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Interactive Comment

# *Interactive comment on* "Eddy covariance methane measurements at a Ponderosa pine plantation in California" *by* C. J. P. P. Smeets et al.

**C. J. P. P. Smeets et al.**

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Interactive author response to RC S1329 Anonymous referee #1

Eddy covariance methane measurements at a Ponderosa pine plantation in California by C.J.P.P. Smeets et al.

General comments As mentioned by the referee we agree that the measuring period is too short to draw reliable conclusions on annual average emission rates but we must remember that there are only very few data available with which to compare at all. Therefore we think that comparing with other data is still useful. We will leave out the comparison with the global inventory study but we will compare our results to monthly model values. Further argumentation is given below.

For the experimental area the summer diurnal circulation pattern is very persistent and present for 72% of the time (see line 3-4 p. 5211 and several references). During the experimental period this circulation pattern was shown to clearly dominate the boundary layer conditions (p5210-5211). Our observations therefore represent the typical diurnal variations that are present throughout summer most of the time which justifies a comparison with monthly model results. The referee is correct that we should use model results from August instead of July (the values remain equal). We will change this accordingly.

Feasibility of $CH_4$ measurements by this type of FMA was addressed before by Hendriks et al., 2007. Our paper presents the second study performing EC measurements with this type of FMA. However, in contrast to the referees remark, we employed this system under conditions of much lower flux levels then before ($>$13 times lower than Hendriks et al. 2007: the average flux emission reported by Hendriks et al. 2007 was about 475 ng/m2s while for the Blodgett data we find 35 ng/m2s). We explicitly mention this at p5212, line 25 and p5213, line 1-4 together with several other studies dealing with EC methane measurements performed under conditions with much higher flux levels.

Furthermore, the referee has doubts about the added value of the analysis shown since all of the corrections applied were published before. This may be due to the fact that we show all the calculations in the appendix. However, although the individual corrections are indeed described in several papers, the complete set of corrections in the setup with the combined open and closed path systems has never been used in a comprehensive way. First, a typical problem arises when applying the FMA under low flux conditions which is first addressed in our paper. The signal-noise becomes so important that it pollutes the cospectral estimates in the high frequency range erratically. It is important to be aware of this problem in the FMA data. The way this problem is solved in the paper is new; first we calculate the low frequency part of the methane flux from the cospectral estimates below f=0.3Hz and then estimate the relative contribution in the high frequency part (f$>$0.3 Hz) from all other scalar cospectra assuming cospectral



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similarity of the scalars. The combination of open and closed path sensors is not a common technique. Usually, when $H_2O$/$CO_2$ are measured, an open or closed path EC set up suffices. However, when measuring $CH_4$ we are still depending on closed path systems. Therefore, we think it is still extremely useful to summarize in our paper all relevant corrections. In particular, the phase shift of the $H_2O$ signal in a closed path system is still not very much applied or only in part, because it is not yet very well documented. This correction can have severe consequences specifically for methane fluxes as is shown on our paper.

Organization of the paper We will define our objective more clearly with a description of combined open and closed path instruments for eddy correlation measurements, and the presentation of the full suite of necessary corrections. For clarity we will include part of the Appendices in the text and check on the duplication of text mentioned by the referee.

Specific comments First the referee #1 comments are reprinted followed by the corresponding authors response starting at $>$.

Abstract Line 11 Relatively high noise level. make it quantitative. What is relatively high?

$>$The noise level was about +-5ppbv which is high when related to the overall variations of methane concentrations within a 30 minutes run. As a result a clear white noise signal is apparent in the power spectra (not shown). These aspects of the high noise level are discussed in the first paragraph of Section 3.3 High frequency noise.

Line 12 A software problem is really vague. What was the real source of the problem? How did you solve this problem? What was the improvement after solving this problem?

$>$The problem we are referring to is a software upgrade of the FMA system. From laboratory measurements, for which we usually use the internal pump, it appeared that the noise was clearly decreased down to 1ppbv. We expected the same thing to



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happen for the measurements in a EC set-up when writing this paper. We recently discovered that this actually does not help to reduce noise for our EC set-up. We now suspect that part of the noise is caused by using the external pump in the EC set-up. The very high pump capacity causes pressure fluctuations that add noise to the methane signal. This means we will rewrite the text in places where we mentioned the software problem.

Page 5203 Line 14 Higher emissions. make it quantitative.

>Quantification of missing sources: Frankenberg satellite data up to 4% higher concentrations than modeled values, Carmo et al. 2006 and Miller et al., 2007 quantify significant emissions for upland forest areas (2-21 mg CH4/m2d and 27 mg CH4/m2d, respectively).

Page 5204 Line 11 Skip line. in the near future.. basin >OK

Line 15 Typing fault. change t he EC data in the EC data >OK

Line 24 Observation period from 11 to 19 August, however, all Figures include a shorter period, why?

>The measurements shown are from 14-19 August because for this period we have a continuous time series. We will reformulate. Note that for the period 11-14 we observed comparable maximum daytime values for the methane flux. We will use this information to strengthen our point that it is realistic to compare out flux results with monthly model values.

