

## ***Interactive comment on “The bi-directional exchange of oxygenated VOCs between a loblolly pine (*Pinus taeda*) plantation and the atmosphere” by T. Karl et al.***

**Anonymous Referee #1**

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The study provides some interesting data of the exchange of oxygenated volatiles between the plants and the atmosphere. The manuscript attempts a synthesis of lab experiments and flux measurements, whereas the latter data are also being prepared for publication separately. While the overall point of the study is good, the presentation is heavy going, and this reviewer had the feeling that the MS is too heavily loaded with all sorts of information. Putting together flux and lab data highlights several contrasting issues, and at the end I felt confused rather than armed with new knowledge. The way the material is presented further amplifies the heavy style of the MS. All tables have units that need to be multiplied with something and are also far from SI. SI unit

for time is s not h. Unit 1/žC does just not have a place in a scientific journal. If we are interested in the amount of carbon that is emitted/deposited g is just fine, if we are interested in chemistry, we should present the data in mol. The world of units is rich in things like nano, piko and femto confirming to IUPAC standards. I recommend getting rid of the multipliers in units in the text, tables and figures. Table 1 is a complete disaster not only because of the units, but also because of cumbersome reference to data. Why not to put a separate column telling which data are in which row rather than referring to specific lines and columns in each occasion in the text? I fully lost the track in several cases. The data are also not properly defined in this table, E30 in rows 3-4 refers to standardized rate. Then again, in rows 7-8 we have a standardized rate that is E0, which apparently differs from E30 by an order of magnitude (if the multiplier is right). We learn much further in acetaldehyde part that E0 is the intercept. Similar problems are with all other tables and this should be radically improved. I guess that the senior authors can be of help in getting the established standards for presentation right. In the tables, we have flux rates in the same units as in the lab experiments. Why on the earth, we move further to flux units cm s<sup>-1</sup> as the presentation of data goes on?

When I read the results, it occurred to me that it is a combination of Results and Discussion, and I said fine. Then I got through this part, and there was again a part called Discussion. Either the latter part should be removed or the Results should be cleaned from speculations and discussions of other data. In general, Results do not contain a single literature reference. Both parts together right now are overtly long and exhausting.

To further underscore the problems with presentation, Figure 3 is cited first in p5883,126, followed by citing Table 3 in p5884,12. Then Table 2 and Fig. 1 come on p5884,112.

The determinations of the compensation point in Fig. 1 are not always convincing, especially for younger needles. e.g., in lower left panel, flux rate first increases with increasing outside concentration then decreases and then increases again. How does

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this constitute a fit? There are recent detailed models on oxygenated VOC emission/uptake available, partly explaining such effects by stomata. These fluctuations need discussion.

Nowhere is demonstrated how the temperature dependences of the compensation points look like, but the quality of data is crucial. As for this specific figure, the panels should be better labelled to get rid of upper/lower left/right terminology, and also extensive statistics for every point are likely not needed. Simple error bars would be ok.

Several aspects of diurnal variation in fluxes contrast to lab measurements as authors also admit. Can we have at least a rough idea of the breath of error bars in flux measurements, e.g. are really the leaves of Liquidambar a sink during the day? Can the contrasting effects be understood if we look at diurnal courses of humidity and temperature? Right now there are too many things open and the paper may be stronger if the flux data are completely removed (especially given that they will be also published separately) and temperature curves of compensation points provided or the open issues should be furnished with a understandable context.

The difficulties associated with the dichotomy of the data become particularly clear in the Abstract, where lab and field are completely mixed. For instance, Abstract tells of ozone experiment, but it is not clear whether it was conducted in the field or in the lab. From where the evidence for specific statements comes is also not clear. Different sets of data should be better separated.

Conclusions are too long and lack the bottomline of what new we have learned from here and to where we should go.

- p5876,13. MVK+MAC is nowhere defined. Not that it does not make sense to me, but this paper likely tries to address a broad audience, I guess, not only specialists.

- p5876,113-14. This exponential increase is nowhere shown.

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-p5878,l10. This sentence needs help.

-p5878,l19 tells that a mechanistic model has been developed. Why not to use this here or what we need to know beyond this model?

-p5879 add USA to equipment manufactures.

-p5879,l13. What is zero air? Air without CO<sub>2</sub>? or without CO<sub>2</sub>, O<sub>2</sub> and water vapor?

-p5880, middle This effect seems that important that a figure is warranted. How small is small in line 17?

-p5880 last line. This is too specific for those not familiar with Duke FACE. Fingerprint modelling (p5883,l6-7) suggests that at night some of the flux originates from elevated CO<sub>2</sub> site. Could this have an effect on the data?

-p5881 Baker et al . 2005 paper. I would not leave the crucial details for this paper. However, it seems that only the gradient analysis is used in the current MS, why to describe then everything?

-p5882,l1. The problem with Pt catalyser in PTR MS is that it leaves a certain part unburnt and this is strictly concentration dependent. Check it out, accounting for this residual VOC term is important for calibration.

- p5883,l21. Specific leaf weight is a meaningless term. Isn't that Jarvis published in 1985 a paper entitled Specific leaf weight equals 1.0. Always!

-p5884,l25-30. This is not visible in Table 2. -p5906. Figure legend should tell that these are the compounds formed due to oxygenation rather than oxygenated compounds to agree with the main text reference to Holzinger.

-p5888. MeOH. Why not CH<sub>3</sub>OH then.

-Appendix is not needed.

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Interactive comment on Atmos. Chem. Phys. Discuss., 5, 5875, 2005.

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