

Author response to comments of Referee #2

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We are very grateful for the detailed and constructive comments provided by Anonymous Referee #2. They especially helped us to improve the results, discussion and conclusion parts of our manuscript. Below we listed the comments of Referee #2 followed by our responses which are marked in blue.

1. Line 218: What does 'enhanced' mean here – is this still simply a lookup table method or does it include something else?

Our “enhanced” Look-up Table (LUT) approach corresponds to the Marginal Distribution Sampling (MDS) approach (see e.g. Moffat et al. 2007). The term “enhanced” indicates an essential modification in comparison to the standard LUT: missing NEE is filled with the mean value of data under similar meteorological conditions (radiation, air temperature and vapour pressure deficit) of a fixed margin within a moving window. Thus, the temporal autocorrelation of NEE is exploited. The algorithm varies in case of incomplete meteorological data (see Reichstein et al. 2005). To adapt to the common terminology we replaced the abbreviation “LUT” by “MDS” at all occurrences in the manuscript and changed page 8 lines 218-221 to: “A Marginal Distribution Sampling (MDS) approach proposed by Reichstein et al. (2005), available as web tool based on the R package REddyProc (<http://www.bgc-jena.mpg.de/REddyProc/brew/REddyProc.rhtml>) was applied for gapfilling and partitioning of NEE measurements (LUT_{CO2nofoot}), with air temperature as temperature variable.”

2. Line 251: The outer pair of brackets is not needed here.

We agree and deleted the outer pair of brackets.

3. Line 300: The statements about the water level are confusing when comparing them with line 112 in the site description. There the water depth was said to ‘range from 0.1 and 0.7 m’ (does this refer to spatial or temporal variation?) and here the temporal fluctuations are shown to be 0.36 and 0.77 m as visible from Fig. 2. How do these two statements fit together?

We apologize for the confusion and the declaration of a rather misleading water level range. The range “0.1 to 0.7 m” on page 4 line 112 and page 7 line 206 is the generously rounded range of the mean annual water level 2008-2012 generated by measurement based water level modelling. For a long-term range we refer to Zak et al. (2015) reporting water levels between 0.2 m and 1.2 m above the surface at a specific gauge between 2004 and 2012. We replace the range “0.1 to 0.7 m” on page 4 line 112 and add in brackets “2004 to 2012; Zak et al. 2012”. We deleted the water level information on page 7 line 206 as we declare the temporal range for our study period within the results part. This range is measured at one single position close to the tower, including the snow cover on ice covering the shallow lake. This measurement is not representative for the whole shallow lake, as the study site is characterised by a distinct microtopography due to previous shrinkage and subsidence of the peat in consequence of drainage and degradation.

4. Line 304: Why were median fluxes instead of averages or totals given here? I think this is not very common and should therefore be briefly explained.

We present median values for our flux measurements as this is the best measure of a central tendency in a skewed dataset due to not evenly distributed gaps.

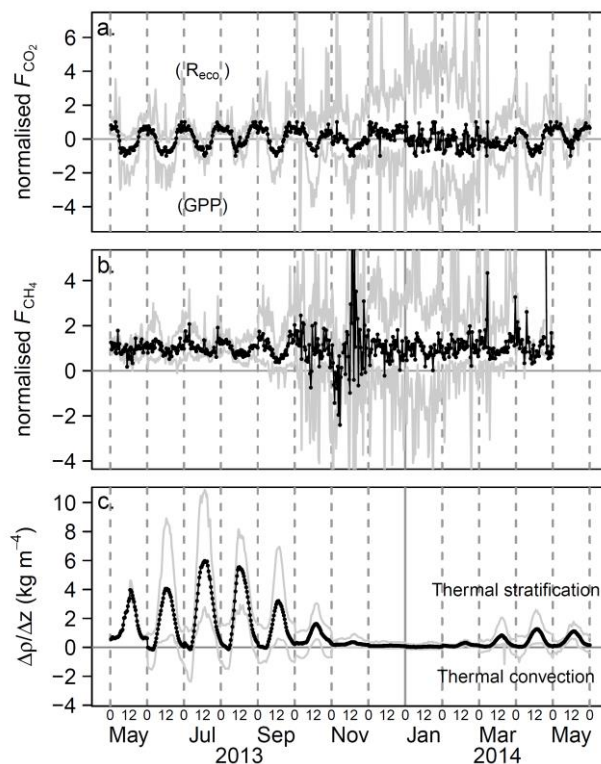
5. Line 309ff: Why were the CH₄ fluxes normalized but not the CO₂ fluxes?

