

Interactive comment on “Effects of land use intensity on the full greenhouse gas balance in an Atlantic peat bog” by S. Beetz et al.

Anonymous Referee #2

Received and published: 30 July 2012

General comments:

This paper presents 2-year GHG flux measurements on an atlantic peat bog with different management including wetland restoration. The measurements have been done with closed chamber technique in 3-4 week intervals. The subject and the experimental arrangement are interesting and the theme is relevant and well within the scope of the BG. The data seems to be novel. Despite the fact that the origin and background conditions of the three sites are not fully identical – the cultivation typically takes place in the more fertile parts of a peatland while the ombrotrophic centre is often considered too poor for farming and is hence left untouched – I think the experiment is as well planned as possible in this kind of studies, and the fact that all the three sites are part of the same peat bog complex guarantees a similar climate for all sites. This compari-

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son aspect is definitely a strength of this paper. Nevertheless, the authors could shortly discuss the fact that the comparison is not purely “blind”, i.e. the sites are of different fertility as indicated also by pH and C/N ratio in Table 1.

To my opinion, however, the presentation of the MS is a bit sloppy: it is missing a proper and logical use of terminology, signs and definitions, the values in different tables and text do not always match, and the text is difficult to follow because of that. I have three major points of criticism:

1) It seems that one main motivation of this paper is the impact of peatland restoration on GHG fluxes. Now the whole rewetting story is, I think, a bit hidden between the lines. You start the conclusion section by discussing the effect of rewetting, but before that not much has been discussed about it. Another example is found in the abstract, where you mention “restoration sequence” in line 7 and then rewetting suddenly on line 25. It is not before this conclusive sentence when I started to wonder if the impact of rewetting was one of the main research questions. This is then made clear in Mat&Met, but it should be highlighted already earlier. It should definitely be mentioned already in the abstract that one of the sites has been rewetted.

2) Regarding the NEE and net carbon balance, it was really difficult to follow the results. These two seem to be somewhat mixed in the text and tables. For example, in Table 2 you speak about NEE, but the numbers in NEE column do not match the sum of Reco and GPP. Instead, nearly correct NEE numbers are shown in Table 3, but the signs of the components do not follow the same logics as in Table 2 (C addition in the ecosystem is negative). In addition, the numbers in Ch 3.2 and Tables 2 and 3 do not exactly match. More comments regarding this can be found below.

3) The authors present an uncertainty analysis which is fine, and not yet regularly required in chamber flux papers. However, the current analysis only accounts for the standard errors of model parameters. This leaves out e.g. the measurements bias, and at least as importantly, the error related to the “gap-filling” of very long time periods (3-

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4 weeks). When using the chambers to measure NEE with such a low frequency as here, the real observations only cover a few percent of the annual period. For example, here the fluxes were measured 29 times during two years. Assuming that each day covers a 12 h period of measurements, the measured data covers on average 2 % of the year. As result, 98% of the temporal gaps need to be filled in with modelled values.

Were these measurements done with the eddy covariance method, such low coverage would have been highly criticized; nowadays most of the papers presenting annual balances by the EC method are required for an analysis of uncertainty, arising not only from the measurement uncertainties, but also from the gap-filling. Can we assume that the data covers adequately the different situations and environmental conditions, and that the response functions are valid for the whole period between two measurements? For example, it is typical for chamber studies that they take place on sunny days. This has a great impact on measured NEE through the VPD, which is one of the main controls on GPP. If one aims to produce an estimate of the annual CO₂ balance of a single site, I would not consider chamber method with 3-4 week intervals accurate enough. In this case, when the chambers are used for comparing three differently managed peat soils, I think the chamber technique is acceptable. However, I suggest that the authors present a thorough error analysis, trying to cover all important sources of error, not only the model parameters, which probably has a minor contribution. Furthermore, a short discussion covering the above-mentioned limitations of the used measurement technique would be useful.

In addition, I do recommend that the MS will be revised by a native English speaker. As my conclusion, the MS could be considered for publication after a major revision. A list of more detailed comments follows.

