

Interactive comment on “Simultaneous high C fixation and high C emissions in *Sphagnum mires*” by S. F. Harpenslager et al.

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Dear Sir/Madam,

We thank you for your critical review of our manuscript. Please find our responses to all separate points raised by Referent #1 below. Furthermore, we have attached a pdf of our revised manuscript (with revisions indicated in blue) and an additional supplementary figure. We feel that comments of Referent #1 were highly relevant and have helped to improve our manuscript considerably.

Yours sincerely, on behalf of all authors,

Sarah Faye Harpenslager

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Interactive comment on “Simultaneous high C fixation and high C emissions in Sphagnum mires” by S. F. Harpenslager et al. Anonymous Referee #1 Received and published: 16 April 2015

The manuscript investigates the relation between soil pH and Sphagnum moss population in transitional mires using a mesocosm experiment. Species succession and changing soil properties are timely research subjects relevant for Biogeosciences. The manuscript is well written but I have some concerns about the data analysis, which will have to be addressed before the manuscript can be considered for publication.

We thank Referee #1 for his or her critical review of our manuscript. The highly relevant points raised by this referee have been carefully considered and we feel that this has improved our manuscript considerably.

Comments: 1. P. 6: explain why you grow species of Sphagnum on a substrate they do not favour in natural conditions.

Transitional mires are peatlands that are characterised by a transition from buffered to acid conditions. In these systems, Sphagnum species are ecosystem engineers and as such responsible for the production of acid. While some species, such as *S. magellanicum*, are more sensitive to HCO_3^- and will thus grow towards the more acid part of the system, other species, such as *S. squarrosum*, are more tolerant of buffered conditions and typically start the acidification process and succession towards more acidic vegetation types. Heterogeneity in the system will therefore result in diversity in the Sphagnum species pool, as is illustrated by the habitat preferences of these species (Clymo, 1973; Vitt and Slack, 1984). We chose our species based on the natural gradient that exists in transitional mires. There are many studies that present examples of such heterogeneous systems, where different Sphagnum species are present, including Kooijman and Bakker (1995), van Diggelen et al. (1996), Bragazza and Gerdol (2002) and Hajkova and Hajek (2004).

2. Line 199-204, P. 11: I do not understand the correction factor applied to the C-fluxes

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based on the ratio of the surface areas of the mosses. You assume that all growth occurred horizontally, which is probably not the case. What you measure is the net flux per chamber volume and you must clarify what this is an underestimate of. If the aim is to quantify the increase in C-fluxes due to changes in biomass, you could consider normalising the fluxes at time 0 (t_0) and time 1 (t_1) with the bulk volume of the mosses at t_0 and t_1 respectively. Using the ratio of surface areas most probably over-estimates the fluxes you are trying to characterise.

We regret that our description of the correction factor was not clear in our Method section. By mentioning that the growth occurred horizontally, we mean that the natural vertical elongation of moss fragments causes them to fall sideways in our setup, since the moss patches were too small to provide support for long fragments. The entire moss patch should, as it were, have been erected and would then have fit underneath the chamber. During measurement, however, only part of the long fragments was present in the chamber, which means that the total flux of the patch with long Sphagnum fragments (*S. squarrosum*) was underestimated. We thus corrected for this underestimation by multiplying with the area covered by all these moss fragments. We have made this more clear by replacing “expansion occurred partly horizontally” in the Methods section with: “elongated moss fragments fell over, causing an apparent horizontal growth”.

3. Line 199-204, P. 11: these numbers must be revisited in the light of the previous comment.

We hope to have clarified the confusion concerning our correction factor in question number 2 and have, consequently, not altered our calculations.

4. Line 358, P. 19: You forget the plant respiration term (the biomass increases, and hence the respiration too).

We fully agree with the referee that we should include plant respiration here. We have therefore changed this in the revised document. Although we cannot present actual

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respiration data for our experiment, we have estimated the contribution of respiration to the total C emission by using literature data. In literature, Sphagnum respiration ranges from 12 to 31 % of photosynthetic C fixation (Haraguchi et al., 2003; Laine et al., 2011; Kangas et al., 2014). From this range we have taken the maximum value (31 %) to determine a conservative estimate of the C emitted as HCO₃-derived CO₂. and present these values in Table 4 in the revised manuscript.

5. P. 17: you use C and CO₂ inter-changeably. Please, change to CO₂, especially when defining the terms of eq. (1) which all seem to have to do with respiration, etc... Not sure where CH₄ fits in.

