REFEREE COMMENTS:
Referee #2

We greatly appreciate all comments from the reviewer. These detailed comments have greatly improved the quality of the manuscript.

MAIN COMMENTS TO THE AUTHOR(S)

1) My greatest concern is that there is insufficient testing of the results via thorough reference to the wetland flux literature. Specifically, the CO2 flux component (NEE, GEP, Re) magnitudes are compared in detail with results from other types of ecosystems in the region that the authors are familiar with (Table 2), being forests and grassland, but not with relevant wetland studies.

[Response]

We agree with the comments from the referee and have added the following text on additional comparisons to wetland studies at the end of Section 4.3.1 (at line 254):

"The annual NEE in this study was more negative than in the majority of previously reported NEE values for pristine temperate peatlands, which were weak sinks, typically in the range of -50 g C m^{-2} $year^{-1}$ (Roulet et al., 2007; Christensen et al., 2012; Humphreys et al., 2014; McVeigh et al., 2014; Peichl et al., 2014, Pelletier et al., 2015). Values that are comparable to the current restored wetland were reported in five pristine temperate wetlands: −248 g C m^{-2} $year^{-1}$ (Lafleur et al., 2001), −234 g C m^{-2} $year^{-1}$ (Campbell et al., 2014), -210 g C m^{-2} $year^{-1}$ (Fortuniak et al., 2017), −189 g C m^{-2} $year^{-1}$ (Flanagan and Syed, 2011), and −103 g C m^{-2} $year^{-1}$ (Lund et al., 2010). The few datasets in the literature for NEE of restored wetlands showed a wide range of values. Some were CO_2 sources, with NEE ranging from +103 g C m^{-2} $year^{-1}$ to +142 g C m^{-2} $year^{-1}$ (Strack and Zuback, 2013; Richards and Craft, 2015; Järveoja et al., 2016). Other measurements in restored wetlands, however, were sinks, all of them stronger than in this study, with NEE values ranging from -804 g C m^{-2} $year^{-1}$ to -270 g C m^{-2} $year^{-1}$ (Hendriks et al., 2007; Badiou et al., 2011; Herbst et al., 2013; Knox et al., 2015; Anderson et al., 2016). In this study, values of R_e and GEP were lower than those found for a restored wetland at a comparable latitude in the central Netherlands with slightly lower annual temperature and precipitation (Hendriks et al., 2007). R_e and GEP in this study area were also lower than values for most pristine peatlands at comparable latitudes (Helfter et al., 2015; Levy and Gray, 2015). Comparably low R_e and GEP were reported from the 'Mer Bleue' boreal raised bog (Lafleur et al., 2001; Moore et al., 2002) and from an Atlantic blanket bog (Sottocornola and Kiely, 2010; McVeigh et al., 2014), both of which experienced a lower mean annual temperature."

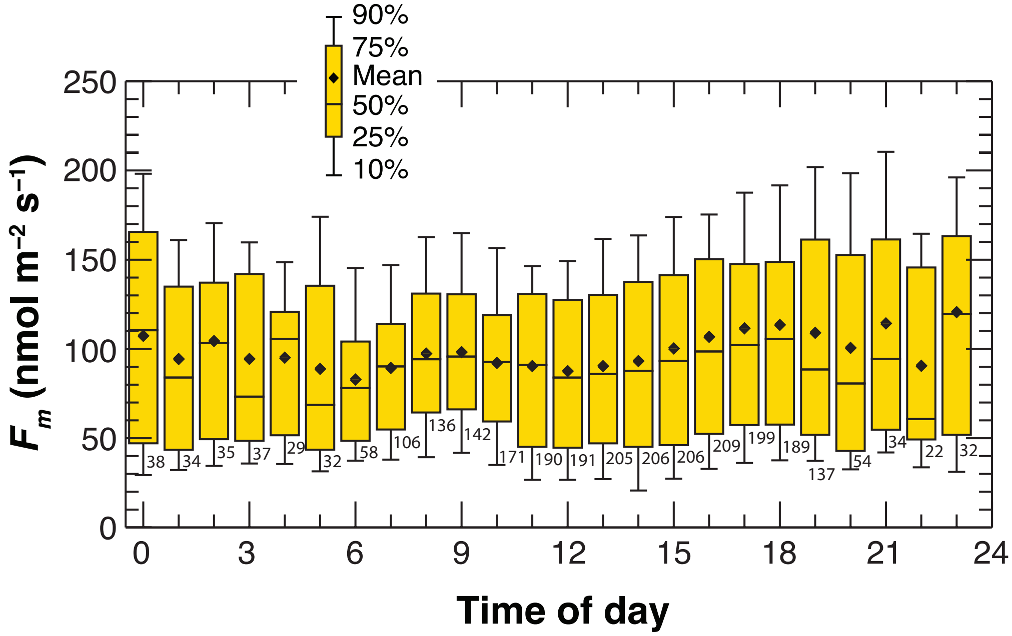
2) There is a growing body of literature reporting annual and sub-annual FCH4 data from EC sites over wetlands, yet little reference to this literature is made.
[Response]
We appreciate reviewer's comments. In order to provide a more comprehensive comparison of our CH_4 fluxes, we added the following paragraph to Section 4.4.1 (it starts at line 298):

