Please note the reviewer comments are marked in black and the author comments marked in green.

Interactive comment on "Dynamic C and N stocks– key factors controlling the C gas exchange of maize in a heterogenous peatland" by M. Pohl et al.

Anonymous Referee #3
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General comments

The manuscript describes the results of a multiyear field study dealing with the carbon gas exchange of maize in a heterogeneous peat land. The results from this study will contribute to a better understanding of the environmental controls of the carbon exchange above cultivated peat lands and improve the upscaling from such sites. I recommend publishing of the manuscript after minor revisions.

- Comment: The concept of dynamic C and N stocks is presented in the abstract as a hypothesis. I would suggest to present the validity of this concept as a major result of the study. One of the surprising results of this study is that the AR soil having the lowest SOC turned out to be a source for CO2 to the atmosphere, while the two other soils comprise a sink. To me the potential reasons for this finding do not become clear from the discussion. On page **16150 line 24** the authors state that the soils contain a large stock of decomposable carbon. How was the decomposable fraction of organic carbon in the soils estimated? Given that the GWL has a pronounced effect on the carbon turnover, is it possible that the groundwater itself interacts with the respiratory CO2 or acts as a source for CO2?
  - Unfortunately, we cannot implement the first recommendation, as it was a main objectives of our study to show that new insights can only be achieved if one follows a system-oriented conventional approach from the start, rather that formulating them afterwards based on the study results. Basically, we want to convey the message that changes in a system's source or sink function for C gases can only be interpreted and predicted if all relevant C gas fluxes and their regulating factors in the plant-soil system are considered simultaneously. To stress this, we revised the introduction as follows:
    - Please refer to the reply to comment 5 of reviewer#2 (page 16138, lines 1-10)
    - Page 16139, lines 11-18: "Despite the system orientated approach mentioned above, it can therefore be assumed that the amounts of soil C and N located above the temporally variable GWL – hereafter referred to as dynamic C and N stocks – are of essential relevance to plant- and microbially mediated C gas fluxes on drained peatland soils. Moreover, investigations into the effects of dynamic C and N stocks may yield new insights into the mechanisms controlling the C dynamics at these sites. This would be a significant advancement with respect to a comprehensive and generalizable understanding of the CO2 and CH4 source and sink capacity of drained arable fen peatlands.

  - The explanations above similarly apply to the comments and questions regarding the large stocks of mineralizable C (page 16150 line 24). Unfortunately, the respective paragraph was

not precise enough in its wording. First, the amount of mineralizable C was not determined in this study – this was only an assumption. Second, we didn't make it clear enough that the results can only be correctly interpreted on the base of a simultaneous analysis of all relevant C gas fluxes and their impact factors. The respective section was rephrased to (now lines 402-407): "Surprisingly, the C-rich drained organic soils showed a strong net CO_2 uptake (Table 2), while the C-poor Arenosol was a small net CO_2 source. This observation cannot be entirely explained by the interaction between GWL and the potentially mineralizable soil C stocks. Hence, an integrated consideration of all relevant C gas fluxes and their regulation within the plant-soil system is required, which is discussed in detail below."

- Comment: For a reader not familiar with the statistics used here it is difficult to follow the outcome of the statistical analysis. To my perception the manuscript would benefit from a more broad description of the statistical method. Moreover phrases such "xy% of the variability can be explained by …" would be helpful.
  - The Generalized linear model (GLM) analyses were performed to determine the relative importance of several environmental controls and their interactions on the cumulated annual CH_4, R_{eco}, GPP, and NEE balances. GLM analysis is not aimed at variance partitioning, but at identifying the main significant influential factors, which we subsequently used in multiple nonlinear regression analysis to derive functional relationships with concrete distribution of variability on the individual factors.

- Comment: In figure 3 the NEE are extrapolated to SOCdyn levels of 0 kg C yr-1 for groundwater levels ranging from -1.6 to -0.2 m y-1. This suggests somehow that soils containing no SOC would act as a source for CO2 what is rather implausible. I suggest to limit the regression model to the data field covered by the measurements and to stress out the limits of the regression model in the manuscript.
  - The authors are very thankful for this advice. In order to show the range of values more clearly, figure 3 was modified to include an illustration of the interpolation and extrapolation range. In the figure caption we added: "… over twelve GWL classes per site (for model statistics see Table 4). Displayed grid represents the derived model surface with i) estimated model area covered by direct measurements (solid black) and ii) non-empirically approved model area computed by extrapolation (grey). Modelled NEE is separated according to positive (solid lines) and negative (dashed lines) values."
  - As well we specify the sentence on page 16154 in line 13: " and plausible for the range of measured GWL and soil C stocks, …"
  - see also below, comment page 16149 line 28

Specific comments

Page 16136 l. 8.: I wonder how the dynamic carbon and nitrogen stocks control methane emissions as methanogenisis is expected to take place under suboxic conditions and thus below the GWL. Do the authors think that the methane flux is mainly controlled by methanotrophy above the GWL? This should be specified.

