

Author reply to the comment of referee #1

(Interactive comment on Biogeosciences Discuss., 10, 11283, 2013)

We are grateful for the valuable and detailed comments provided by referee #1. They helped us to considerably improve the manuscript.

General comments

We changed the title as suggested.

In the material and method section, we now give more details on GHG gas flux measurement and CO₂ modeling. We also provide additional figures, which illustrate specific aspects of the flux calculation approach as well as give examples of the Reco and NEE fits. We now also provide figures of the annual time series of N₂O and CH₄ in the supplement.

Specific comments

p. 11284 Line 14-16: 6: *“clarify statements that GHG balance is independent of water table level and that GHG emissions are linearly related to water table”*

We clarified this to: “The net GHG balance reached 7-9 Mg CO₂-C eq. ha⁻¹ yr⁻¹ on soils with sand mixed into the peat layer water tables from 14 cm to 39 cm. GHG emissions from drained histic gleysols (i) ... (ii) increase linearly from shallow to deeper drainage,”.

p. 11285 Line 4: *move references to end of sentence*

We moved references to end of sentence.

Line 7: *what is BÜK 1000? Richter, 1998?*

We removed BÜK 100 (=soil survey map, explained in references), since “Richter 1998” is sufficient to give the reference.

Line 8: *join sentences by ‘and’ to avoid staccato*

We joined the sentences with “and”.

Line 10-11: *more correct to limit the statement to: ‘caused emission of high amounts of CO₂.’*

We limited the statement to “causes emission of high amounts of CO₂ and often N₂O”.

Line 10: *‘causes’ instead of ‘caused’*

We replaced “caused” by “causes”.

Line 13: DCE (2012). *Please cite the report as Nielsen et al., 2012 [Nielsen, O.-K., Mikkelsen, M.H., Hoffmann, L., Gyldenkærne, S., Winther, M., Nielsen, M., Fauser, P., Thomsen, M., Plejdrup, M.S., Albrektsen, R., Hjelgaard, K., Bruun, H.G., Johannsen, V.K., Nord-Larsen, T., Bastrup-Birk, A., Vesterdal, L., Møller, I.S., Rasmussen, E., Arfaoui, K., Baunbæk, L. & Hansen, M.G. 2012. Denmark’s National Inventory Report 2012. Emission Inventories 1990-2010 - Submitted under the United Nations Framework Convention on Climate Change and the Kyoto*

Protocol. Aarhus University, DCE – Danish Centre for Environment and Energy, 1168 pp. Scientific Report from DCE – Danish Centre for Environment and Energy No. 19. <http://www.dmu.dk/Pub/SR19.pdf>

We changed the citation of the report as proposed.

Line 13-14: *suggest joining sentences by: ‘Such loss of peatland. . .’*

We joined the sentences by “Such ...”.

Line 16: *‘the dominant land use on peat soils in. . .’*

We changed this as suggested.

Line 19-20: *Note both cited studies represent laboratory studies with artificially changed GWL. Also note that Aerts and Ludwig (1997) generally found higher CO₂ emission from soils with high GWT – only as a response to weekly oscillating GWT did they measure higher CO₂ emission.*

We added “In laboratory studies, ...” and discarded Aerts and Ludwig (1997).

Line 23: *‘Histosols...’*

We corrected this.

p. 11286 Line 2: *for consistency use ‘deep’ rather than ‘profound’*

“profound” was replaced by “deep” in the whole manuscript.

Line 4: *the Danish report uses the value 12%, rather than 15%*

We corrected the Corg value to 12%

Line 16: *“please also consider the role of vegetation in this statement”*

In accordance with Reviewer #2, we removed this sentence.

11287 Line 2-3: *‘small-scale’*

Changed to “small-scale”.

Line 19: *Do the authors distinguish between effects of sand mixing to improve trafficability and sand mixing from ploughing into strata beneath the peat layer?*

We clarified that “sand mixing from ploughing into strata beneath the peat layer occurred”.