"The annual CH_4 flux in this study area was lower than CH_4 fluxes reported for other restored wetlands (Anderson et al., 2016; Hendriks et al., 2007; Knox et al., 2015; Nahlik & Mitsch, 2010). Despite the study area being flooded for most of the study year, CH_4 emissions were closer to fluxes measured over drained peatlands (Kroon et al., 2010; Schrier-Uijl et al., 2010). Only Herbst et al. (2013) reported an annual CH_4 flux from a restored wetland in Denmark that was lower than in this study (9 to 13 g CH_4-C m^{-2} $year^{-1}$). Our annual CH_4 flux at 16 g CH_4-C m^{-2} $year^{-1}$ was comparable to an average natural temperate wetland CH_4 flux, which is typically around 15 g CH_4-C m^{-2} $year^{-1}$ (Nicolini et al., 2013; Turetsky et al., 2014; Abdalla et al., 2016; Fortuniak et al., 2017). The CH_4 fluxes from a number of temperate and tropical pristine wetlands exceeded the CH_4 fluxes reported in this study, including emissions from marshes in the Southwestern US (130 g CH_4-C m^{-2} $year^{-1}$, Whiting & Chanton, 2001), tropical wetlands in Costa Rica (82 g CH_4-C m^{-2} $year^{-1}$, Nahlik & Mitsch, 2010), and marshes in the Midwestern US (50 g CH_4-C m^{-2} $year^{-1}$, Koh et al., 2009). However, all these studies were conducted using chambers and the sampling frequency was at most once per month."

3) The authors may have made calculation errors in converting 30-minute fluxes through to annual values, certainly this appears to be the case for the methane fluxes shown in Fig. 6, and listed in Table 1.

[Response]

Thank you very much for bringing this to our attention. In Figure 6, we actually plotted only data that was measured (hence the different number of cases in each hour), and we excluded gap-filled data. There were significantly more datasets available from the summer half-year (higher CH_4 fluxes) than from the winter half-year (lower CH_4 fluxes), consequently the data in the figure cannot be simply averaged. To make this clearer, we have changed the caption and corrected the units (it was incorrectly labelled "µmol" instead of "nmol"). The corrected Fig. 6 is as follows:



The new caption reads:

"Figure 6: ~~(a)~~ Diurnal course of ~~filled~~ measured CH_4 fluxes from the EC-2 system during the study period."

Also, we have corrected the related text in Section 4.4.2 as follows:

"The ensemble diurnal courses of the ~~gap-filled~~ CH_4 fluxes ~~(measured CH_4 emissions and gap-filled by modelled CH_4 fluxes)~~ measured by the EC-2 system are shown in Fig. 6 from 16th June 2015 to 15th June 2016."

SPECIFIC COMMENTS TO THE AUTHOR(S)

1) Lines 38-40. Many of the cited studies here are horribly out of date or completely inappropriate. For instance, den Hartog et al. (1994) appears to be only an energy balance study and Schulze et al. (1999) is a forest study. Citing incorrectly at this early stage of a manuscript is a sure way for a reviewer to lose confidence!

[Response]

We appreciate reviewer's comments. The first paragraph of the introduction has been re-written to include more recent studies and omits den Hartog et al. (1994) and Schulze et al. (1999) as follows:

