

## ***Interactive comment on “Long term BVOC fluxes above mountain grassland” by I. Bamberger et al.***

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We thank referee#1 for the helpful and detailed comments on the manuscript.

*Comment number 1: p.87 line 5: "PFA" or "PTFE"*

Reply: "PTFA" was changed to "PFA"

*Comment 2: p.87 line 20: lined = switched?*

Reply: "lined" was changed to "switched".

*Comment 3: p.87 line 24: was the calibration gas mixture at ambient or zero humidity? This can/does affect sensitivity for at least some compounds, so should be stated.*

Reply: we added that the calibration was performed at ambient humidity.

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*Comment 4: p.87 line 29: 0.5 s is long for eddy covariance - but the Table shows this dwell time was not used for flux measurements - suggest a minor rewording to improve clarity.*

Reply: this part reads now: "The dwell-time for each single mass channel was 0.5 s or less."

0.5 s is quite long for eddy covariance. We decided to use this rather long dwell time for methanol due to the somewhat reduced sensitivity (poorer mass transmission efficiency) and the higher back ground signal. Hoertnagl et al. (2010) demonstrated that the measured flux is not influenced very much by integration times in the range of 100 ms up to 500 ms. Therefore we are quite confident that 0.5 s dwell time is justified for methanol.

*Comment 5: p.89 top: the choice of lag time is all-important to achieving accurate flux measurements. More detail is needed on how lag times were determined, and the criteria use for exclusion of data when the lag time was not well characterised. Use of an inappropriate lag time leads to a bias low in the flux - some estimate of the uncertainty from the chosen criteria for data inclusion/exclusion should be presented. There also needs to be some justification for inclusion of data for which an appropriate lag time was not available - setting it to the 'value of a neighbouring measurement' (whatever that means) may not be the best estimate. How much of the lag time was created by the flow/sampling system, and how much by the data-handling system?*

Reply: Chapter 3.1 now gives a more detailed description of how we handled lag times. Also, information about the theoretical expected lag time has been added.

*Comment 6: p.90 line 3: at some stage the absolute contribution of low and high-pass filtering on fluxes needs to be presented - although corrections were made, I cannot see easily by how much the fluxes were corrected.*

Reply: More detailed numbers on low and high-pass filtering are now given in chapter

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3.2.

*Comment 7: p.90 line 8: what is the definition of 'significant background drift' - make it quantitative.*

Reply: this statement was replaced by "...a background drift higher than the sum of the standard deviations of the two adjacent background measurements within an hour"

*Comment 8: p.92 line 6: quote actual reported ranges and conditions - give more detail*

Reply: The discussion was extended:

"The maximum of the averaged diurnal methanol flux during the growing period of the grass was  $6.0 \text{ nmol m}^{-2}\text{s}^{-1}$  (observed around 13:30 CET). The observed maximum flux during the growing period is comparable to the  $7.2 \text{ nmol m}^{-2}\text{s}^{-1}$  reported by Brunner et al. (2007) over an extensively managed grassland in central Switzerland. However, our findings are almost two times higher than the  $3.4 \text{ nmol m}^{-2}\text{s}^{-1}$  which were recorded at an intensively managed grassland during the same study, which was cut and fertilized four times a year and covered with a lower diversity of graminoids and forbs than our field site. The extensively managed grassland in Switzerland consisted of a higher diversity of species and its treatment - three cuts a year and no fertilization - was similar to the treatment of our field site (three cuts and one fertilization)."

*Comment 9: p.92 line 17: did the calibration mixture contain hexenal? If not, how was the signal calibrated?*

Reply: The calibration mixture didn't contain hexenal. The revised table 1 contains information on the sensitivities for the different compound masses. For hexenal the sensitivity was calculated based on the sensitivity of acetone, the transmission of the instrument and the fragmentation pattern was taken into account as recorded by Fall et. al. (1999).

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*Comment 10: p.93 line 11: how were these compounds calibrated?*

Reply: This is also addressed in the revised Table 1: The gas standard contained alpha-pinene.

*Comment 11: line 12: methanol concentrations in Figure 8 appear to be in range 4-7 ppb not 7-9 ppb as stated.*

Reply: we corrected the range to 4-7 ppb.

