

Response to Reviewers – Fest et al., 2016 bg-2016-181

Reviewer comments appear as normal text

Our responses appear in bold and italicised

Reviewer 1

Fest et al. aimed to understand CH₄ dynamics in two Eucalyptus forests in Australia with different precipitation regimes. Fluxes of CH₄ were measured in a high temporal resolution with six replications at each site. In addition, soil temperature and moisture, and inorganic N levels were measured. The data were analyzed using linear regression for casual correlation to explore which factors controlled CH₄ dynamics. Fest et al. concluded that soil moisture regime could explain over 90% of the variability of CH₄ dynamics.

I believe the strengths of this study is 1) very high temporal resolution in CH₄ measurements and 2) air-filled porosity explained CH₄ dynamics in almost the same manner for the two study sites. These are novel, and deserve publication.

We wish to thank the reviewer for a very thorough and constructive review. We agree with most of the suggestions and believe they greatly enhanced the quality of the paper.

However, the current manuscript is no more than a draft. The weaknesses includes 1) statistical analyses, 2) discussion are underdeveloped, and 3) it's not well written.

1) The data should be analyzed using a maximum likelihood framework with AIC or BIC to compare regressions and determine the importance of temperature.

We agree with the reviewer and have reanalysed the data as suggested by the reviewer: We used a maximum likelihood framework to arrive at the AICs for 3 different models (one model containing only soil temperature, one model containing only a measure of soil moisture (we choose AFP) and one model containing soil temperature and AFP as a predictor. The results of this analysis are displayed in Table R1:

Table R1: parameters and coefficients of determination (Adj. R^2) of selected linear models in combination with results of a restricted maximum likelihood analysis (REML) explaining seasonal variability in mean chamber cycle methane flux (F_{CH4}) at a mixed *Eucalyptus obliqua* forest stand, Wombat State Forest, Victoria (AU-WOM) and at a mixed *E. obliqua* and *E. regnans* forest stand, Warra LTER between, Tasmania, Australia (AU-WRR). Predictors: T_S (soil temperature) and AFP (air-filled porosity). REML results: Akaike information criterion (AIC); Estimate of importance for models containing both predictors (in parentheses).

Site	Dependent Variable	Constant (Intercept)	AFP (slope)	T_S (slope)	AIC	Adj. R^2
AU-WRR	F_{CH4}	53.640	-195.378	-	5666	0.855
	F_{CH4}	-19.543	-	-2.215	9657	0.158
	F_{CH4}	55.587	-193.284 (0.997)	-0.254 (0.003)	5629	0.857
AU-WOM	F_{CH4}	53.943	-195.768	-	7648	0.915
	F_{CH4}	-6.320	-	-1.701	13088	0.209
	F_{CH4}	54.766	-201.671 (0.998)	0.147 (0.002)	7617	0.900

The REML and AIC results confirm the interpretation of the original linear regression approach showing that soil moisture (in this case expressed as AFP) is the strongest predictor of soil CH_4 flux in both forest systems. The analysis also shows that the models including soil moisture and soil temperature perform marginally better based on AIC compared to models including only soil moisture to predict soil CH_4 flux. However, the importance rating of the predictors (soil moisture and soil temperature) clearly indicates that in both forest systems soil moisture dominates accounting more than 99% of the proportion of variance explained by the model compared to <0.01% proportion of the variance explained by soil temperature. This reconfirms our initial assessment of the datasets where we stated that including temperature as a variable improved the correlation with methane uptake to a small degree, but it did not improve the predictive capacity. However, it will be important for readers to understand that and we propose to include the AIC statistics in the revised manuscript.

2) Discussion should emphasize the novelty of this study.

We have revised and restructured the discussion to emphasize the novelty of the study

3) I found so many typographical errors throughout the manuscript.

We apologise for the large number of typographical errors Reviewer 1 discovered and we have endeavoured to correct all to greatly improve the presentation, writing and communication.

