

Interactive comment on “Methane emissions from boreal and tropical forest ecosystems derived from in-situ measurements” by V. Sinha et al.

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We thank the reviewer for his/her helpful comments. In this reply we would like to clarify and address his/her concerns. Below, (C) stands for the comments of the reviewer and (R) stands for our response.

General Comments:

(C) : The ms under review presents new CH₄ measurements from two contrasting forest ecosystems. The results are interpreted in the way that they are supporting a night time CH₄ source from vegetation.

(R) : We would like to clarify that we have interpreted our new measurements objectively and the results have not been interpreted in the way ” that they are supporting

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a night time CH₄ source from vegetation ". On the basis of the measurements, while we certainly see a net night time ecosystem flux of methane at Hyytiälä, the vegetation flux derived using the land use approach in the manuscript, is clearly within the uncertainty of the wetland and lake fluxes. This has also been categorically stated in the manuscript (Line 24-25, page 14024). As noted in the manuscript (Section 4.1), the in-situ measurements suggest that the contribution of boreal vegetation to the total methane budget is at best very small (less than 1%), and within the uncertainty of the known sources. Such a result is not irreconcilable with Dueck et al. (2007), whose laboratory based study indicated no significant methane emissions from plants, under aerobic conditions.8221;

Specific Comments: 1

(C) : Used calibration gas - Obviously the authors solely relied on the stated accuracy given by the manufacturer and they did not bother to calibrate their methane standard against internationally accepted standard scales of NOAA or SIO. Therefore, any comparison with other data (see e.g. on page 14022, lines 6-10; page 14025, lines 13-25; page 14026, lines 21-25) and any efforts to explain apparent differences or apparent agreements are only speculative, at best.

(R) : The calibration gas used by us was prepared in accordance with ISO norms and certified to be so. While we agree with the reviewer that for comparing global methane trends and concentrations, having a uniform scale (e.g. using standards calibrated against NOAA standards) is desirable and necessary, it should also be noted that in our study the major point of discussion are methane fluxes, and for fluxes it is the relative difference between concentrations that is important.

Earlier the instrument had been calibrated and tested at Thermo Electron Corporation as well, and results of the calibrations using these two different gas standards were always within the uncertainty of both the standards. The close agreement of our results with the Pallas station data (Aalto et al. 2007: see also reply to reviewer 2), which were

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obtained using NOAA standards gives additional confidence in the standard used by us. With regard to comparisons with the SCIAMACHY data (IR based remote sensing data), it is not clear to us as to why not having used a NOAA calibrated standard would render the discussion invalid. If a carefully prepared standard with known uncertainty is employed for measurements, comparisons can certainly be made with measurements obtained using another standard with possibly different uncertainty. The comment of the reviewer is tantamount to stating that only GC measurements calibrated against NOAA standards can be inter compared against each other worldwide!

2)

(C) I am wondering why the authors do not discuss their results in view of actual CH₄ measurements from a nearby atmospheric monitoring station in Finland (Aalto et al. 2007), which would make more sense than the comparison with satellite data which have a high degree of uncertainty (as stated by the authors, page 14022, lines 14-17.) Aalto, T., Hatakka, J., and Lallo, M.: Tropospheric methane in northern Finland: Seasonal variations, transport patterns and correlations with other trace gases, Tellus, 59B, 251-259, 2007.

(R) We thank the reviewer for drawing our attention to this very relevant work. When compared to data from this site shown in Fig. 2 and Table 1 of Aalto et al. (2007), which shows the monthly mean data for April, 2005 (the same month and year as our measurements) the value is approx. 1868 ppbV, which is quite close to our reported average from Hyytiälä of 1830 ± 38.5 ppbV. This will be noted in the revised version.

3)

(C) page 14026, line 5: The estimate of the tropical CH₄ night time emission flux is not based on measurements of the NBL and therefore it is entirely speculative and should be removed from the text and not discussed further.

(R) In accordance with the reviewers recommendation, we shall remove the tropical

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flux discussion in the revised version.

4)

(C) page 14023, lines 8-11: An extrapolation to the global scale cannot be solely based on a single measurement campaign. Unless other measurements are used together with the data presented here, a global extrapolation is misleading, at best. In this point I certainly disagree with the authors argumentation (page 14023, lines 4-8): I would like to see the global extrapolation to be removed from the text.

(R) In accordance with the reviewers recommendation we will remove the global extrapolation of the flux from the abstract, because we agree that the global estimate is a only a potential source estimate. We shall also make this clearer in the text of the revised manuscript. We disagree however, on the issue of not using bottom up estimates at all. We believe that bottom up estimates help estimate potential source contributions and are thus useful information.

5) Technical Comments

In the revised version we shall cite IPCC 2007 (which had not been released at the time of submitting this manuscript to ACPD) and correct the typo of " SCIAMACHY ".

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 14011, 2007.

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