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Interactive comment on "Topography-based modelling reveals high spatial variability and seasonal emission patches in forest floor methane flux" *by* Elisa Vainio et al.

Elisa Vainio et al.

elisa.vainio@helsinki.fi

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Here we answer all the comments by the Referee #2 in detail.

RC: There are two larger general comments: 1) Concerning the upscaling method:

AC: We re-evaluated our modelling approach based on these Referee comments, and will change the modelling to follow the suggestion by Referee #1: we model the CH4 fluxes directly with the RF technique and using soil moisture as a driver in the model (see Fig.1 in this document).

RC: On top of this, there is never a discussion of why the approach of modeling soil

moisture and then CH4 flux is advantageous. Furthermore, the authors dedicate a large portion of this manuscript to the upscaling exercise, but barely, if at all, discuss whole plot scale fluxes. It would be interesting to hear how much the estimated net CH4 sources offset the plot level sink between the two time periods, and how uncertain their plot level fluxes are. After all, the primary purpose of upscaling is not to accurately predict CH4 flux at every individual point, it is to enhance our predictive capability of large-scale CH4 exchange in a way that reflects soil heterogeneity.

AC: See the previous response, we changed the modelling approach.

We agree with the referee that the primary purpose of upscaling is to get an accurate estimate of landscape fluxes and not at individual points. We will revise the text so that this message gets across to the reader clearly, and add discussion on the whole area flux and the modelling, decreasing the proportion of some smaller details in the discussion. This was actually one of the main reasons why the analysis on chamber location bias was done (Fig. 10). Typically, mean of CH4 fluxes observed at a handful of chamber locations is reported and considered as representative of ecosystem CH4 exchange, however, this neglects any bias stemming from non-representative sampling locations. By using mean upscaled CH4 flux as a reference, we were able to show that 15-20 randomly selected chamber measurement locations (out of 60 locations available) were able to produce as accurate estimate of the landscape CH4 flux as averaging over all the chamber data. This information should prove useful when designing future chamber measurement campaigns in similar locations aiming to achieve accurate landscape-level flux estimates without upscaling with e.g. machine learning techniques.

RC: It would be more useful to randomly subsample the flux observations and build soil moisture-CH4 relationships from the random subsets. Then see how upscaled fluxes based on these relationships compare to the predictions made using the whole dataset.

AC: We acknowledge that the wording on lines 422-425 in the manuscript needs clarification, and this has maybe caused misunderstanding. The idea of the analysis shown in Fig. 10 in the manuscript was to evaluate how many chamber measurement locations were needed to get an accurate estimate of landscape-level flux by only averaging over the measured chamber data without any upscaling with RF. Here the mean upscaled CH4 flux was used as a reference since it accounts for the soil heterogeneity (see above). It was shown that average over 15-20 locations resulted in a similar bias as average over all the chamber measurement locations. This should be useful information for future chamber measurements in similar locations. We will revise the text so that this analysis is clearer and describe the methodology briefly also in Materials and Methods (Sect. 2).

RC: The authors should also report their modeled CH4 predicted fluxes for pixels corresponding to the sample sites, which would help explain whether differences in upscaled fluxes are caused by a model bias or because of the heterogeneity of the predictor variable domain.

AC: Thank you for this comment, we will evaluate this.

Specific Comments

RC: Abstract: If possible, add some descriptive statistics (i.e. mean, min, max) for what CH4 fluxes were observed in each season.

AC: Will be added.

RC: 21: The wording "as well as on the related ground vegetation" is confusing to me.

AC: Spelling mistake, will be corrected "as well as from the related ground vegetation".

RC: 33-35: I do not believe that this is the current paradigm. Observed methane fluxes are the net sum of both opposing processes occurring in the soil.

AC: While it is the prevailing paradigm that the availability of oxygen mainly controls

these processes in nature, and thus in general upland soils are no favourable place for CH4 production, there are possibilities for CH4 production taking place in these soils too, as we state in the next paragraph. The recent study of Treat et al. (2018) reported that during non-growing season upland soils can be a significant source of methane although they are generally considered as sinks during the growing season.