Abstract

P1L13-14. Add “under predicted climate change scenarios” to the sentence.

This has been added

P1L26. Replace “air-filled porosity” with “AFP” as the abbreviation appears in P1L21.

This has been replaced

P1L23-25. I disagree with this statement after reading the results and discussion. Activity of MOB was not quantified in this study, and the results cannot indicate MOB activities were similar between the two sites.

We agree and have rephrased this sentence to now read as:

“Our data suggest that soil MOB activity in the two forests was similar and that differences in soil CH₄ exchange between the two forests were related to differences in soil moisture and thereby soil gas diffusivity.”

P1L24. Check “physiochemical” for definition. It can be a typo for “physicochemical”.

This word has been removed as a consequence of the rephrasing of the sentence above.

P1L24. Here, the differences between the two sites in CH₄ flux were due to “physiochemical” but AFP explained up to 90% of the variability, indicating that the differences were likely caused by moisture regime.

Soil moisture is a physicochemical difference; it is not a biological difference. However, we have agreed to change the sentence and remove the reference to physicochemical and the sentence now reads as:

“Our data suggest that soil MOB activity in the two forests was similar and that differences in soil CH₄ exchange between the two forests were related to differences in soil moisture and thereby soil gas diffusivity.”

Introduction

P2L17. Here “air filled” is not hyphenated. Be consistent throughout the manuscript.

We have standardised this word to appear as “air-filled” throughout.

P2L33-34. This statement needs citation.

This statement has citations.

P2L35-P3L1. There are many ecosystems in the Northern Hemisphere without snow or below zero soil temperature, comparable to the Australian forests (e.g., Southwest of USA, Mediterranean region).

Reviewer 1 is correct in that there are bioregions in North America that have similar temperature ranges as Australian temperate forest soils. However, this statement is a follow up ‘Furthermore,’ statement to the primary one, which states that Australian temperate forest soils are highly weathered and very low in nutrients. No change has been made.

Results

P7L23. Replace “around” with “approximately”

Agreed, we have replaced.

P7L23. Fig. 4 should not appear before Fig. 3 (P8L1) in the text.

Agreed, we have changed the numbering of these figures.

P7L23. The text mentions 85% (0.85), but Table 1 has 0.896. Why are they different? It’s also the case for 19% in the text, and 0.148 in the table.

Table 1 has 0.855 for VWC and we rounded this to 85%. However the 19% was misquoted and we have corrected this to 16% as represented by and R2 of 0.158 in table 1 for soil temperature.

P7. This is not the best way to analyze the data. Use a model selection approach such as Akaike Information Criterion. For instance, see Monteith et al. (2015): Monteith DT, Henrys PA, Evans CD, Malcolm I, Shilland EM, Pereira M (2015) Spatial controls on dissolved organic carbon in upland waters inferred from a simple statistical model. Biogeochemistry 123(3): 363-377

We agree and have re-analysed the data as outlined above. Based on the AIC for each site a model using soil moisture and soil temperature performs marginally better compared to a model using soil moisture alone to predict soil CH₄ uptake (see above). However, the additional amount of CH₄ variance explained by including soil temperature into the model as compared to a model only including soil moisture is less than 1% at both sites. Including temperature in addition to moisture may on a statistically basis improve the model accuracy but not the predictive capacity. Hence, our overall conclusion is still valid. However, we agree that the AIC is the better way of selecting the best model.

P7L30. Avoid starting a sentence using an abbreviation. Spell out AFP.

Agreed – we have changed this throughout the manuscript.

P8L1. There are AU-Wom and AU-WOM, and AU-Wrr and AU-WRR. Stick to one form.

Agreed, we have now standardised these acronyms to all appear in CAPITALS

P8L20-21. This should be SD, not SE. The large sample size (how many?) makes the SE so small and misleading.

We have changed these to Standard Deviations rather than Standard Errors.

