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Biogeosciences Discussions

Interactive comment on "Organic sediment formed during inundation of a degraded fen grassland emits large fluxes of CH₄ and CO₂" *by* M. Hahn-Schöfl et al.

Anonymous Referee #1

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The paper addresses a timely question on greenhouse gas emissions generated during the re-wetting of formerly cultivated organic soils. The topic is important and well in scope for BG, first because there is a brist of studies concerning the climatic impact of restoration activities, and second because re-wetting is often considered as a remedy to stop CO2 emissions from drained organic soils. Some studies have reported high CH4 emissions in re-wetted peatlands. The results are in line with and can explain other observations reported elesewhere. This paper shows nouvelle results using laboratory incubations of peat with and without fresh litter how the presence of fresh organic matter relates to high CH4 and CO2 emissions in anoxic conditions, and helps to identify the situations critical to successful restoration in terms of greenhouse

gas fluxes. In addition they help to understand why the emissions can be low in early phases or restoration. The results on the role of fresh organic matter are derived using induction and in part a "black box" approach, since the quality of OM is not specifically characterised in the peat layers. It is thus not able to show the drivers behind the CH4 emissions. The quality of OM matter could be one important factor. While the discussion paper indirectly points to such factors, references could be given to more accurate work on fresh OM. Suitable references an be found from e.g. Kiikkilä, O., Kitunen, V. & Smolander, A. 2011. Properties of dissolved organic matter derived from silver birch and Norway spruce stands: Degradability combined with chemical characteristics. Soil Biology & Biochemistry 43: 421-430.

As chemical correlates, CN and CP ratios are determined, and the indirect evidence suggested by those measures are solid. Two experiments, peat profile incubation, and top soil surface incubation, are described. The first experiment is used to identify the peat layers' activity in methanogenesis while the other addresses the role of different fractions of residual organic matter in CH4 production.

Since the incubations were performed in room temperature conditions, they cannot be used to mimic natural conditions, and the results are relative to the different layers and fractions used in the present study only. That is correctly acknowledged by the authors. Both experiments support most of the conclusions drawn from the role of presence or absence of fresh OM. However, the authors go further and by siting other work state that introduction of aquatic species with aerenchyma and capability to oxygenate the sediment may potentially reduce the CH4 emissions by means of more recalcitrant litter production and enhanced methanotrophy (page 9292, lines 9-15). I feel that such conclusions fall outside the scope of the present laboratory work and should be omitted from the final version of the manuscript.

Detailed comments

The Results section (pages 9285-9286) could be more fluent, if less numbers were

inserted in the text. Instead, references to the figures showing the dynamics could be used for readability. It is hardly a simplification to express differences such as 8, 13, 43 or 675 times higher.

The statement on lines 4-7 (page 9291) repeats what has been said earlier and is perhaps not needed.

Interactive comment on Biogeosciences Discuss., 7, 9273, 2010.