RC: 40: This is not an instant effect, however, which has implications on the influence of both total soil moisture and its temporal variability. It would be useful to note this here.

AC: We agree that the soils do not turn immediately sources of CH4 after inundation, but there are time lags between these processes. We will add this to the manuscript: "However, there are likely notable time lags between the start of inundation and methanogenesis, complicating the analyses of dependencies between these processes.".

RC: 53-56: I think this is a great point, but I am confused why it is in this paragraph.'

AC: Thank you for this constructive comment but our impression is that this is logically introduced as it is.

RC: 57-62: This paragraph is important but it could be written more clearly. Are the authors trying to say that we often consider ecosystem fluxes in large-scale models but have not adequately accounted for heterogeneous sources/sinks within the ecosystem (which likely respond to environmental changes differently)?

AC: Yes, we are trying to say that the sources and sinks within the ecosystems are not adequately known or accounted. We will add a sentence in this paragraph, clarifying this.

Methods:

RC: 152: Is there an explanation for why one measurement would be so large?

AC: The sample point was located on small water pond, where the water table level was most of the time above the peat surface. The CH4 emissions from this sample point were at the same level with typical peatland emissions. One possible explanation for the highest emission would be ebullition.

RC: 177: It would be good to provide a +/- range for what types of temperature variation were observed here as justification.

AC: We will add these.

RC: 220: Was the TWI then resampled/interpolated or left at coarse resolution? Additionally, I would hesitate to say that the TWI is "not accurate" at fine scales since it is simply a statistical metric and not a measurement of anything. It does, however, have limited application for estimating soil moisture on very high resolution DEMs because the metric itself is very sensitive to surface microtopography and noise.

AC: Yes, TWI was then interpolated with bilinear interpolation to the finer grid (5x5). We agree, "not accurate" is not correct wording in this context. We will modify the text accordingly.

RC: 225-242: I believe this paragraph could be rewritten to be better related to this study. A lot of the information on the inner workings of the RF algorithm can be condensed with an appropriate citation and a brief note on the advantages/disadvantages of RF over simpler techniques like multiple regressions. More information on why the model parameters (ntree = 300, mtry = 2) and predictor variables were selected would be helpful.

AC: Based on this comment, we will condense the details related to the Random Forest algorithm. However, we believe that some description is needed since the method is not necessarily very familiar to the chamber flux measurement community. Minimum number of observations per tree leaf was set to 2, due to limited amount of data. Small number for this parameter helps when trying to capture dependencies at both

ends of the training data distribution, especially when using limited dataset. Amount of trees in the RF model (ntree) was initially set to 300 based on prior experience with RF and value for this hyperparameter did not significantly influence results. However, the number of variables randomly sampled as candidates at each split (mtry) was not changed from its default value (one third of the total number of variables). The predictor variables were selected based on Spearman's rank correlation coefficient.

Results:

RC: 304-306: Having S.D. values or some indicator of variability in soil moisture besides these means would be helpful here and in other parts of the results section.

AC: We will add the SD values in text with the reported mean values.

RC: 326-327: What was the data of the highest emission outlier? It could be nice to see where it and other CH4 measurements fall on the time series graphs above.

AC: The CH4 highest emission (1080 μmol m-2 h-1) was measured on 5 June 2013. We will add the date of the measurement in the text (line 326). It is unfortunately difficult to present an approachable timeseries plot of the measured CH4 fluxes due to high variation (large emissions vs. other data) – furthermore, we think there is no need to add any more figures to this paper.

RC: 333-334 and Fig 3: This is unclear to me. Is this a temporally static correlation between the mean of all CH4 fluxes at each point and the mean of soil moisture at each point?

AC: Yes.

RC: 339: Is "September" supposed to be "October" here?

