Answer to the second Anonymous Referee

GENERAL COMMENT – We thank the referee for the positive reaction with regard to our manuscript. Also thanks for the valuable suggestions, which helped to improve our paper. We addressed the individual suggestions below (the referee comments are reproduced in italics):

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.

Replay: The introduction will be rewritten to take into account the specific points suggested by the referee.

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.*

Replay: This information will be replaced as suggested.

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.*

Replay: P5318, l3 and the beginning of l4 will be deleted. Some additional information about the carbon content and composition of the soil will be added in the revised manuscript. Nevertheless, as noted already by the referee (see points below) in this paper we do not attempt to directly link soil processes to the atmospheric observations. Therefore, the information on carbon content and soil deposition will be kept limited and be based the site description in Andreae et al. 2002.

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.*

Replay: The flux sign convention will be defined.
Part of section 3.1 will be moved to the methodology part in the revised version of the manuscript. However, the text in this section contains primarily measurement results about precipitation and soil moisture and does not include discussion elements.

Section 3.2 is methodology

This section will be placed immediately after section 2.2, or perhaps will be inserted at the end of section 2.2.

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.

Replay: The referee is correct that an important point of this paper is to establish flux and gradient measurements as important analysis tool in the Amazon region. Given that these are the first flux measurements in the region, we think that a thorough quality check is warranted. We then link the different kinds of measurements to show that they are consistent and come up with a value for the soil strength at this location. This is in the range of previously published values, e.g. Miller et al., or do Carmo et al, who already estimated the global impact of emissions of this magnitude. Rather than repeating similar calculations again in our manuscript, we will refer to the results from these papers in the discussion.

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.*

Replay: This will be rephrased and presented a bit more cautiously for the final version of the manuscript, e.g. that the observed gradients are conceptually in agreement with a soil at or near the surface.

P5326, l1-2: *the statement that the nighttime fluxes are identical to the afternoon fluxes is too strong*

Replay: Also this part will be rephrased for the final version of the paper.

l12-13: *"we use this for a rough estimate". Use what? the flushing rate, the height, general characteristics?*

Replay: This will be replaced by… "we use these flushing rates for a rough estimate…"

l18: *how do you know the magnitude is correct? The reference can be wrong as well. In addition, this phrase suggests a precise estimate.*

Replay: We already tried to phrase this cautiously. We do not state that it proves that our flux measurements are correct, but that it "provides independent support that the nighttime methane fluxes are positive and have the correct magnitude". In our opinion, it is important to examine whether the different types of measurements (gradient and flux in this case) are at least consistent, and we show that this is the case. Of course there are several assumptions involved here, which we tried to state clearly (e.g. that the BL heights from Rondonia may be different than at Manaus). In the revised version, we will again indicate with the final numbers that these are rough estimates which show the consistency.

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.*

Replay: This will be rephrased (stating that this assumes the flux at the top of the boundary layer to be zero).

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).*

Replay: Will be changed.

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.*

Replay: Will be rewritten with the assumptions mentioned above, but we don't think that it should be moved to the discussion. It is one of the main conclusions of the paper. Although direct measurements from soil are required for direct support, our results positively suggest that we have emission of CH_4 close to the ground level.

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

Replay: This information will be added.

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.*

Replay: Will be rephrased

P5331, l25: *the spectra are no proof of the high quality, just an indication*

Replay: Will be rewritten as "….This result gives support to the quality of our data…"

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.

Replay: This will be included.

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.*

Replay: All technical correction will be replaced.