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Interactive comment on "High greenhouse gas fluxes from grassland on histic gleysol along soil carbon and drainage gradients" *by* K. Leiber-Sauheitl et al.

Anonymous Referee #1

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Review of the manuscript: High greenhouse gas fluxes from grassland on histic gleysol along soil carbon and drainage gradients, by K. Leibner-Sauheitl, R. Fuss, C. Voigt and A. Freibauer. Biogeosciences Discuss. 10, 11283-11317, 2013.

The work of Leibner-Sauheitl et al. addresses a highly important aspect of evaluating GHG fluxes from organic soils, namely the role of soils with intermediate C contents. Such contributions are highly welcomed. The authors have used static chamber methods to measure emissions of CO2, N2O and CH4 at intervals of two weeks or less in a full year study. Modeling based on temperature and PAR as driving variables was used to derive annual CO2 budgets. The authors show that high NEE fluxes occur from the studied soils and that emission of N2O and CH4 are negligible, which corroborates

previous studies. The proposed title in this respect is somewhat misleading and should be changed to signify only high CO2 fluxes rather than collectively high GHG fluxes.

Overall, the current manuscript is generally well written, though some sections in the methods description could benefit from being somewhat more explicit. Some concern with the manuscript in the present form is the lack of quality checks of the models applied and the lack of presentation of basic measured data. Also, the authors interpret their findings in relation to the mean annual groundwater table (GWT) and emphasizes that their results 'confirms the rule that peat mineralization generally increases linearly when the water table lowers'. I think this conclusion is challenged by the fact that the authors study ecosystem respiration rather than heterotrophic respiration and, indeed, biomass in the present study seems to correlate much stronger to Reco, GPP and NEE that the mean GWL.

Whereas I am favorable for including the manuscript in Biogeosciences, I think the manuscript should be improved prior to publication. I have made a number of suggestions and comments that are intended to assist the authors in this work.

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

p. 11285 Line 4: move references to end of sentence Line 7: what is BÜK 1000? Richter, 1998? Line 8: join sentences by 'and' to avoid staccato Line 10-11: more correct to limit the statement to: 'caused emission of high amounts of CO2. . .' Line 10: 'causes' instead of 'caused' 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] Line 13-14: suggest joining sentences by: 'Such loss of peatland. . .' Line 16: 'the dominant land use on peat soils in. . .' Line19-20: 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. Line 23: 'Histosols. . .'

p. 11286 Line 2: for consistency use 'deep' rather than 'profound' Line 4: the Danish report uses the value 12%, rather than 15% Line 16: please also consider the role of vegetation in this statement

11287 Line 2-3: '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? Line 20: 'reached' rather than 'hit' 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). Line 27-28: I appreciate the designations of sites with the info on C and GWT; much better than some arbitrary designations! 11288 Line 5: indicate distance to the meteorological station

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 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' Line 10-14: this part belongs to the section on GHG measurements rather than modeling. Line 10: What is the meant by diurnal cycles here? Is it from sunrise to sunset? Specify the number of measurements achieved for each frame during the daily campaigns. This will also be needed to evaluate the quality of Reco modeling. Neither model perfor-

mance nor measured data are presented. Line 15: Non-linear models were considered for CH4 and N2O, so why not for CO2? 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. 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.

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? Line 18-21: the interpretation of e0 (generally E0) as activation energy and T0, 227.13 K as a temperature constant for the start of biological processes to me is somewhat misleading. E0 is an ecosystem sensitivity coefficient (a temperature rather than an energy) and T0 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). 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? 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

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. Line 8: maybe write:

'. . .according to a Michaelis-Menten type of equation modified by. . .'. No need to cite Menten and Michaelis (1913) here.

11292 Section 2.5 It seems only the calculation of individual fluxes is mentioned; information on how annual sums were derived should be included Line 1-5: This paragraph belong to section 2.3 Line 15: did you use the appropriate AIC with small-sample correction? Line 19-20: Point (d) - so it could be argued that reverting to robust linear regression caused severe underestimation? How often did this occur?

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

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

11296 Line 1-5:Give reference to relevant figure for description of dynamics Line 19: Give also r for the correlation between GPP and Reco Line 23: refer to Table 3, rather than Fig. 3, for this statement

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. Line 10: How was it whether annual emissions or uptake of N2O was significant? Line 13-14: Table column reads 3.3 to 8.6 (rather than 3.1 to 8.2 as cited in 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? Line 24: use 'are in accordance with' rather than ' confirms'

11299 Line 13-16: can you quantify this statement on robustness of the interpolations?

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. 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.

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.

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. Line 24: '. . .emit as much CO2 as grasslands on histosols.'

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

Table 2 Give means and SE with same number of decimals To avoid confusion, specify

in footnote a why sum of cover can be >100% (here up to 155%) 'Cover values are indicated to nearest 5%'

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?

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

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.

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.

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

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