By normalising the mean half-hourly CH_4 fluxes per month we can illustrate the diurnal pattern of CH_4 fluxes, which was hardly visible in the unnormalised fluxes during months with generally low CH_4 exchange rates. We did not normalise the CO_2 fluxes so far as we can detect a diurnal cycle for the same months based on both normalised and unnormalised fluxes. However, to be consistent we now also normalised the mean half-hourly CO_2 fluxes per month. In addition, we decided to also include fluxes of days where less than five half-hourly flux values are available, thus including mean half-hourly CH_4 fluxes for April 2014, which are based on three days only, due to the dismantling of the sensor.

We modified lines 307-314 on page 11 to:

”To investigate the potential presence of a diurnal cycle of CO_2 and CH_4 fluxes throughout the study period we normalised the mean half-hourly CO_2 and CH_4 fluxes per month with the respective minimum/ maximum and median of the half-hourly fluxes of the specific month (modified from Rinne et al. 2007). A pronounced diurnal cycle of CO_2 fluxes with peak uptake around midday and peak release around midnight was obvious until November 2013 and beginning in March 2014 (see Fig. 3), although less pronounced in these two months. We found a clear diurnal cycle of CH_4 fluxes from June to September 2013 and in March 2014 (April 2014 based on 3 days only and May 2014 not available as the sensor was dismantled) with daily peaks during night-time (around midnight until early morning).”

We changed Fig. 3 as follows:



“Figure 3: Average diurnal cycle of a) CO_2 flux, b) CH_4 flux and c) the water density gradient per month. The numbers at the x-axis denote midnight (0) and midday (12) in UTC. Midnight is also illustrated with a dashed line. Black and grey lines represent the mean and the range, respectively. The CO_2 and CH_4 fluxes are normalised with the monthly minimum/ maximum and the median of the half-hourly fluxes, respectively. Although the zero line is slightly shifted due to normalisation, positive CO_2 fluxes roughly indicate the dominance of R_{eco} against GPP, negative fluxes the dominance of GPP against R_{eco} . The period of ice-cover was excluded from the calculation of the temperature gradient. A density gradient equal to or below zero indicates thermally induced

convective mixing down to the bottom of the open water body of the shallow lake, positive gradients instead thermal stratification.”

6. Line 363: Insert “for the AOI” before “than”.

Done.

7. Lines 384ff: Would convection also affect the CO₂ emissions from the lake? Please discuss whether this is possible – or why you think it’s not.

For our response to this comment we refer to our response to comment 4 of Referee #1.

8. Line 417: Replace “typically” with “typical”.

Done.

9. Line 451: Add “and a higher rate of CH₄ oxidation in the aerated top soil” after “CH₄”.

We agree and changed lines 448-451 on page 15, also considering the impact of soil shading: “Furthermore, soil shading potentially supports CH₄ oxidation, as the growth and activity of methanotrophic bacteria is reported to be inhibited by light (Dumestre et al. 1999, Murase and Sugimoto 2005). Besides, the soil of emergent vegetation stands is generally only temporarily and partly inundated and the water table decreased additionally during the unusual warm and dry summer 2013, probably resulting in a lower rate of anaerobic decomposition to CH₄ and a higher rate of CH₄ oxidation in the aerated top soil.”

10. Lines 495ff: This is one of the (few) weak points of this study: With only one year of data that happened to be characterized by “unusual meteorological conditions” the question arises as to what extent the observation of the wetland being a large GHG source can be transferred to other sites and other years. Other studies have shown multi-year trends in GHG budgets following wetland restoration. I suggest that the authors discuss this in more detail, taking for example the papers by Waddington and Day (2007, JGR) or by Herbst et al. (2013, this journal) and/or the respective references therein into account.

The unusual meteorological conditions during our study period might have caused a differing GWP compared to years with usual meteorological conditions, highlighting the need of long-term measurements. Moreover, based on the few existing studies a consistent picture and development of the GHG exchange behaviour does not seem to exist for rewetted fens, probably due to a variety of driving conditions and processes. We agree to extend our comparison with other studies and for that refer to our response on comment 12 (changes for the paragraph of lines 495-504 on page 17).

In addition, we changed lines 491-494 on page 16f.: “Our results imply a delay of the ecosystem towards a C sink with reduced climate impact, which might be the result of the exceptional characteristics represented by eutrophic conditions and lateral transport of organic matter within the open water body.”