"Wetland ecosystems play a disproportionately large role in the global carbon (C) cycle compared to the surface area they occupy. Wetlands cover only 6% – 7% of the Earth's surface (Lehner and Döll, 2004; Mitsch et al. 2010), but they act as a major sink for the long-term C storage by sequestering carbon dioxide (CO_2) from the atmosphere. For example, strong C sinks (896 to 1139 g CO_2-C m^{-2} yr^{-1} and 1236 g CO_2-C m^{-2} $year^{-1}$) were found in Southeast USA and Eastern France, respectively (Mitsch et al. 2013; Grasset et al., 2016). Other wetlands around the world sequester around 100 g CO_2-C m^{-2} $year^{-1}$ (Petrescu et al., 2015; Bortolotti et al., 2016; Lu et al., 2016). C storage in wetlands has been estimated to be up to 450 Gt C or approximately 20% of the total C storage in the terrestrial biosphere (Bridgham et al., 2006; Lal, 2008; Wisniewski and Sampson, 2012). However, wetlands emit significant quantities of methane (CH_4), a powerful greenhouse gas (GHG), due to anaerobic microbial decomposition (Aurela et al., 2001; Rinne et al., 2007). CH_4 emissions from wetlands are responsible for 30% of all global CH_4 emissions (Bergamaschi et al., 2007; Bloom et al., 2010; Ciais et al., 2013). Peatlands are the most widespread of all wetland types in the world, representing 50 to 70% of global wetlands (Roulet, 2000; Yu et al., 2010). Their dynamics have played an important role in the global C cycle during the Holocene period (Gorham, 1991; Yu, 2011; Menviel and Joos, 2012), and it has been shown that it is crucial to include peatlands in the modelling and analysis of the global C cycle ~~to mitigate the changes in other C reservoirs is highly relevant~~ (Frolking et al., 2009; Wania et al., 2009; Kleinen et al., 2010)."

2) Line 40-41. Again, there seems little rationale for choosing these particular references as representative. Overall, I suggest that the introduction should contain as up-to-date references as possible, especially in the wetland eddy flux discipline where so many recent advances have been made.

[Response]

We appreciate reviewer's comments. The Introduction Section has been expanded by adding up-to-date citations (please see the previous response).

3) Line 46. Details of Mundava reference appears to be incorrect.

[Response]

We appreciate reviewer's correction. This reference has been discarded to avoid using a thesis as reference, and replaced by Roulet (2000) and Yu et al. (2010).

4) Lines 58-59. Poorly written text.
[Response]
We have now rephrased the text in reference to make it clear:
"Additionally, degraded peat increases the risk of peatland fires, which could consequently cause significant CO_2 emissions (Gaveau et al., 2014; Page et al., 2002; van der Werf et al., 2004)."

5) Lines 70-72. The three references supporting this statement about this "other study" appear to be a review followed by two papers describing studies from two different wetlands.
[Response]
Thank you very much for the suggestion. We have corrected the text as follows:
"In other studies, re-establishing the conditions…"

6) Lines 80-84. No mention of the role of DOC flux contributing to the overall net C flux. Exports of C via DOC can make up a major component. This should be acknowledged in the paper, and a justification made for why it was not assessed.
[Response]
We appreciate reviewer's comments. A mention of DOC and its role in net C flux has been made at the end of Section 4.3.1 (at line 254):
"It is important to estimate dissolved organic carbon (DOC) to determine a more complete ecosystem C budget. DOC lost from restored and pristine peatlands have been found typically to range from 3.4 to 16.1 g C m^{-2} $year^{-1}$ (Hendriks et al., 2007; Roulet et al., 2007; Waddington et al., 2008; Koehler et al., 2011), although, Chu et al. (2014) reported a net DOC import for a marsh of 23 ± 13 g C m^{-2} $year^{-1}$. D'Acunha et al. (2016) estimated DOC export for the current study area for Jan – Dec 2016 to be 22.4 g C m^{-2} $year^{-1}$ (15% of annual NEE)."

7) Section 2, Study area. It would be nice to have some more brief details of BB, such as area, mean annual climate statistics (see later comment).
[Response]
Thank you very much for the suggestion. We added the information at line 86 and line 101 as follows:
"Burns Bog in Delta, BC, on Canada's Pacific Coast, is part of a remnant peatland ecosystem that is recognized as the largest raised bog ecosystem (2,042 ha) on North America's west coast."
"… bracing (Howie et al., 2009). Based on the weather data for 1981 to 2010 from the closest Environment Canada weather station, Vancouver International Airport, the average annual temperature was 10.4 ℃ and average annual precipitation was 1189 mm. Following rewetting, …"

8) "... highest emissions under a high water table"? Maybe "... associated with high water tables".

