

## ***Interactive comment on “Field intercomparison of four methane gas analysers suitable for eddy covariance flux measurements” by O. Peltola et al.***

### **Anonymous Referee #2**

Received and published: 6 February 2013

#### <General comments>

In this study, four fast-response methane (CH<sub>4</sub>) analyzers were tested for determining the surface CH<sub>4</sub> flux by applying the eddy covariance technique. It also provided strong evidence that two of the tested closed-path analyzers can work well throughout a growing season for eddy flux measurements. Measurements and data processing/analyses were carried out appropriately. The results will contribute to the flux community, and to thus Biogeosciences, which promotes the progress of full GHGs accounting and CH<sub>4</sub> emission/abortion dynamics in terrestrial ecosystems. However, the present version of the manuscript is not well organized. I have found that many descriptions have been repeated several times in the main text. Further, the Discussion and Conclusions sections do not seem insightful. I believe that the authors can improve the manuscript by

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consciously reorganizing and emphasizing their findings and limitations.

<Specific comments>

P17653, L25: The word “long-term” seems ambiguous. I suggest using an alternative term such as “season-long” to express and to interpret the presented data.

P17654, 2.1: Please clarify the mean height of the vegetation. It would help in understanding the spectral analysis results.

P17655 2.2: Calibration is the basis of any measurement. Hence, I suggest that the authors describe the manner in which they calibrated each gas analyzer: place (on site/off site), method (static/dynamic), date/frequency, standard gases, and relevance of the coefficients.

P17655 2.2.1: Please specify the difference between the LI-COR prototype 7700 used in this study and the commercial version of LI-7700. If the two do not significantly differ with respect to their sensor specifications, the authors could refer to the prototype as LI-7700.

P17660, 2.3.2: This subsection seems redundant, and it is difficult to judge whether the authors’ calculations are scientifically sound. Please reorganize this subsection.

P17660, L11-15: Pressure fluctuations can be neglected only when their effect on the WPL correction is considerably smaller than the other effects. Did the authors confirm this for their system (closed-path measurements) or environment (open-path measurements)?

P17663, L9-29: I did not completely understand this paragraph, particularly the calculation of the water vapor flux that is used for the correction of each analyzer. The authors mentioned that “WPL terms were calculated using RMT-200 methane time lag” in L20-21, but according to my understanding, the LI-7000 analyzer was connected to a different line with a different flow rate. Therefore, I suggest that the authors explain the lag calculation in further detail.

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P17664, 2.4: Thank you for separately estimating the instrumental noises. However, this is not appropriately emphasized in the Results section (P17673, L3-29).

P17671, P2-4: Is this true?

P17673, L3-29: These paragraphs are confusing. The AFFE value was determined using Equations (11) and (14), and thus contains both environmental (one-point sampling) and instrumental errors. The authors should use Equation (15), not Equation (11), if they want to conclude the performance of the instruments as in L18-20. In fact, most of the random error resulted from one-point sampling, not from instrumental noise (Fig. 2).

P17676, 3.4.2: This subsection is also redundant and confusing. For example, the observed diurnal variations of the WPL terms were self-evident according to the WPL theory, and therefore not worth noting in the body.

P17682, Discussion and Conclusions: I strongly recommend that the authors reorganize this section. Many descriptions given in this section have already been mentioned in Results or elsewhere. The authors could discuss the critical points of this study more consciously by considering the generalization/applicability of the presented findings.

<Technical comments/corrections>

Overall: The body is a little confusing and redundant. I found some descriptions in Results that should be moved to Materials and Methods or Discussion and Conclusions. Many sentences in Results are repeated in Discussion and Conclusions. In-depth interpretations of the data that are not mentioned in Results are expected in Discussion and Conclusions.

Overall: Both British and US English are used in the manuscript.

P17658, L2: “L” is used one line before to abbreviate the latent heat of vaporization.

P17658, L5: “u” and “v” are not used in Equations (1)–(3).

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P17659, L2-14: This paragraph can be understood very well but may not be needed for this manuscript.

P17661, L2: “different spectroscopic”?

P17662, L8:  $X(\chi)$  is used for denoting the molar mixing ratio of CO<sub>2</sub> in other places of the manuscript. It is confusing.

P17667, L2: Add the DOY labels to the x axis of Fig. 2.

P17669, L11-23: These sentences do not express a result and should be noted elsewhere.

P17671, L8-9: I do not understand the syntax of this sentence.

P17674, L20-22: This should be noted in Materials and Methods.

P17675, L10-13: This sentence should be moved to Discussion and Conclusions.

P17675, L24: “partly cancel the effect. . .”?

P17676, L15-19: The sentences are already stated in Materials and Methods.

P17676 L20-25: They should be discussed in Discussion.

P17681 L16-17 “. . ., namely. . .”: This is also redundant.

Fig. 1: Please explain the line colors in the caption.

Fig. 2: Add DOY labels to the x axis. Is it possible to combine Fig. 2 with Fig. 10?

Fig. 4: This figure seems less informative. Please emphasize the necessity of this figure in the body.

Fig. 6: The fitted line looks different from the expectation from the solid circles for G1301-f, particularly at relatively high frequencies. Is it true?

Fig. 7: The legend should not overlap.

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Fig. 9: The legends and dots should not overlap. The body states that the small slope value for the left-middle panel (0.886) resulted from some points located at a relatively high position (P17680, L15-17), but I do not agree, because the number of such irregular data is very small in the entire set of data points.

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**BGD**

9, C7982–C7986, 2013

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