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Discussions

Interactive comment on "Impact of water table level on annual carbon and greenhouse gas balances of a restored peat extraction area" *by* J. Järveoja et al.

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We thank Referee #1 for the constructive comments and questions. The following revisions will be made in response to each of the comments (page and line numbers refer to the pdf version of the Discussion paper):

REVIEWER 1:

This paper evaluates the effect of the restoration of peat extraction site on carbon and greenhouse gas emissions. The authors compared three sites, one non-restored and two restored sites having contrasting water tables. Considering the importance of lim-

iting the release of greenhouse gases from such impacted ecosystems the chose topic is very interesting and falls well within the scope of this journal. The main message from this study is that peatland restoration is an effective way to reduce GHG emissions from these areas. Overall the paper is well written and the results are worth of publication.

Specific comments: P80 L23-25. "No study has investigated the impact of contrasting WTLs" I find this claim too strong, for example Tuittila et al. (1999) also looked at different water table and the effect on CO2 at the same restoration site.

Response: We incorporated the reviewer's comment and rephrased the sentence as 'To date, only few studies (e.g. Tuittila et al., 1999, 2004) have investigated the impact of contrasting WTLs on the subsequent ecosystem C balance within the same restoration site.' (P17180 L23-35).

Section 2.6. Were the measurements always carried out at the same time of the day? Did you check for diurnal variations, especially for CH4 at the vegetated sites.

Response: As described in the Material and methods section (P17185 L14-15), the gas flux measurements were carried out in random plot order to avoid diurnal effects on the fluxes when comparing different study sites. Within the time window for measurements (between 10:00 and 14:00), no diurnal variations in CH4 flux patterns could be detected, possibly due to the very low cover percentage (<1%) of aerenchymous plants in the restored treatments. Given the random sampling order, we believe that our estimates of annual GHG budgets were not biased by the timing of measurements.

Section 2.7. Can you specify how many fluxes were discarded after "filtering" of the data?

Response: Out of the total number of individual collar fluxes 11% of NEE, 9% of RE, 21% of Rh, 33% of CH4 and 6% of N2O fluxes were discarded based on the quality criteria. This information has been included in the revised manuscript 'Based on these quality criteria 11% of NEE, 9% of RE, 21% of Rh, 33% of CH4 and 6% of N2O fluxes were discarded from subsequent data analysis.' (P17188 L10).

P89 L26. Did you use the mean fluxes over the year (or growing period) or the individual fluxes? I think the percentage vegetation cover (which is only one measurement) should be related to the annual fluxes only and not to the individual fluxes.

Response: We agree with the reviewer's comment and confirm that we used the mean fluxes over the growing season for each collar in the correlation analysis. We clarified this 1) in the Material and methods section of the revised manuscript 'Pearson's correlations were used to investigate the effects of vegetation cover on mean growing season fluxes.' (P17189 L25-26), 2) in the Results section of the revised manuscript 'The differences in mean growing season NEE, GPP, NPP and Ra . . .' (P17193 L10) and 3) in the Table 5 caption of the revised manuscript 'Correlation coefficients of vegetation (bryophytes and vascular plants) cover (%) with mean growing season CO2 fluxes including the net ecosystem CO2 exchange (NEE), ecosystem respiration (RE), gross primary production (GPP), net primary production (NPP) and autotrophic respiration (Ra) and with mean growing season methane (CH4) and nitrous oxide (N2O) fluxes in . . .' (P17212).

P94 L11 typo, "was lower"

Response: We have corrected this mistake in the revised manuscript (P17194 L11).

P97 L18-20 The mean WTL in res-H and res-L was -24 and -31cm so I don't find it surprising to measure such low CH4 fluxes. It is likely that most of the CH4 produced was oxidized by methanotrophs in the upper layer of the soil. How does the water level in the restored area compare with natural peatlands? Was the restoration successful to restore natural hydrological patterns?

Response: We agree with the reviewer's comment that the low CH4 fluxes are likely a result of the relatively low mean WTL. We have added the following sentence in the

Discussion section of the revised manuscript 'Overall, the CH4 emissions observed in the restored treatments were small which was likely caused by the relatively low mean annual WTLs of -24 and -31 cm in Res-H and Res-L, respectively, resulting in lower CH4 production and increased CH4 oxidation potentials.' (P17197 L18). The comparison of the restored sites vs the unrestored bare peat site shows that the WTL has been raised on average by about 15-22 cm. Nevertheless, the annual mean WTL of -24 cm in the wetter Res-H site and of -31 cm in the drier Res-L site is still deeper than the targeted mean of ca -20 cm and is also fluctuating more than in natural bogs. Considering that the acrotelm plays an important role in stabilizing the WTL, it will take more time until the Sphagnum moss cover in the restored sites becomes thick enough to be able to act as an acrotelm and thereby reduce the WTL fluctuation. Recent work by Karofeld et al. 2015 (Environ. Sci. Pollut. Res.) within the same restoration site has, however, shown that the re-establishment of bog vegetation, specifically Sphagnum mosses, has been successful. Thus, it can be said that the conditions in the restored sites are adequate to support the development of a new acrotelm which will gradually lead to the creation of hydrological patterns similar to natural bogs.

Fig. 3. Do you have an explanation for the peak in methane emission in December 2014? Strange considering that the temperature was close to zero.

Response: During the time of this peak CH4 emission, a rapid drop in the WTL occurred while the soil temperature at 20 cm depth was still at $\sim$6 °C which is sufficient to enable CH4 production. This phenomenon has been previously observed also in other studies where large episodic CH4 fluxes have been reported after rapid drops in the WTL (e.g. Windsor, Moore and Roulet 1992; Moore and Dalva 1993). We have incorporated the explanation of these CH4 peaks in the Discussion section of the revised manuscript 'The autumn peak emissions observed in all three treatments might have occurred due to a concurrent rapid drop in the WTL during which CH4 may have been released from the pore water and emitted to the atmosphere as shown in previous studies (e.g. Windsor, Moore and Roulet 1992; Moore and Dalva 1993).' (P17197

L18).

Fig. 5. So the minimum VWC was recorded at Res-H? How do you explain that?

Response: Figure 5 shows the regression of N_2O fluxes to the VWC measured during the sampling sessions. Thus, this figure does not show all measured VWC values since some fluxes were rejected due to bad quality and their corresponding VWC are therefore not included in Figure 5. The lowest VWC was in fact measured in the drier Res-L (0.3 m3 m-3) although this is not clear from Figure 5.

Interactive comment on Biogeosciences Discuss., 12, 17177, 2015.