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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 #2

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General To estimate the present and future methane emissions from northern/arctic wetlands, modeling tools are needed. The development of the models for general purposes is very demanding because of the high variability in the physical, chemical and biological characteristics of the microhabitats typical for these ecosystems. The

present work is a step forward attempting to improve the modeling of methane emissions by considering especially the hydrological part needed in the models. It is well known that water table is even more important than temperature in the regulation of methane release from northern wetland. However, actual data on water table is not generally available and the hydrological regimes have to be modeled from the climatic data as done here. The specific comments are listed below.

>> Thank you. No further comment.

Specific comments

Abstract

Page 3196/lines 21-23 It is rather clear that hydrology has to be included in the models predicting methane emissions from wetlands. Here the point is that the hydrology, i.e. the position of water table, can be predicted by "a relative simple model" (this means that the other models, if available, are more complex than the model applied here?) from the weather data, which then can be used as a part in the methane model.

>> There are other models more complex than the one described here which use not only weather data for simulating water table but take into account also the topography and watershead characteristics e.g. hydrological models as hydrological module of Wetland DNDC model, and watershed hydrology models as TOPOG or SLURP. Our point is to test a modelling approach that requires relatively simple parameterization, using generic weather data. [We also appreciate that different model types may be use to describe mire hydrology. Moving from the simple 1D approach used here to 2D or 3D models of water flow would introduce explicit description of water table variation due to watershed topography and spatial inhomogeneity in soil/peat and vegetation characteristics. However that would also require more input data and model dependence on the spatial scale of choice.]

Page 3196/lines 23-24 The sentence would be "Our results support the generalization

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in literature that methane fluxes in northern wetland are regulated more tightly by water table than temperature"

>> Thank you. The sentence will be changed as suggested: "Our results support the generalization in literature that methane fluxes in northern wetland are regulated more tightly by water table than temperature"

Introduction

Page 3197 The text in the beginning of the Introduction (first three paragraphs) is a bit loose. Especially the paragraph on nitrous oxide is not tightly related to the key aims of the study. The text could concentrate more on methane, and just refer to other greenhouse gases if needed.

>> Yes, it is right. The paragraph with N2O will be removed and only the general information on CH4 concentrations will be kept.

Page 3199/lines 2-3 The sentence "We study these sites because the hollows nutrient rich and often saturated river delta (lawns) have CH4 much higher than the other microlandscapes" is not associated logically in the previous text, and the message of the sentence is not clear.

>> We agree. The sentence and the previous one will be reworded as follows: Because of the presence of microtopography (hummocks, hollows and lawns, Bubier et al., 1993b) the topography of a wetland has a very high spatial variability which determines also spatial variability in methane fluxes. Bubier et al. (1993b) found that the CH4 flux follows the trend: hollows >lawns > hummocks. We study these sites because the hollows and lawns tend to have much higher CH4 fluxes than other parts of the northern wetlands.

Materials and methods/Results/Discussion Optimizing /calibration of the model:

Was it so that the model was optimized using all the measured data? A way to test the power of the model would be that only part of the data is used for the calibration, and

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the rest of the data is then used to test the calibrated model. This would be possible for the Stordalen data but not for the Kytaluk where the data pool is very limited..

>> We agree that this method of optimization would have been better. However this is indeed only possible for the Stordalen site and then still with a limited amount of data. The model was optimized only for some parameters e.g. plant oxidation type factor, Q10 factor which are very hard to be measured and very little literature is available. The input data for the model are the air temperature, WT and snow thickness and for validation CH4 emissions (measured data) were used to compare it with the simulations. In this case the goal was not the sensitivity of these parameters, but to test the combination of the methane flux model with a hydrological model, and therefore the optimization of the other parameters has been kept relatively simpler. A more thorough optimization and sensitivity analysis is deferred to a later paper using more data. [See also response for referee 1 and the new figure for optimization].

The oxidation fraction is a critical factor to model methane emissions. This fraction surely varied between various microhabitats depending on hydrological and vegetation characteristics. How the factors were selected here (0.6 for Siberia, 0.7 for Stordalen) considering the differences between the two sites including their microhabitats? (values were probably based on the optimization of the model but were in the proper range for the particular habitats?)

>> Sorry, there was my mistake in the text. For both sites the oxidation factor was the same 0.7 (see page 3208, line13-14) as the sites have same vegetation type: Carex, Eriophorum. This parameter needed to be optimized as little information was available (Cao et al., 1996; Walter et al. 1996). Walter 1996 has chosen Pox to be 0.5 but it may increase with increasing the maturity of plants. The parameter has high uncertainties and very little is known about it, therefore was tuned around the range of Walter 1996 considering a higher plant maturity.

Page 3208/lines 5-10 The sentences "The mean soil temperature at the Stordalen site,

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for the years 2004-2006 was 3.76 oC. This is in accordance with the known sensitivity of methanogens to temperature ... However, methane formation may occur at subzero etc. " This text does not form a logical message?

>> Yes, it is right, will be reformulated. I was just trying to make a point about the importance of high temperature and the direct connection to the methanogenic bacteria which produce more methane when high soil temperature is present.

Fig 7 Is there some explanation why the model simulated better the WT in 2004 and 2006 than in 2005 at the Kytalyk site?

>> 2005 was a very dry year compared to 2004 and 2006. The model takes into consideration the precipitation and temperature as input for simulating the WT. The error bar for the measured WT is very high so it is very uncertain so I would agree with the simulated levels instead of the measurements.

Fig 9 The model predicted rather well the maximum methane emissions at Stordalen mire in summer 2004 and 2005 but not in summer 2006 when the model underestimated the emissions. Is there some explanation for this?

>> My mistake, the simulated water table used here is not from Fig 7 but from Fig 6. As you can see there, the water was very low so as the CH4 emissions compared to the previous years. The climatic information is present in the figure 3 (very little precipitation that year so as the WT level, fig 5)

There is a statement in the conclusion that "parameter uncertainty at site level in wetland CH4 process models is an important factor in large scale modeling of CH4 fluxes". Some sensitive analyses could be given how the model output will change using a range of values for the key parameters. If the model needs specific parameterization for every microhabitat and climatic regions there is little hope that it can be used in proper large scale modeling? Now there is some qualitative text on the sensitivity on the page 3208 but real examples of the output could be given when changing the

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values of various parameters.

>> Extensive sensitivity analysis of the model parameters besides hydrological input is not the goal of our paper. We refer to sensitivity analysis made by Walter (2000) and Van Huissteden et al., (2005)

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