

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

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MS-NR: bgd-2008-0036 Version: 1 Received: 4 March 2008, 12:54 CET Title: Methanotrophy potential versus methane supply by pore water diffusion in peatlands Author(s): E. Hornibrook, H. Bowes, A. Culbert, and A. Gallego-Sala

General comments

The authors present field measurements on methane concentration as well as incubation data on potential methane consumption at different depths. Field sampling occurred at 4 different sites and at different times during the growing season. Part of the data has been reported elsewhere, but enough new data are reported to justify a new paper. The study improves our knowledge on methane consumption in the up-

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per soil profile and reports interesting effects of precipitation on methane profiles and consumption.

Specific comments

2 Site descriptions. Could you perhaps indicate dominant plant species/ vegetation types at the measurement points? Alternatively give cover mosses/ graminoids/ ericoids Where board-walks installed around the sampling locations?

3.4 How were the porosity measurements performed?

3.5 At which depths/depth intervals was T measured? Perhaps you could refer to moss surface instead of ground surface in line 29

3.7 Perhaps you could indicate how the temperature used for lab incubations relates to field values? Where the vials shaken during the incubation?

3.8 Could you perhaps indicate the goodness of fit for the fitted relationships (where possible)? What is the uncertainty? Could you indicate why you used different depth intervals for calculating the value of D? What were the criteria?

4.3 You indicate that values above 100umoll/l of methane in the oxicanoxic zone are rare. In your figures 3 and 4 this concentration does not really seem to be so rare. 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?

4.4 Page 2621 lines 3-17. Perhaps this part of the text can be moved to methods? There is also something I do not fully understand, but this could be due to my relative unfamiliarity of the field. You took 3cm as this is the minimum depth for O2 to diffuse into a waterlogged (?) peat soil. Yet from your results we see that the CH40 point often

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lies much deeper than 3 cm below the water table, suggesting deeper O₂ penetration. From your results 5 to 10 cm (excluding rain influence). Is it possible to indicate what would happen with your results if you would take 10 cm instead of 3?

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

Conclusions I suggest stressing in the first paragraph the importance of microdisturbances 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.

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 r² of almost 1.

Technical comments

1 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

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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.

3.1 Maybe you could indicate the the sampling dates for the different locations here instead of in the legend of fig2

3.7 line 12: control to confirm absence of net CH₄ production. Insert net? Reason: there could be production, but it could be consumed so no net production.

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.

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)

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

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.

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

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