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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.

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General comments: Parmentier and co authors have (1) measured CH4 fluxes and environmental variables over 20 days in a flooded Russian arctic tundra wetland with a focus on comparing CH4 emissions between areas with submerged Sphagnum moss present and without Sphagnum moss (the latter having a substantially larger vascular plant cover, but of the same Carex and Eriophorum species as in the Sphagnum plots); (2) incubated Sphagnum plants with apparently non-rate-limiting concentrations of CH4 and O2 and measured CH4 oxidation potentials in vitro and scaled up measurements

to the level of in situ net efflux; and (3) performed model sensitivity analysis with a previously described model used by one of the co-authors at this and other sites to help identify if/that presence or absence of Sphagnum mosses plays an important role in CH4 efflux rates observed between these 2 sets of sampling locations.

I generally agree with the unsolicited comments from Dr. Knoblauch. I would find it appropriate for the authors to also consider his comments in their revisions (rather than just providing the rebuttal). This is an interesting paper and there is clearly a recent precedent in the literature for conducting this work. I also greatly appreciate that this work was carried out in a representative globally important high latitude site that is logistically challenging to access. Particularly because of the latter point, I find overall merit in the reasonably small scope of field and modelling work that was conducted. I also found the manuscript to be appropriately succinct and generally easy to read.

However, even in light of the model sensitivity analysis, I feel that the authors should present their conclusions more cautiously. The study is fundamentally observational and based in only a few measurement locations in one site over a very short period of time. Presence or absence of Sphagnum moss may very well be the key factor driving observed differences, but this, as well as CH4 production, water-table position, and vascular plant cover (that could supply substrate for more rapid methanogenesis as well as represent a potential gas conduit) are not controlled experimentally. I also am also particularly wary of the direct comparison of lab-derived CH4 oxidation potential rates to field-based net CH4 flux rates.

I provide more specific comments below that I hope will prove constructive for the authors as they revise this manuscript. Sincerely, Nate Basiliko (University of Toronto)

P8523 Line 5 (and throughout): How important is this cooperation? Is it a true symbiosis? One might imagine that stable habitat (and perhaps a local O2 supply) for CH4 oxidizing bacteria in these mosses might lead to larger bacterial populations than if living freely in water. However, would moss NPP decline in absence of the methanotrophs? It is hard to imagine Sphagnum mosses, which often exist above a decomposing organic soil profile, are typically C-limited and that in absence of CO_2 production by bacterial endophytes they could not obtain this amount of CO_2 from elsewhere. There is some evidence that elevated CO_2 concentrations in peatlands might actually favour vascular plants over Sphagnum mosses (see Fenner et al. Ecosystems 4:635-637)

P8523 Line 19: The reference to the response of methanotrophic bacteria to a warmer climate in the last line of the abstract does not really seem to fit with the scope of the study.

P8527 ca. Line 26 (and elsewhere): I don't think that this point can be made conclusively: O_2 concentrations were not actually measured, and winds described above in the methods section could create turbulent mixing in this standing water (others, e.g., Strack et al. Global Biogeochemical Cycles GB4003, have reported standing water in a boreal peatland to be a zone of CH_4 oxidation). There appears to be a real difference in the water table elevations between these two vegetation communities (perhaps as much as 10cm mean difference) in Figure 3, with a lower (as low as 2-3cm above the surface) and more variable water table in the sampling locations that also had smaller CH_4 effluxes. One might expect water table position to play a role in observed differences in flux rates. These points are one of the key reasons for my asking the authors to be more cautious in their conclusion about the role of Sphagnum presence or absence as the key driver of differences in observed CH_4 efflux.

P8525 ca. Line 17: What impact could human disturbance associated with collar installation and chamber measurements have made? Were boardwalks present? If not, and perhaps particularly in flooded conditions, CH_4 ebullition could have been triggered by walking (repeatedly) to these measurement areas. Also, do you think that soil compaction occurred during sampling and would this have had any important effects?

Page 8529 Line 15 (and elsewhere): "microbiological analysis" is not an appropriate description of what was actually done. Please reword to (some variation of) "CH_4

consumption potential measurements". Could there have been issues associated with sampling peat in 2008 for these measurements while flux measurements appear to have been made in 2007? As well as with other concerns I express below, there might be problems with assumptions that the methanotroph community, biomass, and activity rates are stable over time.

Page 8530 Line 11: Were these always linear? (my 'back of the envelope' calculations might be wrong here, but): the in vitro oxidation rates seem quite fast; if only about 50micro mol CH_4 was added to each vial and oxidation rates were about 50 micro mol/g dry peat/day, I would expect to have seen an exponential decrease in mass of CH_4 over the time frame of the incubations. If this was the case, it might be best to calculate linear rates over an earlier portion of the incubation or to fit a non-linear regression model. Interestingly, the CH_4 oxidation potential rates reported are very similar for those that colleagues and I have measured in portions of submerged, but not emergent, Sphagnum majus in an inundated margin of a southern boreal Canadian bog (See Fig 4b in the cited 2004 Wetlands paper).

Page 8530 Lines 12-19 and elsewhere: I really think that it is too far of a stretch to compare CH_4 oxidation/consumption potential measurements to field efflux rates in an absolute sense. At best, when performed over short/initial periods of time following sampling, this lab assay can report relative rates of CH_4 oxidation given large/non-rate limiting amounts of CH_4 and O_2. The cited Sundh (1995) paper indicated that this might represent relative viable methanotroph biomass only as I recall (and many other subsequent publications have generally corroborated this). Because "low-affinity" methanotrophs typically exhibit first-order (substrate-availability controlled-) CH_4 oxidation dynamics, the rate observed in the lab where plenty of CH_4 and O_2 is supplied evenly to mosses can't represent the conditions in the field (that is, the entire mass or volume of the 3-dimensional 0.25m2 "cube" of Sphagnum is not evenly exposed to 10,000ppm CH_4 and 200,000ppm O_2). Again I strongly feel that these measurements can only be used to illustrate potential relative differences across treatments or environments. This was not done, and the quantitative scaling-up is inappropriate, I am afraid. As such, I suspect that the data by themselves are not very useful. Perhaps a comparison could be made to potential rates published using similar techniques in other Sphagnum-dominated wetlands. If not, I suspect that this section should be removed from the paper.

P8534 ca. Line 25: Would physically removing Sphagnum be a potential (and easy) means of testing its role more directly/experimentally?

P8535 Lines 1-14: I fully agree that this supports the conclusions of the paper (i.e. that moss cover and not vascular cover in important in observed CH_4 emissions), however given the very short duration of the study and that one site with relatively few measurement locations were observed, one must be more cautious in relying on such a coarse relationship (e.g. that % cover would conclusively predict variability in short-term net CH_4 emissions across a small spatial scale). Likewise with the model results, they certainly support the conclusions, but are based on a simplified/idealized understanding of CH_4 dynamics in these systems. Again, I feel that the appropriate way to deal my concerns is to report the conclusions (in the abstract, discussion, and conclusions sections) a bit more cautiously.

P8535 ca. Line 25 (and elsewhere): The emphasis here seems to be on the potential for vascular plants to allow CH_4 to bypass CH_4-oxidation, but roots also likely represent a key source of relatively labile C substrate that could increase rates of methanogenesis. I think this could also potentially be important to consider.

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