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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

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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)

+General comments:

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Discussion paper

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.

+Specific comments:

-Page 2, line 7: it is Heiskanen et al (2015) rather than 2014

-Page 2, line 11: "... a small part of a lake": rather vague...

-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.

-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?

-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 equilibra-

tion 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.

-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.

-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...

-Page 8, line 34: why more frequent sampling should necessarily lead to higher fluxes than the ones reported by Miettinen et al? Give explanations.

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

-Page 9, line 1: please add reference to support hypothesis on lateral CH4 transport from catchment linked with precipitation event.

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

-Page 9, line 8: detail explanation in Schubert et al for lower kCC results, if relevant for this study.

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

-Page 9, line 16: EC increase seems be lower than a factor 3. See Table 2.

-Page 9, line 20: 3 μmol m-2 s-1: calculated from which BLM model?

-Page 9, line 25: "The same result. . .": that is, kCC lower than both kTE and kHE?

-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?

-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.

-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?

-Page 10, lines 9-10: is horizontal turbulence assumption consistent with kHE variability given in previous sentence?

-Page 10, line 15: maximum is rather reached at noon than during the afternoon

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

-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.

-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.

-Page 10, lines 27-28: add wind speed on plot for better comparison.

-Page 10, line 29: precise which Fig 9 panel.

-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.

-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

-Page 10, line 33: "detached gases...": detached does not seem an appropriate word

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

-Page 11, lines 9 and 11: again highest fluxes around noon seems more correct.

-Page 11, lines 13-14: Not clear, are you discussing comparison between day vs. night fluxes, or mixed/stratified periods?

-Page 11, line 16: " This may be caused by increased convective transport of CO2 from deep waters to the surface": any other reason possible?

-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.

You use 'median' for 'standard diurnal variation' throughout the section.

-Page 11, line 28: quote reference(s) that show that anchored chambers can enhance artificially the turbulence and subsequent fluxes.

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

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

value

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

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

-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).

-Discussion on Figure 4 and 5 should be supported by statistics on difference/similarity between the different fluxes assessments.

-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.

-There were no measurements for CH4 from the automatic system on September 22, 23 and 24?

-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.

-Figure 6: add wind speed.

-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.

-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.

-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

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