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Interactive comment on "The greenhouse gas balance of a drained fen peatland is mainly controlled by land-use rather than soil organic carbon content" *by* T. Eickenscheidt et al.

Anonymous Referee #1

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The manuscript of Eickenscheidt et al. presents very valuable estimations of GHG fluxes from a drained organic soils (intensively managed as croplands and grasslands) which significantly differed in their SOC content in the top soil. The results clearly indicated that drained mollic Gleysols managed as arable lands or grasslands can be considered as hotspots for GHG emissions. The authors concluded that management and not the land-use type itself or the soil organic carbon content is responsible for the emissions of GHGs from intensively managed drained organic soils. The results discussed are of very high scientific importance and potentially might have very significant implications for reporting of GHG emissions under LULUCF/AFOLU. The paper is very well structured and is written with very good, fluent language. However, I have some

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

Major comments:

1) This is not very clear if chambers used for trace gas fluxes measurements are equipped with fans or not (Lines 202-15) which mix the air inside the chamber headspace? This information is missing, although Droesler (2005) reference is referred here, I would clarify this issue in the text. The latest papers of e.g. Pihlatie et al (2013) or Christiansen et al. 2011 are showing that if the air is not well mixed inside the chamber, the fluxes, especially if they are calculated based on linear approach tend to be significantly underestimated. From the description of the measurements procedure we know that the closure time was relatively long (60 minutes, and even longer for a bigger chamber) hence I would assume that at the relatively high N2O fluxes as well as CH4 fluxes (when the site was flooded or very moist) you should see the non-linear gas concentration development in the chamber. If the fluxes are calculated based on linear approach and only based on 4 points then they might be very significantly underestimated. There were quite a lot of statistical analyses performed by authors to prove that the uncertainties of the results are small, but there is no such a discussion where the above listed sources of errors would be addressed. This might be important also for the interpretation of the reported very small cumulative N2O fluxes (in relation to other studies from similar drained peatlands and IPCC emission factor).

2) the infrared analyser used in the study (LI820) is measuring only CO2 concentrations (Lines 244-245). What about water vapor? It is well known that the H2O concentration inside the chamber headspace is increasing over time causing several problems 1) condensation might occur if closure time is too long, or evaporation very strong, 2) CO2 is diluted by H2O which may cause overestimation of GPP and underestimation of Reco (see e.g. Application Note #129 of LICOR) and 3) there is also an issue of cross-sensitivity and band broadening. All these factors together may seriously bias the measured concentrations of CO2 and calculated fluxes. I assume that the H2O

concentration was not measured by the authors, but I would suggest to discuss this issue in the paper at least in order to critically assess or refer to these potential sources of errors.

3) I would suggest to not combine datasets from two arable plots with similar SOC content and two different crops to present differences in fluxes between different soil types (Line 462-463, fig. 6). If soil respiration would be presented here, then I would say, yes, this figure would show the differences between soil related fluxes. But, in this experiment there were two different crops (maize, oat) cultivated in rotation. That means autotrophic respiration might be different at both plots with different crops (Ra/Rh ratio is unknown, but one may assume it would be different for both crops and for sure for both years with so different weather conditions), hence according to me Reco cannot reflect differences between soil type depended fluxes.

This way of analyses may indicate that the measured/modeled fluxes should be/or are dependent only on the SOC (which was one of the aim of the study) and soil type, while the cultivated crop and crop specific management (which is very different for oat and maize) have no impact on the fluxes. Please reconsider this issue. It would be better to compare average fluxes for the type of soil with similar SOC content (high, medium), but for the same kind of crop. By doing so, you can combine datasets from two years when the same crop was cultivated (A1 – corn, A2 oat in 2010 and reverse in 2011), to reflect some interannual variability (highly impacted by differences in ground water table), but at the same time to exclude the crop specific management effect on the measured fluxes. Maybe then you can conclude about the differences in fluxes between different soil types/SOC. I have the same concerns in relation to fig. 3, and 8. To be honest this concern has an impact on farther analyses and discussion (paragraph 4.3, lines 618-622) and should be considered by the authors. Concerning above, I would not conclude about differences in Reco (line 462-463), GPP (line 473) and NEE (line 482-483) between two soil types.

Other minor comments and suggestions:

Lines 24-25 – yes, fluxes were measured, but only for CO2 fluxes were determined for both years, be more precise

Lines 25-28- from this sentence one may understand that only NEE fluxes were measured with close dynamic chambers, while Reco and GPP were modeled. This is not truth, please rewrite, be more precise

Line 28, add “static” chamber

line 81-82 – This is not clear what is the conversion factor and 1.72 for mineral and 2 for peat soils?

Lines 133-135, please add information about the surface area of the peatland. This information is missing

Lines 136-138, I suppose that authors refer here to 30-years mean temperatures and precipitation from 1961-1990, taking into account that the IPCC baseline temperatures refers to this period, but at the same time the average values of climatological variables would be significantly different if the last 30-years period is considered. I would suggest to refer to the last 30 years period, rather than to the one authors refer to.

