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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Modelling trends in OH radical concentrations using generalized additive models" *by* L. S. Jackson et al.

Anonymous Referee #2

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Jackson et al. used a new statistical method (Generalized Additive Models, GAM) to analyze TORCH field campaign data for modeled and measured OH. GAM model identifies photolysis rate, o-xylene, acetone, formaldehyde, water five variables for measured OH. For modeled OH, GAM identifies photolysis rate, peroxyacetyl nitrate (PAN), carbon monoxide (CO), isoprene, and ethanol. GAM models explain 78% variation for measured OH and 83% variation for modeled OH respectively. Then Jackson et al. examined the dependent variables produced from GAM_{ME} and GAM_{MO}. The authors also made an effort to predict OH by GAM with realizing the potential problems.

This paper reads well. But I am not really convinced by the conclusion of this paper. As said in the paper, one of the biggest problems for HOx chemistry is that model/obs HO_2/OH ratio has strong NO dependence, which was not addressed or related to the

results of this paper. As a number of papers reported, the models behave quite differently under different NOx conditions regarding the model/measurement discrepancy. I would suggest the authors to split the dataset into two subsets: high NO and low NO to run this GAM model again. This will help the reader to think about the possible reasons behind the model/measurement discrepancy. My guess is that the dependent variables would be totally different for high NO and low NO conditions except photolysis rate, which is the dominating driver for OH. Since this dataset include a wide range of VOC-NO_x, I think the authors should also try to analyze HO_2 and HO_2/OH ratio with this GAM method, and this will be much more helpful to find the reasons causing the difference between modeled and measured OH from the big picture.

My another question is that, does the deviance really represent the main factors affecting OH concentration? In Page14624, Line 22, "Isoprene concentrations were remarkably high during the TORCH campaign, which explains the impact on the model results. However, its lack of impact on the measurements is surprising." Beside the reasons proposed in the paper, this could also be due to the extra cycling from isoprene to OH(Lelieveld et al., 2008). If extra isoprene is added into the system, it may change OH production and loss, so it may not change OH concentration very much but it actually changes the HO_x cycling a lot. If this is the case, this could be a disadvantage of this GAM method for identifying this kind of insensitive-to-OH but critical species. On the other hand, it could be beneficial for finding the discrepancy if this species has different sensitivities to OH in model and measurement.

Special notes:

1)Page 14622, Line 23, "Each sample, comprising 75% of the data, was used to calibrate the GAM model." Please specify how exactly to calibrate the GAM model.

2)Page 14615, Line17, "The value for the intercept was 1.32x10^6 molecule cm^{-3}, the mean value of the measured OH radical concentrations." The mean model value was also set for the intercept of GAM_{MO}. Was the intercept set at this value on purpose?

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Why? What is the physical meaning of the intercept? Is it the expected value?

3)The author spent almost two pages on describing the acetone, which is just one dependent variable out of five. This seems unnecessary.

4)Since the weakness in prediction is realized by the authors, I would think the authors need to find a way to quantify the accuracy of this prediction.

References

Lelieveld, J., Butler, T. M., Crowley, J. N., Dillon, T. J., Fischer, H., Ganzeveld, L., Harder, H., Lawrence, M. G., Martinez, M., Taraborrelli, D., and Williams, J.: Atmospheric oxidation capacity sustained by a tropical forest, Nature, 452, 737-740, 10.1038/nature06870, 2008.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 14607, 2008.

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