P8L7-9. Awkward sentence, and I cannot find “inter annual” differences were displayed well in figures.

Agreed, this sentence has been deleted. It effectively replicated, in a confusing way, the statement in the previous sentence and was therefore redundant.

P8L9. VWC in the text, but soil moisture in the figure. Be consistent.

We have standardised this to volumetric water content (VWC) throughout the manuscript where appropriate. We use soil moisture as a general term that stands for the different ways soil moisture can be expressed (AFP, VWC and WFPS).

P8L10. Fig. 4a in the text, and Fig. 4A in the figure. Be consistent.

We have standardised reference to figures with upper-case e.g. “Fig. 4A”

P8L28-P9L7. What is the point of presenting daily CH₄ flux in relation to soil environmental variables, if it is not better than that in finer time scales, and does not add much?

We disagree with the reviewer since a lot of available flux studies only ever cite daily flux values and the relationships of daily flux values with environmental variables. We believe that the inclusions of this information in the manuscript will especially be of interest to modellers and will help to put our data in the context of CH₄ flux studies globally.

P9L5-8. Integrate this section to the first paragraph of the Results.

Given that the annual site CH₄ flux budgets are calculated based on the daily flux data presented in the two preceding paragraphs of the result section, we believe it is more logical to leave this section where it is.

Discussion

P9L10. 1-2 years? I thought the measurements were for two years. Spell out numbers smaller than nine.

We agree this is confusing. We have changed this to read “>12 months”

Start the discussion on the most exciting findings. I believe the significant correlation between AFP and CH₄ flux for the two sites is most interesting in this study. Comparing the daily CH₄ flux values with past studies is not too exciting.

Agreed. We have rearranged the Discussion sections to open with the discussion of AFP and CH₄ flux.

P9L25-31. Delete the paragraph. I do not think the statement is true that cool wet temperate eucalypt forests are often compared to rainforests. It’s interesting that the annual CH₄ flux is comparable between the eucalypt forest and a tropical rainforest, but no more than that. Plus, net CH₄ flux is

determined by not only MOB activities, but also methanogens as well, especially in wet sites. Thus, there is not much point for the comparison.

We agree with the reviewer and have removed this section from the discussion

P10L1-4. This paragraph should be presented first in the discussion.

We agree and now open the Discussion with this paragraph

P10L10-11. Check the order of the citations.

We have checked that our citation style is consistent with the BGS style format throughout the manuscript

P10L13-15. Is this an assumption? Delete (i.e. atmospheric CH₄ concentration) and add “between soil and atmosphere” after “the concentration gradient” (L14).

We have made this suggested change

P10L21-L23. The coefficient of determination for the relationship between WFPS and CH₄ uptake is mentioned in the text, but not shown in Table 1 or 2. The relationship is shown in Fig. 4D, but the coefficient is not shown. If it's discussed in the text, it should be shown somewhere.

Agreed. We have added coefficients of determination to Figure 4

P10L28-29. Delete the sentence, and cite Farquharson and Baldock (2008) for the previous sentence.

Agreed, we deleted the last sentence of the paragraph and placed the citation after the previous sentence.

P10L35-P11L2. This sentence needs to be integrated into the context, otherwise it does not make sense. The paragraph is discussion about temperature on CH₄ flux. Then, out of the blue, the sentence on CV of CH₄ flux appears without relating it to temperature. It's confusing. I am not quite convinced that temperature did not affect CH₄ flux with the current analyses. The better way to test the temperature effect is that 1) construct two models (CH₄ flux is a function of moisture, and moisture + temperature) and 2) compare the two models via AIC. This will provide a more concrete answer.

In response to the reviewer's suggestion we have removed the sentence about the CV from the paragraph. In response to the second part of the reviewers comment please see our responses above. We have performed the suggested analysis with the result that the AIC indicates that at both sites models including soil temperature and soil moisture perform marginally better as compared to models only including soil moisture to explain CH₄ flux variability.

