

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 #5

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This paper presents an interesting comparison of NEE at two sites located close to each other in southern Germany. One of the sites is pristine, while another is defined as “a drained peatland forest”. Base on eddy covariance measurements done for two years at both sites, the drained site is a stronger CO₂ sink when trees are accounted for. In addition to the NEE measurements, the authors have estimated the long-term contribution of tree CO₂ fixation and peat degradation from which they give the value of peat C loss uptake during the last 44 yrs for the forest. They conclude that, despite of the present CO₂ sink at the drained site, the peat has lost a significant amount of C in the history.

The subject and theme of the paper falls well within the scope of BG. The obtained results are highly valuable for the C flux and peatland carbon cycle science community.

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The study is nicely planned, the results extremely interesting, and the paper is well written. It was a pleasure to read it. I have, however, some criticism against some parts of the paper.

First of all, I agree with the opinions from some of the earlier reviewers that the measurement setup of using two different IRGAs is problematic, and the possible systematic error attributed to this should be tackled somehow. The minimum requirement is to make the so called “Burba correction” to the fluxes measured with the open path analyzer, mentioned earlier. Similarly to some others, I also agree that the authors should try to quantify the impact of the storage effect, particularly at the drained site with a tall canopy.

My main worry related to this paper is linked to the interpretation of the C sink/source status of the drained site. The conclusions based on the peat subsidence and the tree growth rate, interpolated for the last 44 yrs, are vague. It seems that the authors have not measured the tree growth, which may vary a lot, depending on, e.g., age, soil conditions, climatic conditions. Therefore, I think that it is not possible to say for sure if the peat soil is currently a sink or source of CO₂ (the ecosystem definitely is, but the peat - we do not know). The tree growth rate (and the uncertainties associated to the method) should be determined for this. The comparison to subsidence rate is interesting, but also there the used methods are questionable. See more detailed comments below.

In my opinion, the authors should remove the subsidence and tree growth part from the paper, at least from the main conclusions (and abstract), and concentrate discussing the results they have from direct measurements. The current tree growth rate would be needed to say anything about the present CO₂ balance of the peat in the drained site. The subsidence and tree growth discussion can be included in the discussion, but nothing related to current CO₂ balance can be justified based on those speculations. Please keep the facts and speculations clearly apart. I recommend publication of the MS after a major revision.

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More detailed comments: -p.2193 line 17: Which LAI? Total, projected or half of the total? What about the LAI of the sedges and heather?

-p. 2194 line 2: Could you please give the CN range here, and add them to Fig. 2 as well?

-p. 2194 more information on the peat cutting and agricultural history is needed. How much peat was removed during the peat cutting activity? How long was the site cultivated; from the beginning of the century until 1967? Was the site fertilized or prepared otherwise? All this history makes me think whether the site should be defined as “afforested (agricultural) peatland” or “afforested cutover peatland”, instead of “a peatland drained for forestry (which typically means only drainage, sometimes also fertilization).

-P. 2194, line 10: You mention that the site has suffered from a still high water table, but from line 16 onwards you highlight the impacts of good drainage. There is a slight conflict, please clarify.

-p. 2194, lines 13-14: Again, define which LAI you are talking about.

-p. 2194, line 20: how was the peat thickness measured? In how many points, and how were these distributed in space? Please give the SD. This variable is very important, as you are deducing the peat loss rate by observing the peat depth.

-p. 2194, line 23: Also here, please give the range of observed CN ratio

-p. 2202 line 13 onwards: what about Lohila et al. 2007 (Boreal Environ. Res.), that’s afforested site, isn’t it? Also that of Meyer et al. 2013 (in BG) could be added here, except if you are referring to only bogs, which you should state on line 13. For the more general discussion on the C balance in forestry-drained peatlands I might recommend adding the paper of Simola et al. 2012 (European J. Soil Sci.)

-p. 2202 lines 20-21: what do you mean by similar site conditions? The Finnish site had not experienced agriculture or peat cutting. Please clarify.

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- p. 2208 lines 10-14: what about the Burba correction, or the impact of storage fluxes, which were likely manifold in the drained site?

Chapter 3.4: The tree uptake has been given by referring to an unavailable document. If the tree growth is to be used in the main conclusions of the paper, it is essential that the biometry model and the assumptions used in it is described in the mat&met. This is a highly important number if one wants to say something about the current state of the peat soil CO₂ sink/source. Why not to calculate the present tree C uptake using the same model? How was the model calibrated for the Mooserauch site? Or is the presented value some kind of general mean for forests of this age? Did you make any biometric measurements at your site?

About the peat subsidence: the authors have made many assumptions which make the uncertainty (which was not estimated) of this calculation huge. For example, how can you know that the bulk density of the already disappeared peat layer has been the same you have found from the site now? Also, the bd values used are all from farmed peat soils, how well do they represent the forest conditions?

Technical comments: -p. 2205 line 8 → is/was based

-p. 2206 line 6 Saarino → Saarnio (not in the reference list)

- p. 2209 line 10: remove "and"

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

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