

This paper describes novel measurements of glyoxal in a region where the VOC reactivity is dominated by biogenic compounds. Because of the wide suite of co-located measurements, the authors are able to make predictions using a 0-D model that is well-constrained by observations. The model is significantly biased high compared to the measured glyoxal, and the authors explore several explanations for this. One of the most interesting aspects of the paper is that model predictions of glyoxal are highly sensitive to OH. This sensitivity is not necessarily linear and depends on the generations of VOC oxidation during which glyoxal is formed as a product and also the importance of OH as a glyoxal sink, relative to photolysis. Could the authors use the model to explore the power of the OH dependence, over a reasonable range, and test whether it is different for isoprene and MBO? While the range of values found may only be applicable to this specific location, it would be interesting to provide context for future analyses that include glyoxal.

In general the paper is well-written, however that authors should address the following issues in a revised submission for publication in ACP.

Page 13665, line 1-14 By what mechanisms are glyoxal and O<sub>3</sub> deposited? Maybe the lack of correlation between the two reflects differing mechanisms, e.g. glyoxal is dissolving in surface water whereas O<sub>3</sub> is being oxidized by BVOC (see Fares et al., 2010). A warmer, drier night might be expected to increase ozone loss rates and decrease glyoxal loss rates. Does the loss rate of glyoxal have any dependence on RH?

Page 13665 Line 20-22. This sentence is a little unclear as written, since it seems that the issue is that the production is slow, rather than that it exists at all. Perhaps end with “as according to the model there was nighttime glyoxal production, albeit at a slow rate of xx.”

Page 13666 Line 22 - When introducing the model mechanism, it would be useful to convey more clearly that the mechanisms are consistent with chamber measurements of glyoxal yield made using the same instrument (e.g. Chan et al., 2009 and Galloway et al., 2011). This is mentioned/implied later in the text and in the conclusions, but would be valuable for readers to know at the outset.

Figure 4 –Would the time-of-day dependence of the predicted glyoxal from the MBO-only curve (not plotted) better match the observations? Could that offer some insight into what the problems are with the model?

Page 13668, line 10 – Can this point be stated more clearly? Do you mean that because the chemistry is in the low NO<sub>x</sub> regime, the main way to reduce higher generation oxidation products is the increase HO<sub>x</sub>-HO<sub>x</sub> chain termination reactions, but that it requires unphysically large rate constants to have an impact? When you say “In addition, the model employed here matched low NO<sub>x</sub> chamber studies of MBO and isoprene very well (Galloway et al., 2011)”, what aspects of the model match the chamber studies – certainly not the glyoxal production.

Page 13670, lines 10-26 - This section is somewhat difficult to follow. Given that the OH measurements are so important, can more information be provided about the technique in the Supplemental section? Since the ‘PSU group’ is among the co-authors of this paper, can more information be given about whether the instrument was operated in the same manner in the 2007

and 2009 campaigns. Is it reasonable to assume that an across-the-board factor of two decrease is an appropriate correction?

Page 13670, lines 19-22 I think the sentences

“The implied lower OH concentration refers to the OH concentration experienced over the lifetime of glyoxal and not necessarily the OH concentration at the measurement site. However, there is no reason to expect the OH concentrations to be substantially different.”

would flow more naturally at the end of preceding paragraph (line 10), since these statements not really related to potential over-measurement of OH.

Section 4.2.3 In the truncated Lagrangian run, what values are used for OH?

Page 13671, line 21 Rather than “the edge of the MBO-emitting region”, do you mean the edge of the isoprene-emitting region, since you are talking about losing glyoxal formed from isoprene in this sentence?

P13672, line 5 – Why would you expect the impact on glyoxal to be larger than the impact of glycoaldehyde? If glyoxal has other sources, I would expect the impact to be smaller. Why is the impact bigger for glyoxal and glycoaldehyde: because other precursors are also affected, because there is a feedback on the modelled OH, ...?

Supplemental, Line 83 – even ozone was assumed to have a zero background concentration? That seems like a poor choice, though I am not sure it will have a big influence on your results.

Reference section – it seems that the page on which the reference appears is listed after each reference – is this intentional?

## References

Chan, A. W. H., Galloway, M. M., Kwan, A. J., Chhabra, P. S., Keutsch, F. N., Wennberg, P. O., Flagan, R. C., and Seinfeld, J. H.: Photooxidation of 2-methyl-3-buten-2-ol (MBO) as a potential source of secondary organic aerosol, *Environ. Sci. Technol.*, 43, 4647–4652, doi:10.1021/es802560w, 2009.

Fares, S., M. McKay, R. Holzinger, A.H. Goldstein, Ozone fluxes in a *Pinus ponderosa* ecosystem are dominated by non-stomatal processes: Evidence from long-term continuous measurements, *Agricultural and Forest Meteorology*, 150, 420-431, 2010.

Galloway, M. M., Huisman, A. J., Yee, L. D., Chan, A. W. H., Loza, C. L., Seinfeld, J. H., and Keutsch, F. N.: Yields of oxidized volatile organic compounds during the OH radical initiated oxidation of isoprene, methyl vinyl ketone, and methacrolein under high-NO<sub>x</sub> conditions, *Atmos. Chem. Phys. Discuss.*, 11, 10693–10720, doi:10.5194/acpd-11-10693-2011, 2011.