P11L2. Replace “will” with “would”.

Agreed, this change has been made

P11L9-14. I do not think the statement is valid. First, the authors measured soil moisture only to 10 cm in depth, and did not measure soil moisture in deeper soils. Methanotrophs in deeper soils can contribute to CH₄ oxidation if the surface soils are dry. The only way to tease out methanotroph activity from physical constraints of soils for CH₄ diffusion is to measure CH₄ flux as well as gas diffusivity (see von Fischer et al. 2009). von Fischer JC, Butters G, Duchateau PC, Thelwell RJ, Siller R (2009) In situ measures of methanotroph activity in upland soils: A reaction~AR~ diffusion model

and field observation of water stress. Journal of Geophysical Research: Biogeosciences (2005–2012) 114(G1):

We have removed the strong statement that our data clearly demonstrate that there was no moisture limitation of MOB activity at the beginning of this paragraph and now simply make a statement that we didn't see any indication that soil CH₄ uptake was moisture limited in our data. This paragraph it now reads:

“Furthermore, our data also show that soil CH₄ uptake still continued at a very low WFPS of 10% (VWC = 0.07 g cm⁻³, AFP = 0.59 cm³ cm⁻³) with CH₄ uptake ranging between -62 to - 80 μg CH₄ m² h⁻¹ at this time. We can therefore hypothesize that MOB activity was not severely limited by moisture at the AU-WOM and the AU-WRR sites during the measurement period.”

However, we also want to point out that the suggested test following the method described by von Fischer 2009 can not necessarily provide the information needed to assess if there is in-situ moisture limitation of methanotrophic activity because this method treats the soil as a one layer and provides a bulk methanotrophic activity measurement and a bulk soil diffusivity measurement without any information on where along the soil profile methanotrophic activity happens at any given point in time. Which means that if as a result of increased diffusivity a larger area of the profile compensates for lower MOB activity in the top soil layer (caused by moisture limitation) it will not show up.

P11L17. Why is “air filled porosity” used here, instead of AFP? Be consistent throughout the manuscript.

Agreed, we have changed to AFP

P11L17. Replace “same” with “almost identical” (they are not “same” based on Table 1).

Agreed, this change has been made

P11L18-19. I disagree with the statement. It is possible that AFP governs the CH₄ flux across the landscape for eucalypt forests, but there is also a possibility that the casual correlations between AFP and CH₄ flux happened to be very similar for the just two study sites. It's not reasonable to extrapolate the results to all the same type of forests in Australia.

Agreed – this statement was an over-reaching. We have reworded this to now read as:

“This means that future research should investigate whether simple information about soil bulk density can be used to estimate CH₄ uptake across different eucalypt forest ecosystems in Australia, or in other similar ecosystems globally.”

Tables

P16L5. “S” in “TS” should be subscript.

Agreed, we have made the subscript change in the Table caption.

Is "-" missing for 195.768 for the AFP parameter at AU-Wom?

Yes this has been corrected

“Soil water content” is used in the caption. Is this the same as “soil moisture content” (e.g. P30L6)? If so, use only soil water content.

We are using soil moisture content in the manuscript as a general term that represents the three different ways soil moisture content can be expressed (AFP, VWC and WFPS). We believe this is clear and not incorrect.

P17. Table 1. Are “constants” intercepts? Are “parameters” slopes for predictor variables?

Yes, to clarify this we have added a sentence to the caption.

Are both “unstandardized and standardized coefficients” in parentheses?

We have clarified this in the caption only standardized coefficients are in parentheses

Table 1 shows results of four regressions: 1. VWC 2. Soil temp 3. VWC and soil temp 4. AFP

But the corresponding Fig. 4. has: 1. VWC 2. AFP 3. Soil temp 4. WFPS Why the inconsistency?

