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Interactive comment on "Exploring the response of West Siberian wetland methane emissions to future changes in climate, vegetation, and soil microbial communities" *by* T. J. Bohn and D. P. Lettenmaier

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We thank referee #1 for his/her comments and insight. This referee makes some good points, which we will address with suggestions for how we intend to modify the experiments. We do, however take issue with some of the referee's comments, as noted below. We have also included a supplement describing our proposed changes to the paper.

This referee's comments center on concern about our use of methane emissions parameters optimized to match observed emissions, and in turn to infer soil microbial behavior from them. In particular, the referee argues that the values of optimized parameters (e.g., Q10) may be influenced by the lack of observational constraints of other parameters (e.g., plant oxidation parameters, which we did not calibrate), and/or other processes not represented in our model. The referee is particularly concerned about our Q10 values in the southern half of the WSL (9.6-11.7), which he/she believes are unrealistically high, and may be artifacts of the optimization. While we attempted to justify our parameter values by noting that they fall within the ranges found in laboratory and field studies reported by Lupascu et al. (2012), the referee points out that the statistically significant differences between Q10 values found by Lupascu et al (2012) were determined only by wetland type, not latitude. The referee also expressed frustration that we did not describe our optimization procedure in more detail.

First, with respect to the referee's objections to our optimization approach: We did not describe the optimization approach in detail because the parameters were optimized in a previous study (Bohn et al (2013)), to which we referred the reader. To summarize, we ran several thousand Monte Carlo simulations, sampling parameter values uniformly from ranges reported in the literature, and evaluated them using Bayes' Theorem. The objective function was the maximization of the joint likelihoods of the mean simulated emissions across latitudinal bands and water table depths, relative to the observed means and errors on the mean. The parameters so estimated were r0 (the tuning parameter), xvmax (the maximum oxidation rate), rq10 (the temperature dependence of methanogenesis, i.e., the Q10 parameter of concern, above), oxq10 (the temperature dependence of oxidation), and rkm (the Michelis-Menten constant for oxidation). We would be happy to include a similar summary of the optimization procedure in the current manuscript – still referring to the Bohn et al. (2013) paper for details.

Regarding our (methanogenesis) Q10 values: we maintain that our optimized values are realistic (parameter equifinality notwithstanding; see below), but perhaps we should have provided a clearer justification for them. It is true that Lupascu et al (2012) did not

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find a statistically significant difference between Q10 values on the basis of latitude, nor on the basis of presence/absence of permafrost. However, Lupascu et al (2012) did find a statistically significant difference in mean Q10 between sphagnum-dominated (mean of 8, ranging from 2.5 to 35) and sedge-dominated (mean of 4.3, ranging from approximately 1 to 8) wetlands across the high latitudes (see Figure 7 from Lupascu et al., 2012). In the WSL, sphagnum-dominated wetlands are the most common type of wetland south of 63 N latitude (see Figure 5 from Peregon et al., 2008). Thus, in the WSL at least, wetland type is strongly correlated with latitude (as is permafrost extent), and our southern Q10 values fall comfortably within the range of values expected for the sphagnum-dominated wetlands found there. We acknowledge that we did not make this as clear as we should have, and that our wording could be interpreted as claiming that it is permafrost, rather than wetland type, that determines Q10 values. We certainly can clarify appropriately in a revised manuscript.

Regarding parameter equifinality: we agree with the referee that our optimal Q10 values could have been influenced by the choice of plant oxidation parameters, given the high sensitivity to Pox and Vtransp, and the interactions between those two parameters, as found by van Huissteden et al. (2009). In addition, we recognize that the omission of one process in particular – the oxidation of methane in the water column under inundated conditions – could have influenced our results (in that a lower Q10 value in the northern WSL could be compensating for the omission of oxidation of methane in the water column, given that the northern wetlands experience more inundation than those in the south). Therefore, we propose to include in the paper the results of additional experiments that address these points, as outlined in our attached "Proposed Changes" supplement.

The more detailed comments by reviewer #1 (preceded by a "*"), and our responses to them, are listed here:

* Page 16333, lines 21-24 and 26-28. The fact that the eutrophic mires in the south emit more methane than the tundra wetlands, is not necessarily an effect of climate

and vegetation alone, but likely also relates to trophic status. In eutrophic wetlands the availability of nutrients allows a higher primary production and hence a higher methanogenic substrate production. Many tundra wetlands on the other hand are meso- to oligotrophic. The paper by Glagolev et al does not infer that the observed differences are related to climate only, as is suggested in line 26-28. Moreover the paper of Glagolev is based on surface flux measurements, while incubation experiments would be a more appropriate to assess climate-related differences between microbial communities.

