Biogeosciences Discuss., 9, C7813–C7816, 2013
www.biogeosciences-discuss.net/9/C7813/2013/

Biogeosciences Discussions

Interactive comment on "Methane fluxes measured by eddy covariance and static chamber techniques at a temperate forest in central ontario, Canada" *by* J. M. Wang et al.

Anonymous Referee #2

Received and published: 30 January 2013

The manuscript by Wang et al. focuses on methane fluxes measured by eddy covariance from a forest ecosystem. Up to date only few studies reported on continuous methane fluxes from forest ecosystems and therefore the presented results contribute to the currently available knowledge on methane exchange between ecosystems and the atmosphere. In general the authors provide a well structured and analyzed manuscript and I think stating that pressure pumping could actually lead to higher CH4 uptake rates in the soil is a new but also challenging statement. Even though some points are critical: (1) the rather short time period of measurements (5months) which seems to focus on publishing methane CH4 data as soon as possible even though more detailed analysis of methane fluxes in relation to environmental parameters but

also on the variation throughout the year could have been accomplished. (2) A more severe point before publication are the uncertainty issues as already mentioned by Reviewer 1, which I absolutely agree with.

General Comments: The authors add further information with supplementary figures, which given the still relatively little available date on CH4 flux post processing etc could be of further interest the scientific community. I therefore encourage the authors to include figure S4 and S5 in the manuscript. This will further strengthen the manuscript, particularly since the currently presented results have already been shown within other CH4 flux studies.

How did the authors define the detection limit of the FGGA? In order to do so, one could use the full setup above a system, which emits or takes up a defined amount of methane. Otherwise to determine the noise within the measurement one could go to a non-release/non-uptake study often referred to as "carpark"-studies. Figure 1 as presented by the authors does neither show the uncertainty nor the detection limit of the instrument though one might easily tend to think so when seeing the results.

In terms of data availability the authors mention the removal of rather vast amounts of data, and it remains unclear whether each percentage given is cumulative to the previously stated number, therefore a stepwise removal and therefore it is unclear how many data remained for this analysis.

Specific Comments: The inlet of the FGGA was rather far positioned from the Sonic Anemometer, having a sensor displacement of 0.7m. Why?

Why did the authors define outliers as values diverting from the mean by 8 SD? Especially when having methane fluxes and possible peaks caused by ebullition in mind this seems to be a random pick.

How much data were lost when removing all fluxes having wind directions between 0-93°?

On page 17750 the authors refer to the importance of biologically active plants influencing CO2 fluxes but not methane fluxes. However previous studies, even though not in forests have shown a clear link between productivity and methane fluxes.

A more detailed information on the chambers would clearly improve understanding the presented results – even though the authors refer to the chamber measurements, considerable little information is provided on this – except the quick comparison of the general pattern of fluxes across the 5 months of measurements.

In page 17752 the authors state a higher mixing ratio might be occurring due to the 200km distant Greater Torronto area, which I believe is rather far and why would the larger values be caused be the often mentioned patchiness of the site? Please comment.

How was the comparison and the averaging done when comparing canister with eddy flux measurements? Were potential gaps in the data filled in order to derive a two day average? Please explain.

The authors mention the slope rather often. Have the authors checked whether advection of methane occurs and possible sources are resembled in the actual flux measurements?

The end of paragraph 4.2.: Even though I appreciate the statement of enhanced methane uptake caused by higher windspeeds the things written are contradicting, particularly when looking at the October data, where the uptake rates decreased even though wind speeds were higher. Please comment.

Technical Corrections: Page 17747,l.20: masl –> m a.s.l. Page 17748,l.21: lpm -> l m-1 Page 17750,l.19: every 2.5weeks (on eight separate days) – unclear, please clarify, also I suggest adding a figure which presents the study site including the location of the tower, of the chambers and if possible the reliefs in order to understand the topography would be really helpful Page 17751,l.5-11: The authors first state about

measurements of Tsoil and Theta adjacent t each chamber but than only two sites are mentioned which measured theta and there only one site is used in the analysis. Please clarify. Page 17753,l.12: high emission rates – how high is high, please provide numbers or values for the different chambers Page 17753,l.15: (+/- standard deviation) –> (+/- SD) Page 17753,l.22: with ranges of 8-100% and 55-100% - this is unclear since the averages including SD have been given before Page 17754,l.2: remove “soil moisture” Page 17754,l.13-17: Why is this relevant? Page 17756,l.1: how narrow were the bins – be precise Page 17756,l.18: 163nmol m-2 s-1 – This is a very large value and if these are averages what were the maxima and what were the minima? Page 17757,l.24-26: What about possibly deeper installed sensors? Page 17758,l.7: just because of “shallow” soils and commonly occurrence of methantrophs in the upper soil layers I think it is a wild guess to argue this way. Therefore I suggest stating the typical occurrence of methanotrophs in the upper soil layers and leave it at this information. Figure 2: does this represent available data from all months? What about data gaps? Might there be a bias towards daytime data? Figure 5a: This is the same as Figure 2 – redundant, therefore I suggest to remove or add the additional variables to Figure 2! Figure 5d: no change in soil water content is visible, adjust the scale. Which measurement location is this, dry or wet? Figure 4: bin-averaged slope – please explain Figure 7: I disagree with the statement that chamber fluxes agree well with the results found using EC measurements, particularly on Jun 7th, Jun 21st and Aug 2nd this is absolutely not the case. Please correct.

Interactive comment on Biogeosciences Discuss., 9, 17743, 2012.