Table 1 – I think it should be Mean GW level “ below” surface instead of “above”

Line 153, write “and” instead of “or” – these are two different soil types

Line 157 – although this information is published in other paper of the first author, I would suggest to add at least basin information about the used organic fertilizer (e.g. C/N ration, N content, the amount applied (to know how much N was fertilized). I consider this information very important for the discussion in the current paper

Line 229, write chamber instead of camber

Line 229-230 again, what about a fan? Was the air mixed in the chamber? Please deliver any information about the average air speed etc.

Line 235 temperatures

Line 293- it should be in reverse: "relationship between Reco and temperature" (Reco depends on temperature, while temperature does not depend on Reco)

Line 317 – can you be sure that this is a matter of PAR absorption? What about reflection?

Chapters 2.4.1. and 2.4.2 - the modelling of Reco and NEE is based on all measured data from the plot and for each campaign. It is fine as from supplement we know that the number of measurements for each plot was limited, However, how to define the site specific variation of the fluxes expressed e.g. in average plot specific flux+SD? This is not clear to me, as although there is no any SD values at the modelling parameters (in supplement) the cumulative fluxes (in table 3) are presented as some average +/- SD. Can you please explain how you proceed?

Line 366, add "test" after differences

Line 370 used

Line 371,385 non-parametric

Lines 390-398 how the temperatures were calculated? There was only one station installed in 2010 between the two land-use and soil types (I assume in the center part of the experiment presented in fig1). As the distances between sites are so close there should be no significant difference between air temperatures at 200 cm. At the same time, by having only one station is difficult to describe differences in temperature between sites. Is that means that the average temperatures presented here are just average of temperatures measured during the campaigns? If yes, this is not correct in my opinion. Or at least this should be written here, to make it clear. Why only average temperature for 2010 are mentioned here? To better understand differences between years I would suggest to inform about temperatures in both years. At the same time I am just wondering how Reco was modelled – which temperatures were used at each

of the sites? Can you please describe it more clearly? Did you model temperature for each site?

Lines 428-429 – as there were two different crops cultivated I would suggest to be more precise and separate yields for corn and oat,

Also in table 3 I would clearly write what kind of crop was cultivated in each year and each plot, although this might be taken from tab. 2

Lines 428-429 and Table 3. – different units for biomass yield might be confusing

Line 428, 431 – I would not use one term "crop yield" for yields of grass. This might be confusing as this term is usually used for a cereal, grain or legume. Please use the biomass yield instead.

Lines 432 -is this 73% of the 2010 yield or 73% lower than in 2010 – please be more precise

Lines 437-439 considering this difference please clarify it also in table 3, where there is not clear which yield consists of what

Line 444 add "depth"

Lines 458-462 I would suggest to add detailed information about the plots/crop/year for the certain cumulative range of fluxes presented in the text e.g.

At the grassland sites, annual sums of modeled RECO ranged from 3521 $\pm$ 1041 (G2Chigh) to 4316 $\pm$ 562 g CO2-C m$-$2 yr$-$1 (G2Chigh), which was significantly (P < 0.001) higher compared to the arable sites where RECO ranged from 2012 $\pm$ 284 (A1Chigh,maize) to 2992 $\pm$ 230 g CO2-C m$-$2 yr$-$1 (A1Cmedium, oat) in 2010 and 2011 respectively

The same comment is for GPP (lines 470-472) and for NEE (lines 480-482).

I would even suggest to rewrite these sentences and especially in case of arable crops there should be presented the crop specific ranges of cumulative fluxes (separately for oat and maize). Without this modification the information presented in the text are useless.

Lines 487-488 what was the reason for this peak? How you would explain this?

Line 494, I do not understand this statement. Form fig 8 is clear that N2O fluxes from C_high plots exceeded significantly fluxes measured at C_medium. “this was not valid considering the arable land separately” Refer here to table 3!

Lines 494-497, the same as above

Line 504, add “maize” in bracket (A2C_high, maize)

Line 507-508, how you can explain these peaks

Line 517 add “oat” in bracket (A1C_medium, oat)

Line 518 – please use other expressions than “controls” Ch4 peak cannot control, can e.g. determine

Line 521-522 – “CH4 exchange to NEE”? something is wrong here

Line 523 – the same comment as for 458-462, 470-472 and 480-482

Line 571-572 -is this emission factor for drained arable lands related to organic soils? I am not sure

Lines 600-603 – how this statement refers to your findings/ results

Line 642-643 – there is no any information about soil moisture in the text nor in the tables

Line 699, below instead above?

Line 704 I would add information that the plots were flooded

Line 755 taken instead of take

Line 769-770 as mentioned already before the information about N content the fertilizers would help to understand the observed differences

Line 790-797 why N2O fluxes measured are so low, if there is so big N supply?

Figures 2 and 7 – improve quality. In the current version is hard to differentiate time series of data

Interactive comment on Biogeosciences Discuss., 12, 5201, 2015.