Lupascu et al. (2012) found statistically significant differences between the methanogenesis Q10 values of sphagnum- (mean of 8) and sedge-dominated (mean of 4.3) wetlands; the mean Q10 across all wetlands was 5.6. According to the wetland map of Peregon et al. (2008), the wetlands of the southern WSL (south of 63 N latitude) are primarily sphagnum-dominated. If we were to use these mean values in the Walter-Heimann model according to the dominant wetlands within each grid cell, we would end up with a similar spatial distribution of Q10 values to our calibrated values (higher in the southern half than in the northern half), although South-North differences would be somewhat less pronounced. We propose to replace our optimized Q10 values with these observation-based estimates in our revised paper (for details, please see the attached "Proposed Changes" supplement).

* Page 16335 line 20-23. It is not clear how the parameters for the wetland methane emission model were calibrated. This information is crucial however. The Walter-Heiman model has a large number of parameters that are poorly quantified and often need to be calibrated, including microbial population parameters like the methane production rate, the temperature sensitivity, but also other parameters like plant transport rate and oxidation within the root system. These parameters tend to influence each other, as is demonstrated in a paper on sensitivity analysis of the Walter-Heimann model by Van Huissteden et al (Biogeosciences, 6:3035-3051, 2009). If several of these parameters are poorly constrained by experimental data, it is impossible to obtain

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reliable estimates of one of the parameters by optimization, since equifinality of parameter sets may occur. Equifinality results in widely differing parameter sets, all with an good model-data fit. So, if you use optimization to obtain a Q10 value for methanogenesis, the constraints on other microbial population parameters and any other parameter that influences the surface flux, should be well defined. If not, the resulting optimization may give invalid results. For instance, in the Walter-Heimann model the value assumed for oxidation during plant transport may strongly influence optimization results for the methane production rate and temperature sensitivity parameters, as is demonstrated in the sensitivity analysis cited above.

We did not describe the optimization approach in detail because the parameters were optimized in a previous study, described in Bohn et al (2013), to which we referred the reader. In the revised paper, we will include a more detailed summary. Our proposed changes to the recalibration procedure (see "Proposed Changes" supplement) address the equifinality issue by replacing the differences in Q10 values resulting from optimization with differences found in field and laboratory studies. We will also include error bars arising from parameter uncertainty in our results.

* Page 16335 line 27-30. Q10 values between 9.7 and 11.7 are unrealistically high. Data from incubation experiments in general do not indicate such high values. This indeed makes me worry about the optimization from which these values have been derived.

We disagree. Lupascu et al (2012) found Q10 values ranging from 2.5 to 35 in sphagnum-dominated wetlands, with a mean of 8. These sphagnum-dominated wetlands are the major wetland type in the southern half of the WSL (Peregon et al., 2008). Our optimized values fall comfortably within this range.

* Page 16336 line 1-2. Again, the paper by Lupasco does not consider latitudonal differences on the scale of the West Siberian Lowlands, but differences between wetland vegetation types.

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It is true that Lupascu et al (2012) did not find a statistically significant difference between Q10 values on the basis of latitude, nor on the basis of presence/absence of permafrost. However, Lupascu et al (2012) did find a statistically significant difference in mean Q10 between sphagnum-dominated (mean of 8, ranging from 2.5 to 35) and sedge-dominated (mean of 4.3, ranging from approximately 1 to 8) wetlands across the high latitudes (see Figure 7 from Lupascu et al., 2012). In the WSL, sphagnum-dominated wetlands are the most common type of wetland south of 63 N latitude (see Figure 5 from Peregon et al., 2008). Thus, in the WSL at least, wetland type is strongly correlated with latitude (as is permafrost extent), and our southern Q10 values fall comfortably within the range of values expected for the sphagnum-dominated wetlands found there. We acknowledge that we did not make this as clear as we should have, and that our wording could be interpreted as claiming that it is permafrost, rather than wetland type, that determines Q10 values. We certainly can clarify appropriately in a revised manuscript.

* Page 16336 line 3-4. Q10 is not the only microbial population parameter in the Walter-Heimann model. Why is the methane production rate R0 not considered here – this may be equally responsible for the high fluxes in the south?

