Author responses to comments of Referee #1 (Biogeosciences Discuss., 12, 17393–17452, 2015)

We are grateful for the valuable comments provided by referee #1. They helped us to improve the introduction of our research questions and to reduce or simplify tables.

1. *„I think that would be interesting use approach of ecophases (Mitsch, 2009). "*
The concept of ecophases characterizes the aquatic environment of a site at any moment (Hejný and Segal, 1998). It allows to describe ecoperiods what are sequences of different ecophases and by this to illustrate certain trends in the environment (cf. Krovolá et al., 2013). In our study, however, there were no shifts of the studied sites from one ecophase to another because water level, probably because water levels have been monitored only for two years and were quite stable within and between both years. The sites BA *Eriophorum–Carex* and BA *Carex–Equisetum* always belonged to the limosal and the sites GK *Phragmites–Lemna* and BA *Phragmites–Carex* to the littoral ecophase. The floating mats of *Carex–Lysimachia* and GK *Typha–Hydrocharis* could be also assigned to the littoral ecophase because the relative small water depths above surface just resulted from swimming on a larger water column. So, as there are no trends between ecophases, there is no realy need to use the concept of ecophases. Moreover, we are concerned that the application of the concept of ecophases in our study would make it more complicate. We found for example, that shallow flooding is a better measure to arrive at stable and low GHG emissions than deep flooding. However, with "shallow flooding" we do not only mean the limosal sites BA *Eriophorum–Carex* and BA *Carex–Equisetum* but also the littoral site BA *Phragmites–Carex*. The other littoral sites are not stable and most of them are strong GHG sources. We could argue that the definition of limosal by WL of 20 cm below to 10 cm above ground should be seen flexible and could also include BA *Phragmites–Carex* with average water levels of 15 cm above and maximum up to 20 cm above surface. However, this could lead to misunderstanding. As the water level dynamic is clearly presented by figures and tables, we do not see the advantage to classify the sites accordingly to ecophases.

Hejný, S. and Sega,l S., 1998: General ecology of wetlands. *In:* Westlake D.F., Květ J. and Szczepański A. (eds.), The Production Ecology of Wetlands, Cambridge University Press, Cambridge, 367–404.

Krolová M., Čižková H., Hejzlar J. And Poláková S. 2013. Response of littoral macrophytes to water level fluctuations in a storage reservoir. KNOWL MANAG AQUAT EC 408, 07. doi: 10.1051/kmae/2013042

2. *„As the result are presented modeled data only. Directly measured data are not presented and reader cannot compare actually measured data with modeled (theoretical) data. By my opinion, actually measured data have a higher value than modelled and estimated data. "*

For methane emissions we presented both, measured and modelled data. This was not possible for CO_2 exchange, because the timelines show daily averages but CO2 fluxes change strongly during a day. Showing modelled versus measured CO_2 fluxes would have required an additional figure. But instead we had compared modelled and measured data by leave-one-out cross-validation (see methods): "Stepwise one measurement campaign was left out after the other and the modelled R_{eco} and NEE fluxes obtained for the left out campaigns based on the remaining campaigns were compared with the measured fluxes. Model performance was assessed by the Nash–Sutcliffe efficiency (NSE, Moriasi et al., 2007)." The result are given in the results section for the CO_2 models: "Model performance tested for the H-approach was good for both years and all site types and plots. Cross-validation resulted in a median NSE of 0.78 (range from 0.38 to 0.90) for the R_{eco} models and of 0.76 (0.21 to 0.91) for the NEE models." and for the methane models: "The Lloyd–Taylor methane models performed well for all sites except for the second year of BA *Phragmites–Carex* and GK *Phragmites–Lemna*. NSE for all but the *Phragmites australis* sites ranged between 0.38 and 0.85 (median 0.58). Models of the *Phragmites australis* sites were acceptable in the first year (median NSE 0.37, range 0.05 to 0.82) but performed poor in the second year (median 0.01, range -0.25 to 0.24). Models of GK *Phragmites–Lemna* III and BA *Phragmites–Carex* III did not explain the high emissions in August 2011 (Figs. 3h and 4h). Both and the model of BA *Phragmites–Lemna* I overestimated emissions in spring and early summer 2012. Annual emissions calculated alternatively for the mentioned plots and second year by linear interpolation were 25, 28, and 118 g CH_4–C m^{-2} yr^{-1}, compared to 30, 32, and 139 g CH_4–C m^{-2} yr^{-1} derived by the temperature driven Lloyd–Taylor methane model, and lie within the 90% confidence intervals of the latter

(Table A2 in the Annex). The Lloyd–Taylor models were therefore accepted despite of negative NSE."

3. *„Maybe it would be preferable omit the N2O fluxes. In the case of the N2O, authors argue that the role of N2O exchange was negligible for the GHG-balances of all sites). "*
The GHG balance of peatlands consists of CO_2, CH_4 and N_2O. We could skip the N_2O data and cite other studies from rewetted peatlands. But there are still not so much studies of GHG emissions from rewetted peatlands and only few have monitored all three GHGs. Moreover, there is no study of annual GHG emissions from rewetted peatlands in Belarus. Therefore we decided not to rely on other studies but monitor N2O fluxes ourselves. This was no additional work because our gas chromatograph analysed CH4 and N2O concentrations from the same air sample. Now we see from our results that N_2O emissions were indeed negligible. And with respect to the few studies on N_2O emissions from rewetted peatlands we think that it is useful to present these results.

4. *„The overall feeling of presented paper is embarrassed without clearly formulated "home message". This is probably due to missing hypotheses in the Introduction sections. Filling of knowledge gaps is not scientific aim. "*
In the introduction we showed that shallow inundated cutover fens may become CO2 sinks and CH4 sources but that the combined GHG balance is unclear. The main interest of our study was to find out what GHG emissions can be expected when such fens are rewetted. There was no reason to assume that they would remain important GHG sources or even become small GHG sinks. The literature on comparable sites is rare and not equivocal. Therefore we decided to formulate our main questions instead of hypotheses. This was different in our former paper on the impact of shading by chambers on methane fluxes from *Phragmites australis* (Minke et al., 2014). In the mentioned study most of the literature indicated that there should be a significant impact and we consequently hypothesized to find significant lower methane emissions with opaque as compared to transparent chambers. Formulating clear hypothesis was not possible in our present study but in our opinion it is also the task of scientists to ask questions and try to answer them, even if they can not expect a distinct answer in front.
We changed the last paragraph of the introduction as follows:
"Whereas earlier studies indicate that the radiative forcing of such methane emissions may be compensated for by the simultaneous very strong net CO_2 uptake (Brix et al., 2001; Whiting and Chanton, 2001), recent observations described *Typha* dominated

wetlands as often only weak CO_2 sinks (Rocha and Goulden, 2008; Chu et al., 2015; Strachan et al., 2015; but cf. Knox et al., 2015).
Given the not univocal results regarding the potential of plants to compensate for methane emissions by correspondingly high CO_2 uptake, it is unclear how the GHG emissions from cutover temperate fens develop after inundation and establishment of wetland plants. Therefore we measured the CO_2, CH_4, and N_2O emissions from representative vegetation types along water level gradients in two rewetted cutover fens with different nutrient conditions in Belarus. Our objectives were: (i) to assess GHG emissions from rewetted temperate cutover fens recolonized by wetland plants (ii) to analyse the effect of water level, vegetation and nutrient conditions on GHG exchange."

Minke, M., Augustin, J., Hagemann, U., and Joosten, H.: Similar methane fluxes measured by transparent and opaque chambers point at belowground connectivity of *Phragmites australis* beyond the chamber footprint, Aquat. Bot., 113, 63–71, 2014.

Page 17397 Lines 1-2: *„The claim that the plants are strong sources of methane is not true. The role and effect of plants in this case is enhancing of greenhouse gasses emissions from soil profile and its partial biochemical interactions. Please change the sentence : : :" of plants in shallow water of Typha and Phragmites australis, i.e. of species that are potentially strong sources of methane..."*

We changed the sentence: "Such fens differ from those in the above cited studies in particular by the massive establishment in shallow water of *Typha* and *Phragmites australis*, i.e. of species that are potentially strong pathways of methane (Kim et al., 1998; Brix et al., 2001; Whiting and Chanton, 2001; Kankaala et al., 2004; Hendriks et al., 2007; Chu et al., 2015; Knox et al., 2015; Strachan et al., 2015)."

Page 17397 Lines 4-5: *„ The radiative forcing in term of the IPCC (IPCC 2007) and I think that for processing studies of different ecosystems is more suitable use amount of Carbon (C) in different form such as C-CO2 and C-CH4. Biochemical processes used and transform (sequestered) a carbon and important role of wetlands is long-term store of this C in soil. "*

We agree that for process studies the element base is more suitable. However, the role of peatlands is important to both, the carbon balance and the climate impact. Therefore we present both, the exchange of CO_2 and CH_4 and the resulting carbon balance on an element base (cf. Table 3) but also the GHG balance (cf. Table 5). Rewetting of peatlands aims at both, restoring the carbon balance and reducing the GHG emissions. We were confident that reed beds of *Phragmites australis*, *Typha latifolia* and *Carex* are net carbon sinks. But we were concerned that these reed beds could be strong GHG sources because of the high GWP of methane. Therefore we addressed the GHG aspect in the mentioned sentence.

Page 17398: *„ A map to shown the site location at both the local and regional scale would be helpful. "*

We agree and prepare it.

Page 17401 Lines 19-22: *„I recommend shortening this paragraph "*

The results of the diurnal studies of methane emissions and the impact of shading for all sites were important for the construction of annual methane models because they showed were we needed to correct for the shading impact and how safe the annual estimates are. Therefore we would like not to skip this paragraph.

Page 17401 Line 25: *„ Meteorological parameters for the flux models were recorded in two climate stations at distance 5.6 km and 6.3 km. I think that climate stations are too far from places where chambers measurements were made. "*

Climate stations were indeed quite far. However, we did not only construct transfer functions using the correlation between site temperatures measured during GHG campaigns and data from the climate stations, but also calculated the error of the transfer functions and included it into the emission estimates. Correlations were very close for air temperature (R2 between 0.95 and 0.97) and also strong for soil temperature (R2 between 0.93 and 0.96). This gave us confidence that the constructed temperature timelines for the sites were reliable what was confirmed by the good results of the cross-validation.

Page 17402 Line 15: *„ It is true that in the eddy covariance community a positive sign refers to a flux from the ecosystem to the atmosphere and a negative sign to an ecosystem sink. But it is depending of our consensus; I think that organic production based on consumption of CO2 from the atmosphere cannot be negative. Production is positive fundamental process of the organic mass formation."*

We think that using a positive or negative sign is both okay, as long as this is clearly explained. In our study we decided for the atmosphere perspective because, as stated above, we were in the first line interested in the GHG balance of the rewetted peatlands. We oriented on the IPCC 2014 as well as on numerous studies on GHG emissions from peatlands cited in our manuscript.