- Thank you for pointing out this inconsistency. Indeed, the influence of C and N stocks located above the groundwater level on CH_4 fluxes is not identical to their impact on CO_2 fluxes, as CH_4 fluxes originate from soil zones under suboxic conditions, i.e below the GWL. However, due to the dependency on the GWL, CH_4 fluxes are very likely also – albeit inversely – related to dynamic C and N stocks. We clarified this in the manuscript (now lines 22-23) by rephrasing as follow "… play a key role in the regulation of plant- and microbially mediated CO_2 fluxes of these soils and, inversely, for CH_4."

Page 16139. L.13: Please remove "also".

- Implemented.

P. 16143 l. 18ff.: I wonder whether changing moisture inside the chambers is an issue for the flux measurements . Can the authors comment on this?

- In advance, we tested the influence of humidity on gas fluxes. Given the used chamber type, however, the influence of changing humidity levels is negligible due to a high chamber volume, with associated flux errors of < 1%. Therefore, when choosing measurement instruments, we decided to use CO_2 sensors without humidity quantification.

Page 16144, l 16. Some information on the uncertainty is given in the Supplemental material. However it would be fair to provide an estimate of the uncertainty in the manuscript.

- Uncertainty estimates are already presented at several locations throughout the manuscript. First, the uncertainty of annual CO_2 and CH_4 fluxes are provided in Table 2 as the model error (±95% confidence interval), and are also pointed out in the text (page 16147, line 28 and page 16148, lines7-8). Second, the confidence intervals for daily CO_2 fluxes are also shown in Figure 2. And finally, the uncertainty of daily CH_4 fluxes is included in the supplement figure S3.
- For additional indications please refer to reply of comment below (page 16147, line 13)

p. 16145 line 3. Is this the total number of datasets or the number of datasets per sampling site?

- We specified the sentence in line 259 "… resulting in a total of 111 datasets (37 per site) for…"

Page 16147, l. 1ff: The authors state that exceptional high methane emissions occurred during periods of flooding or high GWL. To my perception flooding or high GWL are linked to strong precipitation and thus to climate variability. In contradiction to this the authors state on **page 16148**, **l. 23** that climate played a minor role in determining annual methane fluxes could the authors clarify this?

- The Authors specified the sentence on page 16148, line 23 (now lines 350-351)**:** "While climate played a minor role in determining annual CH_4-C emissions via the effect of precipitation on GWL, climate controls were more relevant for CO_2 exchange (Table 3)."

Page 16147, l. 13: How large was the uncertainty. See also previous comment.

- We included the uncertainly from line 313 to 314: "… higher uncertainty (±3.7 g CH_4-C m^{-2} y^{-1} in 2007/08 vs. ±0.5 and ±0.2 g CH_4-C m^{-2} y^{-1} in 2008/09 and 2009/10; Table 2)."
- And also from line 309 to 311: "…to 28± 4 g CH_4-C m^{-2} y^{-1}, and ... the following years (0.3±0.5 and ±0.2 g CH_4-C m^{-2} y^{-1}) and … for AR and GL (< 1.2±0.6 g CH_4-C m^{-2} y^{-1}; Table 2)."

Page 16148 line 2ff: For a reader not familiar with the statistics used here the meaning of significant or" highly significant" as frequently used in **chapter 3.3** is not apparent.

- In order to improve the consistency of wording with respect to the significance of statistical analysis, now the significant levels are defined in line 342 "… significant (*p*- value ≤ 0.05)…", line 343 "… highly significant (*p*- value ≤ 0.001)…" and line 369 "… no significant (*p*- value ≥ 0.05)… "
- Also, *p*- values are implemented as a footnote in Table 4

Page 16149 line 28: I may be wrong but the statement NEE being always positive for SOCdyn<4.3 kg refers to the regression model. In a more generalized sense it implies that carbon free soils could act as a source for CO2. IS this statement substantiated by flux measurements carried out under these conditions?

- Sentence changed in line 379 to "However, the shown relations cannot be assumed as valid outside the measured ranges of SOC_{dyn} and GWL."

Page 16154 line 5: Please replace und by and.

- Implemented.

Figures

Figure 1: This figure needs some rework. It is difficult to differentiate the different sites from the color-coding. Further I can only find data for two soils in the two lower panel of figure 1. Please remove the a in the brackets at the end of the figure caption.

- We revised the Figure 1, the different sites are now clearly separable due to different line types and we removed the "a" in the brackets and in the figure.

Figure 3: Please check the legends of figure 3. Why is SOC given in Kg C yr-1 and why is the groundwater level given in m yr-1?

- We modified the legends of Figure 3 to "Mean annual SOC_{dyn} [kg C m-2]" and "Mean annual groundwater level [m]".

Supplement:

Fig. 2 Please provide a description of legends in the figure caption. What is RWI and what is the time scale?

- The authors modified the axis label and completed the legend description of Figure S2: "Diurnal variability as detected by wavelet analysis of modelled half-hour NEE data from 07/07/2008 to 31/07/2008. The upper graph displays modelled NEE time series and corresponding smoothing spline (solid red line). The lower graph shows the continuous wavelet transform and cone of influence (hatched area) within the respective time frequency domain. The wavelet power spectrum is thereby defined as the squared absolute-value of the wavelet coefficients (correlation between wavelet and data array)."