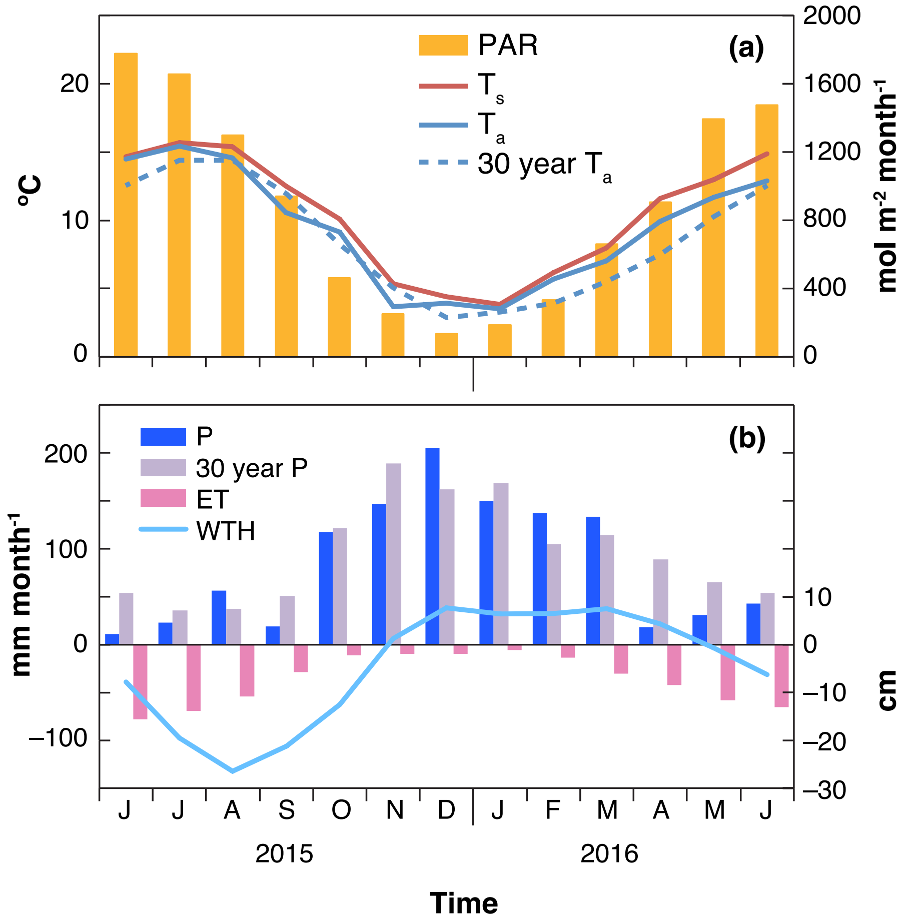
[Response]

Thank you very much for the suggestion. The suggested correction has been made:
"… highest emissions associated with high water tables."

9) "... reduced ET as a consequence of senescence." Are there data on this? Reference to another study? Implies a definitive finding, which would be a worthwhile result on its own, but no EC water vapour flux data were presented in the manuscript.

[Response]

Yes. We have continuous ET data which were gap-filled using REddyProc (Max Planck Institute for Biogeochemistry). Monthly ET values have been added to the figure showing the annual course of weather variables:



To make it further clear, we have now added more details at line 104 as follows:
"In September and October, a water table rise due to the increase in precipitation and reduced evapotranspiration (ET) as a consequence of reduced available energy and senescence of sedges was observed, which is similar to water table observations in other temperate wetlands (Lafleur et al., 2005; Rydin and Jeglum, 2006)."

10) The detail that the CSAT3 samples at 60 Hz is unnecessary.
[Response]
We appreciate reviewer's suggestion. This information was edited as follows (at line 127): "The CSAT-3 measured the longitudinal, transverse and vertical components of the wind vector and sonic temperature and output data at 10 Hz."

11) Lines 130-131. Please describe at least whether fluxes were calculated on-line by the dataloggers or during post-processing. It would be useful if the URL for the Crawford et al. report were provided in the reference list.
[Response]
We appreciate reviewer's suggestion. The fluxes were calculated in the post-processing, and this information has been added as follows:
"… were calculated in post-processing of 30-min data blocks following the procedures documented in Crawford et al. (2013)."
Also, the permanent link (http://hdl.handle.net/2429/45079) was added in the reference list.

12) Line 143. There is no Lee et al. (2016) reference provided, but there is a Lee (2016) MSc thesis. Generally, referring to a thesis should be avoided.
[Response]
We appreciate reviewer's suggestion. As suggested, reference to the Lee (2016) thesis has been removed:
"Gaps in the climate data (<1% of the year) were filled using measurements at nearby climate stations."

13) Line 152. Isn't GEP normally defined as gross ecosystem production (i.e. equivalent to GPP)?
[Response]
Yes, GEP usually stands for gross ecosystem production or productivity, which is equivalent to gross primary production (GPP). GEP can also stand for gross ecosystem photosynthesis which is equivalent to gross ecosystem productivity. In order to be consistent, we modified the definition in Section 3.3.1 (line 153) as follows:
"…and gross ecosystem productivity (GEP), i.e. NEE $= R_e -$ GEP."
Also, the name of Section 4.3.4 was corrected to:
" 4.3.4 Gross ecosystem productivity"

14) Line 165. Range of annual Re: Table 1 lists an even larger value.
[Response]
Thank you very much for pointing out the discrepancy. The sensitivity test of window sizes on gap-filling was re-run on a more comprehensive scale based on comments from Referee #1, as a result of which this sentence has been modified as follows:
"However, the sensitivity of choosing different window sizes on gap-filled R_e was small, varying the annual value between 226 and 245 g C m^{-2} $year^{-1}$."