IPCC 2014, 2013 Supplement to the 2006 IPCC Guidelines for National Greenhouse Gas Inventories: Wetlands, Hiraishi, T., Krug, T., Tanabe, K., Srivastava, N., Baasansuren, J., Fukuda, M. and Troxler, T.G. (eds). Published: IPCC, Switzerland

Page 17403: *„ Where measured soil temperatures for modeling? Soil temperatures from too far climate stations cannot be used for flux models..“*

We did not use soil temperatures from climate station for the flux models, but constructed transfer functions based on temperatures measured at the sites during GHG campaigns and temperatures from climate station (see comment above). This is described in the methods Page 17402 Lines 2-4: “Regression between site and climate station temperature data was subsequently applied to derive continuous half-hourly time series for each site.”

Page 17405 Line 15: *„Why add the annual random error of the approach one to the uncertainties of annual emission.“*

We decided to use two approaches because we wanted to be more confident about the result. Hoffmann et al. (2015) found that identical CO_2 modelling approaches can lead to very different estimates when seemingly small aspects are dealt with differently. Both approaches, that of Leiber-Sauheitl et al. (2014) and Hoffmann et al. (2015) are very sound and reasonable, but, however, differ in some aspects, like estimation of measured fluxes, importance of significance of fits, and equation used to estimate GPP parameters. It is not possible to clearly decide on what way is more appropriate. Therefore we used both approaches and, surprisingly, arrived at similar estimates. But we did not skip one approach because we can not clearly say what is more realistic. Instead we assumed the mean of both as flux estimate and the difference between both approaches to represent one part of the uncertainty. The other part of the uncertainty, the random error, we estimated for the approach of Hoffmann et al. (2015) by bootstrapping. The random error accounts for the error of the temperature transfer function and the error of fitting the R_{eco} and GPP parameters. To arrive at more realistic error estimates we accounted for the random error and for the difference between both approaches and defined the confidence intervals as the difference between the annual sums of both approaches plus two times the annual random error.

Hoffmann, M., Jurisch, N., Albiac, B. E., Hagemann, U., Drösler, M., Sommer, M., and Augustin, J.: Automated modeling of ecosystem CO2 fluxes based on periodic closed chamber measurements: a standardized conceptual and practical approach, Agr. Forest Meteorol., 200, 30–45, 2015.

Leiber-Sauheitl, K., Fuß, R., Voigt, C., and Freibauer, A.: High CO_2 fluxes from grassland on histic Gleysol along soil carbon and drainage gradients, Biogeosciences, 11, 749–761, doi:10.5194/bg-11-749-2014, 2014.

Page 17409: *„This section is long. I recommend shortening this section and data presented in a table.“*

We agree and strongly reduced the first paragraph (Lines 3-19): "Mean annual temperature at Barcianicha during the first measurement year was 6.5 ℃ which corresponds to the long term mean (6.4 ℃, 1979–200 8). The second year was slightly warmer (6.9 ℃). Annual precipitation in the first year was higher compared to the long-term mean (740 vs. 665 mm), and in the second year lower (633 mm). Giel'cykaŭ Kašyl' was generally warmer and drier as compared to Barcianicha (long-term mean 7.3 ℃ and 594 mm, respectively, 1979–2008). Also here the first year was wetter (804 mm) and the second year drier (500 mm) while annual temperatures of the first year agreed to the long term mean but were higher (7.9℃) in th e second year."

Page 17410: *„ Differences in production of the Phragmites australis it may be caused by different density of stand. What is density of the reed stand?“*

The density of *Phragmites australis* was less at Giel'cykaŭ Kašyl' but biomass larger as compared to Barcianicha. Number of green shoots of *Phragmites australis* at Barcianicha was 204 per m² in 2011 and 123 per m² in 2012. At Giel'cykaŭ Kašyl' there were 48 green shoots in 2011 and 82 in 2012. This is because *Phragmites* culms at Giel'cykaŭ Kašyl' were much higher and thicker than at Barcianicha. This is obviously due to different nutrient availability. We did not add information on shoot density because there is already very much information and the site description in the method section states that *Phragmites* culms at Barcianicha were up to two metres high and at Giel'cykaŭ Kašyl' three metres.

Page 17440: *„ I recommended add the Table 2 in the appendix as a detail information of plant species cover..“*

We agree and move it to the annex.

Page 17448 Fig. 2: *„Results of comparison of the different chamber types would be used in different paper which will be focused in this topic. In presented paper this point is not important detail of methods. What is main result of paper?“*

The results of the diurnal studies of methane emissions were important for the construction of annual methane models. We learned from the diurnal studies that we had to correct the growing season methane flux estimates of GK *Typha–Hydrocharis* and GK *Carex–Lysimachia* by a factor of 1.2. Also we learned that we did not need to correct fluxes from the other sites. The annual methane models rely on the results of the diurnal studies. The figures of the *Phragmites australis* sites have been published by some of us before (Minke et al. 2014). We could skip them and cite the other publication. However, we would still need to discuss them because the mentioned publication did not discuss the implications of the findings for annual methane models. Therefore we prefer to keep also the figures of the *Phragmites australis* sites, and we are convinced that the presentation of the diurnal methane flux dynamic for all six sites in one figure supports the reader in following our argumentation regarding the construction of annual methane models. We suggest to move Fig.2 into the annex. This would help readers to concentrate on the main topic but allow them to check for details of the methane model.

Pages 17449 - 17450 Fig. 2: *„ Figures 3 and 4 could be merged into a single image with left and right panel of graphs. “*

We agree and combine them.

Page 17439 to 17446 Table 1 to 7: *„ Too many tables. I recommended simplified Table 1 (Site characteristics). Water level fluctuations are presented in Figure 3 and 4. Characteristics of individual plots on the site can be probably merged (averaged). “*

We agree and simplified Table 1, and moved Fig. 2 and Table 2 into the annex.

Please see below the simplified Table

Table 1. Site characteristics.

Site	Annual median water level (cm above surface) 1st year	2nd year	Above Ground biomass[a] (g C m^{-2})	Surface peat pH[b]	C[c] (%)	N[c] (%)	C/N ratio	Profile description, top down[d]
BA *Eriophorum–Carex*	-3±2	-3±2	117±34	6.2±0.2	42.2±1.7	2.3±0.1	18.5±0.2	0–9 radicel peat (H6), 9–14 silty gyttja, 14–43 radicel peat (H4, H3), 43–119 brown moss peat (H3, H4), below: middle sand
BA *Carex–Equisetum*	8±1	8±1	55±22	6.1±0.0	43.0±0.2	2.6±0.2	16.8±1.1	0–15 radicel peat (H6), 15–30 radicel brown moss peat (H3), 30–34 *Alnus* peat (H4), 34–85 brown moss peat (H3), 85–95 clayey gyttja & coarse sand, below: fine sand
BA *Phragmites–Carex*	14±2	14±2	296±79	6.1±0.1	43.8±0.3	2.7±0.2	16.8±1.1	0–13 lost, 13–40 radicel peat (H5/H4), 40–67 brown moss peat (H3, H4), below: gravel
GK *Typha–Hydrocharis*	11±2	2±3	259±103	5.6±0.1	41.4±3.2	2.8±0.2	14.8±0.3	0–20 lost, 20–30 radicel peat (H5), 30–55 very highly decomposed peat with radicels (H8), 55–90 radicel peat with *Phragmites* (H5, H3), 90–103 brownmoss–radicel peat (H3),
GK *Carex–Lysimachia*	10±3	4±3	299±73	6.3±0.4	43.3±2.5	2.6±0.4	16.7±2.3	103–113 woody radicel peat with *Phragmites* (H4), 113–140 radicel peat with *Phragmites* and brown mosses (H4), 140–150 organogyttja, below: sand
GK *Phragmites–Lemna*	104±6	74±6	586±121	5.7±0.1	37.1±4.1	2.4±0.2	15.2±0. 5	0–10 very highly decomposed peat with radicels (H8), 10–100 radicel peat with *Phragmites* (H4, H5), 100–170 radicel peat (H5), 170–185 organogyttja, below: sand

Given are means±standard deviations, n = 3 plots

[a] harvest at Barcianicha (first three sites) 2012-10-29, and at Giel'čykaŭ Kašyl' (last three sites) 2012-09-11, [b] pH (KCL) mean of three samples, [c] total carbon and nitrogen content, one sample, [d] von Post peat decomposition scale: H3 very slightly, H4 slightly, H5 moderately, H6 moderately highly, H8 very highly decomposed peat

Author response to comments of Referee #2 (Biogeosciences Discuss., 12, 17393–17452, 2015)

We are very thankful for the very valuable, detailed and constructive comments of anonymous referee #2. They helped us to considerably improve the manuscript.

General comments

1. We kept using both approaches to estimate annual CO_2 fluxes. Accordingly to Hoffmann et al. (2015) even changes in only one aspect of identical approaches can lead to strongly different results. As both approaches that we used differ in several aspects but both solutions are reasonable, we were not able to identify the "better" approach. Therefore we regarded the results of both as equally justified and considered the difference between both results as a better measure of uncertainty as compared to the random error of only one approach alone.

Hoffmann, M., Jurisch, N., Albiac, B. E., Hagemann, U., Drösler, M., Sommer, M., and Augustin, J.: Automated modeling of ecosystem CO2 fluxes based on periodic closed chamber measurements: a standardized conceptual and practical approach, Agr. Forest Meteorol., 200, 30–45, 2015.

2. Regarding the englisch language quality, we received from the BG editorial support that the paper will be send out to our in-house copy editors when it has been accepted for the final publication. The editors will typeset it and send it to the copy editors before sending it to me for proofreading.

Specific comments

Page 17395 Lines 1-2: *„Please give a half sentence reasoning, why. The informations on colonizing species goes into a separate sentence"*

We changed the sentence into: "Rewetting of temperate continental cutover fen peatlands usually causes inundation of areas that suffered intensive height losses while less deeply extracted parts remain at or above the water level. The flooded areas are – dependent on water depth – colonized by helophytes such as *Eriophorum angustifolium*, *Carex* spp., *Typha latifolia* or *Phragmites australis*."

Page 17395 Line 6: *„Merely describes? Or rather analyses? To keep active voice, try: "Here, we analyze" instead.“*

We replaced “This paper describes“ by „Here, we analyze“.

Page 17395 Line 9: *„Greenhouse gas“*

We completed “Greenhouse“ into “Greenhouse gas“.