Line 20: *‘reached’ rather than ‘hit’*

We replaced “hit” by “reached”.

Line 22: *I guess Corg calculated from LOI was only used as indicative levels, since elemental analyses are mentioned later. But see Pribyl (2010) for a discussion on the so-called ‘van Bemmelen’ factor and its validity (Pribyl, D.W. 2010: A critical review of the conventional SOC to SOM conversion factor. Geoderma, 156, 75-83). Actually, Corg calculated from LOI was only used as indicative levels.*

We are aware that LOI can only be used to make a rough estimation of Corg and that the ‘van

Bemmelen' factor is only of restricted validity according to Pribyl 2010. We clarified in the manuscript that these values were only used for a first assessment.

11288 Line 5: indicate distance to the meteorological station

We added the distance.

11289 *Section 2.3 on GHG flux measurements should be expanded. Starting paragraphs of section 2.4 and 2.5 actually belongs to section 2.3*

We reorganized and expanded this.

Line 5-6: *'For CO₂ three sites were measured per day, i.e., including CmedW39, ClowW29 and ClowW14 on one day and ChighW11, ChighW22 and ChighW17 on another day'*

We changed the sentence as proposed.

Line 10-14: *this part belongs to the section on GHG measurements rather than modeling.*

We moved the sentences to Section 2.3.

Line 10: *What is the meant by diurnal cycles here? Is it from sunrise to sunset?*

We clarified "diurnal cycles of CO₂ fluxes ranging from sunrise into the afternoon and covering the full range of PAR and soil temperature" (GHG flux measurement section).

Specify the number of measurements achieved for each frame during the daily campaigns.

We added "During each campaign each frame was sampled by opaque chambers three to six times and by transparent chambers five to eight times depending on season and weather conditions." (GHG flux measurement section).

This will also be needed to evaluate the quality of Reco modeling. Neither model performance nor measured data are presented.

The quality of Reco modeling can be evaluated using the width of the confidence intervals e.g. in figure 2. During long periods of the time series confidence intervals of Reco are small. Only after cuts in autumn, they become larger which is also reflected in NEE.

We added figures with measured vs. modeled Reco as well as PAR data showing a good model performance (Fig. S4A and S4B in the Supplementary). In an additional figure we show exemplary a Reco and a NEE fit of a measurement campaign (Fig. S3A and S3B in the Supplementary). The Reco models of the measurement campaigns had a median R² of 0.98 and a minimum of 0.83. The NEE models of the measurement campaigns had an median R² of 0.97 and a minimum of 0.60. We added the model performance to the manuscript.

Line 15: *Non-linear models were considered for CH₄ and N₂O, so why not for CO₂?*

Since we measured NEE as well as Reco using short closing times (maximal three minutes), CO₂ fluxes could and needed to be calculated by linear approximation. In contrast, closing times for CH₄ and N₂O flux measurements were 60 minutes.

Line 17: *the exclusion of fluxes where temperature changes was >1.5°C could be problematic. These (NEE) fluxes would preferentially have been taken during the growing season, which has a large impact on the annual budget. The number of discarded fluxes should be stated, or, preferably, a simple scaled temperature function should be used to correct the effect of*

temperature.

We stated “Exclusion criteria of CO₂ fluxes were PAR changes larger than 10 % of the starting value and more than 1.5 °C increase in chamber temperature.” They represent the quality criteria during the measurement campaign in the field (now GHG flux measurements section). We extended “For cooling of the transparent chambers thermal packs were used.” Therefore, only very few measurements had to be discarded afterwards during flux calculation.

Line 25-26: I think the models used by Alm et al., 1997 are seasonal models including both water table and temperature as driving variables. Also, Drösler 2005 modeled Reco with a dataset from the entire year, but using only T as driver as water table as a variable did not improve the fit of his respiration equation. I think the authors should be specific on how and which models were used for their daily modeling. Indeed, I feel some confusion about the aspects of daily vs annual modeling.

