Interactive comment on "Methane and carbon dioxide fluxes over a lake: comparison between eddy covariance, floating chambers and boundary layer method" by Kukka-Maaria Erkkilä et al.

Anonymous Referee #1

Received and published: 20 March 2017

Scientific significance: Good: It is not clear whether the whole data set is new, but the comparison between EC measurements and Boundary Layer Methods (BLM) offers new insights on these data
Scientific quality: Good: Applied method are valid, and the research group has a well expertise on this topic. References are appropriate, very few might be added to support assumptions (see comments)
Presentation quality: Good: English is good, figures and tables are all necessary. Some of the figures could be improved to support discussion (see comments)

We thank the anonymous referee #1 for your very good and on point comments. We have carefully read through all the comments and prepared responses to all specific comments below.

+General comments:
This manuscript by Erkkilä et al deals with the important question of assessing CO2 and CH4 fluxes from water bodies, boreal lake in this case. This paper is fallen well in the scope of the Biogeosciences journal. MS present a set of Eddy Covariance flux measurements and BLM flux calculations, all data of interest for the research community and GHG inventory compilers. This MS is generally well written, is timely and interesting to understand the parameters of influence on carbon emissions from lakes. The authors have done a good job in data collecting and study design though the study period is quite short (15d), but still, interesting by the contrast it is showing between stratified and mixed conditions. Some aspects of the discussion elements should be reworded to make the main conclusions more evident. Some of the conclusions, for example on the difference between day and night time, do not seem so evident based on the figure analysis. Those figures should also be improved to ease comparisons between fluxes and controlling parameters on one side, and between approaches on the other side.

Thank you! The study period is short due to intensive manual data collection that was timed to the start of the lake mixing period. All the comments will be taken into account and the figures improved.

+Specific comments:
-Page 2, line 7: it is Heiskanen et al (2015) rather than 2014
Yes of course, this will be fixed.

-Page 2, line 11: ". . . a small part of a lake": rather vague. . .
Will be changed to "…is representative of the measurement point only"

-Page 2: lines 27-29: "Because current up-scaling estimates are based on these methods, comparison is needed to reduce the uncertainties in current estimates of the role of lakes in global carbon cycle". More generally, the role of freshwaters need to better assessed.
"lakes" will be changed to "freshwaters"

-Page 5, line 12: with 27% and 32 data coverage for CO2 and CH4 fluxes are quite low, though not critically low. What is the coverage for heat fluxes? Is there any estimate of the impact of gap filling with bulk model on those fluxes?

27% and 32% coverage for lake eddy covariance measurements is not that low, keeping in mind that we also have to discard data according to wind direction on top of other quality filtering. Coverage of heat fluxes was 83% and 80% for latent and sensible heat fluxes, respectively. The effect of gap-filling is probably not large, because the few gaps in heat fluxes most probably coincide with gaps in gas fluxes and will not affect the comparisons. The bulk-transfer relationships are widely used, and well-established in-situ coefficients are reported in Mammarella et al. (2015).

-Page 8, lines 2-5: do you assign difference in CO2 concentration between the automatic and manual systems to the same reason as for CH4 (too short time of equilibration in the automatic system?). With 40min, do you really think time of equilibration was too short? What percentage of dissolved CH4 do you think you were retrieving? This issue needs to be discussed.
We don't know if the equilibration time was too short or not but this a possibility. This is why we included the manual headspace samples to get a feeling for the uncertainty of the absolute values. Headspace samples were taken at different locations than the automatic system, so some variation between the two samples is expected. Without additional headspace samples close to the automatic system it is impossible to say anything absolute about the accuracy, but we still trust the gradient.

