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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 #1

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Vainio et al. studied the magnitude, spatiotemporal patterns, and drivers of forest floor CH4 flux. This study provides new insights into the mechanisms driving CH4 fluxes and utilizes a relatively novel methodological framework that focuses on spatial heterogeneity and statistical upscaling, therefore representing a substantial contribution to scientific progress. The manuscript is well written and has clear main aims. However, the Methods section still needs some technical clarifications and the structure of the Discussion could be revised to make sure it is easy to follow.

Here are a key few topics that I think still need to be clarified (more explanations in the line-specific comments):

1. The modeling framework

Line 225: How many observations did you have for May-July and August-October? Did you have many measurements from one point in your model (e.g., early May measurement, late July measurement)? Can you be certain that the soil moisture measurements conducted within one study period (e.g. in early May and late July) can be directly compared and used in the same model, even though the soils tend to get drier during the summer? I think it's important that the measurements that you use for your response variable (i.e., soil moisture) are fully comparable with each other.

225: Could you also describe why you decided to use soil moisture as a predictor of fluxes instead of using the different topographic indices directly? Also, did you consider creating a continuous vegetation type raster based on your vegetation classes and the gridded layers for the study domain? This could have been a useful predictor for CH4 fluxes as well.

256: Why didn't you use the similar framework that you used for soil moisture to predict CH4 fluxes with soil moisture? You could have created a RF (or a GLM/GAM or some other) model with the measured soil moisture as a predictor, and then used that model to predict fluxes across the landscape using the predicted soil moisture. And this could have been repeated over the different bootstrapped soil moisture maps to get CH4 flux uncertainty map as well.

240: Could you add the response graphs (partial dependence plots) describing the relationship of these indices and soil moisture to the Appendix?

2. Description of the model performance

Line 20: Somewhere here I would add a sentence about how the statistical models performed, and how reliable your results are.

377: I would be interested to see a scatterplot of the observed and predicted (CV) fluxes to see how well the model predicts high and low soil moisture values. Same

applies to CH4 flux (line 406).

542: Somewhere in the Discussion you should also discuss how well your upscaling performed compared to previous studies. What are the main uncertainties, and how can these uncertainties be reduced? What predictors are you missing? How about other RS-derived indices, such as NDVI?

Here are a few more minor suggestions to the manuscript:

Title: I would consider adding the word “statistical modelling” somewhere in the title

Line 19: I would say “using digital elevation model-derived topographic indices” instead of “topography”

28: I was a bit surprised to see this methodological suggestion as a final sentence concluding your study. I would consider changing this to something more broader, e.g. to the sentence on line 565-572.

71: “Large amount of measurement points” is a rather subjective statement as for some people this might mean hundreds or thousands of observations. Maybe define the rough amount of measurement points instead, and mention that this is more than has previously been used

75: With one driving parameter (i.e. soil moisture), right? You didn’t have many driving parameters to make the upscaled CH4 flux map?

76: But what about Kaiser et al., 2018?

105: You could add an index map to this figure showing e.g. the location of Hyytiälä too

210: I would describe these gridded layers in their own paragraph, similar to the other environmental measurements, and dedicated this one to the models only.

220: Could you provided a little bit more information about what parameters were

chosen for the different indices? For example, TWI can change quite a bit depending on what parameters you use in the calculation.

255: Should be Aalto et al., (2018), not (Aalto et al., 2018)

370: This figure could be moved to the supplementary – it’s not so useful for the reader because there are so many different points.

400: This is just an idea, but you could also replace these two maps by a map that describes the mean summer soil moisture and a map that describes its change over the growing season. It might be easier for the reader to spot the areas that are drying this way.

431: I would use the same color scheme that you used in Fig. 8 for the Fig. 9 as well, to make sure that you are using different color schemes for soil moisture and CH4 fluxes.

440: The discussion is rather long and without subtitles it is a little bit hard to follow. Could you consider adding a few subtitles and structuring it according to your main aims of the study (spatial variation, drivers and upscaling, hot spots)?

560: Again, I was a bit surprised to see this discussion here as it was not motivated in your introduction or it wasn’t one of your main aims of the paper. Maybe include it to the introduction or remove it completely?

567: If you want to discuss the sampling strategy, I would provide some more details here. E.g., how should the sample points be selected (e.g. systematic grid, gradient, random sampling, researcher-defined)? What is the number of temporal replicates required to understand spatiotemporal variability in this system? Further, in the abstract you mention that capturing the environmental variability requires 15-20 sample points. But do you think using statistical methods (e.g. random forest) with 15-20 points is reliable?

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