**Biogeosciences
Discussions**

# *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.

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We thank the referee for their 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.

Anonymous Referee #1 (Received and published: 7 July 2008) 1. Page 2613, lines 10-15: Pore water profiles were collected throughout the summer of 2003 and $CH_4$ kinetics determined from cores collected in 2005. How appropriate is it to apply the $CH_4$ kinetics from 2005 to calculate oxidation rates for comparison to $CH_4$ diffusive flux from 2003? To answer this you need to consider; how variable is $CH_4$ concentration profile between years? How do $CH_4$ oxidation kinetics vary between years? How do $CH_4$ oxidation kinetics vary with time over the growing season (i.e. can September

kinetics be applied throughout the summer?).

Response: The referee makes a valid point about the application of CH4 kinetics data determined on peat collected in 2005 to CH4 pore water concentration data collected in 2003, which is why we noted on page 2613 (lines 10 to 15) that the two data sets are not coeval. Pore water CH4 concentrations change from month-to-month in peatlands. We do not have the data to establish whether CH4 kinetics vary in a similar manner. We were unable to conduct the methanotrophy kinetics measurements in 2003 and thus, have only the data from 2005 for these sites. Such data are scarce for a single year for peat soils and to our knowledge multi-year CH4 oxidation kinetics for an individual peatland site have not been published to date. While our analysis has its limitations, it does provide a starting point for future studies to compare.

2. Page 2618, line 7: How was porosity determined in situ; I do not think I saw this explained anywhere. It would also be useful to give a range of the values you determined in the text somewhere.

Author Response: The following text has been added to Section 3.4: Subsamples were extracted from the peat blocks in the laboratory to determine bulk density (g cm-3) and porosity (unitless). A specific volume of peat was dried to constant weight to determine bulk density. Porosity was calculated from the equation:

Porosity = 1 - (bulk density/particle density)

Where particle density was assumed to be 1.4 g cm-3. The section title has been changed from: 3.4 Peat cores to 3.4 Peat cores and properties. The porosity values are used in the Eq. (5) (old Eq. 4) to correct the coefficient for diffusion of CH4 in water and are not used elsewhere in the manuscript in a raw form. Hence examples of the range of values for the sites are not listed anywhere in the paper (although the ranges of values for peat in the acrotelm of the four sites are listed in the response to Comment 15 from Reviewer 3).

3. Pages 2618-2619: Most of the first paragraph in section 4.2 should be moved to the figure caption (a lot of it is already in the captions and is thus repetitive here). Right now when I look at the figures I do not know what the grey bar was, because that information is not in the caption - only the text. So, moving this to the caption not only tidies up the text, but also helps make the figures understandable without referring to the text.

Response: The suggested change has been made. Also, common legend details for Figs. 3 to 6 are summarized in the caption of Fig. 3 and then referred to in the captions of Figs. 4, 5 and 6 as suggested in Comment 25 by Reviewer 3.

4. Page 2620-2621, section 4.4: I have several questions about this section and the comparison of the diffusive flux and calculated methane oxidation. Firstly, what was the criteria used when determining the concentration gradient used for the Ficks Law calculation? Looking at Figs. 3-6 sometimes the regression extends quite deep, other times just to within the 3 cm layer below the CH4 (0) level and sometimes it seems to ignore some measured values (Figure 5a). I think if would be useful to give some more details in the methods on how these were determined.

Response: The linear regression analyses to determine CH4 concentration gradients were performed in most cases on pore water [CH4] data in the 5 to 10 cm depth interval immediately beneath the depth where [CH4] = 0 $\mu$M. Typically CH4 concentration profiles possessed this short linear interval underlain by higher concentrations of CH4 exhibiting a greater scatter of values (e.g., profiles in Figs. 3b-c, 3f-g, 4a-h, 5c-f, 6a-b, 6e). In some instances the regression analysis extended deeper (e.g., Figs. 3a, 4i-j) because the linear concentration trend continued with depth and extending the regression had no impact on the calculated rate of CH4 diffusive flux. In a few instances, the regression was deeper because a data gap occurred immediately beneath the depth [CH4] = 0 $\mu$M and that was our only means to determine a [CH4] gradient (e.g., Figs. 6d, 6f, 6h). In Fig. 5a the reviewer has identified the one instance where we made a judgment call to exclude a shallow high CH4 concentration point in favour of a clear

linear trend that existed below the high concentration point. We did so because of observations during sampling which suggested gas bubbles were present at that depth. In none of the other 32 profiles does a similar situation occur. In general, if more than one option existed for fitting of the regression curve through CH4 concentration data beneath the depth [CH4] = 0 $\mu$M, the steepest [CH4] gradient was selected (e.g., Figs. 4e-f, 6a-d) because of our contention in the manuscript that pore diffusion transports negligible quantities of CH4 relative to plant-mediated flux or ebullition and thus we thought it best to err on the side of maximizing pore water CH4 diffusion rates.

5. Secondly, how appropriate is it to apply the kinetics determined in the lab to the field to determine CH4 oxidation potential rates? I am not surprised that the potential CH4 oxidation rate is always higher than the diffusive flux because disturbance effects in the lab may overestimate CH4 oxidation potential and oxygen in the field may be limiting even within the 3 cm layer. I think you have shown pretty clearly by referring to your isotope data that most if not all the CH4 diffusing across the water table interface is actually oxidized in the field, but I think some recognition of these shortcomings is warranted in the discussion.

Response: While methods exist to determine the efficiency of methanotrophy in the field, there are no techniques to our knowledge that allow CH4 oxidation rates and kinetics to be measured in situ. We agree with the reviewer that laboratory determinations of CH4 oxidation rates are potential rates that almost certainly are higher than in situ rates. We did not measure [O2] in situ and cannot comment whether rates of methanotrophy in the peatland were O2 limited. As noted by the reviewer, stable isotope data published previously for the sites (Bowes & Hornibrook, 2006; Hornibrook & Bowes, 2007) confirms that diffusive transport of CH4 contributes little to CH4 surface flux. Moreover, the depth of [CH4] = 0 $\mu$M relative to the water table level also supports that conclusion (i.e., CH4 diffuses across the water-air interface in only 5 of the 33 profiles). We noted already in Section 4.4 (pp. 2620-2621) that CH4 oxidation rates determined in the laboratory have been integrated across a 3 cm thick section of peat

using in situ CH4 concentrations and equation (1) in order to generate a conservative estimate of methanotroph capacity for CH4 consumption. Also discussed in that section is that our estimates of CH4 uptake using this approach are significant less than other attempts to integrate CH4 oxidation rates in peat soils reported in the literature yet still greatly exceed the supply of CH4 that diffuses into the oxic-anoxic transition zone.

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