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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Methane flux, vertical gradient and mixing ratio measurements in a tropical forest" *by* C. A. S. Querino et al.

Anonymous Referee #2

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GENERAL

This paper on methane budget of a forest site is probably mainly intended to document the measurements, which is valuable in itself. In addition, the paper comes with an overview of the first results. The interpretation and discussion contain interesting elements as well. The manuscript is reasonably well structured and written, although it can be improved in some places. The subject is suitable for publication in ACP. I would recommend publication in ACP after some relatively small improvements and corrections.

SPECIFIC COMMENTS

The introduction can be condensed and needs to be streamlined. The present version

is distracting at places. It takes a long time before the reader knows what the subject of the paper really is. Please, focus sharper on the scientific problem at hand, that is, the methane budget of a tropical forest. Remove distracting parts, for example, on ecological services and land use change or long discussions on details of existing measurement techniques. Instead, be much more specific on the scientific question that drives the research. "To learn about the CH4 budget" is not specific enough. The introduction ends quite abruptly, which could be repaired by specifying those questions just before the methodology.

P5317, l19: replace "daytime or weakly stable conditions" with "conditions with well-developed turbulence". These conditions may be present or absent during both daytime and nighttime, that is, daytime per se is not a guarantee for well-developed turbulence.

In the methodology, the remark on the flora and fauna (p5318, l3) is irrelevant. Topographic characteristics are relevant, of course. Information on the carbon content and composition of the soil should be added since those characteristics are crucial in the interpretation of the methane production and consumption. In fact, the authors themselves refer to that importance quite often.

Please, explicitly define the flux sign convention used in the paper (positive upward). Section 3.1 should be reduced in length (delete discussion elements) and moved to the site description or perhaps to section 2.4. These results are quite relevant for interpretation (although not used for that purpose), but are beside the focus of the paper and distract too much from the real subject.

Section 3.2 is methodology.

Although the interpretation and discussion contain interesting elements it seems to be focused mainly on the credibility of the measurements. That is a pity, because the results probably allow interesting analyses on the relation between methane budget components and environmental drivers. Consider adding some more analysis (instead

of just comparison) in that respect. I would at least expect a comparison between the C budget related to methane and carbon dioxide, respectively, and a comparison in terms of GWP, in spite of the fact that the flux observations ran for a relatively short period. If the introduction is successfully restructured, this kind of analyses should not increase the paper length too much.

P5324, l11-12: a methane concentration decreasing with height within the canopy can in principle also mean that there is some strong methane sink in the upper part of the canopy. Admittedly, this is unlikely, also given the observed flux direction above the canopy, but the interpretation in l11-12 is not necessarily correct without further evidence. The planned flux chamber observations will become important in that respect. Restructuring the discussion a bit should place the remark in l11-12 in a proper context.

P5326, l1-2: the statement that the nighttime fluxes are identical to the afternoon fluxes is too strong; l12-13: "we use this for a rough estimate". Use what? the flushing rate, the height, general characteristics?; l18: how do you know the magnitude is correct? The reference can be wrong as well. In addition, this phrase suggests a precise estimate.

P5327, l15-16: this remark is strictly speaking not true, since it is only a flux gradient that can change the concentration. Such a flux gradient may be expected most of the time, though, in particular during the night since the flux and the top of the nocturnal boundary layer may be assumed approximately zero. In fact, during the night we have an atmospheric flux chamber measurement.

P5328, l13: 5 h is not entirely consistent with the 6 h mentioned on p5325, l19. (Also remove the zero in front of the 5 (or 6, whatever it should be).

P5328, l21-22: Statement cannot be proven without further analysis and soil based measurements. Please, add further evidence in the results/discussion. At least the site description should be extended to give some idea of the carbon characteristics (see remark under methodology). Otherwise, this does not belong in the conclusions

and should be moved to discussion.

P5329, l17 ff: Include reference to the Cabauw site.

P5331: Please, rewrite the explanation and discussion on Figure 9. This part is confusing and I do not entirely understand the arguments. If the average of the measurement is below the detection limit, a significant fraction of individual observations must be as well. Why then conclude that the flux is realistic if the average contains many undetectable fluxes? Or do you mean to say that the detection limit should be compared on the basis of individual half-hourly fluxes, since it is much different for each half hour. In that case, the presentation in Fig. 9 is not appropriate. It is concluded that during the night the fluxes are well above the detection limit, but I also see quite a significant time during the day where the average flux exceeds the detection limit. P5331, l25: the spectra are no proof of the high quality, just an indication; line29: specify the type of noise (noise "color"). Noise uncorrelated with wind speed should not have an effect on the fluxes. Certain noise, notably white, may have some effect.

TECHNICAL CORRECTIONS

P5315, l8: specify relevant timescale of the GWP, especially since the number quoted is a bit unusual.

P5315, l9: delete "per kg" since it is in the definition of GWP.

P5320, l15: "obteined" should be "obtained"

P5323, l2: add "CH4" between "the" and "flux data".

P5324, l2: delete ", which requires in-situ measurements".

P5326, l8: replace "a different" by "another". L15: delete volumetric flux unit.

P5329, l13: insert "of" between "factor" and "2"; l25: delete "and displayed"

P5330: add primes to "C" or "Cm" in covariances.

P5344, Fig.4: in x-axis, replace “Dez” with “Dec”.

P5346, Fig.6: Include bold lines in caption/legend.

P5348, Fig.8: delete “various” in first line of the caption; change “empiric” to “empirical” in the legend.

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 5313, 2011.