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Biogeosciences Discussions

Interactive comment on "Modelling CH_4 emissions from arctic wetlands: effects of hydrological parameterization" *by* A. M. R. Petrescu et al.

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Modelling CH4 emissions from arctic wetlands: effects of hydrological parameterization A. M. R. Petrescu, J. van Huissteden, M. Jackowicz-Korczynski, A. Yurova, T. R. Christensen, P. M. Crill, and T. C. Maximov

Below response is given to the referee comments. The author responses start with >>

Anonymous Referee #1

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#General comments:

The paper addresses the interesting topic of CH4 emission from (sub-)arctic wetlands. To improve our understanding of climate change processes and to predict future

changes in atmospheric greenhouse gas balances, it is needed to quantify terrestrial carbon exchange. It is recognized that northern wetlands may play a significant role in terrestrial emissions of CH4. Modeling these emissions may be done for several reasons, e.g. providing spatial and temporal coverage (including prediction of future emissions) or studying the relative importance of various processes and factors. The paper focuses on the latter and shows us the sensitivity of CH4 emission from two northern wetlands to water table variations. This stresses the need to include hydrology in ecosystem models to improve simulations of CH4 emissions, especially for larger spatial scale.

>> Thank you. No comments.

Specific comments

Abstract and Introduction

The purpose of the study is introduced not before the end of the introduction. This may be done for the specific research question, but for the reader it would be helpful to have some idea of the scope of the paper right from the start. Why are the two sites compared? What is the reason to model the emissions? In other words: what is the big picture? The purpose itself (page 3199, lines 14-17) may be rewritten. I think that the purpose is not to test a model or to compare two sites, but to quantify/study the effect of water table, temperature etc. on CH4 emission, by means of field measurements and a modeling approach.

>> We agree that the purpose could have been stated more clearly. The purpose will be changed according to your suggestions: to quantify/study the effect of water table, temperature etc. on CH4 emission, by means of field measurements and a modeling approach. We will reword the abstract and the introduction in this sense.

Materials and methods

Why are the CH4 measurements at the Kytalyk site limited to the lower terrace

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and the floodplain (ombrotrophic to minerotrophic) and at the Stordalen site to the minerotrophic mire area?

>> We have chosen the two sites due to the similarities present concerning the region, permafrost pattern, CH4 emissions and vegetation types. Mainly the CH4 fluxes for the Siberian site are coming from the flooded part of the area (lower terrace and floodplain) therefore a similar wet site in Stordalen was chosen in order to have comparable CH4 values and same vegetation which may play an important role in the CH4 emission patterns.

Model description

Model optimization and parameter calibration is mentioned (page 3203, line 7 and 16), but it is not clear what method is used. Which parameters are calibrated and what model output was optimized (what was the objective function)? Are the mentioned parameter values resulting from the calibration? In that case the values should be moved the ‘Results’ section.

>> The method used for the optimization was tuning of the model by simple fit by eye. The 'objective function' in this case was the agreement of modelled and measured values, and the extent to which the model represented the growing season cycle of the CH4 fluxes. The range of variations for most parameters it is known from literature. We tuned the parameters from which quantitative measurements are difficult to obtain, such as the methane production rate (as suggested by Walter, 2000). These are site specific parameters as Q10, plant oxidation and methane production rate. We agree that a more sophisticated procedure for optimization should have been used, but the limited amount of data from the Kytalyk site did not allow this. Moreover, this will be the subject of a later paper. The text was moved as suggested to the Results section. Parameter estimation for Q10 and R0 was done testing a range of values for R0 and Q10. The best fit was R0=0.22 with Q10=4.4, very close to the fit by eye values. The new figure will be introduced within the text of the revised manuscript on page 3206.

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Why are two different Q10 values used for one site? The sensitivity analysis is not described, although later on in the paper results and conclusions about parameter sensitivity are described.

>> It proved that for the Kytalyk site, the model performed slightly better when the water table simulation input was combined with a higher Q10, closer to that for the other site. The modelled water table information is better than the water table based on extrapolation from the scanty observations. Clearly, better water table input improves the model to such an extent that also a better adjustment of the Q10 parameter can be achieved, yielding more similar values for the two sites. We will discuss this more extensively in the text. The discussion on the sensitivity of the parameters is based on sensitivity tests done by the author of the model (Walter, 2000) as indicated in the text.

How was lateral inflow of water described in the MMWH model? The water table at a given position in the landscape is also depending on hydrological processes in the upstream area. Sites with the same soil characteristics, precipitation and evapotranspiration may experience different water tables, due to their position in the wetland. Drainage is also important (see page 3205, lines 24-28). Because WT is an important factor in the paper, some more words on the choice for the MMWH model, the strengths and weaknesses would be helpful. What parameter values were used in the MMWH?

>> The MMVH model as applied here accounts only for the lateral export (outflow) of water from the specified sites and no description of lateral inflow is included. Certainly, description of localized water flow would improve model performance, but that would also require more knowledge of ground water regime, both local and regional. In accordance with our final purpose-to construct the model that can be applied for the sites where little data is available-we only tested here the improvements that can be gained by using a relatively simple approach. We justified that mire groundwater dynamics is not a commonly monitored, and spatially explicit model that can describe it is far too sophisticated choice for our purposes.

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Results

In general: some assumptions and methodology are mixed with the results. For example: page 3205, lines 5-8, 10-11, 13-14; page 3206, lines 16-22.

>> The sentences describing the measurement techniques will be moved to the 2.2 section.

Page 3205, lines 16-20: the numbers in table 2 and figure 7 (not figure 8!) do not correspond to each other. Why are only the measurements of the floodplain used and not of the lower terrace? I do not understand what is meant by the interpolation of the water table. Is this done to obtain a continuous record for input in the model? How is seasonality applied in the interpolation of summer measurements? Figure 7 shows the WT of the floodplain(?). In line 27 (page 3205) it is said that there is a better agreement between measurements and modeling results at the lower terrace. A figure may support this.

>> Sorry, my mistake, figure 7 indeed and yes some errors for the values. The correction will be made and figure changed according to the Table2. It is true that I refer sometimes to the terrace but the simulations and the data are only from the floodplain. This will be changed accordingly in the text and only the floodplain values will appear. The interpolation for the WT depth was made linear in order to have a continuous record (daily time step) used input for Peatland VU model. The lower terrace is not present in the simulations therefore will be removed.

Page 3207: figures 10 and 11 belong to the floodplain site? Or is it a combination of floodplain and lower terrace? For both WT and CH4 I am a bit confused about what belongs to what site. I expect the two sites to differ from each other, due to differences in WT dynamics and vegetation.

>> Yes, the both figures are showing simulations of methane emissions at the floodplain. The figure text will be changed to be more explicit.

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Discussion and Conclusions

Page 3208, lines 17-24: the sensitivity of the model parameters is not supported by numbers or figures. Therefore the conclusions are not as strong as they could be. There could also be some words to physically explain why WT is more important than temperature, vegetation type and NPP.

>> We refer to the explanation given above to the remark on Q10 values.

Technical comments Figures: the axes are not consequent: time-axis of figures 5, 6, 8 and 9; WT-axis of figures 5, 6 and 7.

>> The time axes will be changed for all figures as the ones present in figures 8 and 9. The WT axis will be made for all figures 'cm below surface', with same step.

Interactive comment on Biogeosciences Discuss., 4, 3195, 2007.