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Discussions

Interactive comment on "Can a bog drained for forestry be a stronger carbon sink than a natural bog forest?" *by* J. Hommeltenberg et al.

Anonymous Referee #4

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The manuscript Hommeltenberg et al. reports very interesting dataset of two-years eddy covariance measurements of CO2 fluxes conducted at two different forested peat bogs with different land use history, but with the same genesis, climate and age. The paper is of high scientific importance, as comparative studies of pristine and nearby-located natural forested bogs are rather rare in literature. The authors came to conclusion that 44-years old forest plantation of Picea Abies and Pinus silvestris on a previously drained bog are stronger CO2 sink then the near-pristine forested bog, with a big age span of Pinus mugo (from sapling to 150 yr). The differences in age of both ecosystems (the natural one have already reached a climax state, while the drained one is still in a phase of dynamic growth) should be well addressed in the paper, as it might have impact on the analyses performed and conclusions. Although analyses are performed based only on two-years of measurements, which might lead to mislead-

ing, uncompleted conclusions, I consider this dataset long enough and scientifically extremely interesting to be presented in BIOGEOSCIENCE. However, this is very pity that the methane fluxes were not estimated for both sites based on EC or chamber measurements, as to have overall impression about a total carbon budget of both sites, this would be very crucial. As the authors are focusing only on CO2 budgets, I would not find it critical to include CH4 in a reviewed paper. The paper is very well structured and is written with very good, fluent language.

I have some concerns and a number of suggestions, that I believe will improve this manuscript once addressed.

Major comments: 1) The analyses of CO2 budgets for both sites are based on measurements conducted with two different EC approaches, which may biased the calculated CO2 budgets. Were these two systems compared at the beginning of experiment to demonstrate and prove that the fluxes are comparable? You may also add a sentence supported with some reference that these EC systems are working comparably. Although both systems have different advantages and disadvantages, the data loss should be higher in case of LI7500, especially during wintertime, but also in summer when raining, hence fluxes estimated for natural site for these periods of the year might be more biased as they are more related to modeled values.

2) The authors refer (Page 2194 lines 25-28, and page 2204, lines 25-30) to the old map of peat depth from 1940 concluding that due to anthropogenic activity the peat layer decreased by 1 meter within the last 70 years. And based on this assumption the estimated total carbon loss from this bog within 44 years between 1967 and 2011. Although this might be a truth, I would not trust so much the old maps, as we do not know how they were prepared and what is accuracy of a peat layer depth assessment. As this is uncertain I would use e.g. expressions as" potential peat loss" or "possible peat loss" on Page 2194 lines 25-28). Are the same maps available for natural site? What they would tell us about a peat depth in 1940? If the maps are available, than I would mention in the site description about the average peat depth at Schechenfilz site

in 1940. If these values are comparable, then the analyses conducted at the drained bog would be more strong and trustful. This imply also later analyses conducted in chapter 3.5.

3)Footprint analyses were not performed for natural site (Page 2196 lines 24-26). As Authors do not mention about the height of the EC installation above canopy level (we know only the height of the towers) we do not have good impression about how big the footprint might be for the site. What is more, we do not know even what is the prevailing wind direction. The height of EC tower at the natural site is 6 meters (in average 4 meters above canopy level) while in the drained site – 30 meters (9 meters above canopy level). Hence: 1) first specify what is your area of interest at natural site – looking on figure 1a we have bog-pine forest nearby the tower surrounded by single pine trees area– is this area also considered? If yes, please write it clearly in the methods section, otherwise, if we consider only bog-pine forest, then, according to my estimation, we have from 50 to 250 meters to the edge of this area. That means that your footprint should be bigger, assuming that EC system is installed 4 meters above canopy level. The same refer to the drained site, where we may suppose that EC system is 9 meters above canopy level and there is only 300-350 meters to the bog border in the west-east direction (but here luckily the footprint analyses were performed and data are filtered). Please deliver information about the footprint size and prevailing wind direction (wind rose) for both sites in the methods)

Other minor comments and suggestions:

Page 2193 lines 10-15 from the description of the natural site we know that the northern part of the natural site was affected by cutting and restored in 2001? How big was the degraded part of the peatland, how the site was restored and how far is this area from the footprint area of EC tower. This is just a question, If this restored peatland area may affect the measured fluxes? I think it might be useful for the interpretation of the data to add the main footprint area to the figures of both sites.

Pages 2193, 2194, 2195, LAI – how LAI was derived? If it was derived just from measurements conducted with optical method (based on Sunscan DELTA-T system), as it is written in page 2195, then the values given in the paper are related to an effective Plant Area Index (PAI), which includes foliage but also branches and stems. To derive LAI you need to subtract the area of branches and stems from PAI, or which is more reasonable – to change LAI to PAI in the paper. Another issue is, if the PAI values presented in the paper are related to trees only, or includes also sedges etc. The optical method used by the author assumes that PAR is measured just above the soil surface (in lowest part of the canopy) and above canopy level (where the reference PAR measurements should be done). My question is, how it was measured in Mooseurach site, where trees have 21 meter height? Can it be clarified in the method section? The paper of Chen (1996) may be useful here as a reference. Chen, J.M. (1996) Optically-based methods for measuring seasonal variation in leaf area index in boreal conifer forests. Agricultural and Forest Meteorology 80, 135-163.

