Biogeosciences Discuss., 5, S1736–S1739, 2008
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Biogeosciences Discussions

Interactive comment on "No detectable aerobic methane efflux from plant material, nor from adsorption/desorption processes" *by* M. U. F. Kirschbaum and A. Walcroft et al.

M. U. F. Kirschbaum and A. Walcroft et al.

Received and published: 19 September 2008

General comments:

We are pleased with the generally favourable assessment by the reviewer.

Specific comments:

1. The reviewer questions our assumption with respect to the applicability of methane adsorption /desorption observed on filter papers to plant tissues a) The reviewer states that we assumed that "in plant tissues, molecule adsorption/desorption were not related to numerous metabolism processes". The important point is that we found negligible adsorption / desorption. That means that any observed emissions can now be

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related directly to metabolic processes without needing to consider the confounding effect of adsorption / desorption.

b) The reviewer states that we assumed "that maximal CH4 concentration in plant tissues/materials were close to the atmospheric concentration". It is indeed true that we assumed that the methane concentration inside any measured tissue would come to equilibrium with the concentration in the surrounding air, or at least that the effective concentration that comes to equilibrium with adsorption sites would change with that in the surrounding air. We still believe that this assumption is likely to be true. Hence, we do not understand why that assumption, or any departure from it, would cause any problems with the interpretation of our findings.

c) The review states that "CH4 concentration were even distributed in plant tissues/ materials". As for point b), we do not see how that consideration would negate our interpretation.

2. The reviewer states that "on each measurement occasion, individual data points scattered around mean values by 20-40 ppb (Page 2781, Line 16-17). Could you explain it? 40 ppb scatter is approximately equal to an increasing of mean CH4 concentration during four days (Fig. 2). We worry that measured scatter largely disturbed to acquire an actual mean value". The scatter appeared to be random and corresponds to the resolution of the instrument. The experiment had been designed to measure larger fluxes, which we would have been able to detect more easily against the background noise. So, if we had observed fluxes of a larger magnitude we would have been able to do so with greater confidence. However, as the observed fluxes were close to zero, the measurement uncertainties became disproportionately large.

3. The reviewer states that the "authors also monitored aerobic CH4 emission by plants, although they were very small (Page 2783, Line 12-15). Authors discussed a possibility that it is indeed possible for methane to be produced by plants under aerobic conditions, at least by some plant materials and under some conditions (Page 2785,

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Line 9-15)". This statement was made as part of the Discussion where we reported that other researchers have found aerobic methane release in experiments that had apparently been well-designed and conducted. So, while we found no, or at most extremely small, fluxes under the conditions of our experiment, we are open to the possibility that under certain other conditions, there may indeed be larger fluxes. The recently corroborated findings of aerobic methane release under the influence of UV radiation may be such a condition that differs from the conditions in our experiment which was conducted under low-light conditions.

4. The reviewer states that the "authors presented some explanations on gas leakage (Page 2783, Line 16-22). I once compared gas leakage between using plexiglass chamber and quartz glass chamber, and found significant leakage in plexiglass but no leakage in quartz glass. Plexiglass probably has numerous nano-pores, through which gases easily run due to pressure differences between inside and outside. For a fine experiment, it seems that plexiglass is not the best materials". This is an interesting observation by the reviewer, but we were unable to locate any literature references where such observations have been reported or quantified. We recognize that plexiglass is not an ideal material for our type of experiment, but we believe that we were able to adequately deal with any potential leakage problems by using blank chambers. Issues such as possible diffusion also only emerged as possible problems because measured adsorption/ desorption fluxes were negligible. Had those rates been more substantial then any measurement uncertainties due to leakage or other causes would have been minor.

5. The reviewer states that "in addition, some references were incompletely understood and wrongly described. In Wang et al. (2008), for example, a key result is that some xerophyte shrubs directly emitted CH4 in aerobic conditions (see Table 1 in Wang et al., 2008). The statements, It is particularly interesting that only one of the studied species produced methane while the others did not (Page 2775, Line 17-18) and The apparent storage of soil-derived methane in woody stems (Page 2784, Line 20-21),

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are not true". In re-reading the work of Wang et al. (2008), we recognize that it is not plants with woody stems, but the plants with non-woody stems that store soil-derived methane. That error has now been corrected. We have also changed the first mentioned statement to "one of the studied species was clearly shown to produce methane while most of the others did not".

Interactive comment on Biogeosciences Discuss., 5, 2773, 2008.