We agree that using both C and CO₂ inter-changeably causes confusion. In the revised document, we now first discuss the CH₄ fluxes, which we compare to ranges from literature. In the rest of the discussion, we then focus on CO₂-fluxes, since these show the most relevant results of our study. To avoid confusion, we have now made clear in these paragraphs whether the C fluxes we discuss concern CO₂, CH₄ or both.

6. Line 367-370, P. 18: you omitted the temperature dependence of the fluxes (e.g. respiration follows an exponential relationship of the form $R = a \exp(bT)$). You may find that the daytime fluxes > night-time fluxes due to this temperature dependence. This might give rise to a “residual” respiration term, which would then feed into the “C” term of eq. (1) and thus overestimate it.

We thank the referee for remarking on the importance of temperature dependence in C-dynamics. Although the temperature in our set-up was fairly constant due to the use of a temperature controlled water-bath, daytime and night-time temperatures differed slightly due to the heat generated by the lamps during daytime (reaching maximum daytime temperatures of 23 °C). This indeed increases the confidence interval of our “conservative” (see point 4) estimate of HCO₃- derived CO₂. Although assumptions (point 4) and the likely higher respiration rates during the day impede an exact quantification of this HCO₃- derived CO₂, the fact that we find CO₂ emissions during the

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day in growing Sphagnum patches clearly points out that there is a considerable chemical CO₂ source. To acknowledge this uncertainty we have added “Although the likely higher respiration rates during the day and the use of estimated Sphagnum respiration impede an exact quantification of factor C, the fact that we find CO₂ emissions during the day in growing Sphagnum patches clearly points out that there is a considerable chemical CO₂ source” to the discussion.

7. The values of C-fluxes in the rest of the manuscript must be revised in light of the previous comments.

We now point out that the exact magnitude of the HCO₃⁻ derived CO₂ flux should be interpreted with caution, due to reasons mentioned above.

8. Overall, CH₄ fluxes seem underused or amalgamated into a general C-term. It would be interesting to treat CH₄ and CO₂ independently and compare them to literature values.

We agree that the contribution of CH₄ to the total C-flux could be emphasized more. We have therefore added the range of CH₄ emissions that were measured in our experiment in our Results and Discussion section, and have compared this range to data reported in literature in the revised manuscript. For CO₂, this comparison to literature data was already included on P20, L388. To include the effect of these emissions on the climate change, we now also include the global warming potential (GWP) of CO₂ and CH₄ to look at the effect of the greenhouse gas emissions from these floating soils covered by Sphagnum spp. in a climate perspective.

Specific comments: 9. Line 64, p. 4: in which way is Sphagnum “strongly influenced” by water?

We agree that we should specify this influence of water on Sphagnum. We have therefore changed the sentence “As Sphagnum spp. lack stomata, water conducting tissue and roots, they are strongly influenced by the surrounding water (Robroek et al., 2009)”

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into “As Sphagnum spp. lack stomata, water conducting tissue and roots, their growth, nutrition and vitality depend on the chemical composition of the surrounding water (Robroek et al., 2009)”

10. Line 68, p. 4: again, qualify the “direct influence”.

We agree that we should specify this “direct influence of buffered surface water”. We have therefore changed the latter part of the sentence “. . . and the direct influence of buffered surface water in the moss layer is therefore relatively low” into “. . .and are fed by rain water, which accumulates in the top (moss) layer and dilutes the buffered surface water.”

11. Line 112, p. 6: move (n = 8) to the end of the parentheses block, which provides details of the monolith dimensions.

We thank the referee for this comment and have changed this according to his/her advice.

12. Line 118, p. 7: how was the organic matter content determined?

We thank the reviewer for spotting the missing method explanation on determining OM content of our soils and the Ca content. We have added the following paragraph to this section of our Methods: “The peat had an organic matter content of 92.7 ± 0.4 % (determined by loss on ignition; 3h at 550°C) and contained 3.6 ± 0.4 mmol kg⁻¹ fresh weight (FW) of Ca (determined by digestion of 200 mg of dry soil with 4 ml of HNO₃ and 1 ml of H₂O₂ using a microwave oven (mls 1200 Mega, Milestone Inc., Sorisole, Italy), after which digestates were analysed by inductively-coupled plasma spectrometry (ICP-OES iCap 6000; Thermo Fischer scientific))”.

