

Interactive comment on “The effects of surface moisture heterogeneity on wetland carbon fluxes in the West Siberian Lowland” by T. J. Bohn et al.

Anonymous Referee #1

Received and published: 2 June 2013

The paper describes an interesting modeling project on wetland methane and GHG dynamics in the western Siberian lowlands (WSL). It is a comprehensive study with the conclusion that heterogeneous moisture regimes in wetlands (and thus, the extent of distribution of saturated vs. unsaturated wetlands) has to be taken into account when modeling CH₄ emissions and dynamics of carbon exchange. Further, in the far North, parameters for the CH₄ model need to be adjusted for realistic representation. Many individual models have been combined to arrive at the model results presented. The authors have given all needed information on the individual model in tables, figures and text and, thus, model structure is principally clear. Further, control simulations employing a uniform water table scheme were performed to demonstrate the importance of spatial heterogeneity of natural wetland types on C fluxes. These control runs showed, on the one hand, that the model is not so sensitive to the spatial heterogeneity, largely

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because unsaturated zone dominated the areas. On the other hand, methane fluxes were strongly dependent from the water saturation level and thus the water level fluctuations played a significant role for temporal trends and at the site-level.

I find the manuscript generally very interesting. It is one of the first models which tries to integrate permafrost into process-based methane models and points out that different wetland types need to be distinguished (even though sensitivity analysis concluded no major impact on an areal basis), which is a clear contribution of the paper. However, I have some critical comments and suggestions for improvement which are outlined below.

My main critics lay in the variable settings for modeling CH₄ in the North and South, which are a little suspicious to me. Even though the authors try to justify that, I still find no real good argument which would convince me to use parameters towards less productive methanogenesis, other than “a better fit” with observed data (which I also do not see). I understand that CH₄ emissions decrease with temperature, but what is that “permafrost” factor exactly which gives reason for changing the sensitivity settings? This is further questioned by the fact that the model results substantially underestimate fluxes far in the North (one out of three groups). From a quick view in the literature I find that, e.g. CH₄ emissions can be quite higher than what is reported in Fig. 5 for CH₄ emissions in permafrost areas (e.g. Heikkinen et al. (Global Biochemical Cycles 18, GB1023, doi:10.1029/2003GB002054, even though from a different region), which would further underestimate the fluxes. . . I have my concern that enough evidence is provided here to justify the use of different settings for north and south, especially with respect to global models. Are there enough validation data especially in the North? It would have been interesting to see results on the control simulations the authors refer to on page 6530 (lines 11-19) as we could evaluate better whether there are reasons not keep methanogenesis rates constant. And: do the authors think that this is a phenomenon just valid for the WSL? This is important whether the model can be used for global simulations. Generally, concerning methane model: did the authors consider the

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occurrence of so-called “floating fens” where the peatland surface adjust the water table fluctuations maintaining high water tables even during drought? Such fens are quite common in the north. In such wetlands, CH₄ emissions correlate often positively with LAI, which indicates a tight link between net primary production and methanogenesis (opposite to the methane emission model used here). Could the authors discuss the impact of such behavior on the overall model outcome by considering the abundance?

Secondly: have the simulations of NPP and Rh of the wetlands been validated by observations? The model results on these component fluxes seem to be not very well corroborated by experimental ones.

Lake CH₄ emissions are not simulated. However, lakes can have significant CH₄ emissions and should be taken into account especially if “regional” or “areal” fluxes are computed. I suggest generally to tone down discussion on global warming impact of the region (see below).

Specific comments

Page 6519, line 6-77: make the last part of this sentence read “. . .and, therefore, the net climatic impact” Page 6521 Line 15? which modification? Would be interesting to know more details here Page 6523 Line 4-7 it is likely that NPP and Rh decrease with higher water saturation in wetlands, however, in periodically inundated zones NPP and also Rh is unlikely zero. Respiration continues as long as oxygenated electron acceptors (and also dissolved oxygen) is available, and sedges can be submerged in water with active photosynthesis. Discuss how this would change the model results Page 6524 Line 12 Explain for readers who are not so familiar with the terminology the abbreviation JJA

Page 6527 Lines 13-24 and Table 4 even though the WSL is dominated by wetlands there are also significant upland soils abundant. This paper only models wetland C dynamics. Thus, the authors cannot refer to regional GHG balance. CH₄ emissions may dominate the global warming potential of the wetlands, but this is certainly not the

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case for the uplands. Delete any conclusion made with respect to total climatic impact of the WSL.

Interactive comment on Biogeosciences Discuss., 10, 6517, 2013.

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