-Page 8, from line 23, section 3.2.1 There is a main issue here in defining detection limit, uncertainties, errors. If detection limit is approximately 2 nmol m-2 s-1, then you cannot write that "CH4 fluxes. . . were small (less than 1 nmol m-2 s-1)". Identically, you cannot say that "the difference between manual and automatic BLM fluxes remained below 0.4 nmol m-2 s-1". All this is not consistent. You should give indications on how you determine the flux detection limit.
Detection limit was not calculated for this site but a reference was taken from Peltola et al. (2014) to have a rough estimate of detectable flux by this analyzer. A site specific detection limit may be calculated as the standard deviation of the cross-covariance function values far from the maximum values (flux), as was developed in Wienhold et al. (1995), and added to the text. As the EC fluxes are presented as daily median values, the detection limit should be scaled by $\sqrt{n}$ where n is the number of measurements per day (n=48), giving a daily flux detection limit of approximately 0.29 nmol m-2 s-1. We will take a closer look on the detection limit and make the text more clear on this.

-Page 8, lines 18-20: you expect an enhancement of CO2 concentration at the surface due to up-welled methane. You mean CO2 from oxidised CH4? There is at least a factor 10 between CO2 concentration at the surface and CH4 concentrations at 11m depth, so the proportion of CO2 to be expected from methane oxidation between 11m and the surface would remain low in all cases...
I mean the upwelling water that would also bring CO2 to the surface from deeper waters. However, the CO2 concentration difference between surface and 11m is not as drastic as for CH4 (surface water CH4 concentration is only 3% of the 11m concentration whereas surface water CO2 concentration is 20% of the 11 m concentration) and thus upwelling water does not make such a big rise in the surface water CO2 concentration. This will be made more clear in the text.

-Page 8, line 34: why more frequent sampling should necessarily lead to higher fluxes than the ones reported by Miettinen et al? Give explanations.
More frequent sampling would include also nighttime cooling periods and perhaps windier daytime episodes in the measurements. When all kinds of weather conditions and cooling periods are included, the fluxes might end up being higher.

-Page 8, line 34: Give value of high fluxes reported by Ojala et al.

The high fluxes after rain events were about 6 nmol m-2 s-1, which is much higher than we measure during the stratified period. The value will be given in the text as well.

-Page 9, line 1: please add reference to support hypothesis on lateral CH4 transport from catchment linked with precipitation event.
Ojala et al. (2011), Rantakari & Kortelainen (2005), will be added to the text.

-Page 9, line 3: ". . . that kTE and kHE were similar. . .": add "and comparable to EC measurements" to that sentence.
Will be added.

-Page 9, line 8: detail explanation in Schubert et al for lower kCC results, if relevant for this study.
Eddy covariance and floating chambers gave 8 and 7 times higher cumulative fluxes than kCC method in Schubert et al. (2012). This will be added to the text.

-Page 9, lines 11-13: make consistent, CO2 flux or fluxes, singular/plural
Will be made consistent.

-Page 9, line 16: EC increase seems be lower than a factor 3. See Table 2.
This value is not seen from the table. Table 1 & 2 give linear fit values to the comparison between EC and BLM fluxes for CH4 and CO2 fluxes, respectively, during the whole measurement period. CO2 flux increases from about 0.5 µmol m^{-2} s^{-1} to 1.7 µmol m^{-2} s^{-1}, thus a factor 3 increase.

-Page 9, line 20: 3 µmol m-2 s-1: calculated from which BLM model?
Good point, 3 µmol m^{-2} s^{-1} from kTE and kHE models, whereas kCC increased to 2 µmol m^{-2} s^{-1}. This will be corrected in the text.

-Page 9, line 25: "The same result. . .": that is, kCC lower than both kTE and kHE?
"The same result…" refers to the previous sentence that compares BLM and EC methods, not the different k models.

-Page 9, lines 28-29: again, what is the impact of using bulk formulas on the calculations of heat fluxes and subsequent kHE and kTE?
Probably not large and the heat flux coverages were quite high in any case and the bulk formulas are well established.

-Page 10, section 3.3. This section is rather confused. If EC is taken as the reference (line 21), then discussion on CO2 diurnal variation should try to explain why BLM show a diurnal variation which is not expected at the end, as seen from EC measurements.
EC is not taken as a reference, but it is compared to other methods. We do not assume any method being "more correct" than others and different methods may have different diurnal variations.

