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Biogeosciences Discussions

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

Anonymous Referee #2

Received and published: 23 September 2020

Recommendation: accept with major revisions

General Comments

This study investigated spatial variability of CH4 fluxes across a hilltop study site in a boreal forest, as well as their relationship to vegetation and soil moisture. The data generated by this study is very useful, as boreal forests are a critical ecosystem in global GHG dynamics. The methods for collecting the field measurements were rigorous and well done. However, 1) I have major issues with the upscaling approach and how it was used within the narrative of the manuscript. Additionally, 2) I am confused by the analysis of how many measurement points are necessary to model plot scale

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fluxes.

1) What is the advantage of first modeling soil moisture and then using modeled soil moisture to model CH_4 flux. Were TWI, slope, DTW, and TRI also evaluated for their relationships to CH_4 flux, and could this be directly modeled without first modeling soil moisture? It seems that uncertainty is being compounded by introducing the uncertainty associated with the soil moisture-CH_4 relationship as well as the RF model uncertainty. I understand that measuring soil moisture is logistically much easier than CH_4, which would be useful for temporal and potentially spatial gap filling, but this study is only looking at average CH_4 flux for two time periods. On top of this, there is never a discussion of why the approach of modeling soil moisture and then CH_4 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 CH_4 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 CH_4 flux at every individual point, it is to enhance our predictive capability of large-scale CH_4 exchange in a way that reflects soil heterogeneity.

2) The number of points analysis is highlighted in the abstract and results and discussion, but it is not mentioned in the methods. Unless I am mistaken, the conclusion is only based on the fact that there are similar means between the predictions and observations at N points. I do not agree with the authors on this and believe that substantially more work would be needed to demonstrate the necessary number of points. It would be more useful to randomly subsample the flux observations and build soil moisture-CH_4 relationships from the random subsets. Then see how upscaled fluxes based on these relationships compare to the predictions made using the whole dataset. Finally, the manuscript struggles to clearly communicate model and equation uncertainty at many points, which I tried to note below. The authors should also report their modeled CH_4 predicted fluxes for pixels corresponding to the sample sites, which would help

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explain whether differences in upscaled fluxes are caused by a model bias or because of the heterogeneity of the predictor variable domain.

Specific Comments

Abstract: If possible, add some descriptive statistics (i.e. mean, min, max) for what CH_4 fluxes were observed in each season.

20: No comma needed after flux.

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

Introduction:

32: Remove word “also”, perhaps provide estimated percentage contributions of each global sink for context.

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.

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.

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

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 heterogenous sources/sinks within the ecosystem (which likely respond to environmental changes differently)?

Methods:

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

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

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.

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.

Results:

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.

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.

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

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

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

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.

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 CH_4 uptake, or if the domain of the entire study area happens to be drier on average leading to a larger estimated CH_4 uptake.

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!

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

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.

Discussion:

444: This is unclear. The RF model was just used to estimate spatial distributions of soil moisture, which were then used to predict CH_4 flux based on a linear model, correct?

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

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 CH_4 flux rather than soil properties other than moisture.

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

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.

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

503-509: It would be useful to communicate whole plot scale CH_4 flux estimates, but net sums and total source and sink strength.

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

565-566: Good point. Not only is it that the heterogeneity is well-represented, the sample set must also account for the relative coverage of landscape features for a mean value to be accurate.

568-571: Yes. But why did this study focus on modeling soil moisture and not directly modeling CH_4 flux based on landscape features?

Conclusions: This section could be filled out more completely. Differences in CH_4 flux based on vegetation type was an interesting finding, for example.

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