The reasoning behind displaying WFPS in to CH₄ flux relationship in Figure 4 was that we wanted to show that if WFPS is used as a measure of soil moisture the slopes of the relationships with CH₄ flux are different at each site. We will add WFPS to the tables 1 and 2

Figures

18. I am not sure if the data are presented in the most effective manner in Fig. 1 and 2. The current figures have; A: CH₄ flux B: Air temp, CV of CH₄ flux, and precip C: Soil temp and soil moisture Is there rationale behind the combinations? I am not convinced that the arrangement makes sense. How about rearrange the combinations; A: CH₄ flux and CV B: Precip and soil moisture C. Air and soil temp

Or A: Ch₄ flux B: Precip, soil moisture and CV C. Air and soil temp

We arranged the figures this way because the CV% of the CH₄ fluxes would convolute figure A to a point where CH₄ flux and CV% cannot be separated visually. We decided to leave the figure presentation as it stands.

P18. Fig. 1. AU-Wrr and AU-Wom are Fig. 1A and 1B, respectively, but “A” and “B” letters in the parentheses do not match up. Are these typo?

Yes, we have corrected this typographical error in the Figure caption

Replace SE with standard deviations. SE partly depends on the sample size, which is not described in the text, thus the tight error bars can be misleading.

Agreed, we have changed to SE to SD in Fig 1A and 1B

P19. Fig. 2. The description is confusing. Are the individual symbols means of measurements over four hours for each chamber, or average of 10 chambers? Spell out "four" instead of "4".

Agreed – this can be interpreted several ways. We have rewritten this line in the Figure caption to now read as:

“Soil-based flux of CH₄ at a mixed Eucalyptus obliqua and E. regnans forest stand. Warra LTER, Tasmania, Australia (AU-WRR). Panel A shows CH₄ flux cycle means of ten chambers measured within a four hour time period, panel B shows in black closed symbols site air temperature averaged over the chamber cycle period, daily rainfall sums (bars) and coefficient of variance of the CH₄ flux cycle mean shown in Panel A (grey closed symbols). Panel C shows soil temperature in the top 0-10 cm averaged over each chamber cycle (grey open symbols) and corresponding volumetric soil water content (grey closed symbols) at the site

Replace "moisture" with "water".

In the text (P7 L20), it seems like CV was calculated using average and SD of 10 chambers, but it seems like CV was calculated using average and SD over time for each chamber.

We agree that this was unclear. CVs are calculated using average and SD of 10 chambers in one measurement period. We have changed the caption to:

“Soil-based flux of CH₄ at a mixed Eucalyptus obliqua and E. regnans forest stand. Warra LTER, Tasmania, Australia (AU-WRR). Panel A shows CH₄ flux cycle means of ten chambers measured within a four hour time period, panel B shows in black closed symbols site air temperature averaged over the chamber cycle period, daily rainfall sums (bars) and coefficient of variance of the CH₄ flux cycle mean shown in Panel A (grey closed symbols). Panel C shows soil temperature in the top 0-10 cm averaged over each chamber cycle (grey open symbols) and corresponding volumetric soil water content (grey closed symbols) at the site.

P21. Fig. 4. Add regression equations and R² values on the figures.”

Agreed, the Figure has been annotated with regression equations and R².

P22. Fig. 5. I do not think this is the best way to present the data. In the text, the authors want to show there is no significant correlation between CH₄ flux and inorganic N contents. Then, scatter plots should be used to show the data.

We disagree, our intention was to give the reader an idea of the temporal variability in soil nitrate and ammonium concentrations, what can clearly only be achieved with a figure that can accommodate a timeline. The R² and P values of the regressions between nitrate and ammonium concentrations and CH₄ flux are listed in the caption of the figure. We believe that it is quite clear from the figures that there is no significant relationship between these parameters.

