

***Interactive comment on “Rate coefficients for the reaction of methylglyoxal (CH<sub>3</sub>COCHO) with OH and NO<sub>2</sub> and glyoxal (HCO)<sub>2</sub> with NO<sub>3</sub>” by R. K. Talukdar et al.***

**Anonymous Referee #2**

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This manuscript presents rate coefficients for the reaction of OH with methylglyoxal and NO<sub>3</sub> with glyoxal and methylglyoxal. These compounds are of much current interest and the results presented in the manuscript will enable a higher degree of confidence for modelling studies of these compounds.

I only have a few technical comments, as the manuscript is well written and the work as always from this group is thorough and of very high standard. The main question in my mind when reading the manuscript was whether the work is more appropriate for a more specialized journal, e.g. for chemical kinetics, or is suitable for ACP. For the latter the implications section of the manuscript is central, which centres on figure 1. I believe

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the implications warrant publication in ACP but recommend that the authors attempt to strengthen the implications section and take into account the comments below:

1. The manuscript discusses OH and NO<sub>3</sub> chemistry, which typically are day and night. I am not certain of this, but I could imagine that it might make sense to split figure 1 into day and night-time or address this point in some other manner.
2. The comments of anonymous referee #1 with respect to the fate of the CH<sub>3</sub>COCO radical are pertinent to this and should be addressed.
3. The authors do not discuss dry deposition much. However, especially at high relative humidity I would not be surprised at all if ground-level measurements could be strongly affected by this, in particular at night in stable night-time atmospheres. Another important point worth mentioning is the work by Karl et al. 2010 that showed that deposition can be much higher than expected. It is likely that for the alpha-dicarbonyls photolysis and/or reaction with OH will remain the dominant term, but I believe including all terms that can affect them is important.
4. The authors state that the reaction of MGLY+OH is HO<sub>x</sub> neutral (p.18232 line 3) and go on to discuss PAN. The PAN formation is a net radical loss (termination) as a OH and NO<sub>2</sub> radical are lost. Clearly, PAN gets destroyed again to reform these but if this is what the authors mean an important point is that for a specific location the reaction of MGLY+OH could be a radical sink due to export of longer-lived PAN. This aspect could be emphasized more and play into the implications of the manuscript.
5. I think it could be useful to include formaldehyde in the comparison of GLY/MGLY with acetaldehyde and acetone+NO<sub>3</sub>.
6. The work of Kroll et al. 2005 should be added and contrasted to the work of Fu et al. as Kroll et al. found no measurable uptake of MGLY on ammoniumsulfate aerosol. I believe this is required for balance. It should also be clarified whether the uptake of Fu et al. implied cloud processing (i.e. chemistry) or simply partitioning.

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Technical Comments:

Table 3 contains the explanation of the acronym MS, but I did not see MS show up anywhere in the table.

Figure 2: How much are the OH profiles offset.

References: Karl, T., Harley, P., Emmons, L., Thornton, B., Guenther, A., Basu, C., Turnipseed, A., Jardine, K. Efficient Atmospheric Cleansing of Oxidized Organic Trace Gases by Vegetation, *Science* 330, 816-819, (2010).

Kroll, J.H., Ng, N.L., Murphy, S.M., Varutbangkul, V., Flagan, R.C., Seinfeld, J.H., Chamber studies of secondary organic aerosol growth by reactive uptake of simple carbonyl compounds, *Journal of Geophysical Research*, 110, D23207, 10.1029/2005JD006004 (2005)

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