AC: Thank you for pointing this out. These were actually calculated for May–Sep, which has been related to some previous version of the manuscript, but it obviously makes more sense to report these for May–October. Thus, we will correct this as follows: “The mean measured CH4 flux in May–October at the site was -4.88 μmol m-2 h-1 (median -6.43 μmol m-2 h-1) in 2013 (n=339), and -6.46 μmol m-2 h-1 (median -5.90 μmol m-2 h-1) in 2014 (n=373), however, the difference was not statistically significant.” Furthermore, we noticed that in line 290 we have reported mean air temperature in May–September for different years, and these will be changed to cover May–October, as well. Mean air temperature for May–Oct in 2010–2017 has been 10.0–13.2 C-degrees, in 2013 12.4 and in 2014 12.7 C-degrees.

RC: Fig 4: It would be nice to break these plots up by May-July and August-October observations.

AC: Thank you for this suggestion, we will replace Fig. 4 with a new one with the two seasons separately (see Fig. 2 in this document), and also modify the text accordingly. We also added information about statistically significant differences between the seasons among each vegetation group (plus signs). Moreover, we noticed that there was a mistake in the figure caption of Fig. 4: the triangles are actually medians, whiskers are 25th and 75th percentiles, and asterisks are means.

RC: 361-367: Again, reporting the only the mean is limiting, also report SD (or some other metric of variability) within these sample groups.

AC: We will add SD values.

RC: Fig 5: Was this variability maintained between the early to late summer transition? It would be good to show both groups on this plot, but might make things too cluttered.

AC: We explored the possibility to plot the seasons separately in two different plots (see Fig. 3 in this document). However, it does not provide much new information compared to the modelled flux maps – mainly the wettest sample points shift from emission to uptake of CH4. Also, the figure splitted to two seasons is not very easy to read, as the Referee expected, and moreover the purpose of this figure is to show the spatial variation at sample points. Hence, we think it is best to keep this figure as it is.

RC: Table 2. It would be very helpful here to report the modeled statistics for both the whole area and at the sample points. Currently it is unclear whether the modeled soil moisture is systematically lower and therefore causing systematic overestimations of CH4 uptake, or if the domain of the entire study area happens to be drier on average leading to a larger estimated CH4 uptake.

AC: We will modify Table 2 as the Referee suggests.

RC: Fig 7: Normalizing the uncertainty at each pixel by its predicted value would help communicate the spatial patterns in the consistency of the RF ensemble output. I would also suggest that the authors add a note on interpreting this uncertainty, which is more of a measurement of the agreement of predictions among multiple RF iterations than the error between predictions and observations like RMSE. I am a major supporter of reporting ensemble uncertainty along with model metrics like RMSE, but the wording can get very confusing!

AC: Thanks for this comment. We will plot the relative uncertainties instead of absolute uncertainties as the Referee suggests, however it is good to note that this will then alter the message conveyed with the figure. We will also modify the text so that it is clear that this figure shows the uncertainty related to the upscaling procedure alone and does not include uncertainties related e.g. to possibly biased training data due to biased sampling locations. For the overall uncertainty, cross-validation metrics such as RMSE are better, like the Referee points out.

RC: 418-421: This is another place where normalizing the uncertainty of the ensemble predictions is useful.

AC: Figure 9 will be modified as the referee suggests, as well as Fig. 7 (see Figs. 4 and 5 in this document), however note that for locations with close to zero fluxes the relative uncertainties will inflate. This is one of the reasons why we opted to plot absolute uncertainties in the first place.

RC: 422-424: I may have missed it, but I do not remember seeing this approach described in the methods and it is kind of unclear here. I am also confused by what this is supposed to demonstrate.