Page 17395 Line 10: *„What the heck are few-weekly intervals? I'd like more explicit information. For instance you could state, that measurement were run with intervals from one week to XX days. If few-weekly is retained it has to be written with normal dash and without spaces, an em-dash has a different meaning.“*

We replaced “in weekly to few – weekly intervals“ into “every two to four weeks”

Page 17395 Line 10: *„I am no native speaker either, but shouldn't it be simplified to "over two years"?.“*

We replaced “over a two years period“ by “for two years”.

Page 17395 Lines 15-16: *„Also netto? I think "sequestering" is reserved to the net amount of C that is stored. Maybe you'd better go for "took up" in this case?“*

Yes, netto. Both *Phragmites australis* sites were strong net CO_2 and Carbon (accounting for fluxes of NEE and CH_4) sinks. But as “sequestration” is reserved to C we replaced it by “took up”.

Page 17395 Lines 18-20: *„Could be simplified to: "Shallow, stable flooding seems better to arrive at low GHG emissions than deep flooding. The risk of high GHG emissions after rewetting is larger for eutrophic than for mesotrophic peatlands [and maybe you add an half sentence of reasoning here]". “*

We agree and modified as suggested: “Shallow, stable flooding seems better to arrive at low GHG emissions than deep flooding. The risk of high GHG emissions after rewetting is larger for eutrophic than for mesotrophic peatlands because of a strong link between site productivity and methane emissions.”

Page 17396 Lines 9-10: *„ within a few years“*

We added the missing “a”: “within a few years“.

Page 17396 Lines 16-17: *„In this order? I would prefer, that you use the sorting that is suggested by the literature, which means, that water level has to go first and ? most likely ? vegetation has to come last."*

We reordered the factors accordingly: "water level, nutrient conditions and vegetation".

Page 17396 Lines 28-29: *„I think, this is the point here...."*

Yes.

Page 17397 Line 2: *„sources? or rather pathways?"*

We changed "sources" into "pathways".

Page 17397 Line 9: *„the more abundant species in European wetlands in general!"*

Yes, but to improve this section accordingly to suggestions of the first referee we skipped this sentence. Now it is: "Whereas earlier studies indicate that the radiative forcing of such methane emissions may be compensated for by the simultaneous very strong net CO_2 uptake (Brix et al., 2001; Whiting and Chanton, 2001), recent observations described *Typha* dominated wetlands as often only weak CO_2 sinks (Rocha and Goulden, 2008; Chu et al., 2015; Strachan et al., 2015; but cf. Knox et al., 2015).

Given the not univocal results regarding the potential of plants to compensate for methane emissions by correspondingly high CO_2 uptake, it is unclear how the GHG emissions from cutover temperate fens develop after inundation and establishment of wetland plants. Therefore we measured the CO_2, CH_4, and N_2O emissions from representative vegetation types along water level gradients in two rewetted cutover fens with different nutrient conditions in Belarus. Our objectives were: (i) to assess GHG emissions from rewetted temperate cutover fens recolonized by wetland plants (ii) to analyse the effect of water level, vegetation and nutrient conditions on GHG exchange."

Page 17397 Lines 10-12: *„Really? This is crazy."*

We did not find any publication additional to Brix et al. (2001). However, we skipped the whole sentence, see above.

Page 17397 Lines 15-16: *„Temperate but strongly continental. "*

Yes, we state this in the site descriptions: “Greenhouse gas fluxes were measured at two sites in Belarus with a temperate continental climate with fully humid conditions and warm summers (Dfb after Köppen, 1936; cf. Kottek et al., 2006).

Page 17398 Line 6: *„ or peat?“*

The reference says “земляные перемычки“, what is „earth dams“. They are made from the subsoil.

Page 17398 Line 6: *„Check phrase“*

We replaced “over” by “on”: “water level was raised on 60 % of the area”

Page 17398 Line 11: *„reed beds?“*

Yes, we added “beds”: “Vast reed beds …”

Page 17399 Line 2: *„ how close in meter?“*

We added this information: ”…, both three metres from each other.”

Page 17399 Lines 7-8: *„degree of decomposition was assessed visually? “*

Yes, the degree of decomposition was assessed visually in the field accordingly to the ten-stage scale (H1 to H10) of Von Post (AG Boden, 2005). In the Von Post method peat is taken in the hand and three aspects are analysed:

1. quality of plant structures visible in the peat
2. peat is pressed by the fingers and the flowing water is characterized with respect to colour and amount of peat substrate what is mushy enough to come out with the water through the fingers
3. the structure of the peat left in the hand after the water has been pressed out

Page 17399 Lines 22-23: *„ Why not equipping each site with a diver? “*

We agree that with one diver per site the estimation of the water level dynamics would have been easier. However, we were able to purchase only about 50 divers and needed them to study the linkage between vegetation and water level in pristine, drained, and rewetted fens and bogs all over Belarus. There were some hundred study sites and therefore we tried to use always one diver for several sites. The three sites studied at Giel’cykaŭ Kašyl are within one water body and close to each other and therefore one diver in between them was regarded to be sufficient. To develop the necessary transfer functions we established additional manual water level tubes at each of the sites. The same was true for the sites at Barcianicha.

Page 17399 Line 28: *„ Check phrase “*

We rephrased: “Because of strong peat oscillation this approach did not work for GK *Typha–Hydrocharis* and GK *Carex–Lysimachia*.”

Page 17400 Lines 1-3: *„ How exactly can this lead to water levels? “*

We concretize: “Photographic documentation (monthly during vegetation season, one time per winter, WL estimation error < 5 cm) was used here instead to reconstruct relative water levels for linear regression with Diver records.”

WL estimation on photos was supported by soil collars and their parts with known size, like width of battens and their distance to the collar’s top. Based on this we estimated the WL from photos with an error of less then 5 cm.

Page 17400 Lines 6-8: *„ How did you decide on row direction and distance within row? Why no other alignment was chosen? “*

We added one more sentence: “The row was East West oriented and the north side was the working side to minimize artificial shading during measurements.”

Distance of 40 cm was optimal for moving the chambers from plot to plot while the gas analyzer, connected by a five metre tube, was situated during the day at one point.

Page 17400 Lines 13-16: *„Please separate into to sentences and check phrasing. “*

We modified the sentence: “CO_2 exchange was measured with transparent chambers made of plexiglas (88% light transmission, ice packs for cooling, Drösler, 2005) and opaque chambers made of grey ABS plastic covered with a white film. Both were equipped with fans for air mixing and had an inner size 72.5 cm × 72.5 cm × 51.2 cm.”

Page 17400 Line 16: *„ of what size? “*

We added the size: “Opaque and transparent extensions of same area and 31.2 or 51.2 cm height with open tops…”

Page 17400 Lines 21-22: *„ Why so slow? “*

One value per five seconds was sufficient for flux calculation and allowed to use the data logger for somewhat more than one day before the memory was full.

Page 17400 Lines 25-26: *„ Why not recorded together with the air temperature and the “*
This would have been better, but our budget did not allow to purchase soil temperature probes for all sites. Next to this we were afraid to leave expensive equipment in the field. Therefore we used regression of manually recorded site soil temperatures with automatically recorded meteorological stations soil temperatures to reconstruct continuous site soil temperatures. The related error was accounted for by the error calculation.

Page 17400 Lines 27-28: *„ I'd prefer a sentence that specifies under which conditions measurements were conducted. In the current phrasing it could be misunderstood as referring to only one day.. “*
We clarified: “For CO_2 measurements bright or hardly cloudy days were selected to capture the complete PAR range from zero to solar noon. During each measurement campaign eight to ten transparent chamber measurements of two to three minutes were carried out on each plot from dawn until late afternoon.”

Page 17401 Lines 5-7: *„ This is quite a large interval. Do you have an explanation? “*
Three to four weeks is a typical interval between CO_2 exchange measurement campaigns by chambers (cf. Beetz et al., 2012; Beyer et al., 2015; Eickenscheidt et al., 2015). Indeed the gaps are large but it was not possible to conduct CO_2 measurement campaigns in smaller intervals because of site number and limitations in work capacity, equipment and sunny days. As described in the methods the relationships established separately for each of two measurement campaigns between GPP and PAR, and between R_{eco} and temperature were used to model CO_2 exchange between both campaigns, assuming that the relationships change gradually. This assumption was supported by the fact that biomass was not harvested and the water table was rather stable. Leave-one-out cross-validation resulted for all plots and years and positive NSE’s indicating that the model filled the gaps sufficiently reliable (see results).

Beetz, S., Liebersbach, H., Glatzel, S., Jurasinski, G., Buczko, U., and Höper, H.: Effects of land use intensity on the full greenhouse gas balance in an Atlantic peat bog, Biogeosciences, 10, 1067–1082, doi:10.5194/bg-10-1067-2013, 2013.

Beyer, C. and Höper, H.: Greenhouse gas exchange of rewetted bog peat extraction sites and a *Sphagnum* cultivation site in northwest Germany, Biogeosciences, 12, 2101–2117, doi:10.5194/bg-12-2101-2015, 2015.

Eickenscheidt, T., Heinichen, J., and Drösler, M.: The greenhouse gas balance of a drained fen peatland is mainly controlled by land-use rather than soil organic carbon content, Biogeosciences, 12, 5161–5184, doi:10.5194/bg-12-5161-2015, 2015.

Page 17401 Lines 10-11: *„ Any specific reason why? “*

Yes, we needed to transport the chambers a lot and for this it was advantageous to stack them into each other.

Page 17401 Line 13: *„ Do you know how well this thing measures? I.e., how precise and accurate the measurements were? “*

To ensure accuracy the GC was calibrated every day using three point calibrations for CH_4, N_2O and CO_2. Additionally always after 12 samples calibration gases were analysed and later used to correct for the drift. Precision of the GC was tested by repeated measurements of calibration gases and subsequent calculation of range limits (minimal detectable concentration changes) using the function flux.calib of the R package “flux 0.2–1” (Jurasinski et al., 2012). Range limits till end of 2011 were for CH_4 150 ppb and for N_2O 12 ppb. End of 2011 we adjusted an additional equalization valve what decreased the range limits, being then for CH_4 14 ppb and N_2O 9 ppb. Precision was accounted for during flux calculation, i.e. fluxes were assumed zero when concentration changes were below the range limits.

Jurasinski, G., Koebsch, F., and Hagemann, U.: Flux: Flux Rate Calculation from Dynamic Closed Chamber Measurements, R Package Version 0.2-1, Rostock, 2012.

Page 17401 Line 9: *„I guess "uncertainties" “*

Yes, we corrected it.

Page 17402 Line 24: *„measurement “*

We corrected it.

