Review of *Annual greenhouse gas budget for a bog ecosystem undergoing restoration by rewetting* by Lee et al. (first revision)

In general the authors have made appropriate changes to address many of my comments on their first submission. I have two substantial comments followed by a number of more minor ones.

Substantial

There has been no attempt to discuss or quantify the likely size of uncertainties of the fluxes, especially as they relate to annual fluxes and CO_2-e values. It is not really acceptable to publish these data now without considering the likely size of uncertainties. Given my comments below about gap filling CH4 fluxes, I strongly suggest that a due consideration of the magnitude of uncertainties at the annual timescale is warranted.

My comments about gap filling CH4 fluxes have not really been adequately addressed. I suggest that the nature of the relationship settled on in 3.3.2 be supported by a figure, even if it is in supplementary material. I do find it surprising that an r2 as high as 66% was found, unless it is simply the result of the extremely wide range of flux values. If that were the case, I'd be more concerned about r2 within the region of the relationship where most of the annual flux was derived (i.e. during warm temperatures). This is also why an estimate of uncertainty is required. Moreover, the authors hinted that little CH4 flux data were available in winter, increasing uncertainty. They also hint (lines 328-329) that there may well have been hysteresis in the temperature vs Fm relationship. It may well be more defensible to gap fill using the means of daily 30-minute measurements to construct daily means. But without seeing the data, it's difficult to judge. Suggest looking at Goodrich et al. (2015) JGR-BG (DOI: 10.1002/2014JG002844) for a situation with strong hysteresis in methane fluxes from a wetland, where a "simple" regression-based approach was demonstrated to be inadequate.

Detailed comments

Throughout: "eddy-covariance" should be "eddy covariance". The hyphen is unnecessary.

Line 1 of abstract: peatlands have been drained for many other purposes that have also changed them into C sources.

Introduction and lines 38-40. I suggest that the authors might consider constraining their introduction (or at least detailed examples of fluxes) to peatlands. Wetlands are so incredibly diverse that it is almost meaningless to provide context for a study focused on a restored peatland. The Grasset et al. (2016) paper seems a rather strange choice because this appears to have been an aquatic study and not even focussed on annual fluxes. I couldn't find the CO2-C values cited in this or the Mitsch et al. (2013) papers. It is really important at this stage of a manuscript that the authors concentrate on verifying their source material and providing relevant and authoritative literature sources.

Line 73 onward. Using the term "long-term" suggests multi-year. Clearly this paper is not multi-year so, at least in the conclusion, the authors might need to acknowledge the additional uncertainties affecting their conclusions caused by there being a single year of study.

Lines 130-131. The change to the 60 Hz detail has not been made.

Line 146. "Small gaps … and H2O fluxes"?

Line 154 "atmosphere" is mis-spelled.

Use of terms Fc and NEE. In general there is inconsistent use of these terms which, to an unfamiliar reader, might be very confusing. For example, why on line 155 use "NEE and Fm" rather than "Fc and Fm"?

Lines 167-168. It is not really accurate to state that the parameters "were determined separately for each day of the year", when the dataset for each day consisted of a window of 120 days width! Consider a more accurate description.

Lines 173-182. This does not actually describe completely how GEP was modelled. The first step, surely, was to partition GEP out from measured values of NEE (using modelled Re), and <u>then</u> the light response function was fitted.

Line 207. Include reference to (the new) Fig. 2.

Line 246. "Despite…". It is not quite clear what this sentence means. Maybe replace with "Compared to …".

Line 247. This should be a reference to Fig. 3, not Fig. 4.

Line 264. Replace "pristine" with "more or less pristine". For example, the Campbell et al. (2014) study was for a drainage impacted peatland. It is hard to judge what "pristine" actually means, and so most wetland scientists will avoid the term.

Lines 277-281. There needs to be some details provided about the methodology used to calculate DOC fluxes, since the reference is unpublished. At least it is nice to see some uncertainty value provided!

Line 284. Where is the "daily maximum in GEP" shown?

Lines 286-287. "The rapid decrease in monthly Re (Figure 3?) from May to June …". Fig. 3 does not support this statement.

Lines 319-321. How were these controls investigated? How did you determine "the effects of Ta on GEP were <u>approximately limited</u> between 10 and 15 C."? (The underlined words make no sense!) It is notoriously difficult to isolate the effects of temperature versus light quality versus VPD, because they are all strongly correlated.

Line 326. Still using the inaccurate term "significant". Do you mean "Much larger"?

Lines 328-329. Hysteresis in the Ta vs Fm relationship is hinted at. This suggests that the simplistic gap filling method might be quite inaccurate at these times.

Lines 346-347. Why is this surprising when it has been reported by many other studies?

Line 365. "… magnitude of fluxes changed differently …" is fairly meaningless. What do you mean?

Lines 366-367. Why should they be expected to be representative?

Lines 368-371. This paragraph is a very weak response to Reviewer 1's comment. It is barely coherent. It does not address the long-term nature of C accumulation (from CO2) in wetlands vs the short lifetime of CH4 in the atmosphere.

Line 383. It is surely unusual that there is such a huge seasonal variation in Fm, especially given the temperate climate and high water table in winter. Can the authors suggest why this might be the case? The proper place for this is in Section 4.4.1.

Line 387. If the flux of DOC is taken into account, the annual C sink strength is less than this.

Figure 2. Caption does not mention ET. Was it measured at the site and gapfilled or is it estimated from climate station values?

Figure 3 caption. The x-axis label (F_CO2) has not been corrected.

Figure 7 caption is inadequate. Insert “Ensemble”. Note that mainly summer is represented.

Figure S3. It would be preferable to use a common x-axis scale on all of these panels (as was done for Fig S4).

Figures S3 and S4. It is still a little confusing what “on the first day of each time period” means. Presumably this is the function derived for the first day of the period – which is actually derived from data sourced 60 days prior to 60 days after. Surely it would make more sense to choose a day close to the center of the time period.