- In many places throughout the MS, change “neutrally” to “neutral”, and write “ghg” in capital letters

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Abstract

-Lines 1-6: rewrite. This is not the point you are bringing out in the paper. Rather start by, for example, saying something about the importance of restoration, what does it mean in wetlands, and what are the current trends in peatland management and rewetting in Germany and elsewhere. This is the place where you sell your paper for the potential reader. Please use it for justifying your work – why did you measure such peatlands, what are the gaps in knowledge, etc. Now you are not doing it.

Introduction: p. 6796 lines 26-29: most of the selected references do not actually support your statement very well: Kettunen et al. studied the CH₄ emission only, Alm et al. studied only the winter fluxes, Maljanen et al. 2001 studied the forest floor GHG exchange, which cannot be considered as full GHG balance of that ecosystem, Ojanen et al 2010 also studied forest floor fluxes, and for CO₂ only respiration. None of these

Results Ch 3.2 - Nothing has been mentioned about the amount of C exported from the site during the cuttings. You mention on lines 18 onwards about the CO₂ source but nothing is said about what is included in these numbers. However, in Table 3 you seem to have included also the harvests, but nothing is mentioned about that. Also, the C balances in chapter 3.2 and Table 3 do not match exactly, please correct.

- Please make a clear difference between NEE and NECB throughout the manuscript, according to Chapin et al which you already refer to in Mat&Met.

- line 24: bad English, rephrase

Ch 3.3 - line 8: “.estimated hourly methane flux...” this may give a feeling that you tried to establish a relationship between the estimated (=interpolated) fluxes and abiotic factors. I suggest replacing estimated with measured.

Ch 3.5 - use imperfect throughout the text

Discussion

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The first chapter of discussion on p.6805: - please do clarify the terms: make it clear when do you speak about CO₂ exchange (=NEE), and when do you discuss the C balance - I do not understand why the C balance is given for the calendar year 2008? The balance of 434 g C m⁻² seems a bit odd if one compares them to those of 2007/08 and 2008/09 (548 and 817 g C m² yr⁻¹, respectively). It seems to me that using the calendar year of 2008 is not representative for the whole 2-year period. It is also a bit confusing to have many different yearly estimates from different time windows. - please correct the spelling of the name, not Veenendahl. It makes me to question, why did you not calculate the net C balance of your site? Are there any biomass data available from your site? If not, you should take this into account when showing comparison to others' studies.

- p. 6805 line 23 - p. 6806 line 5: Why should higher frequency of cutting increase emissions? Perhaps it is not just the cutting, but the export of C from the field which increases net C emission. The field is producing a lot, but the biomass is taken away and cannot therefore contribute to the increase in the soil carbon in a form of new litter. Therefore the role of agriculture in controlling the C loss from peatlands is contradictory. If you produce a lot of biomass, then the cost of losing one gram of peat carbon is partially compensated by producing biomass. For example, comparing an annual and perennial crop on peat soil showed that when comparing only NEE, the emissions are much higher from an annual crop (spring barley) as compared to the grassland with two cuts (see e.g. Lohila et al. 2004, JGR 109, D18116). If one takes into account the harvests, i.e. the net C balance, the grassland really seems to have more negative impact on climate. This is of course absurd: the common sense says that it is the ratio of the produced biomass and peat C loss which should be looked at. In other words, if you cause an emission of, let's say, 100 g C m⁻² yr⁻¹ from the peat soil but at the same time produce forage at rate of 300 g C m⁻² yr⁻¹, this is much better option than causing the same emissions but only producing forage at a rate of 100 g C m⁻² yr⁻¹.

- p. 6807 lines 2-3: Why is CO₂ balance of +88 considered "neutral", but -148 as a

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source? Note also that in p. 6803 lines 20-21 you call values of -148 and 88 source and neutral. This conflicts with your earlier sign convention where negative indicates sink and vice versa. Please correct and check the sign throughout the manuscript. Also, use uniform terminology throughout the text (NEE / NECB).

- p. 6807 line 8: what do you mean by "cf. Fig 4", how should these NEE results be compared to CH₄ /N₂O fluxes? All over the MS the abbreviation "cf" is, I think, used unnecessarily often, and many of them could be removed.

