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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.

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General Comments: We are pleased with the generally favourable assessment by the reviewer. This is a fast-moving field, and we recognise that a few key papers have been published since our paper had been published on the Biogeosciences web site. These new papers have now been incorporated into the revised final version of the paper.

Specific Comments: 1. The reviewer states that "the satellite observations of plumes of methane from tropical forests (Frankenberg et al, 2005, 2006) have now been qualified (Frankenberg et al, 2008) by a report of problems with data retrieval such that the methane observations above tropical forests may not be so large as previously reported. The manuscript should include reference to this latest development". We

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thank the reviewer for bringing this latest publication to our attention, and the revised manuscript has been amended to reflect the updated findings of Frankenberg et al. (2008).

2. The reviewer states that "there has been a further publication by Keppler et al (2008) using isotopic labeling demonstrating the role of UV radiation in methane production from pectin. There has also been a publication by McLeod et al (2008) reporting the role of solar UV radiation in driving methane emissions from plant pectins and live tobacco leaves at ambient UV levels including sunlight. These new publications and their observations should be incorporated into any revised version of the manuscript. In particular, the authors might consider whether the range of evidence for a potential role of UV might now modify their concluding suggestion that a coherent picture is not emerging. Those studies that did not include UV irradiation (this manuscript, Dueck et al, 2007; Beerling et al, 2008; Wang et al., 2008 - the latter in the dark) all report no or only one species showing methane release. However, studies in sunlight or with UV appear to have all demonstrated some methane release, although experiments in sunlight could also heat samples". We agree with the reviewer that these newest publications sufficiently confirm that aerobic methane release is indeed possible under UV radiation. When we wrote our statement, we only had the work of Vigano et al. (2008) to draw on. While that work already appeared to be sound and convincing, we were still guarded in our statement about the involvement of UV radiation. With the work of Keppler et al. (2008) and McLeod et al. (2008) and further, so far unpublished, work, the pivotal role of UV radiation in driving aerobic methane release is now firmly established. We have consequently amended our previous cautious and guarded statement about methane release under the influence of UV radiation and are now more definite in accepting it as an accepted mechanism.

3. The reviewer states that "it would be helpful to state the diameter of the cylindrical chambers as well as their volume". The diameter of our chambers has now been added to the paper.

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4. The reviewer states that "the authors should also state the computer package used for their statistical analysis and a specific and clear statement of the test(s) used in the text. The main text and figure captions introduce some uncertainty/confusion about what was done." We have now provided some further details in describing the statistical tests.

5. The reviewer states that "Fig 1 caption and Results text P2782, L1 needs to restate that test samples were cellulose filters for clarity." That is a good suggestion, and the suggested addition has been made.

6. The reviewer states that "the paper clearly shows the implications of using Plexiglas enclosures and the importance of having empty/control replicates as the empty chambers do show a slow increase in methane concentration. The authors make a valuable discussion of possible leakage of ambient methane into the chambers as a cause but fail to mention the possibility that many plastic materials do release organic molecules that could form a methane source on degradation. There might also be a direct release of methane by (photo)degradation of the plastic material? These effects would be revealed if the methane concentration in their sealed empty chambers increased at the same rate when filled with air at ambient methane concentration. This is just a comment, not a criticism." We tried to deal with whatever fluxes might have been coming from the wall material by using blank chambers, and we believe that this adequately dealt with any potential minor fluxes or leakage rates. It is important to note here that the fluxes from empty chambers were extremely small and they would have been barely discernable if our sample material had released methane at the rates originally reported by Keppler et al. (2006). It is only under the conditions where sample fluxes were negligible that measurement errors appeared to become relatively large.

7. The reviewer states that "my only serious criticism of this paper relates to the statement in the Discussion that 'A stimulation of aerobic methane release by high UV exposure is thus primarily important for dead plant materials as UV radiation itself would likely damage metabolic pathways that might be responsible for methane release in

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intact plant materials.' The published papers on aerobic methane release all show a strong temperature dependence and an increase in methane release up to temperatures that would reduce the activity or destroy most enzymes. This implies that the mechanism may not involve enzymic metabolism. Publications suggest that UV has an impact on structural molecules and methane release may therefore be dependent only on the original metabolic synthesis of the target molecules. UV is always present in sunlight and plants have a number of mechanisms to cope with the potential damaging effects of the UV component of ambient sunlight. Thus, I feel that the author's sentence is far too speculative and does not follow from their careful experimentation and analysis in the paper. In addition, dead plant material, as litter on the ground, does not receive much UV exposure in many ecosystems as it is absorbed by the canopy above. So, I suggest that UV effects on dead material may not be so important and the authors should consider revising this statement." We recognise that important new work has been published over the few months since our discussion paper had been released, and we agree with the reviewer that these latest findings require a reconsideration of the statements we have made a few months ago. We have changed the text of the paper to reflect that new thinking in light of the newest papers.

8. The reviewer states that "Vigano et al (2008) and McLeod et al (2008) both indicate that UVA radiation can also drive some methane release and this should also be mentioned in addition to UVB." References to UVB have now been changed to UV, or specifically state that UV-A is active as well as UV-B.

References Frankenberg C, Bergamaschi, P, Butz, A., Houweling, S, Meirink, JF, Notholt, J, Petersen, AK, Schrijver, H, Warneke, T, Aben, I. 2008. Tropical methane emissions: A revised view from SCIAMACHY onboard ENVISAT. *Geophys. Res. Lett.*, 35, L15811, doi:10.1029/2008GL034300. Keppler F, Hamilton JTG, McRoberts WC, Vigano I, Brass M, Röckmann T. 2008. Methoxyl groups of plant pectin as a precursor of atmospheric methane: Evidence from deuterium labelling studies. *New Phytologist* 178: 808-814. McLeod, AR, Fry, SC, Loake, GJ, Messenger, DJ, Reay, DS, Smith, KA,

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Yun, B-W. 2008. Ultraviolet radiation drives methane emissions from terrestrial plant pectins. *New Phytologist* doi:10.1111/j.1469-8137.2008.02571

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