Analyzing GHG data of managed peatlands, Lloyd-Taylor (1994) and Michaelis-Menten (1913) are commonly used for GPP and Reco calculations, respectively. Some adaptations to specific cases were accomplished e.g. by Falge et al. (2001) for GPP limitations. We extended the annual Reco fit of Drösler (2005) to a campaign specific one to gain a higher precision of Reco and GPP fluxes.

We clarified the modeling on a daily and annual basis by subdividing the 2.4 in four parts:

2.4.1 Raw fluxes (from p 11289 line 15 to p. 11290 line 2)

2.4.2 Response functions (from p. 11290 line 3 to p. 11291 line 20)

2.4.3 Interpolation to annual models

2.4.4 Error estimation (p. 11291 line 21 to line 23 and including p. 11293 line 16 to p. 11294 line 12)

11290 *Line 8: the Par correction curve shown in supplementary information shows a considerable scatter, impossible to capture by modeling. How confident is the authors that the meteorological station PAR data can be used to represent the on-site dynamics?*

The scatter of the PAR correction plot exaggerates uncertainty since 0.5 h mean values are plotted against values, which were measured on-site at a distinct time point. The meteorological station PAR data can be used to represent the on-site dynamics since it was in close proximity of the sites(100-150 m).

Line 18-21: the interpretation of e_0 (generally E_0) as activation energy and T_0 , 227.13 K as a temperature constant for the start of biological processes to me is somewhat misleading. E_0 is an ecosystem sensitivity coefficient (a temperature rather than an energy) and T_0 is a hypothetical zero- respiration temperature which in the LT model can be fitted but here is constrained to 227.13 K (so not a universal biological constant).

We changed “ e_0 ” to “ E_0 an activation like parameter (K)”, T_0 was chosen according to Drösler 2005 and Beetz et al. 2013.

More importantly, given the focus in the manuscript on the importance of GWT as driver, why was a model incorporating GWT not used in this part of the modeling?

The GWT effect is included indirectly by the repeated measurement campaigns. The site specific datasets are too small to model GWT explicitly and, in particular, to separate its effect from vegetation seasonality.

Line 18(?): *give Rref units as mg CO₂-C m⁻² h⁻¹, rather than CO₂-Cmg m⁻² h⁻¹. Change throughout in the manuscript*

We changed Rref units throughout the manuscript.

11291 Line 1-2: *How was it decided whether a temperature range was too small and how often was that the case? And what was actually the temperature ranges used for modeling? Please specify. I would expect an often rather low temperature range with inherent risk of stochastic variation influencing the goodness of fit.*

We did not decide whether a temperature range was too small. For two winter campaigns it was numerically not possible to fit Reco due to the small variability in the meteorological conditions (1°C temperature range in 2 cm). The temperature ranges were campaign dependent with an average range of 5 to 13 °C in 2 cm. We added this to the manuscript.

Line 8: *maybe write: '...according to a Michaelis-Menten type of equation modified by...'. No need to cite Menten and Michaelis (1913) here.*

We deleted the citation of Michaelis-Menten (1913).

11292 Section 2.5 *It seems only the calculation of individual fluxes is mentioned; information on how annual sums were derived should be included*

We added: "Mean annual fluxes were calculated by linear interpolation between measurement campaigns."

Line 1-5: *This paragraph belongs to section 2.3*

We moved this paragraph to section 2.3.

Line 15: *did you use the appropriate AIC with small-sample correction?*

The corrected AIC is defined as $AICc = AIC + \frac{2k(k+1)}{n-k-1}$, where n is sample size and k is the number of parameters. The HMR function is a three-parameter function and our sample size is n=4. Thus, there is division by 0 in the formula and AICc cannot be calculated. We use the uncorrected AIC and employ condition (d) to counter AIC bias.

