Responses to Reviewer 2

Bohn et al. present really interesting study with great model integration and representation of field conditions. They dynamic representation of saturated and unsaturated wetland areas and lake areas is really impressive, as is the integration of so many different types of models. The authors deserve commendation for this sophisticated and complex undertaking because they do it well. The manuscript is clearly written and easy to follow and the figures and tables are clearly presented. I enjoyed reading it.

The inclusion of wetland microtopography is really neat. The authors subsequently find that not microtopography but classification of wetlands of saturated vs. unsaturated zones, is what seems to be important to regional carbon balance. This is a great advancement for large-scale representation of C processes and has the potential to greatly enhance large-scale modeling efforts as well as inform field measurements of small-scale processes. I think that the conclusions of the authors are valid and may need a little more evidence for the second two conclusions. Both hinge on the use of separate parameter sets between south and north, which does seem plausible, but needs more justification. The short-coming of this study is the representation of methane processes and, secondarily, the representation of lake C fluxes (none).

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

First is the representation of methane processes. If the authors use a single parameter set for north and south, the model simulates the spatial distribution of methane fluxes very poorly (Fig. 7). Note that we don't know how well the single parameter set fits the observed fluxes, so we are unable to evaluate this part of their argument (Fig. 4). If they use two sets of parameters for modeling methane, the model simulates the spatial distribution of methane fairly well, but falls short of simulating the observed fluxes in northern regions (model underestimates observed by at least 90% for group 1) and overestimates methane fluxes for surface zones in groups 3,4,5. So even if methane should be represented with two sets of parameters, as the authors argue, the fit is still poor. Granted, there may be some short comings all the observational datasets and methane flux is an extremely difficult set of processes to model, but this stills seems like a major shortcoming of the model. Consider also the methane flux dataset of Olefeldt et al. 2012 for the circum-boreal permafrost area.

The other reviewer had similar concerns. To address your comments, we have numbered the individual points:

1. Note that we don't know how well the single parameter set fits the observed fluxes: We have added results of single parameter set for comparison to Figure 4.
2. even if methane should be represented with two sets of parameters, as the authors argue, the fit is still poor: We looked at some possible factors that could have contributed to this behavior (in particular, our severe underestimation of the fluxes in group 1): 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 CH_4 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 added the results of the increased NPP calibrations to Figure 4 for comparison. Finally, another factor likely causing us to overestimate CH4 fluxes when the water table was at or above the surface was that we didn't account for some aspects of inundated conditions in the methane model (see our response to the comment on oxidation below). We have added text describing all of these factors to the discussion section.

3. This still seems like a major shortcoming of the model: We hope that, now that we are including the results of the single parameter set and the results of the increased tundra NPP calibration to Figure 4 for comparison, our model results are more convincing. In addition, our results are corroborated by 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.
4. Consider also the methane flux dataset of Olefeldt et al. 2012 for the circum-boreal permafrost area: In retrospect, it might have been better to calibrate and run the model over the entire pan-Arctic (or circum-boreal) domain and take advantage of these other datasets; it would have helped answer the question of how applicable our results are outside of the WSL. However, when we began this study, Olefeldt et al 2012 was not available; our goal was to focus on the spatial variability that was captured by the Glagolev et al 2011 dataset, which no other large-scale modeling studies had attempted to reproduce or analyze.

To me, this suggests that the authors may be missing something in the representation of methane processes or the spatial distribution of methane production (or both). In their spatial distribution of methane fluxes (Fig. 7), the authors scale by dividing methane fluxes by normalizing for grid cell area. However, upland soils may modify spatial patterns of methane flux through methane oxidation. Upland boreal soils are sinks for methane and can consume anywhere from 2-900 mg CH4 m-2 d-1 (Whalen et al. 1992). Please consider including this process for upland soils.

We feel that the factors we considered in our calibration experiments described above, in particular the increased tundra NPP experiment, are the most likely explanations for our poor fit to observations in the North. It is true that we did not consider CH4 oxidation from uplands in preparing Figure 7. However, if anything, this should cause our spatial distribution to represent an ***upper*** bound on CH4 emissions. As we have taken pains to point out, the most striking difference between our (and Glagolev et al 2011) distribution and those of the other studies (including Fung et al 1991) is that the other studies predict much ***higher*** CH4 emissions in the North than we do. If these other studies also accounted for upland CH4 oxidation, this would imply that their wetland CH4 production rates are even more positively biased in the North.

Methane oxidation is also a relevant process to consider in the unsaturated wetlands. It may be implicitly considered in the flux measurements, but likely contributes to the lower fluxes observed at the surface depths in Fig. 4: groups 3, 4, 5.

We are a little confused by your question. The wetland methane emissions model of Walter and Heimann (2000), which we used in this study, accounts for methane oxidation in the unsaturated wetlands. Did you mean oxidation under ***saturated*** conditions? If so, then, yes, we agree that oxidation is a likely explanation for the lower fluxes observed at the surface depths in Figure 4, groups 3-5. Strack et al (2004) explained similarly low CH4 fluxes from inundated wetlands in Canada as the result of relatively high dissolved oxygen and relatively warm temperatures in the water column when standing water is present above the soil surface, compared to when the water table is below the soil surface. While our modeling framework did model the physics of the standing water, it did not account for these effects on methane oxidation. We have added a note to this effect in the discussion section.

Second, the authors neglect respiration and methane production from lakes. They do briefly acknowledge this, but it may warrant a little more discussion of this point. Clearly, it is unrealistic that lakes do not contribute anything to the landscape C fluxes considering inputs of DOC, lake productivity, and the measurements of large magnitudes of methane fluxes from lakes, while highly spatially and temporally variable, are likely not entirely negligible but may be beyond the scope of this manuscript.

We only claimed to be simulating wetland methane emissions, not those of lakes. We acknowledge that we have not been as clear as we could have been about this. Therefore, we have added statements to this effect throughout the text.

Specific comments:

Please clarify "distributed" water table scheme. I assume this refers to the saturated vs. unsaturated zone clarification, but could also refer to a depth-distribution.

Our water table scheme uses a statistical depth distribution, i.e., a histogram of water table depths within the wetland. We had hoped this would be clear from the model description in the appendix. However, we acknowledge that this choice of words could be construed as a model in which grid cells' water table depths depend on those of their neighbors. Therefore, we have replaced "distributed water table scheme" with "heterogeneous water table scheme".

6527 line 8: "This represents 34% (24 to 47 %)" please define numbers in parentheses.

We have inserted "1st and 99th percentiles of" into the parentheses.

Table 3. Please explain meaning of zones in table heading. Not immediately clear.

We have replaced "zone" with "category", and have added footnotes defining the terms below the table.

Fig. 4. Please clarify in figure legend whether these simulated methane fluxes use the single parameter set or the separate parameter sets.

Done.

References

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