all TCO2 production from 4 months onwards is due to SR. This is a consequence of your assumption, and I don't feel that this is well founded. Can you provide more justification for this?

- P11L30: I think you can make an estimate of the time evolution of the relative importance of the mineralisation pathways, which could provide more information than the integrated budget over 1 year (since SR will always end up being the dominant pathway if you wait long enough). It would also improve the impact of the manuscript.

- P12L5: Based on the results from the FeIII – measurement of Lovely and Philips, I believe you can estimate the importance of dissimilatory iron reduction (at least, that is what they teach at the AMME summerschool in Odense every year).

Technical corrections:

- Abstract: I find the paper well written, but I don't feel the same about the abstract, it does not flow very well (e.g. 'So far' at the beginning of a sentence). I would advise revising the abstract in order to improve attraction. - P2L18: "Further it is" -> Furthermore it is - P3L25: is the water in the tanks from the same site? If so, please indicate. - P10L9: aerobic OC degradation contributed to 18 and 10 % to of the total . . . - P11L21 anaerobic TCO2 production, was detected -> remove the comma - Figure 3: I would use different symbols for the different months (when printed in black and white, the colors will be difficult to distinguish). - Figure 4: same remark as for figure 3, and I would consider changing the axes of the right panels (it is impossible to see the different SR rates).

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