Biogeosciences Discuss., 10, C7945–C7948, 2014
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Biogeosciences Discussions

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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Received and published: 14 January 2014

We thank referee #2 for his/her comments and insight. This referee has made some important points that we will address with a proposal for modification of the experiment (attached "Proposed Changes" supplement).

This referee's comments center on the design of our experiment: a) we should make it clearer that we are conducting a thought experiment rather than a well-constrained process-based projection; b) we should give more information on how the Walter-

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Heimann model parameters were derived; c) we should justify our scheme for representing microbial acclimatization; d) we should discuss the effects of climate trajectory and climate extremes on microbial communities and how that might change the possible response from our mean climatological change approach; and e) we should consider a wider variety of soil microbial responses than the simple extreme case of our "population shift" case, and discuss their likelihoods.

a) We agree, and will make this point clearer.

b) We did not describe the optimization approach in detail because the parameters were optimized in the study described in 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 calibrated were r0 (the tuning parameter), xvmax (the maximum oxidation rate), rq10 (the temperature dependence of methanogenesis), 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 (see also our response to Reviewer 1).

c) We chose a multi-year running mean because the reference temperature in the Walter-Heimann model was described as an annual mean soil temperature. It seemed counter to the original intent of the model to use a reference temperature that would vary substantially on a seasonal basis, as this would erase much of the temperature dependence of methane production. However, we admit that the choice of a 10-year period was somewhat arbitrary. To account for a range of potential characteristic response times within the soil microbial community, we propose to replace our current running mean with the slow relaxation formulation of Koven et al. (2011) and Ringeval et al. (2011), and replace our single window length with a 10-member ensemble of characteristic response times (τ) ranging from 1 to 30 years.

d) It is true that climatological extremes can affect soil microbial communities, and that mean climatological shifts do not represent the full range of soil microbial responses. Due to our downscaling method of randomly sampling historical years and applying anomalies, and linearly ramping LAI changes, our experiment is not set up to predict the occurrence of future multi-year droughts, etc. In addition, the Walter-Heimann model does not in its current form account for changes to microbial populations due to climate extremes such as drought. To properly account for changes in soil microbial communities to climate extremes, we would need to (a) redesign our downscaling approach entirely and (b) add representations of how the soil microbial communities respond to extremes in soil moisture. These are major changes to the current study and might warrant a separate study. However, our proposed ensemble of characteristic response times in the computation of the reference soil temperature (see part (c), above) will at least explore microbial sensitivity to extremes in temperature of varying lengths in the downscaled meteorology. We would be happy to acknowledge this limitation in the discussion section.

e) We recognize that the "population shift" scenario is an extreme case, and that it is not as process-based as it could be. We propose to replace this case with a more process-based representation ("wetland migration"), as follows: 1. Assign Q10s on the basis of wetland type, using the wetland map of Peregon et al (2008) and the average Q10s of wetland types found by Lupascu et al (2012); 2. Project shifts in the geographic distributions of wetland types (and their Q10s) as a function of climate (specifically June-July-August air temperature), as given by the CMIP5 models. This will be both more realistic and a more identifiable upper bound on the northward migration of CH4 parameter values (given that the underlying soil conditions – pH, soil carbon density, recalcitrant carbon concentrations, etc. - likely will not have reached equilibrium with the new climate in just a century). In addition, by tying the future distribution of wetland types to the climate given by the CMIP5 models, we will have

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some measure of uncertainty in the effects of “wetland migration” (see “Proposed Changes” supplement for more details).

Please also note the supplement to this comment:
http://www.biogeosciences-discuss.net/10/C7945/2014/bgd-10-C7945-2014-supplement.pdf

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Interactive comment on Biogeosciences Discuss., 10, 16329, 2013.