As described in Bohn et al. (2013), we optimized several Walter-Heimann model parameters, including R0. In fact, the optimal ranges of values we found for R0 in the south (0.016 to 0.022, median 0.019) and north (0.015 to 0.026, median 0.020) halves overlapped substantially. Thus, we did consider R0 and found no evidence to support strong spatial variation across the domain. We will make this fact clearer in our description of the optimization procedure.

* Page 16338 line 22-23. 'Population shifts were modeled as a complete replacement of northern microbial species abundances with those of the south'. This is nonsense. It is completely unknown how the microbial population parameters in the Walter-Heimann model relate to microbial species abundances.

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We agree; we were wrong to ascribe variation in Walter-Heimann model parameters solely to soil microbial species abundances. In our proposed revisions to the experimental cases (see "Proposed Changes" supplement), we will replace the "population shift" case with a "wetland migration" case. The "wetland migration" case will explore the potential future distribution of sphagnum- and sedge-dominated wetlands as a function of changes in future climate (specifically, June-July-August air temperature) given by the CMIP5 models. Because migration of plants does not guarantee migration of soil conditions, there will be two sub-cases of "wetland migration": a "veg" case, in which values of Pox and Vtransp, which are directly related to the presence/absence of aerenchyma, are migrated but all other Walter-Heimann parameters retain their historical distributions; and a "soil" case, in which not only Pox and Vtransp, but also the methanogenesis Q10, are migrated. Thus, we will avoid speculating about future soil microbe species abundances and/or what effect those could have on methane emissions. The non-wetland-migration case will consider one end of the spectrum (no changes in Walter-Heimann model parameters), the full veg- and soil-migration case will consider the other end of the spectrum, and the veg-migration case will consider an intermediate (and more likely) case in which vegetation changes but the underlying soil conditions have not yet caught up.

* Page 16338 line 23-25. Please explain the parameter abbreviations in the text.

The parameters so estimated were r0 (the tuning parameter), xvmax (the maximum oxidation rate), rq10 (the temperature dependence of methanogenesis, i.e., the Q10 parameter of concern, above), oxq10 (the temperature dependence of oxidation), and rkm (the Michelis-Menten constant for oxidation). We will include this description in the text.

* Page 16340 line 21-24. How is GWP calculated? This deserves more attention than it is given here, see e.g. Frolking et al., Journal of Geophysical Research, 11, 2006.

In our revised manuscript, we will exclude our estimates of greenhouse warming potential, as these a) depend on estimates of other carbon fluxes computed in a previous study (Bohn et al., 2013), b) depend on the method of computation, and c) are not very useful when only computed for the wetland portion of the landscape, as we have done here.

* Page 16343 line 12-13. As detailed above, the large increase in methane emission can be quite well an artefact of the optimization of Q10.

This may be true to some extent, but as we have argued elsewhere, we see good reason to expect higher Q10 values in the southern WSL. Replacing our Q10 with the average sphagnum-dominated Q10 of 8 (Lupascu et al, 2012) will still result in a substantial increase in emissions if applied to the northern WSL.

* Page 16348 line 19. More can be added to the list of model oversimplifications. I miss any references to the uncertainties resulting from the optimization procedure. Another oversimplification is that changes in wetland nutrient status, topography and vegetation composition by thawing permafrost are not included. This might even be more important than changes in microbial communities by climate change.

We will add parameter uncertainty to our estimates (see “Proposed Changes” document). We will add the lack of consideration of wetland nutrient status and topography to the list (although broad wetland type as described in Peregon et al (2008) shows little sensitivity to topography - see Figure 1 in our "Proposed Changes" supplement). We believe that exploring the potential future distributions of wetland type as a function of the climate given by the CMIP5 models (see “Proposed Changes” supplement) will help address changes in vegetation arising from thawing permafrost.

* Page 16348 line 24-25. The lack of data, noted here by the authors, confirms that spatially varying microbial population parameters are still rather hypothetical. However, I wonder why the authors did not support their case by using literature on incubation experiments and (eventually) metagenomic analysis of microbial populations, rather than considering surface flux observations only. Surface flux observations do not give

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explicit clues to microbial population variation.

Our proposed application of parameters based on wetland type, as found by field and incubation studies (Lupascu et al., 2012; and Peregon et al., 2008; see "Proposed Changes" supplement) will address this issue and avoid speculation about microbial communities and species abundances.

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/10/C7936/2014/bgd-10-C7936-2014-supplement.pdf

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