Within the conclusions we deleted the sentence “Our results show [...]” in lines 522f. and the sentences in lines 525-528 starting with “In combination with [...]” and changed lines 534-536: “Inter-annual comparison are also necessary to verify what the results of this study imply: that the intended effects of rewetting in terms of CO₂ emission reduction and C sink recovery are not yet achieved at this site.

In this context, the effect of unusual meteorological conditions needs further investigation. More general statements for the climate impact of rewetted fens can only be provided by inclusion of additional sites varying e.g. in groundwater table and plant composition.”

11. Line 514: I suggest adding a phrase like “... and the interannual variability if short-term studies like this one are involved” to the end of this sentence.

We agree and changed the sentence to: “Inter-site comparisons (e.g. with other shallow lakes evolved during fen rewetting) are challenging with regard to the site-specific spatial heterogeneity and further the interannual variability, if short-term studies like the present one are involved.”

12. Lines 517ff: What I miss in the conclusions is some statement or estimate that relates the finding of this study to the situation of drained fen grasslands, at least on the basis of literature data. Does the described method of rewetting (involving the flooding of substantial parts of the area) make the GHG budget worse than that of a drained fen? Or just worse than that of a more cautiously restored fen (with less surface inundation), but still better than that of the drained situation?

The climate impact of our study site is stronger than generally expected for rewetted peatlands, apart from the CH₄ hot spot characteristic of newly rewetted sites. We mentioned in lines 459f. on page 15 that the net CO₂ budget for the EC source area at our study site was higher or similar to those of drained and degraded peatlands under grassland management (e.g. Hatala et al. 2012, Schrier-Uijl et al. 2014). In addition, CH₄ release was remarkably higher than for the referenced degraded sites, resulting in a stronger climate impact of our study site. Time plays an important role for the climate impact after rewetting and success is often achieved only several years or decades after rewetting (e.g. Hendriks et al. 2007/ Schrier-Uijl et al. 2014). Minke et al. (2015) showed still strong GHG emissions even after 25 years of rewetting due to strong above-surface water level fluctuations. However, the effect of water level does not seem to be consistent along different sites, especially for CO₂. Secondary plant succession towards a peat forming vegetation (Zerbe et al. 2013) and terrestrialisation (Zak et al. 2015) are reported to be requirements for peat formation and thus the revitalisation of the C sink function in case of inundated conditions in consequence of rewetting (but e.g. Knox et al. 2015, see response on comment Nr. 10). At our study site emergent vegetation, but especially non-peat-forming *Typha latifolia*, is progressively entering and organic mud is steadily filling up the open water body. Ongoing investigations will show, how the GHG exchange will develop.

We changed lines 400-409 on page 14 as follows and corrected a mistake in the emission factors derived from IPCC (2014):

“The CH₄ emissions of our studied ecosystem were within the range of other temperate fen sites rewetted for several years (up to 63 g CH₄ m⁻² a⁻¹; e.g. Hendriks et al. 2007, Wilson et al. 2008, Günther et al. 2013, Schrier-Uijl et al. 2014). This rate is remarkable higher than the emission factor of 28.8 g CH₄ m⁻² a⁻¹, that was assigned to rewetted temperate rich organic soils, which is in turn more than twice the rate of the nutrient-poor complement (IPCC 2014). In contrast, newly rewetted fens emit its multiple. In the first year after flooding, Hahn et al. (2015) observed an average net release of 260 g CH₄ m⁻² a⁻¹, which is 186 times higher than before flooding, at a fen site in NE Germany. Two years later the CH₄ emissions were considerably lower (40 g CH₄ m⁻² per growing season; Koebsch et al. 2015). However, natural (e.g. Bubier et al. 1993, Nilsson et al. 2001) and degraded fens (Hatala et al. 2012, Schrier-Uijl et al. 2014) release most often less CH₄ than the majority of rewetted fens, with some exceptions (e.g. Huttunen et al. 2003).”