AC: We acknowledge that the wording on lines 422-425 in the manuscript needs clarification, and this has maybe caused misunderstanding. The idea of the analysis shown in Fig. 10 in the manuscript was to evaluate how many chamber measurement locations were needed to get an accurate estimate of landscape-level flux by only averaging over the measured chamber data without any upscaling with RF. Here the mean upscaled CH4 flux was used as a reference since it accounts for the soil heterogeneity (see above). It was shown that average over 15-20 locations resulted in a similar bias as average over all the chamber measurement locations. This should be useful information for future chamber measurements in similar locations. We will revise the text so that this analysis is clearer and describe the methodology briefly also in Materials and Methods (Sect. 2).

Discussion:

RC: 444: This is unclear. The RF model was just used to estimate spatial distributions of soil moisture, which were then used to predict CH4 flux based on a linear model, correct?

AC: Due to the Referee #1 comment we re-evaluated the modelling approach, and opted to follow reviewer suggestion to use RF model also for CH4 fluxes.

RC: 451-454: I am not sure what these lines are doing in this paragraph. They seem disconnected from the point.

AC: We assume that the discussion on the relationship between vegetation and soil moisture suits here, although, we will try to modify the text to connect them somehow better here and make the text more fluent and effective.

RC: 457: How are these two species different in terms of phenology, growth form, and

root structure? If they are similar, I would hesitate to infer that the vegetation is affecting CH4 flux rather than soil properties other than moisture.

AC: This sentence is not based on just two species, but the vegetation classes. This proposition is very moderate suggestion that there might be other effects than soil moisture, something related to vegetation may affect the CH4 flux. We will see if we can add more information on this.

RC: 469-470: Yes, but variability within point clusters was not communicated to the readers. It would be very useful to include.

AC: It is true that there is some variability in the CH4 fluxes within sample point groups (Fig. 5). We will add a comment about that in the discussion.

RC: 470-472: I do not agree with this. The points created the domain of the training data, so we would expect the model output to be constrained by that domain. Additionally, the mean of the data only tells part of the story. It would be much more useful to compare the distributions of prediction values vs. observations.

AC: We partly agree with this comment. RF model cannot predict outside the range of values in the training data and the same applies to our CH4 upscaling procedure. However, we argue that upscaling can fix biases caused by skewed sampling locations (as long as the training data contains at least some data points at both extremes of the pdf) and hence agreement between the mean values is not trivial. We agree also that mean of the data tells only part of the story, however analysis of mean flux is important if accurate landscape-scale CH4 budgets are strived for. Hence, we would like to keep this part of the manuscript as it is, but include also a sentence about the fact that mean tells only part of the story, but comparison of means is important if the target is accurate landscape-scale CH4 budget.

RC: 494-496: This is interesting, what differences in the ecosystems/soil types may account for this?

AC: Unfortunately, we cannot really find a good explanation to this based on the collected data, but think it is relevant to mention.

RC: 503-509: It would be useful to communicate whole plot scale CH4 flux estimates, but net sums and total source and sink strength.

AC: If we understand correctly, the Referee suggests to add discussion about the whole-site mean values presented in Table 2. We agree that the whole-site mean flux and moisture are now getting quite little attention in the discussion, and we will add some discussion on those.

RC: 513-514: This is could also be due to reduced activity of methanogens in deeper soil layers/microsites.

AC: Yes, due to lower soil moisture. At least for the wet areas. We will add this to the same sentence.

RC: 568-571: Yes. But why did this study focus on modeling soil moisture and not directly modeling CH4 flux based on landscape features?

AC: See our answers to the main questions raised.

RC: Conclusions: This section could be filled out more completely. Differences in CH4 flux based on vegetation type was an interesting finding, for example.

AC: Thank you for this comment. We wanted to keep the Conclusion paragraph short. But we can add e.g. the effect of vegetation type here.

References:

Treat, C. C., Bloom, A. A. and Marushchak, M. E.: Nongrowing season methane emissions–a significant component of annual emissions across northern ecosystems, Glob. Chang. Biol., 24(8), 3331–3343, doi:10.1111/gcb.14137, 2018.

