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Interactive Comment

Interactive comment on "Greenhouse gas fluxes in a drained peatland forest during spring frost-thaw event" *by* M. K. Pihlatie et al.

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We would like to thank the reviewer for carefully reading and suggesting improvements to the manuscript. Our responses to the comments are written below.

Specific questions and notes: In the introduction the authors declare their measurements aim ": : :to estimate the net GHG exchange and the importance of different C and N flux components on the total GHG balance: : :" (6114:15); the terms "GHG balance" and "radiative forcing" can be found all over the manuscript. Unfortunately the estimation for the total GHG balance in any form (CO2 equivalents, radiative forcing, whatever else) is given neither in the text nor in the table. Certainly, any form of GHG balance estimation can be discussible, but it may be recommended to choose one and use it as a numerical prove of the conclusions (i.e "Fluxes of CH4 and N2O contributed only insignificantly to the GHG balance" – 6132:25, for the moment this sentence is baseless). Table 1 may be a good place for these numbers.

Response: We added the GHG balance in terms of CO_2 equivalents in the table 1.

Chamber measurements. What was the reason for not measuring CO_2 by manual chambers? Or may be this data is available but chosen to be excluded from this publication? Manual chamber CO_2 fluxes might be interesting not only per se, but also as a quality indicator for CH_4 and N_2O data. The authors state "The development of gas concentrations inside the chamber during an enclosure period was linear" (6119:15) – was it always the case? Which criteria (R^2?) was used to check the concentration data quality? Was any low-quality data filtered out? Was there any prove that the linear regression is the best to be used for the flux calculations?

Response: CO_2 concentrations were not analyzed from the gas samples collected from the manual chambers. The CO_2 fluxes were measured by different manual chambers and that data is aimed to be published in another publication. Chamber enclosure times were optimised so that the concentration change inside the chamber remained linear during the measurement. In the linear regression we used R^2-value of 0.7 as the limit of acceptable data. To test for the best calculation method, we will compare the fluxes calculated based on a linear regression to the fluxes calculated based on a quadratic fit. This test will be done for the manual chamber data and the best flux calculation method will be applied to all the data.

EC measurements. What software was used for EC calculations?

Response: The eddy covariance data acquisition above the forest canopy was carried out by a LabView-based program BARFLUX. The sub-canopy EC data was processed using an eddy covariance software that has been developed by the Micrometeorology group at the University of Helsinki, Department of Physics. We also added more details of the data processing of the EC data as requested by the referee 2.

Soil measurements. The gas sample cups seem to be very big. When sampling 105-110 ml of gas phase from the cup, the same amount of gas from surrounding soil should replace it. If the porosity is, say, 10% it means degassing of about one liter of soil (a sphere with diameter >12 cm). The cups are placed at 5, 22 and 45 cm (6118:5), so in theory may affect each other. Were the cups sampled always in the same order? The same time between? Is there any evidence that 1-2 weeks of exposition is enough to restore the natural concentration profile after so massive disturbance? Why soil CO2 concentrations were not analyzed? Or may be this data exists but chosen to be not reported?

Response: The gas collection cups were placed horizontally approximately 10-20 cm apart from each other. Hence, their effect on each other was minimized. The cups were sampled in irregular order as they were considered not to affect each other. As they were horizontally 10-20 cm and vertically at minimum 15 cm apart from each other, diffusion did not disturb the concentrations in the gas collection cups.

The characteristic time of diffusion is the order of Z2/D, where Z is the distance and D is the diffusivity. As an example for the diffusion of CO2 in humus (organic material similar to peat) this yields an order of magnitude estimation of (0.15m)*2 / 9 x 10-6 m2 s-1 = 33333 s (9 hours). This time is much shorter than the sampling frequency of 1-2 weeks at the site. Naturally in water saturated conditions, as most of the time at the depth of 45 cm at Kalevansuo, diffusion is much slower and the stabilization time for gas concentration profile takes several times longer than estimated above. Hence, the gas samples taken at 45 cm depth represent average concentrations over several days prior to the date of sampling, whereas, the gas samples collected from the 5 and 22 cm depth represent mean concentrations over few hours. CO2 concentrations in the gas samples were not analyzed and hence we have no CO2 concentration data from the soil profile.

Text corrections: response 6124:25 - Figs. 5b and 6b were probably meant instead of 4b and 5b: corrected 6125:5 - Fig. 6b was probably meant instead of 4b: corrected

BGD

6, C4558–C4562, 2010

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Figures. In contradistinction to the text, which is written in general very well, the figures seem to be prepared in a rush. The authors would be suggested to: 1) Carefully analyze what is the aim of every single figure, and what data is necessary and sufficient to achieve it. For example, why the plot of soil temperature is linked to plots of CO2 fluxes (Fig.3), water table – to CH4 (Fig.5), and soil and air temperature – to N2O (Fig.6), while Fig.2 contains all the same environmental data at one graph? 2) Find a single style across the figures, making easier to perceive them one after another. For example, automatic chambers and their data are abbreviated as AC at Fig.1, but AutoChamb at Fig.3 and Fig.6; the same thing is called "tall eddy covariance mast" (Fig.1), "eddy covariance above the forest canopy" (Fig.3) and "above canopy eddy covariance" (Fig.4). The water table data is represented by dots at Fig.2, but by a line at Fig.5. The same symbols and colors are used for different things at 3 parts of Fig.6. 3) Tune the graphs to the best possible emphasis for black-&-white figures, if b&w is chosen. The circles of the same size, filled by different levels of grey, are hard to distinguish (Fig. 3-6). The perception can be much clearer if different shape symbols are used; in many cases the size of symbols can be increased as well. Fig.1 - would be good to mark Ac and Mc by different symbols, or sign all 4 Mc circles – othervice three unsigned Mc may be taken as Ac or whatever else. This figure can be also used to mark the towers footprint and the location of soil measurements. Fig.2a (and 6d) – the temperature lines are really hardly readable! Some other representation may be better, for example daily min-max filled areas.

Response: We reorganized the graphs so that we removed the plots of soil temperature from the Fig 3., water table depth from the Fig 5., and soil and air temperatures in the Fig 6. Also, we harmonized the styles, abbreviations and labels in the figures as suggested, and also checked that the abbreviations in the text and figures were identical.

Fig.3 – is it really informative to show both a) and b) graphs? May be only one of them is sufficient?

BGD

6, C4558–C4562, 2010

Response: We removed the curves of cumulative fluxes as they are presented in the new version of table 1.

Fig.4 – the legend is missing, is it the same as for Fig.3? Why error bars at Fig.4 show standard deviations, and at Fig.5,6 – standard errors? What do they show at Fig.3?

Response: The missing legends were added to the Fig 4., and the error bars were changed to standard errors in all the figures.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/6/C4558/2010/bgd-6-C4558-2010-supplement.pdf

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Interactive comment on Biogeosciences Discuss., 6, 6111, 2009.