Reviewer 2

The authors investigated the soil methane exchange at two Australian forest study sites differing in annual precipitation. Their major finding is that soil moisture is the main controlling factor and that the relationships of the two sites collapse if air-filled soil porosity, instead of water-filled pore space or volumetric soil moisture is used. The paper is of interest to the readers of BG and the main finding is of general interest to the community as it may trigger new approaches of simulating soil methane exchange if verified across a larger number of sites.

I have three major comments:

- (1) The study uses two different measurement systems at the two study sites. How can the authors ascertain that the two measurement systems do not cause systematic differences between the two sites? Without a cross-comparison between the two systems at the same site, how can we believe the differences/ lack of differences between sites when normalised with AFP?

We agree with the reviewer that this is an important point to consider. In our previous research over the last 15 years we have used different measurement systems in many ecosystems in Australia and some of them in the same ecosystem. When using closed-static and closed-dynamic systems in the same ecosystem we never detected large differences in the CH₄ flux magnitude. However, we have not been able to test the two automated systems in parallel at the same site. In fact such a comparison has not been conducted anywhere in the literature as far as we are aware. There have been many studies on chamber design and comparisons between automated and manual systems, mainly for CO₂ and some for N₂O. But a systematic evaluation of an automated closed-static and an automated closed dynamic system for CH₄ flux has not been performed. Furthermore there are examples in the literature where data from different manual chamber systems (dynamic and static) and different automated chamber systems (static, dynamic, different analysers) were used a) to compare CH₄ flux magnitudes and b) to derive general functional relationships between soil temperature and moisture and soil CH₄ fluxes across multiple ecosystems biomes and continents (Curry, 2009, 2007; Dalal and Allen, 2008; Dalal et al., 2008; Del Grosso et al., 2000; Smith et al., 2000). We do not see how our approach is any different. Hence, we acknowledge that it is possible that the two measurement systems could have measured different magnitude of CH₄ flux in the two ecosystems and that by chance the relationship between CH₄ flux and AFP is identical at the two sites. However, we observed a very strong linear relationship between CH₄ flux and AFP at each site and AFP was able to predict around 90% of the flux variation. This is true regardless of the slope of the relationship. So the only difference that the measurement magnitude could make is a different slope of the relationship. Hence, we propose to include a qualifying statement in the Discussion that highlights this possibility.

Curry, C. L.: The consumption of atmospheric methane by soil in a simulated future climate, *Biogeosciences*, 6, 2355-2367, 2009.

Curry, C. L.: Modeling the soil consumption of atmospheric methane at the global scale, *Global Biogeochemical Cycles*, 21, 2007.

Dalal, R. C. and Allen, D. E.: Greenhouse gas fluxes from natural ecosystems, *Australian Journal of Botany*, 56, 369-407, 2008.

Dalal, R. C., Allen, D. E., Livesley, S. J., and Richards, G.: Magnitude and biophysical regulators of methane emission and consumption in the Australian agricultural, forest, and submerged landscapes: a review, *Plant and Soil*, 309, 43-76, 2008.

Del Grosso, S. J., Parton, W. J., Mosier, A. R., Ojima, D. S., Potter, C. S., Borken, W., Brumme, R., Butterbach-Bahl, K., Crill, P. M., Dobbie, K., and Smith, K. A.: General CH₄ oxidation model and comparisons of CH₄ oxidation in natural and managed systems, *Global Biogeochemical Cycles*, 14, 999-1019, 2000.

Smith, K. A., Dobbie, K. E., Ball, B. C., Bakken, L. R., Sitaula, B. K., Hansen, S., Brumme, R., Borken, W., Christensen, S., Prieme, A., Fowler, D., Macdonald, J. A., Skiba, U., Klemmedtsson, L., Kasimir-Klemmedtsson, A., Degorska, A., and Orlanski, P.: Oxidation of atmospheric methane in Northern European soils, comparison with other ecosystems, and uncertainties in the global terrestrial sink, *Global Change Biology*, 6, 791-803, 2000.