15) Section 3.3.2. Gap filling FCH4. Methane fluxes in wetlands are often the result of a complex interplay of drivers, involving multiple transport pathways and balance between production and oxidation. Moreover, the controls on FCH4 can easily change seasonally and from year to year (Goodrich et al., 2015). I doubt that such a simplistic gap filling procedure as described here is sufficient. This is the reason that multipleparameter (e.g. Brown et al., 2014) and neural network (e.g. Goodrich et al., 2015) methods are more standard. Therefore, some more convincing details of FCH4 gap filling are required.

[Response]

We appreciate reviewer's suggestion. We have tested the effects of all other possible controls including WTH, θ_w, oxidation reduction potential, and T_a on CH_4 fluxes. There was no relationship between these variables and CH_4 fluxes. We were forced to use the relationship between T_s and CH_4 fluxes. The strongest relationship was an exponential one with an R^2 value of 0.66 (logarithmic, linear and polynomial relationships resulted in R^2 values of 0.46, 0.52 and 0.54, respectively).

16) Line 190, Eq. 4. Please define the m values for completeness.

[Response]

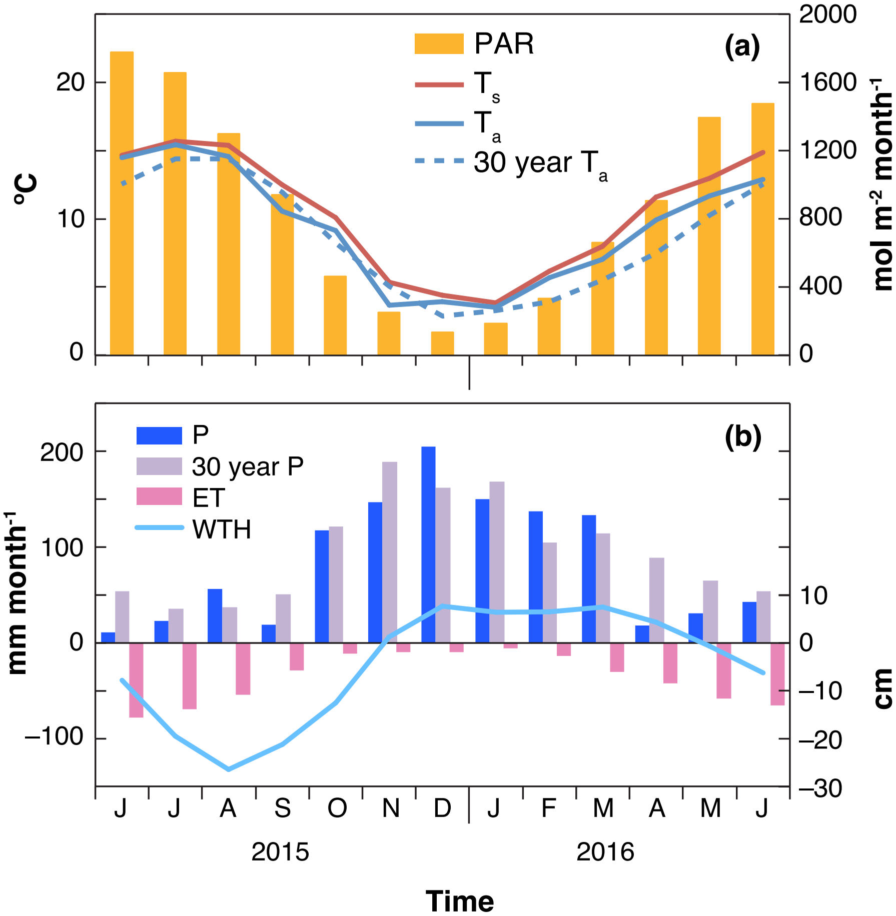
We appreciate reviewer's suggestion. The m values have been included:

"… , m_{CO_2} is the molecular mass of CO_2 (44.01 g mol^{-1}), and m_{CH_4} is the molecular mass of CH_4 (16.04 g mol^{-1})."

17) Section 4.1. Some comparison of seasonal and annual temperature and precipitation to long-term normals would be useful to justify how close to average (or not) the conditions during the study period were. Also (line 200), I don't believe one can justify listing annual precipitation totals to the precision of one decimal place, given the problems inherent in rain gauges!

[Response]

We appreciate reviewer's suggestion. First, we reduced the significant digits of annual precipitation totals to 0. Second, monthly precipitation and temperature measured during the study year at the tower and over 30 years at Vancouver International Airport were plotted in the figure showing the annual course of weather variables:



18) Line 210. Why list the author names (Kormann and Meixner) twice?