-Page 10, line 6: ". . .kCC results in a remarkably lower flux than kHE in general": underestimation seems particularly due to underestimation of fluxes when they are at their maximum. Any reason why?
Early morning times are usually a time of lake cooling, which then causes some additional convective mixing in the water column. This is taken into account in kHE model, but not in kCC model.

-Page 10, lines 9-10: is horizontal turbulence assumption consistent with kHE variability given in previous sentence?
Unfortunately it is not completely clear what is meant by 'horizontal turbulence assumption' within this context. Both kCC and kHE are 1-D models considering only the vertical transport. Neither one of them is capable of addressing horizontal processes, and therefore we think that horizontal turbulence is not the cause of differences between the dynamics of these models.

-Page 10, line 15: maximum is rather reached at noon than during the afternoon
Correct, will be corrected in the text.

-Page 10, line 16: " The BLM flux by kTE is thus also larger in the daytime despite the lower _[CO2]." _[CO2] is the same for all the BLM models, why adding this element in the discussion, it is somehow confusing. . .
The higher kTE is more dominant than the lower concentration, resulting in a higher BLM flux.

-Pages 10-11, section 3.3.2 Whole section is not convincing. Daytime vs. night time fluxes would need to be calculated to support the discussion. First define precisely hours of the day used to separate the two periods. BLM daytime fluxes do not seem to be significantly higher than night time fluxes. Diurnal variation from EC fluxes not well correlated. No bubbling? Figures not very helpful for comparison and to support discussion.
Precise hours will be given in the text and shown in the figures. I would not expect much bubbling from the study area, since the lake is quite deep in the EC footprint area and from the chamber measurements we only saw one or two ebullition events close to the shore (these were removed from the analysis).

-Page 10, lines 26-27: Highest flux value is reached during the late afternoon/evening and during the second part of the night. Not so clear for EC fluxes.
True, also EC fluxes show the late afternoon peak but not the early morning peak. We will rephrase the sentence.

-Page 10, lines 27-28: add wind speed on plot for better comparison.
Wind speed is given in figures 2c, 7b and 9b.

-Page 10, line 29: precise which Fig 9 panel.
Fig 9d, will be specified in the text.

-Page 10, line 31-32: reword sentence: it appears that there is an increase of CH4 in the afternoon just because of less oxidation. It is both possibly due to that phenomenon and to continuous feeding of CH4 from underneath.
This has already been stated in line 29, but can be clarified in lines 31-32 as well.

-Page 10, line 32: ". . .enhanced resuspension from sediments". do you mean lateral advection? Suspension of gases? Not clear to me. Any reference to support the assumption
Resuspension in the littoral zone, bringing methane to lake water and then transported further in the lake by lateral advection. This is of course speculation since we do not measure it. Reference given in the text is study by Bussmann, 2005.

-Page 10, line 33: "detached gases. . .": detached does not seem an appropriate word
Will be replaced by "… enhanced transport"

-Page 11, line 6: any reference to support enhanced night time production of CH4 in sediments?

This is speculation by Podgrajsek et al. (2014), we do not observe higher nighttime CH4 fluxes. This part of the sentence can also be removed, since there is no other reference and it is not related to our study.

-Page 11, lines 9 and 11: again highest fluxes around noon seems more correct.
Yes, this will be corrected.

-Page 11, lines 13-14: Not clear, are you discussing comparison between day vs. night fluxes, or mixed/stratified periods?
Comparison between day vs. night during the mixing period. We will try to make it clearer.

-Page 11, line 16: " This may be caused by increased convective transport of CO2 from deep waters to the surface": any other reason possible?
Algal photynthesis may also play a role here although we do not have any direct result on that.

