Biogeosciences Discuss.,
doi:10.5194/bg-2016-466-RC2, 2017

Biogeosciences Discussions

Interactive comment on "Year-round CH_4 and CO_2 flux dynamics in two contrasting freshwater ecosystems of the subarctic" *by* Mathilde Jammet et al.

C. Wille (Referee)

christian.wille@gfz-potsdam.de

Received and published: 25 May 2017

General Comments

The manuscript presents a 2.5-year CO2 and CH4 flux data set from a fen and lake within a subarctic peatland ecosystem. GHG fluxes from aquatic ecosystems have been identified to contribute to a large but uncertain amount to the global GHG budget. Thus the presented study delivers important data and a substantial contribution to scientific progress within the scope of Biogeosciences. The scientific approach and the applied methods are valid, related scientific work is amply referenced, and the results and conclusions are presented in a well-structured way and in an appropriate style. I support the publication of the manuscript after minor revision.

Specific Comments

1. The manuscript suffers from occasional vagueness in definitions and nomenclature. For instance, "ice-out" is inconsistently used in the manuscript. On first use, in the abstract (p. 2, line 12), it denotes the point in time when the ice is completely thawed. Further into the manuscript "ice-out", "ice-out season", and "ice-out period" are used synonymously denoting the "thaw season" as defined in section 2.7. I suggest to stick to the nomenclature introduced and to replace "ice-out" and its variations with "thaw period" wherever this is applicable throughout the manuscript. Further, in section 2.7 it is not made very clear that the defined "thaw season" not only comprises the actual thawing of the lake ice, but also - and importantly - the subsequent initial overturning of the lake water.

2. Gap filling of highly variable CH4 fluxes is notoriously challenging but at the same time crucial for determining seasonal and annual balances. Therefore, the artificial neural network (ANN) gap filling method should be well presented and discussed. I consent to the detailed description of the ANN method being given in the supplement so as to keep the text concise. However, the environmental drivers used for the ANN is important information which should be included in the main manuscript. Hence I suggest to move Table S2 to Appendix B (which in consequence could be reduced in text). Furthermore, a paragraph on the ANN gap filling performance should be added to the results section, and Fig. B1 should be part of it (and should be improved for better legibility). Especially in case of CH4 fluxes, which span 2 orders of magnitude, a discussion of how the ANN performs - both in case of the slowly varying background flux and episodic high emission events - would be very interesting.

3. There was no gap-filling performed for lake CO2 fluxes after May 2013 due to low data coverage. However, according to table S1, CO2 flux data coverage was back to normal in 2014. Gap-filling should be resumed for 2014 data if at all possible.

4. I like the statistics of measured fluxes presented in figure 4. However, with a data

coverage of typically 30 %, how reliable is this information, i.e. how does the picture change when you look at the statistics of gap filled fluxes? The mean and median fluxes presented in section 3.2 may have to be interpreted with a certain caution, especially concerning the transient lake fluxes.

5. I think there is more potential in the ebullition flux data from bubble traps than the qualitative comparison presented in figure 6. I would like to see the attempt of a qualitative analysis in order to derive an estimate, of how much of the EC flux stems from diffusion and ebullition. Further, the thaw of the lake ice and the initial overturning of the lake water after the "ice-cover" period seem to be well separated in time. Hence I suggest to divide the spring emission peak into a portion which originates from the escape of gas bubbles trapped in the ice, and a portion which originates from the initial overturning of the lake water. This could help to explain the large differences between the total thaw season CH4 emissions in 2013 and 2014.

6. How was eddy covariance raw data logged (type of data logger), and what exactly was done with CH4 raw data during August 2013 - December 2014? As I understand, CH4 concentration was taken from FGGA raw data files and had to be synchronized and combined with sonic anemometer data before being fed into EddyPro. If this is the case, have you checked if this caused any bias in the flux calculation? Please clarify.

7. In addition to the maintenance-caused gap during February - March 2014, there is a large gap in CH4 flux data during December 2013 - February 2014. Was all data of this period rejected by the quality screening? The same question arises for CO2 fluxes during February - March 2014. Please clarify.

8. The "Burba effect" seriously compromises cold season CO2 flux data from the LI-COR Li-7500 which you used. The fact that the "Burba correction" was not applied is important information and should be given in the methods section and not as a sideline in the discussion. To my knowledge, many researchers failed to derive a meaningful flux correction using Burba's method, in which case there is no other way than to use

the CO2 flux data as it is. However, since you used a Los Gatos FGGA analyzer, you could use its CO2 data to calculate another CO2 flux data set to use during winter or to confirm winter time fluxes from your Li-7500. Has this been attempted?

