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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 #2

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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).

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.

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.

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.

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.

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.

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

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

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

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