13. Lines 123-125. P. 7: define “rich” in Ca-HCO₃⁻. Clarify whether the Ca-... rich water was replenished over the course of the experiment.

We agree that “rich” is not a clear definition, although we do also refer to the exact composition of the infiltrating water used in our experiment, which is presented in Table

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1. Here, we show that the infiltrating water, which is being pumped into the aquaria at a rate of 5 L week⁻¹. To clarify that this flushing of the water layer is being carried out with the treatment water, we have changed this sentence to: “The underlying water layer was subsequently refreshed with treatment water at a rate of 5 L week⁻¹ using peristaltic pumps.”

14. Line 132, p. 7: 16 hours of daylight; this doesn't seem realistic for the end of March in the NL!

While we carried out our experiment in March, it was conducted entirely indoors with artificial lighting. We simulated summer conditions for a period of three months, with temperatures and daylight roughly corresponding to the period of May until July. We have clarified this in the revised manuscript by adding the sentence: “This regime of temperature and light was chosen to mimic summer conditions”

15. Line 134, P. 7: it is unclear whether each vegetated monolith had all four species of Sphagnum growing on it or only 1.

We agree that this can be presented clearer. We have therefore added the word “together” to the sentence: On four floating peat monoliths, four different species of Sphagnum (*Sphagnum squarrosum*, *S. fallax*, *S. palustre* and *S. magellanicum*) were planted together.

16. Overall, the linkage between the body of the manuscript and the figures and tables is insufficient. Much of the tables and figures data is described only in their captions and insufficiently referenced in the text. This makes the latter seem data-poor.

We feel that we may indeed have underused the table and figure referred to in the Discussion of our text. We therefore elaborated more on the references to Table 4 and Figure 4 in the Discussion section. Furthermore, we included the CH₄ emission rates and elaborated on these measurements in the Results section of our revised manuscript, as suggested by the referee in point 8.

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62, 1409-1430, 1984.

Please also note the supplement to this comment:

<http://www.biogeosciences-discuss.net/12/C3071/2015/bgd-12-C3071-2015-supplement.pdf>

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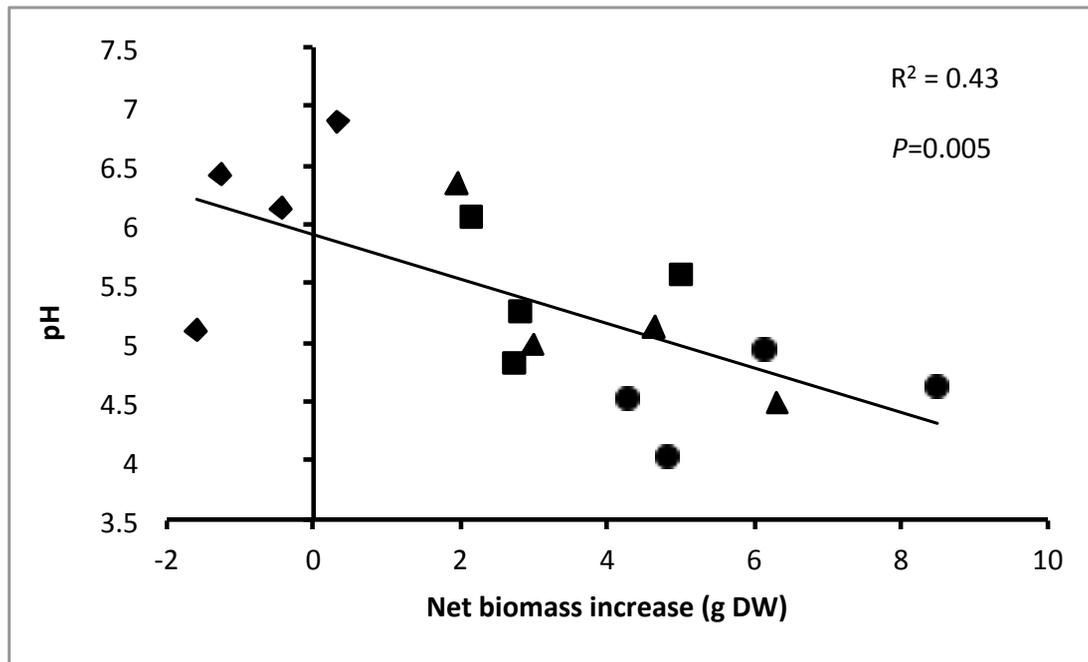


Fig. 1.

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