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Interactive comment on "Comparison of floating chamber and eddy covariance measurements of lake greenhouse gas fluxes" *by* E. Podgrajsek et al.

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We want to thank referee #2 for taking their time to comment on our ms. The comments from the referee together with our answers are provided below:

1. p. 18310 line 7-8, "the two methods agree relatively well during some periods, but deviate substantially at other times." This kind of statements in fact give no information to readers. Should be specific and explain under what conditions the two methods agree or deviate.

In the sentence that follows, line 8-10, we state that the large discrepancy between FCO2 measured with EC and FC might be caused by heterogeneity in partial pressure

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of CO2. As we do not know what is the cause of this difference or at which conditions the two methods disagree we cannot give an exact answer to this statement.

2. p. 18310 line 22, ". . .better quantify. . ." It seemed that there is no result explained that how you quantify the terrestrial carbon sink better?

With this part of the introduction we want to emphasis the importance of studying CO2 and CH4 fluxes from lakes. This chapter will be changed in the revised ms.

3. p. 18313 line 13. Why use the 4-6 chambers mean value? How about the results from these chambers? They agreed with each other or they showed any differences?

Since the EC method measures the flux originating from an upwind area, called the footprint, which is larger than the area the FCs measure from, we believe that a mean value of several FCs is the best way to compare the two methods. In the comparison figs 3 and 7, the horizontal bars represent the maximum and minimum FC measurement during one deployment which shows the variability between the FCs.

4. p. 18315 line 22-25. "The concentration. . ." when you calculated the concentrations of CH4 and CO2 in the water by using the syringe to collect the sea water, did you consider the temperature difference between the lab and the sea surface?

Yes, the water temperature was measured and it was taken into account when the water concentrations where calculated. The procedure is described in detail in Bastviken et al. 2010.

5. p. 18316 line 18-19. Please be specific. How the magnitudes of the difference when you claimed results in 2011 larger than those in 2012?

This is a valid point and the magnitudes of the fluxes in 2011 and 2012 will be added to the text in the revised ms: During 2011 (Fig. 2a), the magnitudes of FCH4EC1 (mean=6.15 mmol m-2 d-1) were substantially larger than in 2012 (mean=4.56 mmol m-2 d-1).

BGD

10, C9606–C9609, 2014

6. p. 18316 line 23. Figure 2 including fig. 2a, fig. 2b, and fig. 2c. In section 3.1, you just mentioned fig. 2a. If fig. 2b and fig. 2c were not necessary for the discussion, please delete them; otherwise some discussion related to them are needed.

We agree with the referee and more discussion related to Fig 2b and 2c has been added in the revised ms: In summer 2012 (Fig. 2c), FCH4 was also measured from an additional EC tower positioned at the shore, FCH4EC2. As expected, because of the position of the tower, the mean value of FCH4EC2 from 13 June 12 – 15 June 12 (mean=1.77 mmol m-2 d-1) was higher than both FCH4EC1 (mean=0.88 mmol m-2 d-1) and FCH4FC (mean=0.89 mmol m-2 d-1) for the same period.

7. p. 18317 line 6-7. When you compared the FC estimate of methane flux with EC method, which values were you used among those 6 chambers? Or did you use the average values of those 6 chambers? How about the difference among those 6 chambers in measured methane flux?

See answer question 3.

8. p. 18318 line 28. Change “then” to “than”

This has been changed.

9. p. 18318 line 19-21. Explain why?

This is a highly interesting question, however we do not have an explanation for this and we believe that this is not in the scope of this article. We, thus, decided to not analyze it further. With this subchapter (3.2) we want to highlight one major difference between the FC and EC methods; EC and FC methods cover different areas making EC advantageous for integrated measurements over larger areas, while the FC approach is suitable for local and spatially well constrained flux measurements.

10. p. 18319 line 1-9. The discussion here is too uncertain; this is not a good way to prove your results, please find some more certain and powerful evidence.

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10, C9606–C9609, 2014

We agree with the referee that this discussion is uncertain and that is why we in the end of the section p 18319 line 8-9 have the following sentence: . . .these discussions are only speculations.

11. p. 18319 line 19. Section “3.3.1” revise to section 3.4

This has been changed.

12. p. 18335 Fig 8: The fig showed that the fluxes of CO2 were measured by floating chamber or calculated according to previous studies all showed smooth variations. However, the FCO2EC1 showed extremely large variations: they were jumping up and down. Can you imagine the drastic changes in a relatively short time period? Were they method errors? Please explain why.

The majority of the EC data is rather well clustered however with some outliers. With the EC method we measured the turbulence in the atmosphere at high frequency. The method is “non-intrusive" and direct responding to changes in the atmospheric flow and other environmental parameters affecting the flux. The results presented here are actually typical and due to the nature of the turbulence, not a method error. Typically instrumental errors for FCO2EC measurements are 10% (Rowe et al. 2011).

Rowe MD, Fairall CW, Perlinger J a. (2011) Chemical sensor resolution requirements for near-surface measurements of turbulent fluxes. Atmospheric Chemistry and Physics, 11, 5263–5275.

Interactive comment on Biogeosciences Discuss., 10, 18309, 2013.