Page 17402 Line 16: *„Why the em-dash? Would be better without. “*
We removed the em-dash.

Page 17402 Line 17: *„as well as “*
We replaced “and” by “as well as“.

Page 17402 Line 21: *„We don't calculate, we estimate fluxes! “*
We replaced “calculation” by “estimation“.

Page 17403 Lines 5-6: *„In APPROACH ONE a moving window of variable time is applied to adjust.. And why you set this in all capital letters? Maybe you find a less offensive terminology for the two approaches? And would you please tell readers why you used two approaches? “*
We improved our explanation why we used two approaches: “Modeling NEE using the approach of Hoffmann et al. (2015) resulted in surprisingly high annual net CO_2 uptake rates of the *Phragmites australis* sites. To check for possible impacts of the calculation routine on the result we used alternatively the approach of Leiber-Sauheitl et al. (2014) and arrived at slightly smaller CO_2 sinks. Both approaches are reasonable, build on the same assumptions but differ with respect to flux estimation, reference temperature, GPP model and importance of the significance of the model fits, as described in the following paragraphs.
To avoid that modelled CO_2 exchange rates would be biased by specific features of only one of the approaches, both approaches were used to model annual CO_2 exchange rates and their means were taken as final estimates. Time series of daily CO_2 exchange rates, however, were drawn solely using results of the H-approach because both approaches show very similar shapes.”

We replaced “APPROACH ONE” by “H-approach” and “APPROACH TWO” by “LS-approach”, for Hoffmann-approach and Leiber-Sauheitl-approach, respectively.
The corrected sentence is: “In the H-approach a moving window of variable time was applied to adjust the starting point and length of the regression sequence accordingly to the regression quality.”
We replaced “APPROACH ONE” by “H-approach” and “APPROACH TWO” by “LS-approach” throughout the manuscript.

Page 17403 Line 7: *„check according to previous comment "*

We made a second sentence: "The optimal flux length was selected in a second step, based on the minimum Akaike Information Criterion (AIC) of the flux fit to the R_{eco} or the GPP functions.".

Page 17403 Line 9: *„Same here like above "*

We reworded accordingly: "In the Leiber-Sauheitl (LS)–approach a moving window of constant length (one minute for all, but two minutes for opaque flux measurements at *Phragmites australis* plots because of large chamber volumes and slow concentration changes) was used to select the regression sequence with maximum R^2 and minimum variance."

Page 17403 Line 16: *„In my opinion an approach is not able to fit something but you, the researchers used the approach to fit something. As suggested above, this should reflect in your language. Please, check the text for formulations like this. "*

We reworded accordingly: "In both approaches for each plot and campaign the Lloyd and Taylor (1994) equation (Eq. 1) was fitted to the regression of R_{eco} flux data on site temperatures."

We corrected similar formulations throughout the text.

Page 17404 Line 16: *„ What do you mean by that? "*

We added an explanatory sentence: "Assuming declining GPP fluxes when PAR drops from 500 to 0 µmol m^{-2} s^{-1} α was set -0.01 and GP_{max} estimated as the mean campaign GPP flux."

Page 17404 Lines 24-26: *„Focus or use solely? If the latter you can skip all the approach one/approach two stuff above and just focus on the one you finally used. I would prefer this for averaging the values resulting from these two approaches as you state in the next sentence."*

Both approaches are well-founded and there is no reason to say that the one is more correct than the other. We do not know what result is closer to the reality because we did not apply an alternative and independent method to estimate the annual CO_2 balances. Both approaches build on the same general model assumption what is a gradually changing relationship between R_{eco} and temperature and between GPP and

PAR from one measurement campaigns to the next. Because of differences in flux estimation, selection of reference temperature, handling of significance and the different GPP formulas both approaches arrive at different though surprisingly similar results. By taking the average of the results of both approaches we account for the impacts of these computing differences and arrive – within the above described general model – at more robust results with larger and more realistic error bars as if we would use only one approach. Therefore we do not want to skip one of the approaches. Of course, there are more approaches described in the literature and it would improve the robustness of our results if we would have used them, too, but this was beyond the capacity of this paper and should first be analyzed in a more methodological study. Regarding the figures illustrating CO_2 time series we used for simplicity only the H-approach because the results of both approaches show very similar shapes.
We changed the sentence into: "Time series of daily CO_2 exchange rates, however, were drawn solely using results of the H-approach because both approaches show very similar shapes." and moved it into the section that introduced why two approaches have been applied.

Page 17405 Line 3: *„measurement "*
We replaced "measuring" by "measurement"

Page 17405 Line 4: *„models were obtained "*
We clarified the sentence: "Stepwise one measurement campaign was left out after the other and the modelled R_{eco} and NEE fluxes obtained for the left out campaigns based on the remaining campaigns were compared with the measured fluxes."

Page 17405 Lines 9-12: *„Please rephrase. "*
We rephrased the sentence "Campaign specific confidence intervals (p = 0.01) were determined for the temperature models, as well as for the R_{eco} and GPP parameter pairs by bootstrapping. Subsequently 100 samples were taken randomly from the confidence intervals and used to compute R_{eco}, GPP, and NEE models."

Page 17405 Lines 15-17: *„Why? "*
We clarified this in the section on Uncertainty, accuracy, and variability: "The random error of the CO_2 models calculated with the H-approach represents the uncertainty of the measuring campaigns, but not of the interpolation. As indicated by the differences

between both approaches the uncertainty of the annual balances is larger. To arrive at more realistic error estimates we accounted for the random error and for the difference between both approaches and defined the confidence intervals as the difference between the annual sums of both approaches plus two times the annual random error calculated for the H-approach."

Page 17405 Line 18: *„harmonize plural or singular. "*

We corrected the sentence: "Inter-annual variability of annual NEE fluxes was calculated as the absolute differences between annual plot emissions and two-year plot means."

Page 17405 Line 24: *„estimated "*

We corrected the sentence: "Methane fluxes were estimated with the R package "flux 0.2–1" (Jurasinski et al., 2012) using linear regression."

Pages 17405-17406 Lines 25 and 1-2: *„First, you state that fluxes with NRMSE >= 0.2 are eliminated and then you state that fluxes were accepted if eliminiert und dann NRMSE < 0.4. How does this fit together? "*

We clarified this: "For normalized root mean square error (NRMSE) < 0.2 the flux with the largest number of concentration measurements was preferred. If NRMSE ≥ 0.2 a set of fluxes was estimated using the maximum number up to at least three concentration measurements. Subsequently the flux with the lowest NRMSE was selected. Fluxes were accepted if NRMSE < 0.4, R^2 ≥ 0.8 and n ≥ 3. This was the case in 639 out of 686 methane flux measurements, with 477 accepted fluxes based on n ≥ 4."

Page 17406 Lines 25 and 7-8: *„Why with single drivers only? Isn't this often a multivariate phenomenon? And did you also test for some vegetation parameters? Such, like LAI, bear often quite strong explanatory power. Especially since you measured on Spots with Typha/Carex/Phragmites which are rather larger emergent macrophytes where LAI or other growth parameters typically perform quite well. "*

We are aware of the combined effects that factors have on methane emissions. Originally we applied multiple regression analysis to develop methane models (using ln(CH_4flux + 1), as Tuittila et al., 2000). This resulted in different factor combinations not only for different vegetation types, but also for different plots within the same vegetation types and for different years of the same plots. The narrow restriction of the models was most likely due to the limited data pool (on average 18 methane flux values per plot and year) and because of this the models seemed pretty random.

We agree that plant parameters would have been helpful in the development of methane models, especially for plants that active ventilate when green. Unfortunately we did not have the possibility to monitor independent vegetation parameters like LAI. Therefore we included GPP, NEE and R_{eco} in the multiple regression analysis. While NEE was never significant and GPP only sometimes, R_{eco} was often significant. However, R_{eco} was seldom together with temperature in one model, but usually both parameters replaced each other. This was because of the strong correlation between temperature and R_{eco} and can be explained by the fact that R_{eco} was modelled using temperature as the driver. The differences among multiple regression models among plots and years of the same vegetation type and the strong dependence of R_{eco} from temperature were the main arguments why we decided against multiple regression analysis and looked instead for the most important, single parameter explaining methane fluxes.

Page 17406 Lines 25 and 8-9: *„What does it mean, "selected"? From which choice?“*

We completed the sentence accordingly: “Second, published nonlinear regression models were fitted to the relation between methane emissions and the driver and the optimal model was selected based on the AIC.”

Page 17407 Lines 17 and 19: *„I hope that you chose both the station data and the site temperature data points 1000 times with the same index. Did you? Otherwise this is flawed.“*

Yes, we did. Station and site temperature points were combined in one data frame accordingly to date and time and then sampled simultaneously by row indices with replacement.

We clarify the sentence: “First, the linear regression between soil temperatures at site and climate station was performed 1000 times with bootstrapped re-sampling of the site and station temperature data points with the same indices.”

Page 17407 Line 20: *„Which mean and sd do you mean here?“*

We mean the methane flux and its standard deviation.

We correct the sentence: “Second, a set of 1000 normally distributed flux values was generated for every flux measurement based on the flux estimate and its standard deviation.”

Page 17407 Lines 20-23: *„Now I am lost. What happens with bootstrapped residuals in the third step? I understand that you have 1000 models for each measurement day and from the whole of your models 1000 are selected in the next step. This seem to not provide a good coverage of the measuring frequency because it is quite unlikely that all models of one measurement day are skipped, isn't it? Anyway, you have to try to get this whole paragraph straight. In its current form is hard to follow.“*

To make the description clearer we added some numbers and separated the third point in two points: “Third, each of the 1000 soil temperature data sets was paired with one of the 1000 flux data sets and 1000 Lloyd and Taylor fits (Eq. 1) were performed.

Fourth, from each of the Lloyd and Taylor fits bootstrap parameter samples were created using bootstrap of the residuals (Efron, 1979; Leiber-Sauheitl et al., 2014). Bootstrap sample size was again 1000. More than 99% of the bootstrap fits were successful what resulted in more than 990000 parameter pairs per plot and year.

Finally, 1000 Lloyd and Taylor fits were randomly sampled from the parameter pairs, combined with the 1000 soil temperature data sets and used to calculate 1000 methane models per plot and year. For each time point and the annual sums 95% and 5% quantiles were calculated to construct confidence intervals of the time series and balances.”

Page 17407 Line 26: *„measurement“*

We corrected this: “As the CH_4 model fits build on all data of a year, the 90% confidence intervals do to some extent also account for the interpolation between measurement days.”

Page 17408 Lines 3-13: *„This should go into the methane section which should be renamed methane and nitrous oxide because these small bits of information do not justify sections and paragraphs “*

We agree and included the text into the methane section.

