Biogeosciences Discuss., 5, S3444–S3452, 2009
www.biogeosciences-discuss.net/5/S3444/2009/

Biogeosciences Discussions

BGD

5, S3444–S3452, 2009

Interactive Comment

Interactive comment on "Methanotrophy potential versus methane supply by pore water diffusion in peatlands" *by* E. R. C. Hornibrook et al.

E. R. C. Hornibrook et al.

Received and published: 2 July 2009

We thank the referee for her positive review and constructive suggestions for improvement of our manuscript. Revisions made to the manuscript in response to reviewer's comments are described below. We provide explanations for instances where we do not concur with recommended changes.

J. Limpens (Referee 3) (Received and published: 18 July 2008) *1. Site descriptions. Could you perhaps indicate dominant plant species/ vegetation types at the measurement points? Alternatively give cover mosses/ graminoids/ ericoids*

Response: Information about ground cover at stations 1 and 2 at each peatland is provided in Table 3, footnote a. No further quantitative information is available about relative cover of different plant types at the stations. Details about dominant plant species in each of the four peatlands have been added to Table 1 (as requested by Reviewer 4, Comment 1).

2. Were board-walks installed around the sampling locations?

Response: Yes, temporary board walks were installed around the membrane equilibrators and flux collars at each site several months before sampling began.

3. 3.4 How were the porosity measurements performed?

Response: See the response to Comment 2 by Reviewer 1.

4. 3.5 At which depths/depth intervals was T measured?

Response: At each peatland soil temperature was measured at depths of 5, 10, 20, 30, 40, and 50 cm and then every 25 cm to either 200 cm depth or the peat-sediment interface.

5. Perhaps you could refer to moss surface instead of ground surface in line 29

Response: The requested change has been made.

6. 3.7 Perhaps you could indicate how the temperature used for lab incubations relates to field values?

Response: Given the number of methanotrophy incubations that were conducted (2 peatlands x 5 depths x 3 replicates x 7 CH_4 (S_0) concentrations x 4 time intervals = 210 incubation vials and 840 analyses), a single temperature (15°C) was employed in the incubations. From May to September 2003, the range of temperatures in the acrotelm (0 to 30 cm depth) was 7.9 to 20.2°C cm and within the 3 cm depth interval used to integrate CH_4 rates the average temperature ranged from 12.5 to 14.7°C.

7. Were the vials shaken during the incubation?

Response: No, the vials were not shaken. Agitation would have promoted physical mixing and enhanced gas transfer in contrast to in situ conditions where gas movement

BGD

5, S3444–S3452, 2009

Interactive Comment

occurs primarily via diffusion.

8. 3.8 Could you perhaps indicate the goodness of fit for the fitted relationships (where possible)? What is the uncertainty?

Response: The regression analyses of [CH_4] pore water concentration data had the following r^2 values: Fig. 3 Crymlyn Bog 0.89 to 0.99; Fig. 4 Gors Lwyd 0.82 to 0.99; Fig. 5 Blaen Fign 0.92 to 0.99; and Fig. 6 Cors Caron 0.78 to 0.99. Only 4 of 33 regression analyses had an r^2 value < 0.90. The standard error (95% confidence interval) associated with each regression analysis was integrated into the diffusive flux rates calculated using Eq. (3) (formerly Eq. (2)) and are incorporated in the +/- error reported in Table 3.

9. Could you indicate why you used different depth intervals for calculating the value of D? What were the criteria?

Response: The diffusion coefficient for CH_4 in water-saturated peat (D_S) is dependent upon both temperature and porosity (Eqs. 3 and 4 on pages 2617-2618 (now Eqs. 4 5 in the revised version)). Porosity varies between months because of swelling and contracting of the peat soil due to differences in moisture content. Temperature vary significantly both daily and monthly. Because the depth interval below the zone where [CH_4] = 0 μM typically changed from month-to-month (i.e., the depths for which pore water [CH_4] data were linearly regressed to determine the [CH_4] gradient for the Fick's diffusion calculation), we calculated values of D_S that were specific to that interval based upon measurements of soil temperature and porosity.

10. 4.3 You indicate that values above 100 μmol/l of methane in the oxic/anoxic zone are rare. In your figures v3 and 4 this concentration does not really seem to be so rare.

Response: The problem lies in the sentence: [These anomalous values appear to result from the disproportionate effects of high CH_4 oxidation rates determined from the small number of incubations having S_0 values >100 umol l^{-1}] where [S_0 values >100

BGD

5, S3444–S3452, 2009

umol l^{-1}] refers to the S_0 = 250 and 500 umol l^{-1} incubations. Such concentrations of CH_4 in pore water are uncommon at the oxic-anoxic transition. We have changed the sentence to: [These anomalous values appear to result from the disproportionate effects of high CH_4 oxidation rates determined from the small number of incubations having S_0 = 250 and 500 umol l^{-1}] and the subsequent sentence to begin [Such concentrations of CH_4 are uncommon in situ at the oxic-anoxic interface in peatlands.]

