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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 presented study examines methanol exchange over a mixed temperate forest in the Belgian Ardennes, covering a total of 10 months in 2009 and 2010. The authors report their site to act as a methanol sink on the long-term scale, a result that contradicts most other studies over forest, where methanol deposition was observed only occasionally. In order to examine the mechanisms responsible for the observed sink behaviour, a simple model describing methanol adsorption/desorption in water films present in the forest ecosystem and further the degradation of methanol was developed.

The presented results of a forest ecosystem acting as a net sink for methanol on a longer time scale constitute an important finding and could further be of importance for the modeling community in better calculating methanol budgets on a global scale. Also, studies on methanol exchange over forest ecosystems are still relatively rare compared to isoprene and monoterpene measurements. Therefore, this study and the presented results are of great interest for the scientific community. I recommend publication in ACP after some clarifications and corrections listed below in the section Specific Comments that the authors might wish to consider.

Specific Comments

P24007, line 8: It would be important to know more about the line heating. Methanol is known to easily stick to tube walls, therefore more knowledge about the (various?) heating temperatures could be an important information. "...the line was slightly heated above ambient temperature." seems too vague.

P24008, line 8: One measurement cycle was 2 s (maybe slightly longer as data had to be stored?), therefore I assume that about 900 data points were used in the flux calculations for each half hour. Provided the lag time was about 15 s another 7 to 8 data points would be lost due to the lag removal. Is this about right? I think the number of data points per half hour that have been used for flux calculations should be mentioned here. Also, I would like to know how many half-hourly flux values have passed all subsequent quality tests and were used for further analyses (see also next comment).

P24008, line 10: More information on the flux calculations is needed. Although the authors refer to the previously published Laffineur et al. (2011), a sentence or two regarding lag time determination, lag time window and also quality controls on fluxes (e.g. stationarity, coordinate rotation...) would be helpful. Regarding the lag time: in the previously published Laffineur et al. (2011) the mean time lag found (for all VOCs?) was 14.8 s, while the theoretical time lag was slightly lower (12.9 s). Lag was searched

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in a specific time window and, if the maximum of the covariance function was not found between 10-18 s, the lag was set to the theoretical time lag. Was this also the case for methanol? If so, it should be mentioned in this manuscript. Also, it would be interesting to know why the theoretical and not the mean time lag was used in case no clear maximum covariance was found.

P24008, line 20, 2.4 Data filtering: One of the crucial points that should be elaborated in detail is the data filtering and I think it is important to provide more information on methanol specific criteria. In section 2.4 the authors write that measurements from the main wind direction could be contaminated by the activities of a wood panel factory situated 3 km from the tower and point out that wood panel production is known to emit high levels of monoterpenes and methanol (Nicholson, 2003). The authors refer to a previously published paper describing isoprene and monoterpene emissions from the same site for data filtering (Laffineur et al., 2011). Fig. 2 in Laffineur et al. (2011) shows a peak of monoterpene emission ratios in the wind section of the wood factory, which is also the main wind direction. This shows that there indeed seems to be an influence of anthropogenic activities on the measurements. In order to increase the trustworthiness of the presented results, it seems important to me to know more about the quality controls used for methanol fluxes. I therefore recommend including a figure like Fig. 2 in Laffineur et al. (2011) for methanol mixing ratios before and after the data filtering which would be of great help in assessing the possibility of human activities on the measurements.

The authors described a filtering criterion based on wind direction only (which would be plausible) as too restrictive and therefore used a quality criterion based on the variance of the monoterpene mixing ratios. With the factory running, this variance was very high for the main wind direction, while it was low when the factory was closed. Therefore, half hours with high variance of monoterpene mixing ratios were excluded from the methanol analysis. One of the flaws of these assumption is the possibility of keeping half hours with constant (i.e. variance is low) high monoterpene emissions due to fac-

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tory production. I would therefore ask the authors to elaborate more on the possibility of having measurements influenced by the factory in their analysis. To what extent can anthropogenic influences be excluded after applying the filter described above? This may be an important question regarding the scope of this publication, as the observed uptake could also be site-specific which could also explain why other sites have not observed similar behaviour. However, even if the methanol sink turned out to be site-specific, it should not distract from the fact that the methanol uptake in itself is an important and interesting observation in understanding biochemical life cycles of plant volatiles. After the exclusion of data with suspected anthropogenic influence the data from the "contaminated" wind sector should yield results very similar to those obtained from "non-contaminated" wind sectors. If the results differ vastly from each other it is possibly due to anthropogenic artifacts in the main wind direction. More information on this issue would be interesting. In order to be able to apply the findings of this study to other forest ecosystems the clarifications described above are of great importance.

P24010, line 16 onwards: With the methanol adsorption/desorption model consisting of two components (ad-/desorption in water films and degradation in aqueous-phase) it would be interesting to link it with a model describing methanol exchange during (relatively) dry environmental conditions.

P24012, line 16-20: Regarding air temperature, please give also numbers for October and spring 2010.

24014, line 2: Due to the size of Fig. 2 it is hard to see the deposition during or following precipitation that is mentioned in the text. If the authors would like to highlight this specific observation an additional figure could be added to illustrate this link.

P24019, lines 9-15: The authors in this study describe a very distinct uptake of methanol by the forest. One could expect that with a pattern this clear a similar sink should have been observed elsewhere at some point – even during considerably shorter measurement campaigns. It might be of interest to the authors that we also

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could not see a clear uptake of methanol over grassland at our study site in Neustift, Austria, in 2008 and 2009 (Hörtnagl et al., 2011), a site characterized by humid continental climate and plenty of dew formation.

P24038, Fig. 7: Please explain the negative values for Maw (negative concentration values?).

Technical Corrections

P24011, line 15: "...with CR0 a residual capacity". Verb missing? If CR0 is the residual capacity of the water films to store methanol this line should be reworded, something like "...where CR0 is the residual capacity...".

P24013, line 20: Change "...in the beginning..." to "...at the beginning...".

P24014, line 5: At some point the abbreviation Rnet should be explained.

References

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