-Page 11, section 4 There is no clear added value of this whole section to the paper. If you have floating chamber measurements and mixing ratio of CO2 and CH4 in the water, you should try to calculate a site specific k value and compare it to the one calculated from Cole and Caraco.
We disagree with this and would like to keep the section. Even though we did not find any "hot spots" with the FC measurements, we think it is important to show that. With this analysis we found that EC measurements give higher CO2 fluxes from the south, shallower side of the lake and the CO2 EC fluxes did not differ from FC fluxes from the north side of the lake. Site specific k calculation was left out of the analysis for keeping the paper more compact, but we think the reviewer have a good point with suggestion this approach as well. However, there is another manuscript in preparation that is focused on gas transport and includes site specific k calculation from this campaign and we think that adding this calculation to this manuscript would make it more broad and complicated, as we try to focus on flux measurement methods instead of gas transport velocity.

You use 'median' for 'standard diurnal variation' throughout the section.
This will be corrected.

-Page 11, line 28: quote reference(s) that show that anchored chambers can enhance artificially the turbulence and subsequent fluxes.
Lorke et al. (2015) found that anchored chambers create artificial turbulence in *running* waters. This has not been found to be an issue for lakes, as studied in Gålfalk et al. (2013).

-Page 12, line 2: see comment on figure 11.
Yes.

-Page 12, line 19: see comment on page 11 section 4 on calculation of site specific k value
Yes.

-Figures 2 and 3: Variation of parameters are hard to see with original figure dimensions
Will be improved.

-Figure 3b: CH4 concentration at 11m: mention that it is the blue line.
Yes, good point.

-Figure 4: you should add in the legend, as in Figure 5: ". . . the outliers are represented with the red '+' symbol." A definition need to be given for "outliers" (>3sigma?). Some outliers seems not so different than extremes values kept in the distribution (in Figure 5 particularly), and sometimes fluxes appearing as outliers where not removed (see
CH4 fluxes on September 22 and 23, panel a, or CO2 fluxes on September 14 and 15, panel b).
Box plots come from Matlab's box plot function. It determines the extreme values (w, that are not outliers) as approximately +/- 2.7 σ and 99.3 % coverage and outliers as larger than q3+w(q3-q1) or smaller than q1-w(q3-q1) where q1 and q3 are the 25th and 75th percentiles, respectively.

-Discussion on Figure 4 and 5 should be supported by statistics on difference/similarity between the different fluxes assessments.
Statistics are already reported in Tables 1 and 2.

-A different Y axis could be given for the stratified period. A dead band corresponding to the detection limit for fluxes could also be added.
While a separate y axis might enhance readability for the stratified period, we think it would make it harder to read the whole figure. A dead band for the detection limit will be added.

-There were no measurements for CH4 from the automatic system on September 22, 23 and 24?
There were no manual measurements 22-24 Sep (Figs. 4b and 5b), but the automatic system was running.

-Figure 5: There are no negative fluxes from BLM model, when such CO2 sink is sometime measured with the EC system. Develop to explain this major difference.
Median CO2 sink was measured with the eddy covariance system only during two days but statistically these measurements were not different from 0 (tested with Mann-Whitney U-test). We can conclude that the fluxes are very small before the lake mixing. It is known that boreal lakes may occasionally act as sinks of CO2 (e.g. Huotari et al. 2011), but at this time of year it is very unlikely.

-Figure 6: add wind speed.
Wind speed is already given in Fig 7b.

-Figure 6 all through fig 10: add shaded area for day/night time periods or add radiation data to better discriminate the two periods you are commenting.
This is a good idea, a shaded area will be added.

-Figure 8: use same scale for fluxes calculated with kCC (up to 20 nmol m-2 s-1) It does not appear as evident that daytime fluxes are higher than night time ones. See implication for the discussion.
Same scale can be used, although then the diurnal variation is not that evident.

-Figure 11: Seems that whiskers are showing smaller flux value than what should be error on EC fluxes (see comment on page 8 about errors, precision and detection limit). Add statistics to comment spatial variability on CH4 FC fluxes
Coefficient of variation or standard deviation will be added.

References:

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