Responses to Reviewer 1

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 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.

Thank you for your comments. We have added our responses to your comments in blue following each comment.

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 methanogensis, 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 off 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 Norht? 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 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?

To address this paragraph, we have numbered the various points you have made for ease of response:

1. *The "better fit" of separate parameter sets in the South and North is not evident:* We have added results of single parameter set to Figure 4 for comparison with the dual parameter set results.
2. *We did not describe the "permafrost factor" responsible for the different parameters:* We believe it is differing soil microbial community species abundances, or a combination of this and different plant species compositions, between permafrost and non-permafrost, that lead to different temperature sensitivity (Q10 values). Our results are corroborated by the recent paper of Lupascu et al (2012), who computed the CH4 Q10 of soils in permafrost and non-permafrost wetlands in Sweden, as well as reviewing Q10s reported in other field and laboratory studies, and found that permafrost wetlands tend to have lower Q10s than non-permafrost wetlands (also sphagnum-dominated wetlands have lower Q10s than sedge-dominated wetlands). We have added text to this effect to the discussion section.
3. *Our model substantially underestimates CH4 fluxes in the far north of the domain (group 1):* We looked at some possible factors that could have contributed to this behavior: a. in comparison with Peregon et al. (2008), our modeled NPP appears too low by a factor of 2-3 in the tundra region (group 1 and part of group 2); b. our estimated annual average soil temperature (derived from simulations) appeared low in the permafrost zone; c. we did not account for spatial variation in plant-aided transport efficiency. We tested each factor by re-running our calibrations in 3 separate experiments: a. with NPP multiplied by a factor of 3 in the tundra; b. with annual mean temperature bounded below at 0 C; and c. allowing an additional parameter to vary: tveg, the efficiency of plant-aided transport. For experiment (a), the CH4 fluxes in the tundra (group 1) increased and bias was reduced; for (c), we found a lower optimal plant-aided transport efficiency for permafrost wetlands than for non-permafrost wetlands. But in all cases, we still obtained distinct ranges of likely Q10 values for permafrost and non-permafrost wetlands, and a better fit from separate parameter sets in the south and north than with a single parameter set. We have added text to this effect to the discussion section. As an aside, we did not re-run our simulations using higher NPP rates in the tundra because we were unable to establish whether the cause was overestimation of inundation (and inhibition) or underestimation of plant productivity in the absence of inhibition.
4. *Are there enough validation data especially in the North:* We agree that the smaller number of observations in the North (approx. 250) relative to the South (approx. 500) may have led to a higher sampling error in the North, and thus may be affecting our results, especially in Figure 4, where we split the Northern data into 3 groups, with 3-4 water table depth sub-groups each. Still, it seems unlikely that simulations for all water table depth sub-groups within groups 2 and 3 would be consistently positively biased due to sampling error alone. We think it is more likely that the low bias in group 1 is due either to sample error, since all observations in this group were taken within a single wetland complex, or a low bias in simulated tundra NPP (see #3). We have added text to this effect to the discussion section.
5. *Do the authors think that this is a phenomenon just valid for the WSL:* We agree that, until we know for sure what is causing the lower CH4 emissions in groups 2 and 3, we cannot say with certainty that these CH4 parameters apply elsewhere within the pan-Arctic. We have softened our claim as to applicability outside West Siberia, instead suggesting it is worth checking outside West Siberia, given the corroboration of Lupascu et al (2012) and given its potential impact on emissions (and lack of such intensive observations throughout most of the pan-Arctic).
6. *Did the authors consider the occurrence of so-called "floating fens":* We do not explicitly account for floating fens (nor do any other large-scale models, to our knowledge). However, we believe we may have partially accounted for them by calibrating our hydrologic model to

reproduce the observed inundation and saturation of remote sensing products (the Schroeder et al (2010) AMSR-based inundation product and the PALSAR classifications within our paper). As shown in Figures 3 and 5, our simulated wetlands in the tundra region are quite wet. We acknowledge that our simulated inundation and saturation will exhibit different temporal behavior than that of a floating fen; our inundated and saturated fractional areas will be highest after snowmelt and heavy rains and lowest in late summer, while actual floating fens may have a more constant inundated/saturated extent throughout the summer. We would therefore expect our CH4 emissions to be too high in early summer and too low in late summer, but our errors (at least those due to extents of inundation and saturation) might be smallest in mid-summer, when the observations were taken. In addition, these errors would at least partially cancel in computing the JJA average. We have added a brief discussion to this effect in the discussion section.

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.

We agree: for NPP and Rh, we drew on only on the net CO2 flux observations from the Zotino Tall Tower Observatory (ZOTTO). As mentioned above, we have added text to the manuscript noting that our NPP values are likely too low in the tundra, from comparison with values reported by Peregon et al (2008). However, outside the tundra, our simulated soil carbon density, which should be approximately proportional to NPP, matched the observations of Sheng et al. (2004).

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).

It is true that lake CH4 emissions can be quite large but also quite uncertain, and there are many lakes in the WSL. We chose to neglect lake CH4 emissions due to the lack of lake CH4 observations in the region, and chose to focus on the patterns of wetland CH4 emissions. We never intended our analysis to be a comprehensive accounting of all CH4 emissions from the region. We have added wording that clarifies our claims to the sections where we discuss greenhouse warming potential.

Specific comments

Page 6519, line 6-77: make the last part of this sentence read ": : :.and, therefore, the net climatic impact"

Done.

Page 6521 Line 15: which modification? Would be interesting to know more details here.

The modification is described in detail in appendix A2, which is part of the body of the manuscript. We feel that referring the reader to the appendix, as we have done in the paragraph in question, is sufficient.

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 oxygentated 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.

Our control runs represent the extreme case of no inundation, no saturation, and no inhibition of either NPP or Rh. Presumably, if NPP and Rh do not go completely to zero under inundated conditions, the result would be intermediate between our fully-inhibited simulations and our control simulations. Given that differences between these simulations were small, we expect that our over-inhibition of NPP and Rh would have only a small effect as well. We have added a comment to this effect to the manuscript.

Page 6524 Line 12: Explain for readers who are not so familiar with the terminology the abbreviation JJA.

Done.

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

We do not claim to be computing regional GHG balance. Every C flux computed in the paper applies only to wetlands. This is stated on page 6523, lines 2-3: "Because this study focused on wetland behaviors, all descriptions hereafter refer only to the wet portions of the domain". "Wet portions" are defined on the previous page to be the non-upland portions of each grid cell. However, we have added reminders to the reader of this restriction to wetlands in a few other places throughout the paper just to be safe.

References

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