Page 17409 Lines 3-19: *„Do you really think, readers need these details? I'd suggest to strive for half the length of the current version and for increased readability by concentrating on the real key issues.“*

We shortened the paragraph: “Mean annual temperature at Barcianicha during the first measurement year was 6.5 ℃ which corresponds to th e long term mean (6.4 ℃, 1979–2008). The second year was slightly warmer (6.9 ℃) . Annual precipitation in the first year was higher compared to the long-term mean (740 vs. 665 mm), and in the second year lower (633 mm). Giel’cykaŭ Kašyl’ was generally warmer and drier as compared to Barcianicha (long-term mean 7.3 ℃ and 594 mm, resp ectively, 1979–2008). Also here the first year was wetter (804 mm) and the second year drier (500 mm) while annual temperatures of the first year agreed to the long term mean but were higher (7.9℃) in the second year.”

Page 17409 Line 27: *„In this formulation readers have to calculate for themselves what water levels prevailed in the second year. Better you keep your reference and change to: "and dropped to about 70cm above surface (you could then skip the "above surface" at the first occurrence)“*

We changed the sentence accordingly: “Water tables at GK *Phragmites–Lemna* (Giel’cyka˘u Kašyl’) were about one metre in the first year, and dropped to 70 cm above surface in the second year (Table 1).”

Page 17410 Lines 7-23: *„Since these parts refer to things that are more stable in time than weather and climatic conditions, these paragraphs should be moved up before the climate/weather results“*

We moved the paragraph to the beginning of the results.

Pages 17411 (from Line 5)-17413 (to Line 15): *„ With three replicates this can be pure chance” … “Rather start the paragraph with this very fundamental finding and then go into some detail afterwards but try to cut down text by half. All these numbers within the text are really hard to read. And it should be "sites".” … “No uncertainties?” … “Please, try to reformulate the whole section in this style: Less numbers and detail, more focus on generalities and important points. There can be some few numbers. But these should refer to really important issues like astonishingly high emissions or surprisingly low ones or the like. If you want them numbers readable, put them in a table.” … “Rather start with the point here: "The largest annual GPP rates” ... This should follow* *directly after the largest annual GPP rates because they obviously belong together. Rephrase accordingly.“… „This paragraph was really an example of extended unreadability. Please revise, following my suggestions above.”*

We revised the whole paragraph:

“**3.2 Carbon dioxide emissions**

Model performance tested for the H-approach was good for both years and all site types and plots. Cross-validation resulted in a median NSE of 0.78 (range from 0.38 to 0.90) for the R_{eco} models and of 0.76 (0.21 to 0.91) for the NEE models.

All sites of Barcianicha were net CO_2 sinks in the first year. NEE was −528 (90% confidence interval -933, -194) g CO_2−C m^{-2} yr^{-1} for BA *Phragmites–Carex*, -86 (-130, -38) g CO_2−C m^{-2} yr^{-1} for BA *Eriophorum–Carex* and −88 (-114, -68) g CO_2−C m^{-2} yr^{-1} for *Carex–Equisetum* (Fig. 5; Table 3). In the second year, resulting from increased R_{eco} and decreased GPP, the net CO_2 uptake decreased. NEE of BA *Phragmites–Carex* dropped to −329 (-431, -220) g CO_2−C m^{-2} yr^{-1}, BA *Eriophorum–Carex* became CO_2 neutral and BA *Carex–Equisetum* lost some 24 (-6, 55) g CO_2−C m^{-2} yr^{-1}. Both, sinks and sources were larger at the Giel’cykaŭ Kašyl’ sites. NEE of GK *Phragmites–Lemna* was −611 (−819, −450) g CO_2−C m^{-2} yr^{-1} in the first and, despite of increasing R_{eco} fluxes, −1175 (−1567, −690) g CO_2−C m^{-2} yr^{-1} in the second year. The high values were attributed to extremely high annual GPP reaching in the second year −2267 (−2733, −1843) g CO_2−C m^{-2} yr^{-1} and therefore twice of R_{eco} (Fig. 5; Tab. 3). At the other Giel’cykaŭ Kašyl’ sites R_{eco} and GPP also increased from the first to the second year, but differences between both fluxes were small. GK *Typha–Hydrocharis* consequently varied between a source of 151 (41, 300) g CO_2−C m^{-2} yr^{-1} in the first and a sink of −113 (−418, 66) g CO_2−C m^{-2} yr^{-1} in the second year. GK *Carex–Lysimachia* was a net CO_2 source in both years, releasing 166 (66, 252) g CO_2−C m^{-2} yr^{-1} in the first and 216 (48, 470) g CO_2−C m^{-2} yr^{-1} in the second year.

Inter-annual variability of NEE fluxes was low for BA *Eriophorum–Carex* (39±12 g CO_2−C m^{-2} yr^{-1}, mean ± SD; Table 4) and BA *Carex–Equisetum* (56±8 g CO_2−C m^{-2} yr^{-1}), larger for BA *Phragmites–Carex,* GK *Carex–Lysimachia* and GK *Typha–Hydrocharis*, and maximum (282±177 g CO_2−C m^{-2} yr^{-1}) for GK *Phragmites–Lemna*. With respect to small-scale variability of NEE the order of sites was similar (Table 4).”

Page 17413 Line 18: *„inside what?“*

It should be “inside of chamber”. We changed it accordingly, see next point, please.

Page 17413 Lines 20-22: *„Cooling tends to have a very strong effect on relative humidity and I don't know a real smart solution for that. The problem is, that changes in relative humidity may strongly affect stomatal conductance inducing bias to the measurements. Thus, it is less important how opaque differ from transparent chambers than how relative humidity develops during chamber placement.. I'd prefer some information on that here. “*

We added information on the increase of relative humidity and temperature during chamber placement in Table S1 of the supplement. To make the table easier accessible for the reader we then moved it into the annex (A1). The modified first sentence is: “Opaque and transparent slightly differently affected air temperature and relative humidity of the headspace. Despite of cooling temperature increased stronger in transparent (up to 3 ± 0.5 ℃, mean ± SE; Table A1 in the Annex) as compared to opaque chambers (up 1.4 ± 0.2 ℃). Relative humidit y, in contrast, increased less in transparent (up to 18.1 ± 3.7 %) than in opaque chambers (up to 14.8 ± 2.3 %), but only at few measurement days the differences were significant (Table A1 in the Annex).”

Please see the updated Table A1:

Annex 1

Mean ± Std. Error of daytime (PAR > 2 µmol m^{-2} s^{-1}) CH_4 flux rates, PAR, T_{in}, and RH_{in} by plot and chamber type (DF = opaque mixed chamber, TF = transparent mixed chamber, D = not mixed opaque chamber). Values with same letter superscript do not differ significantly at P < 0.05 (Mann-Whitney or Kruskal-Wallis test; *post-hoc* non-parametric Nemenyi test), data of *BA Phragmites-Carex II* and *GK Phragmites-Lemna II* from Minke et al. (2014).

Site, plot and date	Chamber type	N	PAR (µmol m^{-2} s^{-1})	T_{in} (℃)	dT_{in} (℃)	RH_{in} (%)	dRH_{in} (%)	CH_4 flux (mg CH_4-C m^{-2} h^{-1})	Methane factor
BA Eriophorum-Carex I	DF	8	$685^a \pm 208$	$16.6^a \pm 1.1$	$1.0^a \pm 0.2$	$90.2^a \pm 2.6$	$7.5^a \pm 1.7$	$2.30^a \pm 0.10$	TF/DF = 1.09
2012-07-18	TF	7	$1145^a \pm 224$	$17.1^a \pm 1.5$	$3.0^b \pm 0.5$	$78.6^a \pm 4.1$	$3.8^a \pm 0.8$	$2.49^a \pm 0.05$	
BA Carex-Equisetum III	DF	7	$937^a \pm 401$	$17.4^a \pm 1.4$	$1.5^a \pm 0.4$	$90.1^a \pm 2.1$	$5.8^a \pm 1.7$	$2.30^a \pm 0.08$	TF/DF = 0.99
2012-07-18	TF	6	$851^a \pm 164$	$17.8^a \pm 1.5$	$1.5^a \pm 0.3$	$80.2^b \pm 3.0$	$4.2^a \pm 1.3$	$2.28^a \pm 0.08$	
BA Carex-Equisetum III	D	14	$482^a \pm 85$	$15.4^a \pm 0.7$	$0.7^{ab} \pm 0.1$	$79.4^{ab} \pm 2.6$	$9.1^a \pm 1.0$	$0.76^a \pm 0.03$	TF/D = 1.07
2012-09-16	DF	14	$535^a \pm 95$	$15.6^a \pm 0.7$	$0.5^a \pm 0.1$	$86.2^a \pm 1.5$	$7.5^{ab} \pm 0.8$	$0.80^a \pm 0.04$	TF/DF = 1.02
	TF	13	$584^a \pm 95$	$15.3^a \pm 0.6$	$1.3^b \pm 0.2$	$75.4^b \pm 2.3$	$4.4^b \pm 0.6$	$0.81^a \pm 0.02$	
GK Typha-Hydrocharis I	DF	9	$869^a \pm 157$	$24.3^a \pm 1.2$	$1.0^a \pm 0.2$	$94.4^a \pm 1.7$	$18.1^a \pm 3.7$	$16.61^a \pm 0.43$	TF/DF = 1.18
2012-07-12	TF	9	$868^a \pm 149$	$24.9^a \pm 0.9$	$1.4^a \pm 0.3$	$88.6^a \pm 2.7$	$14.8^a \pm 2.3$	$19.52^b \pm 1.20$	
GK Typha-Hydrocharis I	DF	11	$821^a \pm 136$	$19.9^a \pm 1.2$	$0.8^a \pm 0.2$	$85.3^a \pm 3.0$	$15.5^a \pm 2.8$	$14.04^a \pm 0.24$	TF/DF = 1.20
2012-07-13	TF	10	$1097^a \pm 146$	$20.7^a \pm 1.4$	$1.7^b \pm 0.3$	$80.3^a \pm 3.7$	$11.8^a \pm 2.1$	$18.00^b \pm 0.20$	
GK Carex-Lysimachia I	DF	9	$923^a \pm 115$	$24.2^a \pm 1.1$	$1.0^a \pm 0.2$	$84.9^a \pm 3.0$	$9.2^a \pm 1.5$	$14.28^a \pm 0.22$	TF/DF = 1.10
2012-07-12	TF	9	$749^a \pm 111$	$24.8^a \pm 1.1$	$1.5^a \pm 0.3$	$82.3^a \pm 2.9$	$7.0^a \pm 1.4$	$15.76^b \pm 0.38$	
GK Carex-Lysimachia I	DF	11	$1207^a \pm 188$	$20.1^a \pm 1.3$	$1.4^a \pm 0.2$	$83.4^a \pm 3.3$	$12.7^a \pm 2.1$	$14.62^a \pm 0.33$	TF/DF = 1.08
2012-07-13	TF	10	$1121^a \pm 177$	$21.1^a \pm 1.5$	$3.0^b \pm 0.5$	$78.8^a \pm 4.3$	$7.5^a \pm 1.2$	$15.81^b \pm 0.23$	
BA Phragmites-Carex II	D	16	$830^a \pm 130$	$19.4^a \pm 1.1$	$0.6^a \pm 0.2$	$81.0^a \pm 3.2$	$11.8^{ab} \pm 1.8$	$9.86^a \pm 1.40$	TF/D = 1.01
2012-08-08	DF	16	$857^a \pm 133$	$19.7^a \pm 1.1$	$0.9^a \pm 0.2$	$81.9^a \pm 3.3$	$13.4^a \pm 2.2$	$10.17^a \pm 1.50$	TF/DF = 0.98
	TF	16	$735^a \pm 121$	$19.2^a \pm 1.2$	$0.8^a \pm 0.1$	$76.5^a \pm 3.7$	$6.0^b \pm 1.0$	$9.95^a \pm 1.51$	
GK Phragmites-Lemna II	D	14	$707^a \pm 130$	$20.6^a \pm 1.2$	$0.7^{ab} \pm 0.2$	$70.4^a \pm 3.2$	$6.0^a \pm 1.5$	$13.70^a \pm 1.68$	TF/D = 1.27
2011-09-21	DF	13	$819^a \pm 125$	$21.7^a \pm 1.3$	$1.0^a \pm 0.2$	$71.1^a \pm 3.1$	$13.8^b \pm 1.8$	$17.42^a \pm 2.39$	TF/DF = 1.00
	TF	12	$893^a \pm 125$	$23.1^a \pm 1.0$	$1.8^b \pm 0.2$	$66.5^a \pm 2.5$	$6.6^a \pm 1.0$	$17.46^a \pm 2.08$	