Line 19-20: *Point (d) - so it could be argued that reverting to robust linear regression caused severe underestimation? How often did this occur?*

Using a linear model for a non-linear process such as gas-transport from soil, could indeed result in underestimation of the gradient, i.e., the flux. However, for a single flux this underestimation is usually quite moderate and the main problem is bias of mean or aggregated fluxes. Severe overestimation from the non-linear models occurs due to the small numbers of data points (n=4), which makes the non-linear fit sensible to measurement error. E.g., there is the specific situation where the flux is extremely low and all four concentrations are practically equal to ambient concentration. Due to measurement error it's quite probable that the first data point is farthest away from the 4-point-mean. The non-linear model will fit such a pattern excellently, resulting in a very good AIC and extremely high gradient at time point zero (i.e., flux). However, in reality such a pattern could only occur if diffusion is extremely fast and saturation (i.e., a concentration much higher than ambient) is reached.

Condition (d) is mainly used to guard against this situation. We have added a graph depicting a problematic concentration pattern to the manuscript (Fig.S5 in the Supplement). N₂O concentrations in the example were close to ambient and (by chance) the second to fourth concentration were almost identical and the first concentration was lower by 20 ppb (which is within typical measurement error). Robust linear fit equaled linear fit with flux = 13 $\mu\text{g N m}^{-2} \text{ h}^{-1}$, $p = 0.2$, AIC=-24. Shaded area depicts confidence band of the linear fit. HMR fit resulted in flux = 96 $\mu\text{g N m}^{-2} \text{ h}^{-1}$, $p = 2\text{E-}5$, AIC=-59. Since the HMR flux estimate is more than four times the robust linear flux estimate, we used the latter.

11293 Line 8-11: *Please specify why these medians of square roots 'demonstrates a sufficient accuracy of flux measurements'.*

Flux standard errors were calculated from the regression employed for flux calculation. After square-root transformation they followed a normal-like distribution. Median standard errors were 12 $\mu\text{g CH}_4\text{-C m}^{-2} \text{ h}^{-1}$ and 3 $\mu\text{g N}_2\text{O-N m}^{-2} \text{ h}^{-1}$. Judging from the empirical two-sigma rule to assess significant difference from zero, these error values demonstrate good accuracy of flux measurements.

11295: *Was there any dynamics in the Nmin contents? Since Nmin was measured on every gas sampling occasion data in Table 1 could be given with the SE estimates and n.*

There were some dynamics in the Nmin contents, so we extended table 1 with SE estimates and n.

Section 3.2: *Results for model performance are missing. Was the NEE model successful?*

As with the Reco model, the performance of the NEE model can be assessed by the confidence intervals of the annual time series in figure 2.

11296 Line 1-5: *Give reference to relevant figure for description of dynamics*

We added reference for description of dynamics.

Line 19: *Give also r for the correlation between GPP and Reco*

We added a mean R² over all sites of 0.67 (ranging from 0.60 to 0.77)

Line 23: *refer to Table 3, rather than Fig. 3, for this statement*

We changed reference to table 3.

11297 Section 3.3 and 3.4: *These sections are too succinct and should give better info on seasonal trends or observation of peak emissions.*

We added two figures S6A and S6B in the supplement showing the annual time series of CH₄ and N₂O fluxes. However, we didn't expand these sections much, since these fluxes were only minor contributors to the GHG balance.

Line 10: *How was it determined whether annual emissions or uptake of N₂O was significant?*
Since the standard errors of the annual fluxes were often larger than the annual flux itself, we interpreted the annual fluxes to be not significant. Now, we calculated the p value with a t-test for all sites which revealed a low but significant uptake on site C_{high} W₁₁.
In the manuscript, reported mean values and standard errors had been calculated by a Monte-Carlo simulation. We have now decided to change this to mean values and standard deviations of replicates, which changed the flux values only marginally.