- p. 6807 line 17: how did you judge about the significance? The NEE was actually lower in the second year, not higher, but the sink was higher. Pay attention to the correct terminology throughout the paper when discussing NEE, NECB, source, sink, etc.

- p. 6807 lines 18-23: how is this linked to your study? Are you trying to say that differences in water level height is the reason for the between-year difference you have observed? If yes, please rewrite this section and make it clear that you are explaining your observations. If not, this can be deleted. Moreover, in section 2.4 you mention that "we did not find any significant relationship between water table and Reco". It seems to me that these sentences are in conflict.

- p. 6808 lines 2-3: remove remaining

- p. 6808 line 20: " Höper et al indicated 19.4 g CH₄ from a peat bog. . ." bad English, rephrase

- Ch. 4.3 line 3: Alm, 1999 → Alm et al., 1999; 1.8 g m⁻² → 1.8 g N₂O m⁻². Note that the unit is different from that (g N₂O-N) used throughout your paper. There are actually much better and newer references available for this chapter, for example: - Maljanen et al. 2003 (Soil Use Manage. 19, 73–79) - Maljanen et al (Soil Biol. Biochem., 36, 1801–1808) - Augustin et al. 1998 (Biol. Fertil. Soils 28, 1-4) - Regina et al. 2004 (Eur. J. Soil Sci. 55, 591–599) - Regina et al. 2007 (Agr. Ecosyst. Environ. 119, 346–352)

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Ch 4.4 (and 3.5) - line7: speaking about the time horizon may be confusing here, since time horizon is an inherent part of the GWP concept. With time horizon, one typically refers to the length of the period during which the impact of a pulse emission is followed. Here, the time horizon used is 100 yrs, not 2 yrs. - It could be useful here to shortly discuss the limitations of the GWP method when applying it for wetland fluxes. See the paper of Frolking et al. 2006 (JGR, 111, G01008) and the discussion therein. For example, you could use two different time horizons to calculate the GWP, and discuss the implications for the GWP observed at different sites. - line 27: It is unclear, what are you here referring to with "both sites" - What is missing is the contribution of different gases to the total GWP

Ch 4.5 - line 4: "...fit of our NEE models..." - lines 10-20: * I would not say 3-4 week interval is very often. I would rather call this a relatively long interval, particularly during the growing season. If your site have 5 cuts from mid-March to start of October, this means approximately one cut in 5-6 weeks. It seems thus likely that there have been only 1-2 flux measurements between two consecutive cuts. This is definitely not very much. * Please add a thorough uncertainty analysis which, in addition to the other error sources, accounts for the error related to this linear interpolation - lines 21-28: this text belongs to the Mat&Met rather than in Discussion. Here, you should discuss the consequences of using winter+summer measurements when deriving the temperature responses. Such an approach has been criticized, since in summer – active ecosystems using the annual T-response overestimates the annual respiration; see Reichstein et al. 2005 (GCB 11, 1424–1439) - p. 6812, lines 4-7: how many of the grassland sites in Schulze et al 2009 are peatlands? Probably not very many. Please refer to some other paper(s) reporting aquatic C losses from peatlands.

Tables and Figures:

- Table 2: In some cases, $NEE = Reco + GPP$, in other cases there is a very small mismatch (rounding error?), sometimes NEE is far away from the sum of Reco and GPP. Here NEE should have similar meaning for all rows, please correct the table. I sug-

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gest introducing new columns for harvested/exported biomass and NECB (taking into account also CH₄). Also, explain in more detail the methodology behind the "GWP"; tables should be self-explanatory.

- Table 3: Please follow here the same sign convention as elsewhere in the MS (uptake by the ecosystem is negative, also fertilization, emission is positive, also harvest)

- Fig 4: please use different symbols for the manure addition, now they are similar with the flux symbols

- Fig 5: are harvests and fertilizations included in NEE here? They should be. In that case, do not use NEE here.

Interactive comment on Biogeosciences Discuss., 9, 6793, 2012.