(2) The results of the two sites should be presented together instead of separately for each site.

We disagree with this suggestion as it is important not to suggest to the readers that these sites can be directly compared. They are independent sites that simply show similar strength of relationship between AFP and CH₄ flux. Furthermore, they span difference time periods, experience difference rainfall conditions which would make the figures far more confusing and with less visual resolution.

(3) English style and grammar are in the need of checking by a native speaker.

Agreed. We have extensively edited the manuscript to improve the presentation communication and grammar. Comments from Reviewer 1 greatly assisted in this.

Detailed comments:

p. 1, l. 13-14: this sentence comes a bit as a surprise

We have added following sentence ahead of this sentence:

“Soils in temperate forest ecosystems are the greatest terrestrial CH₄ sink globally.”

p. 2, l. 2: high compared to many VOC that are present in the ppt range ...

Reviewer 2 is correct. We have changed this sentence to now read as:

“Methane (CH₄) has an atmospheric concentration of ~1.8 ppm as compared to >400 ppm for carbon dioxide (CO₂) the second most.....”

p. 2, l. 24: Q10 values critically depend on the depth of the soil temperature used as a reference due to increasing dampening in amplitude and phase shift with soil depth

Reviewer 2 is correct. However, the statement still holds as we only build upon and refer to the published literature.

p. 2, l. 28: initiate a new paragraph here

Agreed, we have added a paragraph start.

p. 2, l. 15-17: would the authors be able to formulate some hypothesis regarding their research? this would strengthen the paper

Our study was objective driven and we do not believe it would be correct to retrospectively add hypothesis to the introduction that would be based on the outcome of the study itself. The dataset is very strong and we firmly believe that the paper as presented is a very valuable contribution to this field of research.

p. 3, l. 28: density

Yes - we corrected this

p. 7, l. 8: did you check for linearity of tested relationships?

Yes, linearity was critical acceptance of chamber flux data. We used an R^2 threshold of 0.9 for our quality control.

p. 7, l. 17: how many longer gaps did you encounter at both sites?

The figures clearly show when we encountered long data gaps at each site. However, we will add a paragraph to the method section outlining the instrument failure related data gap percentage.

p. 7, l. 29-30: figures should be reference in chronological order, i.e. Figure 3 after Figure 2

Agreed, we have changed the order of the Figures.

Results section: the paper would be much more easily readable, if the results of the two sites would be presented together, instead separately – this would help making a stronger point of the major finding of this study; this argument also applies to Figures 2 and 3, which should be combined in my view

We disagree that combining the results of the two sites would greatly assist the reader in understanding the data. We believe that it is important for the reader to clearly see that the relationship between environmental variables and CH_4 flux is identical at each site – not to compare between the two sites. We have trialled this in a previous draft and it was less convincing.

p. 10, l. 20: diffusion-limited

We have rephrased this to read as: “limited by diffusion...”

p. 10, l. 22: able to demonstrate p.

We rephrased this sentence to:

“This agrees with the theory that soil CH_4 uptake is mainly limited by diffusion in most forest ecosystems (Price et al., 2004) when the sites of microbial CH_4 oxidation are distributed through the surface soil (Stiehl-Braun et al., 2011), and the concentration gradient between soil and atmosphere, which drives the flux, is effectively constant (von Fischer and Hedin, 2007).”

10, l. 31-33: reformulate in proper English

This sentence now reads:

This is most likely due to the fact that WFPS is a proportional measure that relates VWC to the total soil porosity (equation (4)); compared to AFP that is a direct expression of the air filled pore volume in a given soil (equation (5)).

p. 11, l. 3: I do not get the “However”

Agreed. We have deleted “However” at the start of this sentence.

p. 11, l. 11: what does “defined role” mean?

We have reworded this sentence to now read as:

“However, the weak temperature dependency of soil CH₄ uptake is unlikely to greatly influence seasonal variability given that a....”