9. The manuscript would benefit from focusing and shortening. Some examples are given in the next section.

Technical Corrections

p. 3, line 8: Change "explains" to "explain".

p. 3, line 8: Should read "order-of-magnitude-scale uncertainty"; consider simplifying to "large uncertainty".

p. 3, line 30: Change "lake" to "lakes".

p. 5, line 3: Change "lake" to "lakes".

p. 5, line 27: "May be" sounds very weak. The cited paper must have a stronger opinion on this matter?

p. 6, line 4: Change "palsa" to "palsas".

p. 6, lines 4-5: Change the order to "During snow melt, there is a small surface inflow feeding. . .".

p. 6, line 19: Add "height" after "2.50 m".

p. 8, line 11: Remove "the" between "footprint" and "model".

p. 9: The first paragraph and the last sentence of section 2.5 could be deleted.

p. 9, lines 21-22: "The goodness of fit was quantified with . . . the absolute root mean square error (RMSE)." "Absolute" is superfluous and can be deleted. But in fact, table S2 gives the RMSE in %, and it is unclear what these percentages refer to. I strongly recommend to give the RMSE in flux units.

p. 9, line26: Replace “per” by “with”.

p. 10: Section 2.6 could be shortened drastically by focusing on the reliability of the low (winter time) fluxes and on a brief outline of the error propagation method and the bias error.

p. 11, lines 21-22: Simplify “daily energy input (upwelling > downwelling radiation)” to “mean daily net radiation”.

p. 11, line 22: Wrong reference. Change Fig. 2b to Fig. 2c

p. 11, line 23: Wrong reference. Change Fig. 2c to Fig. 2d

p. 11, line 23: Change albedo from 5 % to 0.05 to be consistent with units in Fig. 2.

p. 12, lines 9-10: Differences in mean temperature correlate with differences in total net radiation, or more simply, mean temperatures correlate with total net radiation values. Please correct.

p. 12, line 14: “thermal stratification along lake depth” sounds odd. Consider changing to “thermal stratification of the lake”. (Again in line 16)

p. 12, line 15: “... large” thermal stratification ...” Consider replacing “large” by “strong” if that is what you mean.

p. 12, lines 14-15: Replace “was repeated each year” by “was similar in both years”.

p. 12, lines 15, 16, 22: Wrong reference. Change Fig. 2d to Fig. 2f.

p. 13, line 2: Replace “followed” by “showed”.

p. 13, line 12: Delete “but”.

p. 13, lines 13-14: “The highest CO2 uptake rates were observed during the summer of 2014, which was the warmest summer of the study period with highest solar radiation input (Table 1).” Table 1 lists only total net radiation values. As solar radiation can be expected to have a much higher explaining power for carbon fluxes (as confirmed by

its inclusion in the correlation analysis, table 2), total solar radiation should be reported in table 1.

p. 14, line 15: The correlation between increases in sediment temperature and CH4 bursts from the lake can hardly be seen – I suggest to delete this sentence. The correlation with falling atmospheric pressure described in line 7 is much better visible.

p. 15, line 29: Replace “lead” by “led”.

p. 16, line 1: I would not expect a complete ice cover at a fen dominated by vascular plants as described in the study site section. Unless the water table is very high at the onset of freezing. I suggest to rephrase this passage.

p. 16, lines 27-31: The passage on the Burba correction is pointless, because - as written at the end of the paragraph - it corrects fluxes towards higher values and so cannot explain the too high fluxes during the winter 2013-2014.

p. 17, lines 24-25: Mind the causal connection between temperature increase and decrease of CH4 solubility! Rephrase, e.g. “. . .since a seasonal increase in sediment temperature favors methanogenesis and additionally causes a decrease of CH4 solubility. . .”

p. 18, lines 11-20: The whole paragraph seems inconclusive – how does it relate to your data?

p. 19, line 2: Correct “release” to “released”.

p. 19, line 23: Correct “term” to “terms”.

p. 21, line 21: Change word order to “Alaskan thermokarst lakes”.

p. 22, line 13: Change “period” to “periods”.

Table 1: I suggest to move the dates from the figure caption to the table. Total solar radiation should be added as this is the most important driver of CO2 fluxes during

ice-free periods (in which case total net radiation could be omitted). Tables 1 and 3 should be merged into one table.

Table 2, caption: Wrong reference. Change “Table 2” to “Table 1”.

Figure 2, caption: Add “daily means of” where applicable. Explain shaded area, “PN” and arrows.

Figure 3: Add grid lines, or at least y=0 lines. This helps the reader to determine if small fluxes are positive or negative or fluctuate around zero.

Figs. 4, 6, 8, 9, B1: The axis labels are too small.

Figure 6: Remove temperature plots. The suggested correlation between sediment temperature and CH4 flux can hardly be seen anyway.

Figure 7: One of the two graphs can be omitted, as they show the same data.

Figure 8: I suggest to remove the data of single years. The great variability makes it difficult to extract the important information from the graphs. Add grid lines, or at least y=0 lines.

Figure C1: This figure could be deleted. There is no real gain of information compared to figure 4.

Table S2: RMSE is given in % - of what? Please use flux units. What is the mean random error given in the last table row? Please explain.

Interactive comment on Biogeosciences Discuss., doi:10.5194/bg-2016-466, 2017.