Line 13-14: *Table column reads 3.3 to 8.6 (rather than 3.1 to 8.2 as cited in text)*
The values in the table are correct and therefore, we corrected the values in the text

11298 Line 23: *Klemedtsson et al. 2005 is cited for influence of CN ratio on methane emission; but to my knowledge this reference only concerns nitrous oxide emissions?*
The Klemedtsson citation was placed here by mistake. We moved it into the N₂O section.

Line 24: *use 'are in accordance with' rather than 'confirms'*
We adopted "in accordance with".

11299 Line 13-16: *can you quantify this statement on robustness of the interpolations?*
This is difficult to quantify. We added validation plots for illustration instead.

11301 Line 15-17: *comparing the results of Fig 6 and 5b in the same argument mixes the effects on Reco and NEE; in this case Fig 5 should be made with Reco as response variable rather than NEE.*
We reworded this to make it obvious that two different quantities are considered. However, we prefer to keep NEE in Fig. 5.

Line 15-26: *Reco represents both heterotrophic and autotrophic respiration, but the arguments derived seem to focus on the heterotrophic part. Indeed the influence of vegetation, which has been recognized in previous parts of the manuscript (e.g., p 11296, line 15), should also be invoked here. From the data in Table 2 and 3, regression between biomass and (respectively) NEE, Reco and GPP would be characterized by R² values of about 0.89, 0.99 and 0.99 as compared to the regression between GWT and (respectively) NEE, Reco and GPP which are characterized by R² values of about 0.69, 0.55 and 0.49. Therefore the strong emphasis put on the role of GWT should be given further thought.*

The campaign-wise measurement approach does not allow a direct inclusion of GWT in the interpolation model, but GWT is captured by the variations per campaign. Given the interactions with temperature and PAR, however, our approach does not allow to single out the GWT effect. We have tested previously in a lab experiment (Hahn-Schöfl et al. in prep) the GWT effects on heterotrophic and autotrophic respiration and conclude that the text is correct as submitted.

However, we added the correlation between GPP and Reco in this section: "The assumption that plant respiration was dominating R_{eco} differences between the sites is confirmed by

highly significant correlations between daily mean values of GPP and R_{eco} for each site ($p < 2.2\text{e-}16$, mean $R^2 = 0.64$ for all sites).” We also added “Therefore, differences in NEE can be attributed to varying microbial activity as well as carbon degradation differences between the single sites.”

11302 Line 9-13: *As indicated by the strong correlations shown above, the role of vegetation (biomass) can be interpreted as a strong driver of CO2 fluxes. I don't see how the authors can claim that the fact that Reco and GPP are correlated (i.e., has a rather constant ratio) rules out the influence of vegetation on NEE? This statement also appears in the abstract. Maybe the constant relation can be seen as an indicator of qualitative vegetation similarities; but it does not address quantitative differences that affect the CO2 fluxes.*

This is a misunderstanding. Only when $\text{Reco} : \text{GPP}$ is relatively constant we can interpret NEE as driven by site properties rather than vegetation properties. Assume that one vegetation type is significantly more effective in sequestering carbon than the other (e.g. mosses more than grasses). Then our differences in NEE would be driven by changes in $\text{Reco} : \text{GPP}$, hence by vegetation rather than GWT. We conclude that the text is correct as submitted.

Conclusions: *The conclusion collectively speaks about GHG, but results are based on the importance of CO2 fluxes. I suggest to limit the statements to the role of CO2.*

We decided not to limit the results only to CO2 but kept the addition “mainly CO2”.

Line 24: *‘. . . emit as much CO2 as grasslands on histosols.’*

We compared the GHG balance of the histic gleysol with GHG balances of grasslands on histosols. Therefore, GHG instead of CO2 is correct in this case.

Table 1: *As footnote for Site column, I suggest something like “Subscripts in site designations refer to low (<15%), medium (15-35%) or high (>35%) soil C content and mean annual water table depth (cm)”. This info, I think, could be repeated in all four tables. Mean WTL column and footnote c: Use notation ‘GWL’ as in the rest of the manuscript (rather than ‘WTL’) Specify that Nmin is given as an average and include SE and n.*

We added a footnote for the “site” column to all four tables. We standardized the notation “GWL” in the whole manuscript and added means and SE to Nmin.