[Response]

We removed one of them and rewrote as follows:

"… using an analytical turbulent source area (turbulent footprint) model ~~from Kormann and Meixner~~ (Kormann and Meixner, 2001)

19) Line 217. What grasses? Were these wetland species?
[Response]
Yes, the common name of the dominant plant species (*Rhynchospora alba*) mentioned in Section 2 is white beak-sedge. The explanation has been added to Section 4.2.2 for clarity: "Mosses and white beak sedge (the common name of *Rhynchospora alba*) started to grow …"

20) General comment: a figure showing the annual course of weather variables and water table would be very useful.
[Response]
We appreciate reviewer's suggestion. A new figure was made (see our response to Comment 9 above).

21) Lines 238-239. The "highest increasing rate of NEE" appears to be from March to April, not May.
[Response]
This sentence has been re-written as follows for clarity:
"The highest rate of increase in the magnitude of NEE and the highest magnitude of NEE both occurred early in growing season (Fig. 2)."

22) Line 242 onwards. It seems of very limited usefulness to compare the wetland fluxes to those from forests and grasslands, and it highlights the completely insufficient comparison with other wetland studies, both for restored peatlands and pristine or disturbed peatlands (see main comment above).
[Response]
We appreciate reviewer's suggestion. This comparison gives us information on how different the C exchange of a wetland is compared to other ecosystems in the same region, sharing the same climatic conditions. However, we have now added a detailed discussion comparing this study to other pristine and restored wetlands as follows. See our response to Comment 1 above.

23) Section 4.3.2. As it stands, Fig. 3 adds nothing to the paper other than a pretty picture. It would be of some use if there was a proper comparison made between these diurnal/seasonal patterns with the literature from other wetlands. FCO2 is only ever used in Fig. 3 and is not properly defined.
[Response]
We appreciate reviewer's comment. The label of scale (FCO_2) has been corrected to Fc for clarity. Figure 3 is the only place where detailed diurnal and seasonal trends in F_C are shown, which are valuable data and evidence to support our conclusions. To improve readability, we have now added the following information about Fig 3 (at line 256):

"The seasonally-changing diurnal course of gap-filled NEE with isopleths over time of day and year is shown in Fig. 3. The daily maximum in GEP changed with season resulting in the high magnitude of NEE during midday between May and July (~ -3.5 μmol m^{-2} s^{-1}) with the highest magnitude of NEE occurring in May. Nighttime NEE, i.e., R_e, showed relatively small variation with season, and on average was ≤1 μmol m^{-2} s^{-1} for most of the study period. The rapid decrease in monthly R_e from May to June was caused by low R_e in early morning or at nightfall in June."

24) Section 4.3.3. Again, the magnitude of Re has not been adequately compared to other wetland flux literature, either on an instantaneous basis or seasonal/annual.
[Response]
We appreciate reviewer's suggestion. A detailed discussion comparing R_e from this study to other pristine and restored wetlands has been added at line 254 (see our response to Comment 1).

25) Line 277. I could not find where the measurement of theta_w (moisture content?) was described. Section 4.3.4. Again, this section on GEP is deficient in comparing their values for GEP and various timescales (and light response) with the relevant literature.
[Response]
We appreciate reviewer's suggestion. The information on the measurement of soil volumetric water content has been added to Section 3.1:

"A soil volumetric water content (θ_w) sensor (CS616, CSI) was inserted vertically to measure integrated θ_w from the surface to a depth of 0.30 m."

26) Lines 289-290. "We found out there was the light-independent photosynthesis ...". This sentence is rather perplexing. How was this deduced? Also, the PAR range 300-500 is exactly in the range where GEP seems maximally dependent on light (Figs 5, S4)!
[Response]
We appreciate reviewer's suggestion. The second paragraph in Section 4.3.4 was re-written for clarity as follows:

"Other possible controls on GEP explored were WTH and T_a. We found that WTH was not a control on GEP in the current study as the study area remained fairly wet throughout the year. Furthermore, the effects of T_a on GEP were approximately limited between 10 and 15 ℃."

27) Section 4.4.2. Same comment as above about inadequate reference to relevant literature about CH4 fluxes. Lines 296-297. What do "weak" and "significant" mean in the context of CH4 fluxes when the literature is not referred to?
[Response]
We appreciate reviewer's suggestion. We re-wrote the sentence at line 298 for clarity:
"Seasonally, it was a weaker CH_4 source in fall than in summer … ."

