Biogeosciences Discuss., 10, C4234–C4238, 2013
www.biogeosciences-discuss.net/10/C4234/2013/

Biogeosciences
Discussions
Open Access

BGD
10, C4234–C4238, 2013

Interactive
Comment

Interactive comment on "Saltwater intrusion into tidal freshwater marshes alters the biogeochemical processing of organic carbon" *by* S. C. Neubauer et al.

Anonymous Referee #3

Received and published: 14 August 2013

The manuscript "Saltwater intrusion into tidal freshwater marshes alters the biogeochemical processing of organic carbon" by S. C. Neubauer et al. focuses on the potential effects of saltwater intrusion on soil parameters and microbial organic carbon degradation in coastal wetlands. As a combination of anoxic soil incubation studies and soil characterization this paper presents a lot of work. The project addresses a topic which is of great importance in the context of future climatic changes. However, I have major concerns about the experimental procedure and the data interpretation in the way it is presented, which might be misleading to non-experienced readers. Regarding this, I have questions, comments and recommendations:

1. Major comment: The plots and their manipulations are not sufficiently described.

More specifically: (1) A major part of the discussion speculates on qualitative changes in organic matter as a result of undescribed changes in the plant community due to saltwater intrusion. Although the authors refer to a previous study it seems necessary to me that these changes are described and/or summarized to bring them into context in the discussion. (2) The chemistry of the applied river water and seawater is not addressed at all. This seems to be a critical point in the discussion, as for instance the authors speculate on the inhibitory effect of sulfate on the lowered production of CH_4.

2. Major comment: The description of processing, storage and incubation of the soils samples leaves open several questions: (1) The authors give no information about processing or storage conditions, eg. temperature and O_2 exposure. This might be important for the studied anaerobic CO_2- and CH_4-formation as for example methanogens react very sensitive to O_2 exposure. (2) Why have incubations been conducted at 25°C which is more than 10°C warmer than in situ soil temperatures? (3) Soil cores have been collected and stored for up to 2 months until they were processed for incubation experiments. Furthermore, the soils for incubations addressing 'responses to long-term saltwater intrusion' have been stored one month longer than those for incubations addressing 'responses to long-term saltwater intrusion'. I wonder if the observed effects might be (partially) a result of this long storage time and/or the storage conditions.

3. Major comment: From the Abstract and Conclusions I expected that 'quantified changes' would have been addressed by before-and-after evaluations. However, the deduced effects of saltwater intrusion are based on measurements at the end of the plot manipulations with the assumption that 'all plots were similar prior to the field manipulations'. Most of the observed differences between the plots, more specifically enzyme activities, water-, C-content and N-content, were small but significant. Regarding this, I have some concerns: (1) Soil parameters (water-, C-content and N-content) were assessed from slurry incubations. In the discussion these parameters/variables are regarded as actual field conditions. (2) The error of the estimated salt content (see comments below), which affects the actual dry weight, seems to be unconsidered.

Collectively, I have reservations about the weighty interpretation of the data, which appears to be forced into the direction of changes in the quality of the organic matter and resulting effects on microbial activity.

4. Major comment: The comparisons of incubation based CO_2 and CH_4 production rates to actual field emissions are very interesting. However, this raises the questions (1) why slurry incubations were necessary to assess short- and long-term effects of saltwater intrusion and (2) why field emissions seem to not reflect the results of the slurry incubations.

More specific comments:

P.6 l.1: How was the salinity of the water manipulated? Was it adjusted, as described before, by seawater addition or the addition of salts?

P.7 l.12: What are the detection limits of the LI-COR and FID analyses?

P.8 l.3-17: Where any efforts made to test substrate limitation for the applied enzyme assays?

P.8 l.28: 'the'? 'then'?

P.9 l.4-18: Salinity is estimated by the YSI3200 based on the conductivity - since all the results are based on a salt-free dry weight basis: where any efforts taken to confirm the weight of the solids upon evaporation?

P.9 l.10: Why were these parameters determined with the incubated soil slurries? How did the authors account for evaporation, C degradation and the error of water addition?

P.11 l.4-6: If this is the data from the slurry incubations please refer to Fig. 1.

P.12 l.4: Referring to the treatment effects for total anaerobic C mineralization?

P.12 l.14-21: The authors should elaborate why salinity manipulation effects 'were not clear' (ie. significant?) from the absolute rates but for relative CO_2 and CH_4 production

BGD

10, C4234–C4238, 2013

rates which are calculated from this data.

P.13 l.22-23: 'upon the addition of seawater' and 'responded to the addition of freshwater' implies that a change in enzyme activity was analyzed over the course of manipulation. However, the data only address one time point and assume a response based on the difference between plots. Consider rephrasing.

P.14 l.16: Lower compared to?

P.16 l.10-11: Reference?

P.16 l.11-12: Do you mean microbial activity, e.g. C degradation? CO2 production is a result of C mineralization but it is not the driving force.

P.16 l.26-27: I don't understand this. Please elaborate 'soil characteristics or other site properties'.

P.17 l.7: Please make clear which reference you are referring to.

P.17 l.23-24: Please explain why 'pools of readily desorbable organic matter' should be depleted when you summarize from literature in P.17 l.14-17 that 'modest increases in salinity have no effect on, or even cause decreases in, the release of dissolved organic C'?

P.18 l.19-21: Please elaborate. Which changes did occur and how are they relevant for your speculation on changes in the 'molecular composition of the soil organic matter' (P.18 l.17-18)?

P.20 l.5-8: Why should the concentrations of lignin and cellulose be reduced? Your data shows a change in C- and N-pools and therefore does not provide consistency in terms of quality aspects.

P.20 l.22-23: Why should saltwater intrusion or your data suggest 'increasing nutrient limitation'?

P.21 l.14-17: Since this is speculative the sentence might be misleading.

P.25 l.6-7: It does not seem to me that your data are 'demonstrating' shifts 'of the integrated plant-microbial' since a temporal resolution is not presented (in this case it is assumed) and 'plant-microbial' related data are primarily based on references.

P.25 l.14: I disagree that your data suggest that 'C inputs to the soil' or 'the lability of soil C pools' are affected by saltwater intrusion, as to me the conclusion is based on too much speculation.

Fig. 1: You should make clear that you show potential production of CO2/CH4.

Tab. 1: You should make clear that the slurry based areal fluxes are estimates.

Interactive comment on Biogeosciences Discuss., 10, 10685, 2013.