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Biogeosciences Discussions

Interactive comment on "The role of endophytic methane oxidizing bacteria in submerged *Sphagnum* in determining methane emissions of Northeastern Siberian tundra" *by* F. J. W. Parmentier et al.

Anonymous Referee #2

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General comments:

1) In their manuscript, Parmentier and co-authors address the capacity of methanotrophic endophytes of submerged Sphagnum species to reduce methane emissions from a Northeastern Siberian tundra environment. The authors measured methane fluxes from mid of July to beginning of August, 2007, from two similar sites (TW1 and TW4) one vegetated with Sphagnum and vascular plants and one site without Sphagnum. Also, potential methane oxidation rates were determined at different temperatures in samples from two additional sites also covered with Sphagnum. The obtained

potential methane oxidation rates were up-scaled and compared with field methane emissions. In addition, process based modelling was carried out aiming to understand if the observed differences in methane fluxes from the two sites could be explained by methane oxidation through moss associated bacteria. The manuscript is well written, clear and not overlong. A couple of very interesting papers on the particular interaction between methanotrophs and Sphagnum have recently been published either dealing with factors possibly influencing methane oxidation in Sphagnum (such as water level and moss species) or dealing with the community structure of those endophytic methanotrophs. A quantification of the methane sink provided through methanotrophs in Sphagnum, however, has not yet been approached so that our knowledge on the importance of this process for the greenhouse gas budget of tundra landscapes is little. Thus, the objective addressed by Parmentier et al. is, in my opinion, highly valuable, the more since the authors included methane flux modeling. I generally agree with the comments by Mr. Knoblauch and Mr. Basiliko and consider it unfortunate that the major outcome of this paper (that methane oxidation in Sphagnum is mainly responsible for the 50% lower methane flux from a Sphagnum/sedge site compared to a sedge site without Sphagnum) is not conclusively made. It is well known that typical vascular wetland plants such as Carex and Eriophorum enhance methane emissions. This effect is not only a result of the transport of methane through the aerenchyma but also due to root exudation delivering, directly and indirectly, substrates for methanogenesis. In the manuscript (page 8525, line 1-5), the authors state that methane production is higher in the presence of sedges such as Eriophorum (which, by the way, is absent in the Sphagnum site TW4 according to Tab. 1). So I wonder why it should be surprising that less methane is emitted from a site with a clearly lower vascular plant cover (20-30% compared to 40-90% as shown in Tab.1). Neither data are provided on the below-ground methane stock nor on the methane production rates of both sites, so the differences in methane emissions could as well be a result of higher methane production rates in site TW1. Here I fully agree with the comments by Mr. Knoblauch. It appears to me that the process of methane production is also not included in the model

applied and can thus not be excluded as a possible explanation for the different fluxes. How would the model perform, if differences in methane production were considered? In my opinion, the authors should reconsider the interpretation of their data and draw their conclusions with more caution.

Specific comments:

2) The potential methane oxidation rates presented by Parmentier et al. validate that methanotrophs associated to submerged Sphagnum species are actively oxidizing methane. Given the very remote character, the-in terms of greenhouse gas fluxes-importance of Siberian tundra, and the widespread distribution of Sphagnum in Arctic wetlands, this result is certainly interesting. However, in order to quantify by how much methane emissions are reduced through methanotrophs in Sphagnum (which in my opinion is the interesting and new part), a different and more experimentally defined approach less afflicted with spatial heterogeneities should have been explored. The process of methane oxidation, for example, could be inhibited in the field or in mesocosms using CH2F2. The difference in methane fluxes with and without inhibition would give a more realistic picture on the actual methane oxidation capacity of the bacteria associated to Sphagnum. Alternatively, Sphagnum could be removed from the field site as already suggested by Mr. Basiliko.

3) I agree with Mr. Knoblauch that potentially high methane oxidation rates of TW1, the site without Sphagnum, cannot be excluded since this was not measured.

4) I do not consider it appropriate to compare potential methane oxidation rates based on artificial lab conditions with fluxes measured in the field (page 8530, line 12-19). Thus, I do not think the up-scaling of methane oxidation rates to field methane fluxes makes very much sense. In addition to the points already made by Mr. Basiliko in this respect, in-situ methanotrophic activity in the moss is likely substrate limited due to the very slow diffusion of methane through water. Methane concentration profiles could have shed light on the actual flux of methane into the moss layer.

5) Page 8524, line 14-19: The authors state that methane emissions are sensitive to the position of the water table. Though in their study both sites are inundated, the water table still differs by up to 10 cm (Fig.3 e.g. on 23rd, 25th, 26th of July). This certainly has an effect on methane emissions (e.g. depth of the water column determines how far the gas needs to diffuse; refer, for example, to the paper by Sachs et al., 2010, Global Change Biology and the papers mentioned within). Potential effects of the different water table positions of TW1 and TW4 need to be discussed throughout the manuscript.

6) Page 8528, line 17-18 and page 8529, line 16: What is the reason for the different years of chamber measurements (2007) and the determination of potential oxidation rates (samples from 2008)? Even more importantly: why were the samples for the determination of potential methane oxidation rates not obtained from site TW4 where the flux measurements were conducted but from sites NS1 and NS2? Is there a particular reason for this approach? Also, what means 'similar vegetation distribution' here (same page line 17)? Considering the different years and sites for flux measurements and the determination of potential methane oxidation rates, the up-scaling approach appears to be even less reasonable (see comment 4).

7) Page 8535, line 15-17: Again, what about methane production? Was this considered, too?

8) Page 8538, line 4-6: Here I am lost. What is the study site the authors are talking about? On page 8527, line 21-24 (method of methane flux measurements) it is referred to Huissteden et al., 2005. There, 12 different landscape classes were identified with floodplains emitting most of the methane. Why are now only tundra sites (TW1 and TW4) mentioned to emit methane? Even excluding the floodplains, also TW2 and TW3 were previously reported to emit methane?

9) Page 8524, line 2: Please define 'pressure'. What 'pressure' for tundra ecosystem has already occurred due to rising temperatures? Give references here.

Interactive comment on Biogeosciences Discuss., 7, 8521, 2010.