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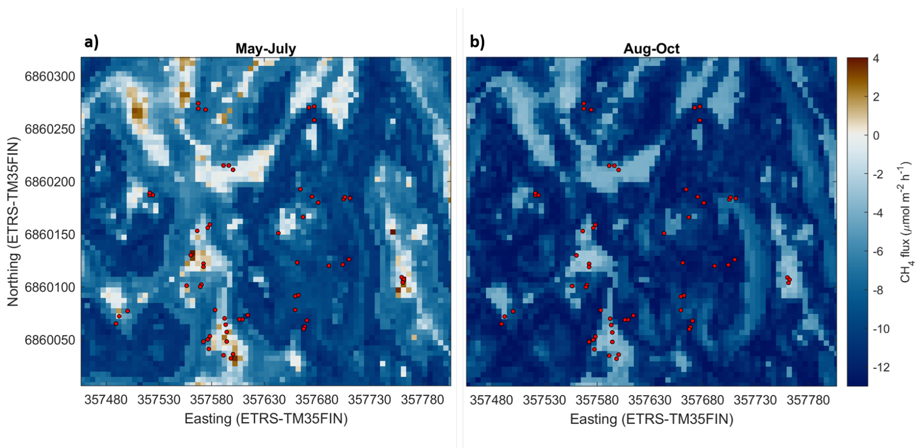

Fig. 1. New Fig. 8: Maps of CH4 Flux with modified modelling approach.

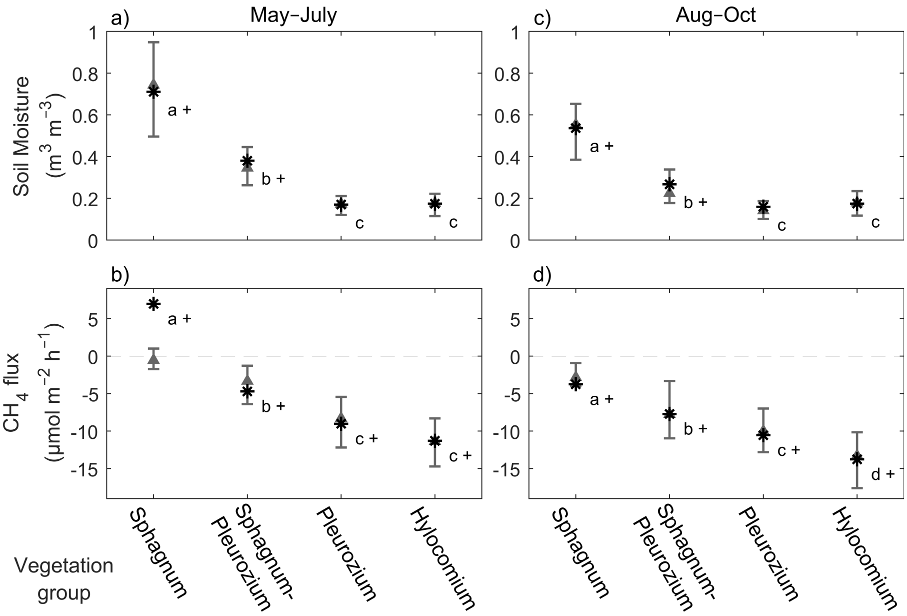

Fig. 2. New Fig. 4: Soil moisture and CH4 Flux of different vegetation groups for two seasons.