*Comment 12: p.93 line 19: the statement about stomatal conductance and diurnal radiation may be specific to this site and this should be noted.*

Reply: The whole paragraph was rewritten. The corresponding sentence reads now "The opening of the stomata largely follows the diurnal course of radiation at this measurement site (Wohlfahrt et al., 2009) and as expected the methanol emission follows the diurnal pattern of the global radiation (Fig. 9 upper left panel)."

*Comment 13: p.95: the discussion is rather short, and lacks any detailed comparison with the published data on methanol fluxes to/from other types of vegetation. There should be a comparison with absolute measured fluxes, patterns of behaviour (no deposition seen here, but observed elsewhere) and a critical evaluation of the measurements in the light of previous results. Moreover, the only discussion appears to be in terms of methanol, whereas a range of other BVOC fluxes were measured. How do they compare with other grasslands/short vegetation?*

Reply:

We extended the discussion as follows:

Methanol emissions from cut grass:

"Fluxes of methanol (up to  $91.5 \text{ nmol m}^{-2}\text{s}^{-1}$ ), acetaldehyde (up to  $19.4 \text{ nmol m}^{-2}\text{s}^{-1}$ ), and the leaf wounding compounds were also detected during and after the grass-cut of a study site in Switzerland (Davison et al., 2008). In general, the maxima of the fluxes which were observed at the study site in Neustift were lower than the

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ones reported in Davison et al. (2008) which might be due to the differences in plant species and the amount of cut biomass."

Methanol fluxes from growing plants:

"Emissions from growing plants are in fact the largest source of methanol to the atmosphere (Jacob et al., 2005). Fukui and Doskey (1998) reported 2-year average methanol fluxes normalized to 25°C of 3.9 nmol m<sup>-2</sup>s<sup>-1</sup> measured at noon during sunny conditions above grassland in Illinois. Methanol is not only emitted by grasslands but also by other types of vegetation. A subalpine forest in the Rocky mountains emitted methanol up to 8.4 nmol m<sup>-2</sup>s<sup>-1</sup> (Karl et al., 2002b); A mixed deciduous forest in central Europe emitted 2.6 nmol m<sup>-2</sup>s<sup>-1</sup> of methanol on an average day (Spirig et al., 2005). All of these results are in the same range as the variation of daytime methanol emissions from the growing grassland in Neustift. However, at our field site no nocturnal deposition fluxes were seen. Karl et al. (2004), reported methanol deposition fluxes applying the gradient method above a tropical rainforest. Due to limitations of the eddy covariance approach during calm and stable nighttime conditions deposition fluxes are hard to detect."

Monoterpene emissions:

"The monoterpene fluxes from the grassland were close to zero and calculated emissions were always below 0.6 nmol m<sup>-2</sup>s<sup>-1</sup>. This is several times lower than average daytime emissions from forest ecosystems e.g., from a mixed deciduous forest (Spirig et al., 2005)."

*Comment 14: Table 1: could/should show which compounds were included in the calibration mixture, and what the measured sensitivities were (cps/ppb or similar)*

Reply: Table 1 was revised. It shows now sensitivities and how they were calibrated.

*Comment 15: Table 2 (and text): was the discrimination between day and night based on time of day or solar radiation level, and how did this change through the seasons? What do 'min' and 'max' refer to? Does this mean that only 31 half-hour*

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*periods were included from a whole month? The table needs better explanation.*

Reply: We clarified the text: "Minimal and maximal amount of half-hours used to calculate the hourly flux medians for diurnal patterns (Fig. 8) in June and October after applying the quality control on m/z 33 and m/z 137 (partitioned to nighttime and daytime according to median radiation). Each hourly flux median was calculated from at least 18 half-hours."

*Comment 16: Figure 3: vertical axis has unusual units!*

Reply: The label of the y-axis was corrected to nmol m<sup>-2</sup>s<sup>-1</sup>.

*Comment 17: Figure 5: label two halves of graph as 'day' and 'night' to help readers*

Reply: The revised version of the figure is separated to a day part and night part.

*Technical comments:*

All changes proposed in the technical comments were applied.

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Interactive comment on Biogeosciences Discuss., 7, 83, 2010.

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