Table 2: *Give means and SE with same number of decimals To avoid confusion,*
done

specify in footnote a why sum of cover can be >100% (here up to 155%)

We added “Since the cover of grasses, sedges and mosses is observed separately and species can overlap, the total plot cover can exceed 100 %.”

‘Cover values are indicated to nearest 5%’

done

Table 3: *Specify the nature of the variability reported; is it mean \pm sd for the three chambers per site or is it an sd estimate based on the bootstrap/monte carlo procedure?*

Uncertainty values for NEE, Reco and GPP represent the accumulated measurement error of \pm one standard error calculated by Monte Carlo.

For N₂O and CH₄ fluxes we report the standard deviation of replicate plots. However, unfortunately we found an error when checking the calculations of means and SDs, which we have corrected now. Corrected CH₄ values are only marginally different to those reported in the previous revision, but N₂O fluxes are somewhat higher, though still not significant and of minor importance for the total GHG balance.

Table 4: *Give means and SE with same number of decimals and specify for measures of variation as in Table 3*

We applied the same numbers of decimals to means and SE and specify measures of uncertainty.

Figure 2: *Include the measured data used for modeling (and verification) as points in this graph; this will not only show which measured data are available, but it will also indicate the model performance.*

We included the calibration dates which we used for modeling as diamonds in figure 2. We do not include actual measured data, because the graph depicts daily mean values, but we measured values on distinct time points.

Figure 3: *This figure is optional; I think it is referred to only at p 11296 in a paragraph where Table 3 is more appropriate.*

We removed this figure.

Figure 4: *Correlation between GWL and annual net C balance seem not to be so strong; net C balance would be the indicator of changes in the soil C pool caused by heterotrophic mineralization.*

The figure is intended to show that our results are in the range of other extensive grasslands independent of the correlation strength. A correlation over many peatlands is expected to be weaker than a correlation within one specific peatland.

Author reply to the comment of Å. Mander

(Interactive comment on Biogeosciences Discuss., 10, 11283, 2013)

We thank Dr. Ulo Mander for his valuable comments on the manuscript.

“First, the second hypothesis (“...peat mixed with mineral subsoil and resulting lower C_{org} concentration emits lower amounts of GHG than unmixed peaty soil with a high C_{org} concentrations”) is not very clear to me because the anthropogenic mixing of peat and subsoil which is always related to drainage will always enhance the mineralization of organic material and thus, leading to higher soil respiration. The C content in peat is probably not so important than the quality of organic material: in many peatlands the lower layers of peat have recalcitrant organic material which does not decay easily. Mixing this recalcitrant peat with mineral material should normally increase the CO₂ fluxes. Therefore, it would be important to touch at least shortly the problems of the C quality in this particular peatland area. It is also related to the long-term history of drainage of this peatland. Most possibly, the quality of organic matter has been changed during the long-term disturbance. On the other hand, I completely agree that the rewetting of this area is a recommended way of restoration and due to the lower potential of CH₄ release will be also less climate-loading than those system are normally during the first decade after the restoration. The problem of the C quality leads to another question which I would like to discuss.”

In the “Grosses Moor” the mixing of peat with sand occurred already several decades ago. Therefore, the subsequent and immediate increase of emission is supposed to have already leveled off.

We agree that peat quality is an important issue concerning CO₂ emissions (Reiche et al. 2010*). Mixing peat with mineral subsoil can either result in C loss by mobilization of C_{org} since aerated surfaces increased or result in a reduction of C emissions by C_{org} stabilization on mineral surfaces (Marschner et al. 2008**; Mikutta et al. 2006***) and/or by C_{org} dilution by mineral soil material (Don et al, 2013****).