11. Especially as you do not really know to what extent (and depth) methane production and consumption occur spatially very close to each other (see Knorr et al. in special issue) in the much more structured in situ peat soils Could differences in measurement technique (indicated by you in the introduction - Popp et al. 2000) explain the (very) high values that you found instead? Or are the values that you compare your data with also derived from lab incubations?

Response: We assume that the [(very) high values] the reviewer refers to are CH_4 oxidation rates that we determined in laboratory incubations. This issue has been addressed in the response to Comment 5 from Reviewer 1. Again, to our knowledge there are no in situ methods for determining CH_4 oxidation rates and kinetic parameters (as opposed to methanotrophic efficiency). We used a conventional incubation technique to measure rates of CH_4 uptake and were conservative in applying those rates to determine the capacity for CH_4 oxidation in situ (Section 4.4, pp 2620-2621). We also ran 0 μmol l^{-1} CH_4 incubations (i.e., a headspace of zero air) to ensure that net production of CH_4 was not occurring within the unstirred CH_4 oxidation incubations.

12. 4.4 Page 2621 lines 3-17. Perhaps this part of the text can be moved to methods?

Response: Integration of CH_4 kinetic parameters and pore water [CH_4] data in the 3 cm interval below $[CH_4]_0$ is secondary data analysis. We prefer to leave the description of this aspect of the study in the Results section.

13. There is also something I do not fully understand, but this could be due to my relative unfamiliarity of the field. You took 3 cm as this is the minimum depth for O_2 to

BGD

5, S3444–S3452, 2009

diffuse into a waterlogged (?) peat soil. Yet from your results we see that the $CH_4$0 point often lies much deeper than 3 cm below the water table, suggesting deeper O_2 penetration. From your results 5 to10 cm (excluding rain influence). Is it possible to indicate what would happen with your results if you would take 10 cm instead of 3?

Response: A depth interval of 3 cm was used to integrate laboratory determined CH_4 oxidation rates with in situ CH_4 concentrations via Eq. 1 (now Eq. 2). A depth of 3 cm was used, in part, because according to Beckmann and Lloyd (2001) O_2 should diffuse at least that distance into wet peat soils. Thus, the reviewer is correct in stating that O_2 penetration into soil may have been deeper; however, by increasing the integration interval to 10 cm, the oxidation capacity would increase significantly because the volume of peat involved would be 3 fold greater. Our aim was to provide a conservative estimate of CH_4 oxidation capacity. The smaller 3 cm depth interval demonstrates that given sufficient O_2 (which can be reasonably assumed in a 3 cm interval) and favourable temperature conditions, methanotrophy is capable of removing effectively all CH_4 that diffuses upwards from the catotelm.

14. 4.4 Page 2621 lines 23-26. How many chamber measurements were omitted? Perhaps this information can be give in the figure legend or methods instead?

Response: Chamber measurements exhibiting signs of ebullition (real or inadvertently induced) were excluded primarily from the Gors Lwyd data set (10 of 24 chamber deployments were excluded). The site has an unstable surface (i.e., it partially floats in places) and the peat soils are rich in gas bubbles during summer months. In contrast only 1 set of chamber measurements was excluded for Cors Caron (1 of 24) and none was omitted from the Blaen Fign and Crymlyn Bog data sets (0 of 30 and 0 of 24, respectively). This information has been added to footnote [d] in Table 3 and the footnote is referred to in the text on page 2621.

15. Could you speculate on the potential effect of porosity on the effect of precipitation on your data in Fig 7? I can imagine that if you have a low porosity the effect of

BGD

5, S3444–S3452, 2009

precipitation could be higher than with a high porosity. About the rain effect. Would it be mainly an increase in methane consumption or an increase in ebullition? Or both?

Response: Less porosity would increase the water table level for the same quantity of precipitation input. The trends shown in Figure 7 are similar for Blaen Fign, Cors Caron and Gors Lwyd, which all had a similar range of porosity in their acrotelms (0 to 25 cm depth) from April to September 2003 (BF = 0.856 to 0.935; CC = 0.846 to 0.931; GL = 0.938 to 0.955). Peat within the acrotelm of Crymlyn Bog was more decomposed and compacted, and thus had lower porosity (0.758 to 0.894). However, the difference in the Crymlyn Bog curve in Figure 7 is best explain by the peatlands topographic position (discussed on pages 2622-2623) and input of groundwater into the site. The rainfall effect shown in Figure 7 most likely reflects simple addition of oxygenated precipitation on top of the existing [CH_4] profiles in the peatland. The point being made is that the depth of $[CH_4]_0$ can vary substantially and rapidly from rain water input rather than microbial processes. Addition of oxygenated rain water may enhance rates of methanotrophy but a higher water table level most likely will suppress ebullition rather than increase it.

