

Interactive comment on “Annual greenhouse gas budget for a bog ecosystem undergoing restoration by rewetting” by Sung Ching Lee et al.

Anonymous Referee #2

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It is interesting to read of research into carbon exchange and the greenhouse gas budget of a part of Burns Bog in coastal BC. Wetland restoration proposals often cite the benefits of reversing the loss of carbon from degrading peat and re-creating a C sink. However, at least in the short-term, emissions of methane from rewetted wetlands can create a net GHG source.

I think that this manuscript is worthy of publication once some of my listed concerns are addressed. 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. While annual NEE indicates a relatively strong CO₂ sink, the magnitudes of GEP and Re are rather small. This is certainly

C1

evident from the comparisons on Table 2, and the authors point this out. However, they make no effort to thoroughly test these results against the wetland flux literature. Most reported peatland flux studies are for higher latitude/shorter growing season sites than the present study, so it can be difficult to find relevant comparisons. For instance, the compilation of annual CO₂ flux data for “inland wetlands” in Lu et al. (2016) have no GEP and Re data for sites with similar mean annual temperature to Burns Bog. In contrast, there are several sites listed that have GEP and/or Re in the range of magnitudes reported for Burns Bog. Equivalently low GEP were reported for sites with much lower MAT, and therefore short growing seasons, whereas Burns Bog had year-round positive GEP. Similarly low Re have only been reported for sites with very low MAT and extremely low productivity. At Glencar blanket bog in Ireland, with MAT similar to BB but with a short growing season, Sottocornola and Kiely (2010) reported annual GEP and Re of similar magnitude to BB. In summary, to gain some confidence that flux partitioning and calculations have been correctly performed, it is essential that these low GEP and Re at Burns Bog are carefully considered and explained. Similarly, the magnitudes and seasonal patterns of light response parameters and respiration should be compared to the established literature. An appropriate Canadian reference would be Humphreys et al. (2006). The same issue arises for the methane flux. 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. 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.

Detailed comments

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!

Line 40-41. Again, there seems little rationale for choosing these particular references

C2

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.

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

Lines 58-59. Poorly written text.

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.

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.

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).

Line 93. “... highest emissions under a high water table”? Maybe “... associated with high water tables”.

Line 105. “... 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.

Line 128. The detail that the CSAT3 samples at 60 Hz is unnecessary.

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.

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.

C3

Line 152. Isn't GEP normally defined as gross ecosystem production (i.e. equivalent to GPP)?

Line 165. Range of annual Re: Table 1 lists an even larger value.

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 multiple-parameter (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.

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

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!

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

Line 217. What grasses? Were these wetland species?

General comment: a figure showing the annual course of weather variables and water table would be very useful.

Lines 238-239. The “highest increasing rate of NEE” appears to be from March to April, not May.

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 compar-

C4

ison with other wetland studies, both for restored peatlands and pristine or disturbed peatlands (see main comment above).

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.

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.

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.

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)!

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?

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.

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

C5

vegetation growth changed (as described), so it is logical to assume that a variety of methane transport processes would have operated.

Line 322. By CH₄ emissions and CO₂ uptake, I presume the CO₂-eq values of these are being referred to.

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.

Line 371. For peak's sake? Peat?

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.

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

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

Figure S4. Same comment about inadequate caption.

References (suggested by this reviewer).

Brown et al. (2014), Evidence for a nonmonotonic relationship between ecosystem-scale peatland methane emissions and water table depth, *J. Geophys. Res. Biogeosci.*, 119, 826–835, doi:10.1002/2013JG002576.

Goodrich et al. (2015), Overriding control of methane flux temporal variability by water table dynamics in a Southern Hemisphere, raised bog, *J. Geophys. Res. Biogeosci.*, 120, doi:10.1002/2014JG002844.

C6

Humphreys et al. (2006) J. Geophysical Research, 111, G04011, doi:10.1029/2005JG000111.

Lu et al. (2016), Global Change Biology, doi:10.1111/gcb.13424.

Sottocornola and Kiely (2010), Agricultural and Forest Meteorology 150 (2010) 287–297.

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