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Atmospheric Chemistry and Physics Discussions

Interactive comment on "First direct measurements of formaldehyde flux via eddy covariance: implications for missing in-canopy formaldehyde sources" *by* J. P. DiGangi et al.

Anonymous Referee #2

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The manuscript by DiGangi describes formaldehyde, HCHO, flux measurements with a new LIF instrument over a pine forest. A well thought through introduction and description of the experiment are presented. The data is analyzed to extract vertical fluxes above the canopy throughout the day. Enclosure experiments of leafs and soil are also presented that supplement the flux data. The authors use a chemical box model to further analyze the data and to test our understanding of HCHO chemistry in a forest environment.

In general this is a very interesting manuscript. The presented data is unique and

should add to our knowledge on HCHO chemistry. The experimental description, as well as the determination of the fluxes is clear and well written. The box model part of the manuscript, on the other hand needs some revisions as it is often difficult to understand how the model calculations were performed. Consequently, I recommend this manuscript for publication in ACP after some revisions. Below are my detailed comments:

Page 18734, lines 0 - 10: Please add the cell length and the air volume of the cell.

Page 18734, line 8,. . . : I am not sure if SLM and SCCM are units understood by all readers. I suggest not using abbreviations, but instead writing them in full word. Alternatively, define the units the first time they are used.

Page 18734, line 15: Is a weekly calibration of the instrument sufficient? Most other instruments calibrate more frequently. Also it appears that with a 4 week long measurement period only four calibrations were performed? How statistically significant is the statement that the calibration only varied by 2.5%?

Page 18735, line 7: Please clarify what you mean by “offline and online sampling”.

Page 18736, lines 5-6: I do not agree with the approach that “OH concentration was assumed to be equal to half the detection limit (2.5×10^5 molec cm^{-3}).” In a statistical sense values below the detection limit are undistinguishable from 0. The only information one can extract from values below the detection are upper (detection limit) and lower (zero) limits for OH concentrations. These limits should then be propagated in any calculations using the OH data.

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Page 18741, line 3: Please explain this more clearly. The decrease in HCHO concentration after 8 am is caused by the rise of the boundary layer. Together with the increasing surface flux, this leads to a peak at 8:00.

Page 18741, line5: This could also be explained by a smaller HCHO loss, for example due to weaker photolysis below the canopy. Please comment on this possibility.

Page 18741, line 19: SO_2 is not a very good tracer for airmass change due to the fact that its atmospheric levels also strongly depend on the presence of SO_2 sources. H_2O, CO, or CO_2 would be better suited for making this argument.

Page 18741, line 28: Was water vapor added to the air flow into the chamber? If not, how does this impact the leaf processes?

Page 18743, chapter 4: In principle it would be more appropriate to use a 1D model, but for what the authors like to achieve here the 0D model may be sufficient. However, the explanations in this chapter are somewhat confusing. The authors have several parameters available at different altitudes in the box, while others were only measured at one altitude. It is often difficult to distinguish altitude dependent terms from those that are not. I propose to add (z) to every term that is altitude dependent and explain this more clearly throughout this chapter.
Equation 3 implies that all terms were calculated altitude resolved first and then integrated. Is this really the case? This should be explained more clearly. The impact of the vertical integration on the results should also be discussed.

Page 18743, equation 3: Please change FHCHO in equation 3 to FHCHO(h) to indicate that the flux at the top of the box is meant.

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Page 18744, section 4.1: I am assuming that the [Ox] term was considered to be constant in altitude. This should be mentioned here. How does the unknown OH vertical profile influence the results of the chemical production? One could expect lower OH below the canopy as all photolytic OH sources will be less efficient.

Page 18745, section 4.2: How does the assumption of altitude independent OH impact the results?

Page 18745, section 4.2: How was the integration of photolytic loss over altitude performed? Was the photolysis frequency or the photolytic loss rate (J_{HCHO}[HCHO]) integrated?

Page 18746, section 4.4: The manuscript nicely shows that there is a considerable HCHO gradient between in and below the canopy. Should the resistance model thus not be applied separately for the canopy region (using the canopy HCHO levels) and the soil (using the below canopy HCHO levels)?

Interactive comment on Atmos. Chem. Phys. Discuss., 11, 18729, 2011.