Page 17414 Lines 1-2: *„ I Don't use abbreviations here. You can save much more text when cleaning up above.“*

We removed the abbreviations D, DF and TF from the text.

Page 17414 Lines 4-5: *„ I don't believe this figure. It is quite unlikely. Do you have any explanation for such a perfect match? “*

You are right; the statement is not precise (cf. Table A1 in the Annex).

We corrected the sentence: “For all other sites the ratio of transparent to opaque chamber with fan ranged between 0.98 and 1.02.”.

There are different possible explanations for the fact that the ratio between both chamber types was close to one for both *Phragmites australis* sites and BA *Carex–Equisetum*. *Carex rostrata* is a passive conduit for methane and chamber closure was to short to change the transport rates. *Phragmites australis* is an active conduit and even short term shading can affect transport rates (see effect of short term shading by clouds in Minke et al. 2014). Here other processes may have sustained similar methane fluxes in both chamber types, for example continuation of gas transport by shoots outside the chamber that are connected with shoots inside the chamber by rhizomes. However, the effect of transparency was significant at least for *Typha latifolia* and this could indeed be the result of slightly stronger increase of relative humidity in opaque compared to transparent chambers what could affect stomatal conductance or just decrease the water concentration gradient between air inside and outside of the plant and therefore reduce inflow of air into the plant and consequently the gas transport.

Pages 17414 Line 16 - 17416 Line 12: *„ Rephrase. Make two sentences.” … „Rather express in terms of model quality and not in terms of NSE value, like "Most models of BA P-C and GK P-L showed rather poor fits (NSE ranging from XX to XX)." The parentheses is not obligatory. Would also be fine without” … „ Check phrasing” … „ Rather: "Small scale spatial variability of methane emissions at BA..."” … „Like before I suggest to skip many of the numbers and to focus on important points like this one. This could well introduce the whole methane section. After all, the whole results section has to become much shorter and should focus in text on the remarkable things. As said before, present numbers in tables and information in text.“*

We revised the whole paragraph:

“**3.3.2 Annual methane emissions**

The Lloyd–Taylor methane models performed well for all sites except for the second year of BA *Phragmites–Carex* and GK *Phragmites–Lemna*. NSE for all but the *Phragmites australis* sites ranged between 0.38 and 0.85 (median 0.58). Models of the *Phragmites australis* sites were acceptable in the first year (median NSE 0.37, range 0.05 to 0.82) but performed poor in the second year (median 0.01, range -0.25 to 0.24). Models of GK *Phragmites–Lemna* III and BA *Phragmites–Carex* III did not explain the high emissions in August 2011 (Figs. 3h and 4h). Both and the model of BA *Phragmites–Lemna* I overestimated emissions in spring and early summer 2012. Annual emissions calculated alternatively for the mentioned plots and second year by linear interpolation were 25, 28, and 118 g CH_4−C m^{-2} yr^{-1}, compared to 30, 32, and 139 g CH_4−C m^{-2} yr^{-1} derived by the temperature driven Lloyd–Taylor methane model, and lie within the 90% confidence intervals of the latter (Table A2 in the Annex). The Lloyd–Taylor models were therefore accepted despite of negative NSE.
GK *Phragmites–Lemna* had the highest methane emissions of all sites, estimated to 100 (90% confidence interval 48, 147) and 101 (61, 177) g CH_4−C m^{-2} yr^{-1} in the first and second year, respectively (Table 3). GK *Carex–Lysimachia* released less methane and GK *Typha–Hydrocharis* was with 60 (47, 77) and 68 (52, 92) g CH_4−C m^{-2} yr^{-1} the smallest source among the studied sites at Giel'cykaŭ Kašyl', but still larger than the Barcianicha sites. BA *Phragmites–Carex* emitted 42 (28, 58) in the first and 36 (22, 52) g CH_4−C m^{-2} yr^{-1} in the second year. BA *Carex–Equisetum* was a much smaller methane source, but the absolute lowest annual methane emissions were estimated for BA *Eriophorum–Carex* being 10 (9, 13) in the first and 11 (10, 14) g CH_4−C m^{-2} yr^{-1} in the second year (Table 3). Inter-annual and small scale variability of methane emissions tended to increase with absolute methane emissions (Fig. 5; Table 4)."

Page 17416 Lines 23-24: *„ This confines the analysis..“*
Yes, and therefore we mentioned the number of plots and years.

Page 17416 Lines 25-26: *„ Start with the strongest and work your way down to the least.“*
We changed the sentence accordingly: "Median annual water level was very strongly with correlated GPP, weaker with NEE and CH_4 emissions, but not with R_{eco}, (Fig. 6)."

Page 17417 Line 11: *„ This is not really surprising..“*

You are right, but we wanted to mention this because the correlations were different when all sites were included (biomass not correlated with NEE but strongly with CH_4).

Page 17417 Lines 19-21: *„See! Remember my statement above about the inclusion of plant biomass parameters like LAI? I guess, this would have been beneficial..“*

Yes, we agree and we regret that we did not have the possibility to monitor LAI. However, it is nice that we still found the biomass relation for annual methane emissions.

Page 17418 Line 4: *„That is inexact since the GWP of the combined exchange of CO2 and CH4 is on the positive site. “*

We agree that the GWP of the combined exchange of CO2 and CH4 for the Barcianicha sites is (with one exception) positive (Table 5). And this means that the sites are GHG sources. Why do you regard our formulation "In both years the Barcianicha sites were very small GHG sources" to be inexact?

Page 17418 Line 4: *„was a small GHG sink“*

We added the missing "was": "… and in the first year BA *Phragmites–Carex* was a small GHG sink, …"

Page 17418 Line 15: *„ This should come later. First present the balances, then write about their robustness. “*

We agree and changed the two sections to each other.

Page 17418 Lines 17-20: *„Interesting and understandable but I would not start the discussion with something this specific. Best would be starting with the general level of GHG exchange on the sites in comparison to the literature. “*

We agree, please see our answer above.

Page 17418 Lines 22-24: *„ This is published elsewhere already by some of you. Therefore, you might use it as an argument when discussing your results or limitations further down but it should not come at the beginning of the discussion. “*

Yes, we agree, please see our two answers above.

Page 17419 Lines 2-4: *„ I don't understand why you argue about diurnal variability here. Just before it was about chambers and in the next sentence you address day-to-day and seasonal variability. However, the next paragraph addresses diurnal variability. Maybe skip this sentence here or move it to the next paragraph?"*

We agree and deleted this sentence.

Page 17419 Lines 10-12: *„But this is just for a specific time period and, thus, cannot be assumed for the whole measurement period"*

We, and this is a problem. We only know that the dynamic is most pronounced during sunny days in the vegetation season, when *Phragmites australis* is green and relative air humidity drops strongly. Outside the growing season no pronounced diurnal emission dynamic is reported for *Phragmites australis*. As we have sampled the dynamic only for very few days of the growing season we do not know if it would be stronger at other days of the vegetation season. However, we can be sure that daylight emissions are higher than night-time emissions, and that measurements around midday will mostly results in larger flux estimates as compared to morning or evening measurements. Consequently, as we usually sampled between about 10:00 and 16:00 we should have most often caught values that were around or above the 24-hour average. Building the temperature model on such flux estimates should result in annual fluxes that do not underestimate but rather overestimate the actual flux. However, we do not have any mean to calculate if we really overestimated the annual fluxes and by how much.

We changed the sentence as follows: "However, a single measurement at any time during daylight does not represent the daily emission average. For the monitored days (Fig. 2) most measurements between 9.00 and 18.00 h resulted in equal or higher estimates as compared to the 24 hour mean. This indicates that also at other days during the growing period daylight measurements will have rather tended to result in flux estimates at or above the daily mean than below it."

Page 17419 Lines 19-20: *„ If you can quantify this, you could also correct for the bias, couldn't you? "*

We can not correct for the bias, because we do neither know how far our single methane measurements during daylight were from the daily mean, nor how the diurnal emission amplitudes of the other days were. We have only good reasons, as given above, to assume that our measurements were mostly at or above the daily average and consequently the annual flux estimates should be at or above the real emissions, too.

Page 17420 Lines 2-4: *„ This sounds a bit like you decided on a gut feeling.“*

Yes, you are right. As you mentioned above, we have only very few diurnal emission data. The ratio 1.2 was the highest observed for GK *Typha–Hydrocharis* and measured at the plot with the highest cover of *Typha latifolia*. We do not know the ratio for other days (it could be lower, but also higher than our observation). However, this ratio was calculated only from measurements taken from sunrise to sunset. At night time there will have been no differences between transparent and opaque and therefore the 24 hour ratio will have been lower. By correcting the emissions with the highest observed ratio of 1.2 we can therefore be quite sure to avoid underestimation of annual methane emissions. Maybe we overestimate the emissions. However, as we can not estimate the overestimation we can not subtract it. In peatland rewetting projects it is better to overestimate the project emissions than to underestimate them, because the project proponent needs to be sure that the estimated GHG emissions reductions compared to the baseline are realistic (a conservative approach, cf. Couwenberg et al., 2011).