16. Conclusions I suggest stressing in the first paragraph the importance of micro-disturbances or water residence time (water table fluctuations/ precipitation/ water movement) for maintaining high methane oxidation rates. When relying on diffusion alone, all processes seem to peter out.

Response: It is unclear what the reviewer is suggesting be added to this paragraph. We already discuss relationships between water table levels and CH_4 oxidation that can be reasonably concluded from the data presented in the manuscript. Our data do not suggest that methane oxidation would cease if it were to rely solely upon diffusion of O_2 from above and CH_4 from below. Rainfall events most likely delivery additional O_2 to the subsurface and we note in the last sentence of the paragraph that a lack of precipitation probably promotes CH_4 diffusion across the water-air interface but it would be difficult to state anything more definitive based upon data presented in the

BGD

5, S3444–S3452, 2009

Interactive Comment

paper.

17. Table 4: Please give n for the relationships. I suggest removing the second remark (b). Since you would have only 2 points left, it is no wonder you would get an r^2 of almost 1.

Response: The value of n has been add in brackets for each equation in Table 4. The footnote has been retained because exclusion of the 25 August 2003 data point leaves n = 3 which is why $r^2 = 0.99$ and not 1.00.

18. Introduction: could be written a bit more focussed. I also miss a sentence where the main aim of the paper is stated - the knowledge gap filled by the paper is stressed. - I suggest: Deleting Page 2609, lines 13: (More than three decades) to .line 25 ..(Chen et al. 2008), as the overview of microbiological research, albeit interesting, does not seem to be very important for the main message of the paper. - Deleting Page 2610, lines 5 to 10 ..Reeburgh, 2000) and lines 23 Despite to line 27 Christensen, 2007) - Moving Page 2610, line 27 In the absence of to Page 2611 line 27 forward to Page 2610 line 5. - Reducing the number of references referring to the high variability of methane consumption to two sentences or something (now about 15 lines). - Adding a line stressing importance underlying research/ main aim research after Page 2611, line 27.

Response: The above comments all relate to eliminating or re-organising content in the Introduction section. The Introduction to the paper is broad but our aim was to be thorough in reviewing relevant literature to lay the groundwork for the paper. The flow of information would be negatively impacted by the recommended changes and we prefer to leave the section as is. We agree though that the purpose of the manuscript could be more explicitly stated and thus, have rewritten the last paragraph of the Introduction section (pages 2611-2612) to clarify the aims of the study.

19. 3.1 Maybe you could indicate the sampling dates for the different locations here instead of in the legend of fig 2

BGD

5, S3444–S3452, 2009

Interactive Comment

Response: The recommended change would greatly cluster the text. The detailed record of sample dates has been left in the caption of Fig. 2.

20. 3.7 line 12: control to confirm absence of net CH_4 production. Insert net? Reason: there could be production, but it could be consumed so no net production.

Response: We agree. The word [net] has been inserted.

21. Results 4.2 I suggest moving lines page 2618 line 22-page 2619 line 7 to first figure legend. In consecutive figures you could then refer back to this first legend.

Response: A similar change was recommended by Reviewer 1 (Comment 3). We have shortened this section of text and moved some of the content to the caption of Fig. 3 as recommended.

22. Discussion 5.1 Page 2622 lines 7-14 I suggest moving this to end of paragraph in order to stress your own results more (Page 2623, line 11)

Response: The recommended change would diminish the clarity of the message in the paragraph. We prefer to keep the paragraph as it is presently written.

23. 5.3 Page 2626 Typo on line 1: surficial should be superficial

Response: [Surficial] is the correct term. We meant emissions from the ground surface.

24. Fig 2: The readability of the figure could be improved by giving the precipitation data during the growing season alone. Furthermore I would suggest removing all the sampling dates in the legend.

Response: The figure is legible in its present form and we feel strongly that research details such as sampling dates should be reported in publications for completeness and accuracy.

25. Fig 3-6 I would suggest referring in figure legends of 4-6 to the legend of fig 3 for the extensive explanation

BGD

5, S3444–S3452, 2009

Interactive Comment

Response: This change has been made. See response to Comment 3 from Reviewer 1 (and Comment 21 above by this reviewer).

Interactive comment on Biogeosciences Discuss., 5, 2607, 2008.

BGD

5, S3444–S3452, 2009

Interactive Comment