Page 5205 Line 13 Raw data was sampled at 10 Hz. Why did the authors use a sampling rate of 10 Hz since the FMA analyzer can sample at 20 Hz. (line 13 page 5204)

>Several arguments can be pointed out to justify this. In general 10 Hz often suffices to capture the full range of flux relevant eddies (e.g. Aubinet 2000). Furthermore, the time constant of the FMA is around 0.1 seconds (see Hendriks et al. 2007), so that



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increasing the sample frequency beyond 10Hz does not add information to the data.

Line 25 Measurement height of 13.5 m indicated for the second time (also at line 10 page 5205) In this section (Experimental set-up), it should be indicated which measurements are performed. For example: it is not indicated with which instruments CO are measured. CO observations are shown in Figure 2. Besides, it is not indicated which micrometeorological measurements are done.

>Correct. We mentioned that we measured at the Berkeleys Blodgett Forest Research Station run by the University of California (Section 2 first line). We should have added that the whole suite of atmospheric data is monitored continuously at this site (meteorology as well as several trace gasses). References for the site are already mentioned.

Page 5206 In this section (data processing), the outline is not well ordered. Most of the topics are addressed more than once. Besides, this section is not easy to follow because of frequent use of the appendix.

>We will rewrite this part in line with the focus on feasibility and methods used to measure fluxes with the FMA. Furthermore, as suggested by the referee, we will include some of the Appendices in the text.

Page 5207 Line 10 How is validated that the damping of temperature fluctuations inside the tube is effective?

>The damping of temperature fluctuations inside a tube is not validated but to our knowledge well known in the flux community and we follow the recommendations from Ibrom et al 2007a as mentioned in Section 3.2 at line 8-9, p. 5207. We will include an extra reference to the temperature fluctuation damping (e.g. Leuning and Moncrieff, 1990)

What is the reason for a different damping effect on temperature fluctuations that on water vapour fluctuations?

>This also follows from the given reference Ibrom et al. 2007a. It is not well known



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what might cause this delay between H2O and other scalars. Ibrom et al 2007 mention that they noticed that the delay time increased in time as a result of particles coating the internal filters or even the entire interior of the intake tube. They hypothesized that the increased damping with RH relates to response of particles to a varying RH.

Page 5208 Line 8 How was the signal to noise ratio determined?

>Visually from the raw data. The value represents a signal amplitude (top-to-top) over a very short time interval, say 10 samples.

Line 9 Recently upgraded software. What is the difference between the old software and the new software? >See the reply on the comment on the Abstract, line 12.

Line 22 Slightly unstable conditions. Could you indicate the stability parameter z/L and the mean wind velocity ?

>We will add the average values for zL and u.

Page 5209 Line 6 If you alter the frequencies larger than 0.3 Hz, what is the effect of aliasing on the low frequencies.

>There are no aliasing effects involved since we merely recalculate the covariance from the cospectral estimates by leaving out the values above f = 0.3Hz (see line 3 and 4 p 5209). The word 'filter' is probably confusing and we will rephrase the sentence.

Page 5210 Line 23 How did they measure CO? > We will refer to a Berkeley University paper Why is it interesting to show CO?

>CO is a clear indicator for combustion processes, and since CH4 behaves the same as CO we suppose that they originate from the same source region.

Line 24 How did they measure all the meteorological variables?

>We will not include a list of all instruments used by the University of California since this is already extensively described in several papers. We will add a reference to a



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paper specifically dedicated to these measurements and will specifically address this in the text.

Page 5213 Line 13 I do not agree with the sentence the uncertainty of fluxes above the detection limit is on average 26%. There are much more uncertainties involved in EC flux measurements. For example: the uncertainty due to one point measurements as described by Businger et al. (1986).

>We will rephrase and include 'obtained from this method'.

Page 5215 Line 16 Empiric change into empirical >OK

Page 5216 Line 11 Missing reference by equation >OK, there is a missing reference to k, the Von Karman constant taken as 0.4. This will be included.

Page 5217 Line 4 Re should be non-italic >OK

Page 5218 Line 4 What is the amount of underestimation?

>In terms of half-power frequency the transfer functions were reduced by 0.3Hz (CO2) and 0.2Hz (H2O) compared to the theoretical ones. Comparing our Figure 4 with their figure 14b resolves about equal additional damping by (presumably) filters in the systems.

Figures In general, the Figures are not very clear. (some examples will be given below)

Figure 1, it is unclear why there is a distance A and B between the (empirical) Kaimal co-spectrum and the measured co-spectrum. Both co-spectra are normalized. Therefore, the area below both curves should be 1. Besides, it is unclear which (empirical) Kaimal co-spectrum is used. The stability and mean wind velocity should be indicated. The symbols of variables should be written in italic. In the Figure caption, the amount of used 30 min EC fluxes should be indicated.

>We do not agree with the referee concerning the position and shape of the co-spectrum. In the Figure caption we state that the universal cospectrum from Kaimal



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(1973) for neutral atmospheric stability is arbitrarily offset (this is done for clarity). This shape is valid for stress and heat fluxes as pointed out by Kaimal 1973. If an italic format for symbols and variables inside Figures is generally applied in ACP than we will of course change our formatting in accord. We will include the amount of cospectra used in Figure 4 (i.e. 87).

Figure 4, the meaning of Ts, Gt corrected and Gt uncorrected should be explained in the Figure caption.

>We will include a description for these variables.

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Interactive comment on Atmos. Chem. Phys. Discuss., 9, 5201, 2009.