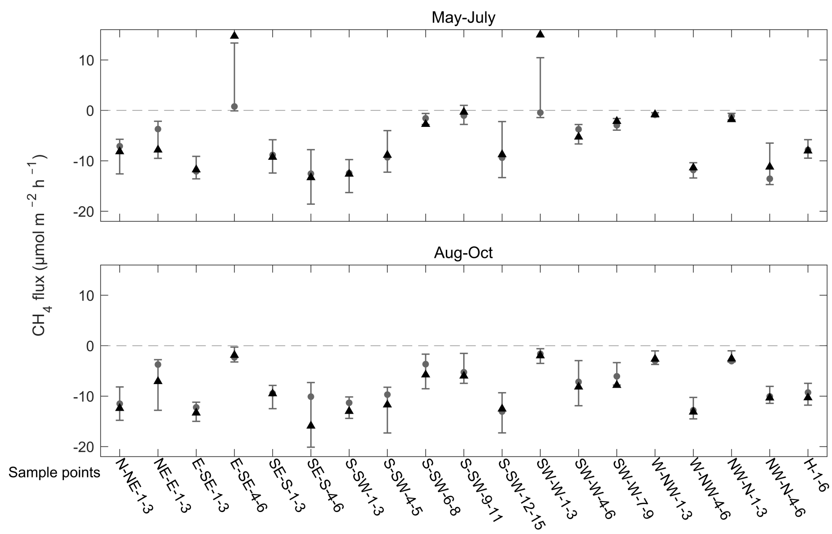

Fig. 3. Fig. 5 splitted to two seasons.

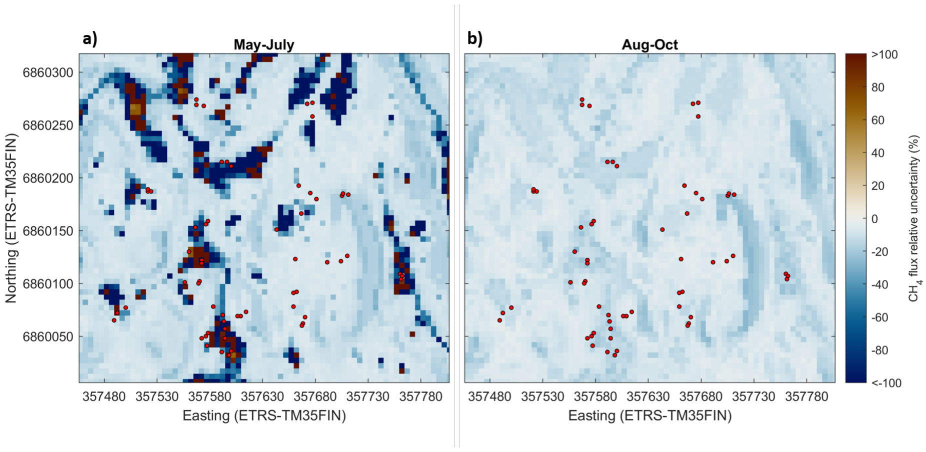

Fig. 4. New Fig. 9: Relative uncertainty of the CH4 flux.

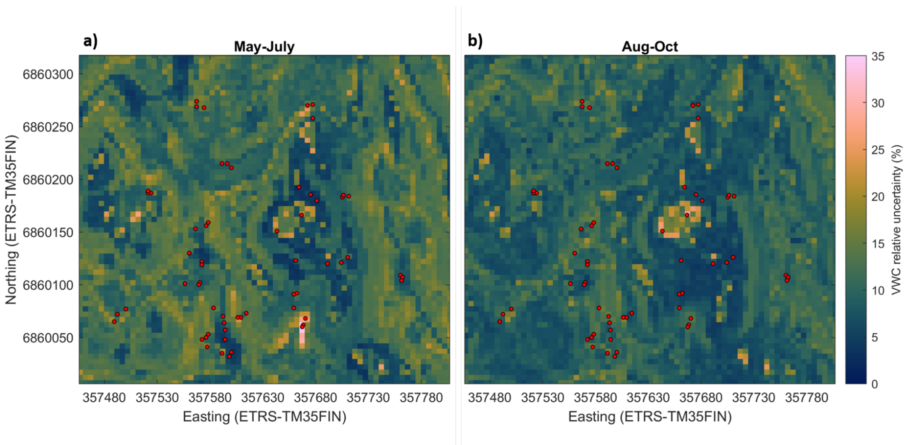

Fig. 5. New Fig. 7: Relative uncertainty of the Soil Moisture.