Page 17420 Line 4: *„We do not calculate annual emissions, we estimate them.“*

We replaced “measured” by “estimated”: “Estimated annual emissions will consequently be at the high end of real emissions from the site.”

Page 17420 Lines 6-7: *„Check phrasing.“*

We modified the sentence: “*Typha latifolia* was not present at GK *Carex–Lysimachia* I during monitoring of diurnal methane emission dynamics at this plot in summer 2012.”

Page 17420 Lines 16-18: *„If you don't correct for shading you would get better fittings? First, how do you know? Second, why then not skip correction?.“*

Model fit quality would be similar because the correction factor was applied to all measured fluxes during the growing season. As there was a significant impact of shading we had to correct for shading because our routine measurements were conducted with opaque chambers. Without correction we would underestimate annual fluxes. The point is that we found a correction factor of 1.1 and the studied plot had no *Typha latifolia* while the other plots of the site GK *Carex–Lysimachia* had some *Typha latifolia*. This plant is known to actively circulate air and this process can be reduced by shading. Therefore we applied the factor of GK *Typha–Hydrocharis* of 1.2 to be sure not to underestimate annual fluxes. Consequently our estimates are rather close to or slightly above the real methane fluxes what is better than underestimation (see above).

Page 17420 Lines 19-21: *„ And so what?“*

We completed the sentence: “The lack of any shading impact on methane emissions from BA *Eriophorum–Carex* and BA *Carex–Equisetum* corresponds to the findings of Joabsson et al. (1999) and Whiting and Chanton (1992) for *Eriophorum angustifolium* and *Carex rostrata*, what supports our decision not to apply any correction factor to the estimated methane fluxes.”

Page 17420 Line 22: *„ Should come first since it did so also in the MM section. And by the way, the section starts with methodological considerations. I think in a kind of standard GHG paper the core results (balance, fluxes) should be discussed first (Either per GHG or together in a section), then you go into detail (then best per GHG) on methodological discussions.“*

We moved it in front of the methodological considerations on methane and both (Robustness of annual GHG balances) behind the presentation and comparison of annual emissions with data from the literature, as you suggested.

Page 17420 Lines 23-27: *„I'm really not sure about these two approaches. Given the length of the MS and the small differences between them, why you just decide for one of the two and use this without making such a fuss about the other one? This could help straighten the text. What is the benefit of reporting on the two approaches?.“*

The benefit of reporting on both approaches is to become more confident in the results. Even obviously small differences in some aspects of in general similar CO2 model approaches can result in large differences of estimated fluxes. For a 14 month integration period Hoffmann et al. (2015) tested the impact of i) linear interpolation of parameters instead of weighted flux interpolation and ii) varying degree of data aggregation during the modelling process. They found for their data that each of both aspects alone changed the integrated NEE by about 100 g CO_2-C m^{-2}. Given that H-approach and the LS-approach differ from each other in more than two aspects it is quite surprising that the results were still quite similar. However, as the decision regarding a number of aspects that differ between both approaches (especially the estimation of measured fluxes, application of Michaelis-Menten vs. Falge2000, and the dealing with not significant fits) is not so clear, it would be arbitrary to skip one of the approaches. Aiming at robust estimates we regard it advantageous to integrate the results of both approaches and to reduce the dependency of the values from a single approach.

Page 17421 Lines 1-4: *„You don't need two approaches to state this since it is well established."*

The robustness of general model assumptions against differences in flux estimation and model parameterization is not well established and, as stated above, differences in some aspects of the calculation routine can have strong impacts on the model results (cf. Hoffmann et al., 2015). In our study the outcomes of the models were indeed similar, but this was not clear before and does not does allow for the conclusions, that both approaches will generally result in similar models. We would very much like to keep both approaches because this gives additional confidence in the flux estimates.

Page 17421 Lines 7-8: *„Check phrasing."*

We corrected the sentence: "Net CO_2 uptake at GK *Phragmites–Lemna* was similar to the estimates of Brix et al. (2001; Table 7) in the first year but two times higher in the second year."

Page 17421 Lines 10-12: *„ For which units and temporal periods?."*

We formulated the sentence more precisely: "Based on dry weight of green above ground biomass assessed at the end of the growing seasons 2011 and 2012 and on published ratios between above ground and below ground biomass production we estimated the net annual primary production (NPP, g C m^{-2} yr^{-1}) of the *Phragmites australis* sites during both GHG measurement periods (Table 6)."

Page 17421 Line 12: *„estimated"*

We replaced "calculated" by "estimated": "Using NPP, NEE, and GPP we estimated heterotrophic and autotrophic respiration (R_h and R_a, Table 6) and evaluated their meaningfulness."

Page 17421 Lines 15-19: *„Way too long and hard to follow. Please make 2 or 3 sentences and rephrase "*

We rephrased as follows: “The ratios of heterotrophic respiration to methane emissions (CO_2-C / CH_4-C) were 2.2 and 2.3 in the first an second year, respectively for BA *Phragmites–Carex* and closer, 1.0 and 1.1 for GK *Phragmites–Lemna*. Similar ratios were found in incubation experiments for organic bottom sediments and the upper peat layer of a flooded former fen grassland (Hahn-Schöfl et al., 2011).”

Page 17421 Line 23: *„ See! You use it here correctly yourself. We should strive to be very clear on that in all instances. “*

We checked and corrected this throughout the text.

Page 17422 Lines 1-2: *„A comparison to a single other study is, in my opinion, not a good basis to build a discussion upon. If you have three studies with similar results and yours differs then this could be a basis. Otherwise it seems a bit erratic because readers don't know why you chose this one and not any other one. “* and

Page 17422 Lines 7-11: *„But why your site was how your site was? That would be the interesting point and not why they were different. And again, single site comparisons are somewhat arbitrary and do not offer much news. Try to generalize.“*

We revised the first paragraph accordingly to both of your suggestion:

“Annual methane emissions from BA *Eriophorum–Carex* and BA *Carex–Equisetum* were of the same magnitude as from similar vegetation types in two rewetted cutover Atlantic bogs (Wilson et al., 2009, 2013). Net uptake and net release of CO_2, however, was smaller for BA *Eriophorum–Carex* and BA *Carex–Equisetum* as compared to the mentioned Irish sites (Wilson et al., 2008, 2013; Table 7), perhaps partly resulting from the more continental climate.”

Page 17423 Line 3: *„ All the time you use the scientific names and now you don't. Would be better to use the scientific names here as well. “*

We replaced the English by scientific names: “Annual methane and CO_2 fluxes from floating *Carex – Typha* mats are not reported in the literature.”

Page 17423 Line 5: *„Should be "pristine, water saturated sedge fen" “*

We added the missing comma: “Methane emissions from GK *Typha–Hydrocharis* and GK *Carex–Lysimachia* were higher compared to a pristine, water saturated sedge fen….”

Page 17423 Line 17: *„Better use the term "source" anywhere here. I had to read three times until I understand that this is about being a source for CO2.. "*

We skipped the first and rephrased the second sentence: "~~Both sites, however, were CO_2 and carbon sources.~~ However, a wet sedge fen in the southern Rocky Mountains (Wickland et al., 2001) and a water saturated *Typha angustifolia* marsh (Chu et al., 2015) were found to be CO_2 sources (Table 7).

Page 17423 Line 23: *„ leaves "*

We replaced "leafs" by "leaves".

Page 17424 Lines 10-12: *„What about the error terms? You should always add them because ? I think ? we would then easily see that the lower ones of these values are kind of meaning less because in the uncertainty range they could also be carbon sources."*

We added the error terms: "BA *Eriophorum–Carex*, BA *Carex–Equisetum*, BA *Phragmites–Carex* and GK *Phragmites–Lemna* had on average low GHG emissions (2.3 (90% confidence interval -1.0, 5.6), 4.2 (2.1, 6.8), −1.7 (-15.0, 10.2), and 4.2 (-26.8, 37.7) t CO_2 eq ha^{-1} yr^{-1}, respectively), and were mostly carbon sinks (−36 (-112, 28), −17 (-89, 63), −390 (-861, -164), and −795 (-1437, -363) g C m^{-2} yr^{-1}), confirming that important aims of peatland rewetting, i.e. restoration of the carbon sink function and reduction of GHG emissions have been largely achieved. Net carbon losses from GK *Typha–Hydrocharis* and GK *Carex–Lysimachia* of the terrestrialization zone (83 (-332, 352) and 276 (140, 539) g C m^{-2} yr^{-1}, respectively), in contrast, were similar as from peat extraction sites (280 g C m^{-2} yr^{-1} – Drösler et al., 2014) and GHG emissions (25.1 (9.5, 37.9) and 39.1 (26.6, 58.0) t CO_2 eq ha^{-1} yr^{-1}) were even comparable to deep-drained temperate fen grassland (26 t CO_2 eq ha^{-1} yr^{-1} – Drösler et al., 2014; 65 t CO_2 eq ha^{-1} yr^{-1} – Eickenscheidt et al., 2015)."

Page 17424 Lines 23-24: *„ levels, also for next occurrence "*

We replaced "level" by "levels" in both occurrences.

Page 17424 Line 26: *„ rather "depth" "*

We replaced "thickness" by "depth".

Page 17425 Line 17: *„ see above "*

We replaced “thickness” by “depth”.

Page 17425 Lines 10-11: *„ Formulation too absolute. Rather "At the study sites water level may have influence methane emissions rather via the plant species distribution than directly" Or so..“*

We adopted your suggestion: “At the study sites water level will have influenced methane emissions of the studied sites rather by plant species distribution then directly.”

Page 17425 Line 12: *„ Nitrous oxide emission were negligible for all sites. which likely resulted from....“*

We adopted your suggestion: “Nitrous oxide emissions were negligible for all sites, which likely resulted from permanent water saturatation and agrees with other studies from rewetted fens (Hendriks et al., 2007; Couwenberg et al., 2011; Wilson et al., 2013).”

Page 17425 Line 16: *„ How did you analyze these. Through indicator values?.“*

We first derived indicator values for species of our sites that were listed in the vegetation form concept (Koska et al., 2001) and then defined the nutrient conditions according to the range where the species overlapped. *Eriophorum angustifolium* for example occurs under oligotrophic and mesotrophic conditions, *Carex rostrata* under oligotrophic, mesotrophic and eutrophic conditions, and *Equisetum fluviatile* under mesotrophic, eutrophic and polytrophic conditions. Together they indicate for BA *Eriophorum-Carex* mesotrophic conditions.