Since peat mixed material remains periodically or permanently water saturated, less peat than under unmixed conditions remains available for mineralization.

*Reiche, M., G. Gleixner, et al. (2010). Effect of peat quality on microbial greenhouse gas formation in an acidic fen. *Biogeosciences* 7(1): 187-198.

** Marschner, B., S. Brodowski, et al. (2008). How relevant is recalcitrance for the stabilization of organic matter in soils? *Journal of Plant Nutrition and Soil Science-Zeitschrift Fur Pflanzenernahrung Und Bodenkunde* 171(1): 91-110.

*** Mikutta, R., M. Kleber, et al. (2006). Stabilization of Soil Organic Matter: Association with Minerals or Chemical Recalcitrance? *Biogeochemistry* 77(1): 25-56.

**** Axel Don • Christian Rördenbeck • Gerd Gleixner (2013): Unexpected control of soil carbon turnover by soil carbon concentration *Environ Chem Lett*, doi:10.1007/s10311-013-0433-3.

“Second, there was a very low, if not to say, zero N₂O emission from this peatland. In analogous soil conditions (if the term “histic gleysol” really means the same: up to 30 cm peat layer above the mineral subsoil) drainage causes a significant N₂O release which is also a main reason of high N₂O potential of some forests on these soils (Mander et al 2010 Landscape and Urban Planning). Thus, it is possible

that the denitrification in this system is complete and all the N₂O will be transformed to N₂. The less calcitrant C available to denitrifiers (which is probably the case!) is a good presumption for that.”

On all sites, low N_{min} concentrations occurred and C/N ratios were around 27 which explain the low N₂O emissions. Moreover, N_{min} contained around 99% NH₄ which shows that nitrification and consequently denitrification are inhibited and the transformation of N₂O to N₂ negligible.

“Third, despite of a very correct and detail statistical arguments you provided, I am doubtful about the usefulness of using non-linear function for calculating CH₄ and N₂O fluxes from chambers. Normally, non-linearity indicates some failure in chamber or measurement technique which always can happen. I am wondering whether the main results would remain the same if excluding these 27% of CH₄ and 16% of N₂O data calculated on the base of non-linear model?”

Non-linearity in closed (non-steady-state) chambers directly follows from physical theory (Hutchinson, Mosier, 1981*; Livingston, Hutchinson, Spartalian, 2006**). Accounting for non-linearity can be expected to result in less biased mean flux estimates than assuming linearity (though at the cost of higher uncertainty). If we excluded fluxes where the non-linear model was applicable, we could exclude preferentially higher absolute flux values. Since we consider them perfectly fine flux measurements that is not desirable. However, if we calculate all fluxes with the robust linear model, the mean annual fluxes are lower by averaged 31 % for N₂O fluxes and 20 % for CH₄ fluxes compared to the mean annual fluxes calculated with our method.

*Hutchinson, G. L. and A. R. Mosier (1981). IMPROVED SOIL COVER METHOD FOR FIELD MEASUREMENT OF NITROUS-OXIDE FLUXES. Soil Science Society of America Journal 45(2): 311-316.

**Livingston, G. P., G. L. Hutchinson, et al. (2006). Trace gas emission in chambers: A non-steady-state diffusion model. Soil Science Society of America Journal 70(5): 1459-1469.

“Fourth, the last sentence of the Introduction “We show, however, that even in shallow histic gleysols and in histic gleysols mixed with mineral soil, GHG emissions remain as high as in deep peat soils and are driven by water table” is already a result and belongs to The Discussion or Conclusions.”

We agree with this and discard the sentence.

“Finally, rewording (may be even skipping) of the second hypothesis and including the C quality analysis, as well, as the analysis of results without the doubtful CH₄ and N₂O data would improve the paper’s quality.”

We disagree to reword the second hypothesis, since a hypothesis should not be changed after testing it. But we include C quality in the discussion now. We did not delete the CH₄ and N₂O data calculated by non-linear regression as stated above.