This manuscript presents two years of methane and carbon dioxide flux measurements at a lake and a fen sites. The dataset is very interesting, the framework analysis and results discussion very comprehensive and well written. I can recommend the final publication in Biogeosciences after the following comments are properly addressed:

1) Measurements of CO_2 fluxes were done by using an open path IRGA (LI-7500). Although this is a convenient or sometimes the only option in remote sites, I would use it with caution for measuring very low fluxes like the one presented here for the lake. The effect of air density fluctuations on CO_2 flux becomes very important, and the validity of WPL correction for low CO_2 flux has been also questioned in the past (see Ono et al., 2008).

2) I was a bit surprised to see that the high frequency corrections for CO_2 flux were so high (31% on average). Usually for open path EC system is much less. I guess this may be because of relatively large separation between the IRGA and the sonic anemometer. What was the separation? What is the value range of time lag for CO2 and which windows have You used? There may be problems with the WPL temperature term, which in theory should be measured in (or close) the path of the IRGA?

3) What would be the reason of relatively high (anti)-correlation between CO2 flux and H during wintertime (from Table 2)? And what about summer?

4) CH4 flux: it is not clear if H2O was measured by LGR. If not, then I guess the H2O fluxes measured by LI-7500 were used in the WPL correction for CH4. What EddyPro does when H2O comes from the LI-7500? The H2O fluctuations in the sampling cell of LGR may be quite different than the ones measured by the open path IRGA. How the authors cope with this issue? Why the compensation for the pressure term was also added? Which pressure data have been used for this?

5) Besides of density fluctuations caused by H2O, spectroscopic correction should be also applied to CH4 flux (see Peltola et al., 2014).

It seems to be some correlation between CH4 flux and LE (table 2). Is this because of points 4 and 5?

Summarizing, I know that there are limitations on including (in proper way) all these aspects, but at least it should be checked how important they are. Finally, the authors should acknowledge more clearly these aspects when discussing the uncertainty of these fluxes.

Minor comments:

Pag 6 L 11. Add also latent heat flux.

Pag6 L.18. “mixing ratio” usually means “dry mole fraction”, but I guess this is “wet mole fraction”, so without dilution correction. For calculating dry mole fraction point by point (high frequency data), simultaneous H2O measurements are needed.

Pag. L.26-28. How the synchronization was done? Just using the time stamps?

Pag.7 L.14. FST<0.3 is quite strict criteria. What about using FST<1? Is there a relevant difference in the data coverage?

Pag.7 L.15. Ustar threshold is taken equal to 0.1 m/s. How this was determined?

Pag.8 L8-10. Could you report some of these values used for the footprint calculation, e.g. roughness lengths?

Pag.8 L13-14. Is longer footprint in winter because of more stable conditions (negative H and low wind speed)? Or why?

Pag.8 L16. Based on what, the criterium $\sigma_v < 1$ m/s was used?

Pag.9L.25. Why the method did not perform well for lake CO2 flux?

Pag10L12-13. The random error of fluxes is usually proportional to the flux magnitude. Do you mean the relative random error (error normalized by the flux) is smaller?

Pag.10 L21. How did you calculate the RE of the fluxes modeled with ANN?

Pag.11. L9. Is the thaw season the same as ice-out season?

Pag.12 L15-16. Do you mean Fig. 2e and Fig 2f ?

Pag. 13. L15. How do you explain this fall burst of CO2?

Pag.15 L5-10. The diel cycle of 2012 H is quite noised respect to the other years. Why?

Pag.16 L10-20. May be some literature values can be added here for comparison.

Pag.16 L22-23. The highest correlation I can see for winter fluxes is with H.

Pag.16 L24. With EC it is not possible to measure advection, however You may see an increase of CO2 mean concentration in the data, which may indicate non-turbulent transport of CO2 from land.

Pag.16 L.27-31. I would say that it could be important to get a rough estimate of this correction. The effect (and direction) of this correction depends on the sign of H.

Pag.17 L.9-11. How the EBC (energy balance closure) plots look like in different years?

Pag.20 chapter 4.3.2. Are there any measurements of pCO2 for this lake during summer? Or chamber measurements? Anything that could support the sink that you measured with EC?

Pag.23 L17. Coordinate rotation is not really a correction.

Pag.23 L19-22 . Please report the values of time windows and time lag. Was the lag maximization applied also for sonic temperature? The lag between w and Ts should be 0.

Pag32 L.17. Please update the reference Rannik et al. (2016). The article is now published in AMT.

Caption of Table 2. Do you mean the std of lateral wind speed?

Figure 2 . Please explain in the caption what are those black arrows pointing down in fig2f.

Figure 6. Please reduced the scale of CH_4 fluxes. It is very difficult to see how EC compare with ebullition data.

Please increase the font size in all figures.

References

Ono et al, 2008. Apparent downward CO_2 flux observed with open-path eddy covariance over a non-vegetated surface. Theor. Appl. Climatol. 92, 195–208

Peltola, O., Hensen, A., Helfter, C., Belelli Marchesini, L., Bosveld, F. C., van den Bulk, W. C. M., Elbers, J. A., Haapanala, S., Holst, J., Laurila, T., Lindroth, A., Nemitz, E., Röckmann, T., Vermeulen, A. T., and Mammarella, I.: Evaluating the performance of commonly used gas analysers for methane eddy covariance flux measurements: the InGOS inter-comparison field experiment, Biogeosciences, 11, 3163-3186, doi:10.5194/bg-11-3163-2014, 2014