In combination with comment Nr. 10 we changed the paragraph of lines 495-504 on page 17 as follows:

“However, the unusual meteorological conditions during our study period might have caused a differing (lower or higher) GWP compared to previous years. CH₄ emissions might have been lower at the expense of high net CO₂ release, whereas under usual meteorological conditions e.g. CO₂ uptake could probably compensate the CH₄ emissions. Inundation is generally associated with high CH₄ emission. Thus, the course of rewetting the water table is generally recommended to be held at or just below the soil surface to prevent inundation and thus, the formation of organic mud (Couwenberg et al. 2011, Joosten et al. 2012, Zak et al. 2015). In contrast to CH₄, the influence of water level on net CO₂ release is not consistent in the few existing studies of rewetted peatlands. In contrast to our site and e.g. Petrescu et al. (2015) and Minke et al. (2015), Knox et al. (2015) reported high net CO₂ uptake to substantially compensate high CH₄ emissions for a site with mean water levels above the soil surface after several years of rewetting (see Table 5). Similarly, Schrier-Uijl et al. (2014) reported high CO₂ uptake rates for a Dutch fen site 7 years after rewetting and even C uptake and a GHG sink function after 10 years with water levels below or at the soil surface. Herbst et al. (2011) present a snapshot of the GHG emissions of a Danish site after 5 years of rewetting with permanently and seasonally wet areas, whereby high CO₂ uptake and moderate CH₄ emissions lead to substantial GHG savings. In contrast, weak CO₂ uptake and decreasing, but still high CH₄ emissions were reported for another fen site in NE Germany with mean water levels above the soil surface (Koebsch et al. 2013, 2015 and Hahn et al. 2015), resulting in a decreasing climate impact after 3 years of rewetting. Interestingly, changes of NEE due to flooding were negligible, although GPP and Reco rates decreased considerable due to the flooding (Koebsch et al. 2013). In comparison to the decreasing CH₄ emissions at this site, Waddington and Day (2007) report enhancing CH₄ release for a Canadian peatland in the first three years after rewetting. A third rewetted fen site in NE Germany with water levels close to the soil surface was reported as weak GHG source 14-15 years after rewetting (Günther et al. 2015).”

We changed lines 538-540 as follows:

“Along with chamber measurements at the open water body our study shows that permanent (high) inundation in combination with nutrient-rich conditions involves the risk of long-term high CH₄ emissions. They counteract the actually intended lowering of the climate impact of drained and degraded fens and can result in an even stronger climate impact than degraded fens, as also shown by previous studies.”

Apart from the suggestions of the two referees we decided to change lines 391-394 as follows: “Apart from convective mixing, highest sediment and soil temperature in the night till early morning might play an important role for the peak emissions of CH₄ due to increased microbial activity. Furthermore, diurnal variability in CH₄ oxidation could contribute to the daily pattern of CH₄ release. Oxygen is supplied to the water, sediment and soil during the day in consequence of photosynthesis and increases CH₄ oxidation. However, convective mixing of the water column during the night might supply oxygen to deeper water depths potentially increasing CH₄ oxidation. We assume plant-mediated transport to be characterised by a reverse diurnal cycle with peak emissions during day-time, as the release of methane is dependent on the stomatal conductance of the plants (e.g. Morrissey et al. 1993). This pathway is limited to plants with aerenchymatic tissue like *Typha latifolia*, which dominates the eulittoral zone at our study site. CH₄ is transported from the soil to the atmosphere, bypassing potential oxidation zones above the rhizosphere (chimney effect). Unusually for wetland plants (Torn and Chapin 1993), complete stomatal closure during night was observed for *Typha latifolia* (Chanton et al. 1993). However, this temporal constraint seems to be superimposed by more efficient CH₄ pathways during the night and early morning.”

Furthermore, we added the following to line 514 on page 17: “Comparisons might be misleading in case the fractional coverages of the main surface types are not considered. Furthermore, as shown by

Wilson et al. (2007, 2008) and Minke et al. (2015) vegetation composition has a remarkable effect on GHG emissions of rewetted peatlands and should be considered in inter-site comparisons.”

In addition, we recognized a mistake in Table 4 due to the erroneous line 7 (CH₄ emission from water) in Table 10 in Hendriks et al. (2007). In alignment with Table 7 in Hendriks et al. (2007) the right value for CH₄ emission from water for 2005 has to be 37.3 g C m⁻² a⁻¹, i.e. 46 g CH₄ m⁻² a⁻¹. We corrected the wrong value in Table 4 and also changed the study year of this observation to “2005”. In addition, we added the annual net CH₄ exchange for 2006 according to Table 7 in Hendriks et al. (2007): 49 g CH₄ m⁻² a⁻¹ at water levels above 0 m.

Additional references

Note that we do not list references, which are already mentioned in the Discussion Paper.

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