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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Abiotic and biotic control of methanol exchanges in a temperate mixed forest" *by* Q. Laffineur et al.

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General comments:

The paper by Laffineur et al. presents a long-term data set of methanol exchange measured above a mid-latitude mixed forest, and analyses the controls of methanol exchange observed by development of a model to describe adsorption/desorption processes and methanol degradation. The paper will be of high value for the scientific community for several reasons: i) the data presented covers a period of more than 10 month from 2 different growing seasons (2009, 2010) while most data sets for BVOC published so far cover only weeks. ii) Laffineur et al. focus on methanol exchange which currently is under-represented in published studies in spite of the fact that methanol is one of the most abundant VOC. Most papers published so far were

dealing with isoprene and/or monoterpene measurements. iii) The data presented here for the methanol exchange contradicts most other studies available as Laffineur et al. observed the site to be a net sink throughout most of the year (except for the spring/ early summer) while other studies published from various sites and ecosystems refer to methanol emissions during most of the time, and observed negative methanol fluxes only during shorter periods, i.e. at night. iv) The paper presented also analyses and discusses the methanol exchange with the help of a newly developed model, which both addresses adsorption/desorption processes as well as degradation of methanol. The paper thus provides important and useful new information which will be very interesting to the community and certainly will help in analysing/interpreting methanol exchange measurements. In fact it will be very interesting to apply the model to another site. Thus, the paper is certainly well suited for publication in ACP, but I think the authors should clarify the remarks/comments raised below.

General comments:

-P24007, L3pp: as the forest stand is quite tall, you were sampling from the top of a 52m tower with a tubing length of 60m and a inner diameter of 6.4mm. This is a high surface to volume ratio for the sampling line, with a risk of wall losses especially for the methanol signal. Could you provide some more details about the sample line heating mentioned? Did you consider any kind of tests to see if there is significant influence of wall effects to methanol (i.e. in comparison to other VOCs presented in Laffineur et al. 2011)?

-Chapter 2.2: can you provide some comments on calibration and determination of instrument background (zero air measurements)? This might be affecting the measured fluxes/concentrations directly and should therefore mentioned here instead of only referring to the Laffineur et al. 2011 paper.

-Chapter 2.4, wind direction: for the data filtering to exclude data possibly affected by anthropogenic influences you again refer to Laffineur et al. 2011, i.e. filtering was

based on monoterpene concentration variance. Why not apply filtering based on the methanol concentration variance (assuming this effect shows for methanol as well) instead of using monoterpenes? You could then also analyse if filtering for monoterpenes and methanol exclude the same data points. At least, you should provide a figure like Fig. 2 from Laffineur et al. 2011 for methanol.

-Chapter 2.4, u*: you briefly discuss why you did not apply u* filtering here and refer to a publication which currently is not (yet) available, so I ask you to give some additional information here.

-P24012, L15pp: if you mention the detailed mean temperatures and precipitation sums of the different periods involved, you should also give the numbers of the long-term averages to compare with!

-P24013, L14pp: changing wind directions (and possibly advection) in combination with a different species composition in the footprint area might have influenced deposition rates in the beginning and end of the night. Can you comment on this?

-P24014, L2: you refer to Fig. 2 to prove that deposition is connected to periods with precipitation, but the scale of the figure is not really suitable to resolve this. Can you add a specific figure for this where this could be seen clearly?

-P24014, L23: Could you add a figure for the relationship u* and methanol flux? Fig. 5 could be used here, but it provides flux/concentration against u*.

-P24016, L2: Would soil temperature measurements be available to get a better estimate of the water film temperature than air temperature (taken from which height a.g.l?)?

-P24019, L9: You state that your model does not take LAI changes into account. This probably is most relevant during leaf unfolding and autumn, however, growth processes (and thus biogenic methanol emissions) take place during the whole growing season (without a significant change of LAI) and might thus be 'masked' by the adsorp-

tion/desorption and degradation processes in the model.

Detailed comments:

-Fig. 7: if Maw is the total methanol content in the water films, how can it be negative? To me, Fig. 7 seems to show the modelled uncertainty of Maw (which might be about +/- 20 mg/m^3)?

-P24022, L14: Fig. 9 instead of Fig. 10?

-Fig. 1: could you briefly add explanations of variables shown to the caption?

-Fig. 3: at least the summer 2010 figure is a mix of wet and dry periods. You show that deposition is mainly occurring at wet situations, so would it make sense to show an extra figure divided into wet and dry conditions?

-Figs. 4-5, 10: I think A and B to mark panels are hardly visible.

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