Page 2195, lines 2-6, from this we know how high the towers are at both sites, but there is not clear at which height the EC systems were installed, at the top of the tower, how many meters above canopy level? please clarify it

Page 2195, line 11-12 was a steel tube heated or not? I know that this has no importance in case of CO2 fluxes, but lack of heating might impact H2O fluxes measurements

Page 2195, lines 14-20, 1) what was the height of T/RH, and PPFD sensors installation? 2 meters above the surface, or above canopy, or near the surface? 2) where the rain gauge were installed? Above or below canopy? Only one rain gauge was used per site- if it was installed above canopy – then it would be fine, but if it was somewhere inside forest canopy then the results might be uncertain. If rain gauge was heated, please mention about it in the text. 3) Can you give any information about distribution of wells? 4) in case of LI190SL please kip in mind that this quantum type sensor measures Photosynthetic Photon Flux Density (PPFD), which has units of quanta (photons) per unit time per unit surface area and not just PAR in watts/m^2. This implies that PPFD and not PAR should be used in the text and in figures.

Page 2195 line 21, 1) I think it should be "indicating" instead of "integrating" 2) once analyzing the CS616 data You should consider that the sensors were originally calibrated for mineral soils, hence the measurements conducted in peat might be highly uncertain, especially in natural site, where the average WTD level was higher than 10 cm below the peat surface (3/4 of TDR was permanently in water) From the manual of the Campbell CS616 we know: " These coefficients should provide accurate volumetric water content in mineral soils with bulk electrical conductivity less than 0.5 dS m-1, bulk density less than 1.55 g cm-3, and clay content less than 30%."

Page 2195 line 25 , 1) use 0.1 m instead of 10 cm; 2) use "thermistores probes" instead of "T-107-probes" and put T-107 to brackets. This is really a pity that peat temperature was measured only at one depth at Schechenfilz site, where there was observed rather high WTD level of a few cm below the surface. That means, measurements of T refer mostly for the water saturated peat layer for most of the analysed period. In fact, this may result in a low daily and seasonal variation of T, which might be used to Reco estimation. If yes, then I suppose that Reco fluxes might be underestimated for this site, what finally may bias the estimation of GEP.

Page 2196 line 16, data coverage of 91% for Mooseurach and 71% for natural site… is it correct, considering longer periods with gaps and more periods when Mooseurach station was not working correctly I would say that it should be in the opposite.

Page 2197 lines 18 –number of missing values might be slightly misleading, as only 9% of data are missing at drained site and the other 29% were rejected at this site because they were outside the footprint area

Page 2198 line 10, -1) use 0.1m instead of 10 cm. 2) As mentioned before- I would be careful with using 10cm depth peat temperature to model Reco at natural site, as T sensor was in a water saturated peat layer by most of the year. How justify to use this T, considering that most CO2 is produced in the near-surface unsaturated peat layer

(when soil respiration is considered) and from autotrophic respiration of plants?

Page 2200, lines 18 and 26, please explain how GPP and Reco were normalized with LAI? Please consider my previous comment related to PAI. I am not sure, but I would assume that in the drained site the ratio of foliage area to stem and branches area might be different (smaller) than in the natural site, hence the GPP and Reco normalization procedure may lead to biased results.

Specific suggestions:

Page 2193, line 15, - delete [LINK]

Page 2198 line 15, Please verify the To value. In Lloyd and Taylor (1994) equation To equals 227.13oC

Page 2198 line 20, I would suggest to use "by the same fitting parameters", instead of "correlation coefficients ", which is not correct

Page 2199, line 9; R2 is a determination coefficient, and not correlation coefficient Page 2199, lines 23-25, as the analyzed 12-month periods are not related to the calendar year I would suggest to use word "period" instead of "year"

Page 2200, line 15, 1) use PPFD instead of PAR, the relationship is between GPP and PAR, not between PAR and GPP, please change the order

Page 2200, lines 21,27 please add " for both analyzed periods respectively" Page 2201, lines 20,23; I would suggest to use "period" instead of "year" Page 2202, line 3, add "net" before "uptake" Page 2202, line 16, 19, 20 is it "net" uptake? Page 2205, lines 15-20, convert tC ha-1 to gC m-2 to be consistent with other values given in the text

Page 2205, line 15 – shall it be gross uptake? Page 2205, line 19, add "net" before "uptake Page 2205, line 27, PAI instead of LAI

Comments to figures and tables:

Fig.1. 1) I would increase slightly the font size of legend, 2) why there is a word “Key” before names of the sites? Is it colloquial name reflecting the shape of the site? If yes, I would recommend to not use it. Fig.3 . 1) Please add the height of air T measurements, 2) depth of soil T measurements, 3) use volumetric water content (%) instead of water content – here you should add that this is the average for 30 cm upper peat layer, 4) average WTD Fig. 4. 1) Are the figures 4a and 4b related to the same periods of 2011? I assume not, at the same PAR in winter and summer GPP should be different. As it may lead to confusion, please explain it in the figure caption. 2) specify the height of Tair measurements. 3) use PPFD instead of PAR 4) why not to use 30 minutes averages, instead of averages of 100 half-hourly measured values? Fig.5 . please explain what you mean by “fraction of expected Reco” – is this REco normalized with temperature? Fig. 6 . I would suggest to use “period” instead of “year”

Table 1. I would suggest to calculate sum of precipitation and average T and RH for 12 month periods analyzed in the paper between July 2010 to June 2011, July 2011-June 2012. This would be more useful for the interpretation of the data. Use RH, instead of rH

Interactive comment on Biogeosciences Discuss., 11, 2189, 2014.