28) Line 305. Why was it surprising that there was not much of a diurnal course observed for FCH4? The authors seem to be completely unaware of why or why not this flux may or may not follow a diurnal course. Figure 6, with the whole annual period included, would almost certainly mask seasonal differences in diurnal patterns. Also, the units for FCH4 in Fig. 6 is surely incorrect. This should presumably be nmol m-2s-1.

[Response]

We appreciate reviewer's suggestion. In Figure 6, we actually plotted only data that were measured, i.e., gap-filled data were excluded (hence the different number of cases in each hour). There were significantly more data available from the summer half-year (higher CH_4 fluxes) than from the winter half-year (lower CH_4 fluxes). Therefore, we edited the text in Section 4.2.1 starting at line 307 to line 309 as follows:

"Surprisingly, there was only small diurnal variation observed for CH_4 fluxes in the summer months, as has been found in other studies (Juutinen et al., 2004; Wang and Han, 2005; Long et al., 2010; Su et al. 2013). In the current study area, with changes in WTH and vegetation growth occurring during the year, there were likely several processes affecting CH_4 transport, which masked the diurnal pattern of CH_4 fluxes. Furthermore, $T_{s,5cm}$ appeared to be the main environmental control on CH_4 fluxes in this study but did not have as strong effect on CH_4 emissions as found in previous studies. Thus CH_4 was continuously emitted at a similar rate during daytime and nighttime. ~~Thermal effects such as recently reported by Poindexter et al., 2016 were not found.~~ From January to March and October to December, the winter half-year, the study site had constant CH_4 emissions of less than 50 nmol m^{-2} s^{-1}, and almost no diurnal variation was observed. July had the greatest CH_4 emissions, and the highest magnitude (>150 nmol m^{-2} s^{-1}) appeared in the evening (3 pm to 9 pm). This corresponded to the lagged effect of soil temperature and may be partly due to convective turbulent mixing caused by cooling during the evening (Gordwin et al., 2013)."

Thank you for pointing out the error in the units in Fig. 6. We have corrected the units to nmol.

29) Lines 305-306. "Thermal effects such as recently reported by ...". This is a bit too cryptic. Were the modelling methods of the Poindexter et al. (2016) followed, or is this just an attempt to justify the apparent lack of a diurnal pattern? Besides, at BB the water table was sometimes above the surface and sometimes below, and the annual vegetation growth changed (as described), so it is logical to assume that a variety of methane transport processes would have operated.

[Response]

We appreciate reviewer's suggestion. This reference was discarded for clarity, and the discussion of the diurnal course of CH_4 fluxes was added, please refer to our response to the previous comment.

30) Line 322. By CH4 emissions and CO2 uptake, I presume the CO2-eq values of these are being referred to.
[Response]
Thank you very much for the suggestion. We changed the text as follows:
"In short, the critical time period for both, CO_2 and CH_4 fluxes in terms of CO_2e, was the growing season when magnitude of fluxes changed differently across the growing season. "

31) Lines 328-330. This is by no means an adequate way to address the lack of comparison of the CO2 fluxes from this study with the peatland (or other wetland) literature.
[Response]
Thank you very much for the suggestion. A detailed discussion comparing this study to other pristine and restored wetlands has been added at line 254 (see our response to Comment 1).

32) Line 371. For peak's sake? Peat?
[Response]
This error has been corrected as follows:
"Chestnutt, C.: For peat's sake: A water …"

