

# ***Interactive comment on “Acetaldehyde exchange above a managed temperate mountain grassland” by L. Hörtnagl et al.***

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Answer to Reviewer #2

We thank Reviewer #2 for his/her comments and suggestions. In the following we carefully addressed his/her remarks and give an overview of revised sections in the manuscript.

Comment #1: "The manuscript presents another solid study from the Austrian research group, describing OVOC fluxes above the Neustift meadow. The study is strong on the descriptive side but somewhat weak on the interpretive side. The extraordinarily rich, multi-year data base should allow a deeper analysis, but such may still be forthcoming. I suggest publication with relatively minor additions/changes as described below, but

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suggest a major reanalysis as further benefit to the research community.”

Answer: We thank the reviewer for his/her supportive comments. The four-year dataset of VOC fluxes at the study site will indeed be used in future publications, in particular aiming a more process-oriented, modelling analysis.

Comment #2: "Some specific comments 1. The DEC method is now relatively well established; however, I miss a statement in section 2.4 (page 26216) on the estimated flux detection/determination limit and random flux error, based on PTR-MS parameters and ambient turbulence. Turbulent fluctuations close to the surface above the (relatively low roughness) meadow are typical much smaller than above a forest. Such a detection limit and random flux error (calculated using, e.g., varying lag times) is important in the interpretation of the flux data presented, such as in functions of VMRs. Possibly some or even much of the discussion on poor regressions can be scrubbed realizing that fluxes during certain periods are simply insignificant?"

Answer: The flux detection limit was calculated according to Karl et al. (2002b) and amounted to ca. 0.1 nmol m<sup>-2</sup> s<sup>-1</sup>. We included this information in section 2.4 of a revised version of the manuscript. This flux detection limit was used as a post-processing quality control criterion on a half-hourly basis, fluxes below the detection limit were rejected.

Comment #3: "In addition, the authors had the rare opportunity to measure with a PTR-ToF-MS side by side the standard PTR-MS. Unfortunately, they did not name the two months of overlap, so the reader cannot judge whether the statement about CO<sub>2</sub> fluxes made (page 26125) is actually for months with significant net CO<sub>2</sub> exchange fluxes."

Answer: The PTR-TOF instrument was installed in addition to the conventional PTR-MS at the field site for a period of approximately three months from June to August 2009. We added this information in a revised version of the manuscript: p.26125, line 20: "For a period of approximately three months from June to August 2009..." Additionally we added a Figure to the supplement showing the excellent agreement

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between PTR-MS fluxes measured at nominal m/z 45 (assigned to acetaldehyde) and PTR-TOF acetaldehyde fluxes.

Comment #4: "2. The authors cover much ground regarding past measurements and results in relation to their own results. One paper they missed was the study by Schade et al. in Denmark (Biogeochemistry, 106, 2011, DOI 10.1007/s10533-010-9515-5), where the authors showed that externally produced acetaldehyde from a mowed hay field was deposited to the neighboring forest, where the measurements were done, with fluxes seemingly dominated by soil deposition. It is unfortunate that soil deposition was not directly investigated by the present authors as part of their project, although past data and their data showed that such deposition was likely important. 3. A soil deposition analysis could still be done with the data. If the authors are so inclined, they could shorten the lengthy regressions discussions in lieu of a Bayesian statistics analysis using the two dominant factors, plant emissions as driven by SWC and NEE or LE, and soil deposition as driven by VMR and SWC (or SHF & T), as priors, and develop posteriors representative of these two individual flux contributors. I think that would be a much stronger analysis to provide to the flux community. Such analysis could be done for both fluxes and compensation points, although it should be realized that the derived compensation points shown in Figs. 6&7 are net values and thus not representative of either soil or plant values unless one of these contributing compartments dominated the measured net fluxes. Thus, once the analysis is redone, the posterior compartment flux should be plotted against the VMR values for soil and plant compensation points, presuming the Bayesian analysis is successful."

Answer: We included the publication by Schade et al. (2011) in section 1 of a revised version of the manuscript. Acetaldehyde soil deposition could not be investigated at the study site, but soil measurements are planned for future projects. Analyses presented in this study are solely based on eddy covariance data, i.e. represent the "bulk" exchange between the entire ecosystem and the atmosphere. Due to the complete lack of VOC soil measurements or process-oriented models we argue that the suggested

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partitioning is out of scope of this study and that results of additional analyses of our dataset with regard to acetaldehyde soil fluxes would not be defensible.

Comment #5:” 4. Minor points Page 26149: The statement on top including “our hypothesis” has become unclear due to the long paragraph ahead of it. Thus the “hypothesis” needs re-statement. Same page, last sentence: I disagree; I suggest the Bayesian analysis as a starting point. At the minimum, you do know that soil is most likely a sink while the plants are source and can occasionally be a sink. Even using flat priors might work.”

Answer: We have removed the sentence including “. . . our hypothesis . . .”. As mentioned above in our reply to comment #4 we believe that given our generally limited understanding of soil acetaldehyde exchange, the fact that both the soil and above-ground plant parts a priori must be expected to exchange acetaldehyde in a bi-directional fashion and the fact that the eddy covariance method is able to quantify only the net exchange to the entire ecosystem (i.e. soil and above-ground plant parts combined), prevents us from disentangling the soil from the above-ground plant exchange, whatever analysis method is used. In order to understand the soil acetaldehyde exchange (and by difference infer the plant from the total ecosystem flux), we believe it is necessary to quantify the soil acetaldehyde exchange. Either directly, which is difficult to near impossible in grassland where the soil surface is not accessible without disturbing the plant canopy which in turn may modify the soil exchange, or to indirectly, assessing acetaldehyde concentrations of the air-filled pore space and above the soil surface, estimating/measuring soil diffusivity for acetaldehyde and applying Fick’s law of diffusion.

Comment #6:” Page 26151, 2nd paragraph: “: : where decreasing SHF resulted in more acetaldehyde uptake on deposition days.” I am not aware of any physical or chemical mechanism by which a heat flux can “result in” (cause?) deposition. The heat flux may serve as a proxy for the actual mechanism, so these sentences should be rewritten. Also: The “unknown process” of acetaldehyde consumption is generally

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oxidation to acetic acid, so if the authors have m/z 61 and m/z 43 data, they can check on those fluxes to potentially find some clues.”

Answer: We did not intend to imply a cause here and reworded the corresponding sentence on page 26151, 2nd paragraph: “This seems to confirm our findings in Table 1, where decreasing SHF was positively correlated with acetaldehyde uptake on deposition days.” Unfortunately we have not targeted m/z 61 and m/z 43 in our measurements.

Comment #7: “Page 26152, section 5, “Conclusions”: This is written as a summary section, not a conclusion section. If you have conclusions, please present them as such.”

Answer: Section 5 was renamed to “Summary” in a revised version of the manuscript.

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/13/C12066/2014/acpd-13-C12066-2014-supplement.pdf>

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Interactive comment on Atmos. Chem. Phys. Discuss., 13, 26117, 2013.

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