We added this information as a third sentence to the methods section (page 17399, from Line 18): “Nutrient conditions of the sites were estimated using plant species groups as indicator (Koska et al. 2001).”

Koska, I., Succow, M., Clausnitzer, U., Timmermann, T., and Roth, S.: Vegetationskundliche Kennzeichnung von Mooren (topische Betrachtung), in: Landschaftsökologische Moorkunde, edited by: Succow, M. and Joosten, H., Schweizerbart, Stuttgart, 112–184, 2001.

Page 17425 Lines 26-28: *„ No question but also not surprising..“*

Yes, but as our data clearly shoes it, we decided to state it.

Page 17426 Line 4: *„ What else should be the strongest control if water levels are at or above ground? “*

Some things are trivial but still worth to be stated.

Page 17426 Lines 5-9: *„ But when you treat CH4 as a GHG and consider GWP the picture changes, doesn't it? Also it should be given in percentage of annual site emissions to be comparable. “*

Yes, if accounted for the GWP there is no difference (small scale variability of NEE = 3.4±4.0 t CO_2-eq ha^{-1} yr^{-1} and of CH_4 = 2.9±3.6 t CO_2-eq ha^{-1} yr^{-1}).

Page 17426 Lines 11-12: *„ As the last comment already suggests, taking the absolute values has only little meaning. Given that we typically measure CO2 in ppm and CH4 in pub I would state that CH4 exchange rates showed much higher variability in space and time (which is about the state of the art). The same holds for the annual comparison because it is not known whether the years were strongly different or not in comparison to an ? unfortunately imaginary ? long-term time series of annual emissions.“*

We agree that our sample number is to small (three plots, two years) for a real analysis of small scale and inter-annual flux variability. We also see the point that it is somewhat arbitrary to decide on reporting the variability on the element base or as GWP. We went for the element base because this is common in the literature (Helfter et al. 2015, Roulet et al., 2007). The latter reference found that NEE is the largest and most variable component of the C balance. We would avoid the decision between element base and GWP, if we would report variability as percentages of the annual flux. However, by this we would have lower variability for stronger sinks or sources and higher for sites with fluxes around zero. Inter-annual variability of NEE would be 221% for BA *Eriophorum-Carex* and 35% for GK *Phragmites-Lemna*. The reader could get the impression that NEE of the latter was more stable than NEE of BA *Eriophorum-Carex*. But this is wrong when absolute figures are considered (cf. Fig. 5). Absolute figures are more important to evaluate the stability of a peatland and the risk of high emissions after rewetting. Our aim was not a thorough analysis of small scale and inter-annual variability of emissions but to find out how strongly emissions differed between plots and years. Interestingly, while the small scale variability of GHG emissions is, as you expected, indeed not different between NEE (3.4±4.0 t CO_2-eq ha^{-1} yr^{-1}) and methane emissions (2.9±3.6 t CO_2-eq ha^{-1} yr^{-1}), the inter-annual variability is larger for NEE (4.2±4.3 t CO_2-eq ha^{-1} yr^{-1}) as compared to methane emissions (1.4±1.6 t CO_2-eq ha^{-1} yr^{-1}).

However, the paragraph on small scale and inter-annual variability is not essential for the manuscript. We therefore suggest the following solution: we keep on stating the variability on the element base in Table 4 (as the element base is often used and can be easily translated into GWP) but skip the paragraph were small scale and inter-annual variability are discussed, Page 17426 Lines 11-14: ~~“Small scale variability, calculated as absolute difference between annual plot emissions and annual site emissions was larger for NEE (92±108 g CO_2–C m^{-2} yr^{-1}) than for methane emissions (8±10 g CH_4–C m^{-2} yr^{-1}). Also inter-annual variability, calculated plot-wise as the absolute difference of annual emissions from the two years mean, was larger for NEE (116±119 g CO_2–C m^{-2} yr^{-1}) as compared to methane emissions (4±4 g CH_4–C m^{-2} yr^{-1}). Both can be explained by the fact that CO_2-fluxes are more directly linked to plant productivity than methane fluxes (Hyvönen et al., 1998; Bonneville et al., 2008; Schneider et al., 2012).”~~

Page 17426 Line 27: *„ Check phrasing, I think there is an "a" missing.“*

We added the “a”: “Plant litter was more abundant at Giel’cykaŭ Kašyl’, certainly because of higher plant productivity, but also because of a longer period since rewetting.”

Page 17427 Lines 10-17: *„ Would be nice in the discussion but seem a bit local for the conclusion in which we should strive to generalize our findings beyond the specific study site or study period..“*

The paragraph summarizes the most important outcomes of the discussion and serves as introduction for the conclusion. Section three of the discussion elaborates one point after the other but we regard it supportive for the reader to mention these outcomes at once combined. Why not doing it in the beginning of the conclusion?

Page 17427 Lines 19-20: *„ Check phrasing..“*

We rephrased the sentence: “This implies that the formulation of robust emission factors for high-productive vegetation types and mire ecosystems requires more long-term and spatially resolved GHG emission studies than for low-productive.”

Page 17427 Lines 26-27: *„Yes, this is a conclusion sentence!!..“*

Thank you.

Page 17440: *„Looks like you were quite a disturbance.“*

Not really, because the species coverages changed in both direction. *Phragmites australis*, for example, grew better at GK *Phragmites–Lemna* in the second year.

Page 17441: *„Although I understand that you want to present all numbers correctly and confidence intervals do not spread evenly around the mean I think the representation is hard to read. What about reporting like 339_364_396 (Reco of BA E-C year 1) or similar to that (e.g. just with spaces between the numbers..“*

We propose to replce “to” by “,”.

Table 3 would then become:

Table 3. Annual fluxes of CO_2, CH_4, and Carbon (C balance = NEE + CH_4 emissions) with 90% confidence intervals.

Site	Year	R_{eco} (g CO_2–C m^{-2} yr^{-1})	GPP (g CO_2–C m^{-2} yr^{-1})	NEE (g CO_2–C m^{-2} yr^{-1})	CH_4 emissions (g CH_4–C m^{-2} yr^{-1})	C balance (g C m^{-2} yr^{-1})
BA *Eriophorum–Carex*	1	364 (339, 396)	-449 (-512, -407)	-86 (-130, -38)	10 (9, 13)	-75 (-114, -30)
	2	406 (368, 458)	-413 (-449, -376)	-7 (-49, 21)	11 (10, 14)	4 (-35, 30)
BA *Carex–Equisetum*	1	232 (196, 262)	-320 (-361, -279)	-88 (-114, -68)	17 (13, 22)	-71 (-92, -56)
	2	327 (282, 371)	-302 (-334, -281)	24 (-6, 55)	13 (9, 16)	37 (8, 66)
BA *Phragmites–Carex*	1	614 (478, 737)	-1141 (-1595, -888)	-528 (-933, -194)	42 (28, 58)	-486 (-873, -156)
	2	706 (568, 842)	-1035 (-1134, -949)	-329 (-431, -220)	36 (22, 52)	-293 (-377, -205)
GK *Typha–Hydrocharis*	1	921 (841, 982)	-771 (-842, -665)	151 (41, 300)	60 (47, 77)	210 (111, 360)
	2	973 (818, 1156)	-1086 (-1476, -862)	-113 (-418, 66)	68 (52, 92)	-45 (-343, 142)
GK *Carex–Lysimachia*	1	1105 (1007, 1207)	-940 (-1081, -774)	166 (66, 252)	86 (63, 121)	252 (145, 356)
	2	1270 (1221, 1362)	-1054 (-1243, -789)	216 (48, 470)	85 (59, 142)	301 (137, 552)
GK *Phragmites–Lemna*	1	936 (733, 1200)	-1547 (-1726, -1386)	-611 (-819, -450)	100 (48, 147)	-516 (-747, -349)
	2	1092 (937, 1210)	-2267 (-2733, -1843)	-1175 (-1567, -690)	101 (61, 177)	-1074 (-1453, -565)

Uncertainties on the site level include the uncertainties of the plot models and the spatial heterogeneity. They were calculated by pooling the plot specific annual models derived by error calculation. Different CO_2 balances of the H-approach and the LS-approach were accounted for by adding the differences randomly to 50% of the respective annual values derived by error calculation with the H-approach. To derive uncertainties of C balances the annual models of NEE and CH_4 derived by plot–wise error calculation were summarized and combined site–wise.

Page 17442: *„ See comment in text. I am skeptical about reporting this in absolute terms..“*

We prefer to keep to the difference on the element base because these can easilily be translated into GWP (see our response above). Reporting variability in percentages of the annual emissions would lead to seemingly strong variability for sites with fluxes around zero and small for sites with large fluxes. However, we aimed at estimating how stable the rewetted sites with respect to emissions are (please see also our response above).

Page 17451: *„ Should be capital letters, like in the figure. And I really don't understand the many bars. Do you give all replicates separately? I strongly advice to put them together per site! There are examples of efficiently bringing the terms together to show them in balance bar plots in the literature..“*

Yes, it must be capital letters.

We present all replicates separately because we aimed at visualizing the differences of GHG emissions among them. This is not often done in the literature. However, it is quite instructive, because it gives an idea of the spatial and inter-annual variability of site emissions. Of course, the small number of years and plots does not allow for conclusive analysis (see our response above), but still we can conclude that emissions from some vegetation types are more stable than from other. We give confidence intervals for all replicates to show the uncertainty and allow to roughly estimate if GHG emission differences between plots are significant. We bring combine the plot emissions and present site emissions later, in Tables 3 and 5. We understand your concern in overestimation the importance of differences among plotsa and propose to skip Table S2 where the emissions of all plots are listed. Still we would like to present them in Figure 5, just to give an impression of the variability.

Page 17452: *„ No, these are scatter plots in which we might see correlations...“*

We modified the figure subtitle accordingly: “Scatter plots of annual NEE, R_{eco}, GPP, CH_4 emissions, median annual water levels (both years for all plots, n = 36), and above ground biomass carbon (second year for all plots, *n* = 18). Spearman’s ρ significant at ‘ $P \leq 0.05$; * $P \leq 0.01$; ** $P \leq 0.001$; *** $P \leq 0.0001$. Spearman’s ρ in brackets without GK *Typha–Hydrocharis* and GK *Carex–Lysimachia* (*n* = 30 for correlations among water levels and fluxes; *n* = 15 for correlations among biomass and fluxes). Small symbols indicate first year, large symbols second year.”