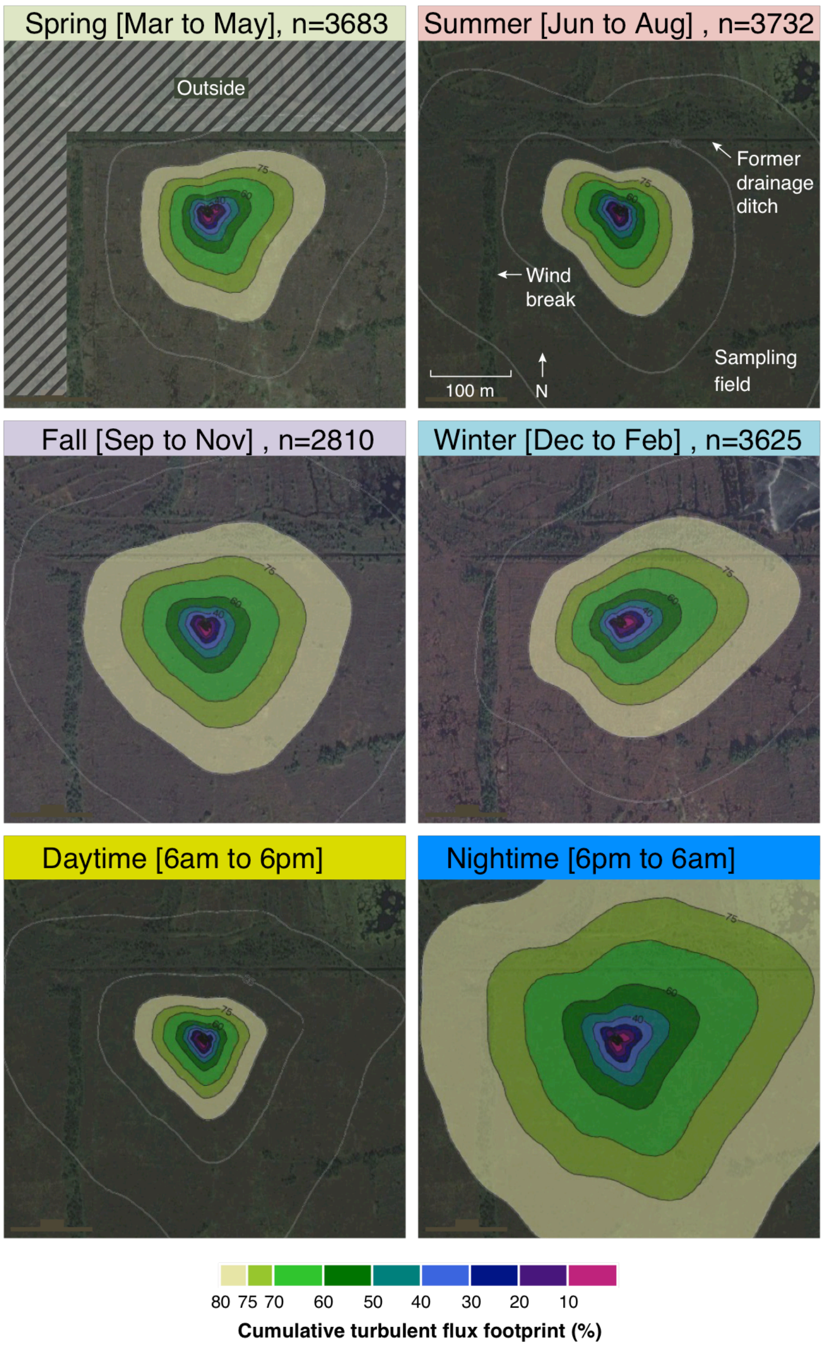
33) Figure 6. Units for FCH4 are surely incorrect. If these are actually nmol m-2s-1, a mean flux of around 100 nmol m-2s-1 should yield an annual flux of around 38 g CH4-C m-2yr-1, not the 16 g CH4-C m-2yr-1 as provided in Table 1. The authors should carefully check their flux conversion calculations, for both CH4 and CO2 fluxes, to provide some confidence it has been done correctly.
[Response]
Thank you very much for the suggestion. In Figure 6, we plotted only data that were measured (as we indicated in our response to Comment 28). There were significantly more data available from the summer half-year (higher CH_4 fluxes) than from the winter half-year (lower CH_4 fluxes), consequently the data in the figure cannot be simply averaged. We have changed the caption accordingly and corrected the unit (we incorrectly used "µmol" instead of "nmol"). The new caption reads:
"Figure 6: ~~(a)~~ Diurnal course of ~~filled~~ measured CH_4 fluxes from the EC-2 system during the study period."

Also, we have corrected the related text in Section 4.4.2 as follows:
"The ensemble diurnal courses of the ~~gap-filled~~ CH_4 fluxes ~~(measured CH_4 emissions and gap-filled by modelled CH_4 fluxes)~~ measured by the EC-2 system are shown in Fig. 6 from 16th June 2015 to 15th June 2016."

34) Figure S1. North orientation should be indicated. Also, note that not all panels show max. contour of 90%.

[Response]

Thank you very much for the suggestion. The fact that not all panels show the 90% contour line is intentional. All source areas were calculated as gridded data for a 1 x 1 km box (open source code see https://github.com/achristen/Gridded-Turbulent-Source-Area). If a contour line for a certain probability reaches the border of the model domain, the exact shape of the probabilities outside the domain are unknown, and hence the contour cannot be drawn, even within the domain. The new figure was drawn for including north orientation and vegetation conditions in different seasons:



35) Figure S3. "Re curves" is not an adequate description. What does it mean "on first day of every two months"? This is not correct.

[Response]

Thank you very much for the suggestion. The new caption for Fig. S3 reads:

"Boxplots of measured R_e (nighttime NEE) plotted against $T_{s,5cm}$ with a fitted curve on the first day of each time period using a window size of 120 days."

36) Figure S4. Same comment about inadequate caption.

[Response]

Thank you very much for the suggestion. The new caption for Fig. S4 reads:

"Light response curves on the first day of each time period using a window size of 90 days."