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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Formaldehyde production from isoprene oxidation across NO_x regimes" *by* G. M. Wolfe et al.

Anonymous Referee #1

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The authors present an investigation of HCHO production over the US based on aircraft measurements, and use the comparisons in smart ways to test current chemical models and their representation of NOx-dependent reaction pathways of isoprene oxidation. The analysis framework is clear and well-thought out, the writing is clear, and overall the work makes a useful contribution to the literature in this area. The paper should be accepted. Below are just a few comments for the authors to consider.

Abstract (and page 31603), "we find that the total organic peroxy radical production rate is essentially independent of NOx, as the increase in oxidizing capacity with NOx is largely balanced by a decrease in VOC reactivity. Thus, the observed NOx dependence of HCHO mainly reflects the changing fate of organic peroxy radicals."

These points appear to contradict two main findings of a paper just out as an accepted

preprint in JGR (Valin et al., "The role of OH production in interpreting the variability of CH2O columns in the Southeast U.S."). Regarding the first point, Valin et al. state that the feedbacks of P(OH) on CH2O removal and production do not offset each other, so that CH2O is not independent of OH. Regarding the second point above, they state: "the yield of CH2O at low NOx concentrations is buffered by high-yield RO2-RO2 reactions (. . .) in isoprene-rich regions, the influence of NOx on CH2O production is primarily due to its feedback on POH, which controls the rate of RO2 formation, and less so through its effect on the fate of individual RO2." It would be worth adding a discussion of these apparent contradictions.

31599, when discussing the yields of HCHO from isoprene, please be explicit about the units to avoid confusion (here, ppb/ppb aka mol/mol) as some previous work has used carbon-based yields

31601, 12-18: in the Valin et al. paper referenced above, they argue that a steady-state assumption is justified for HCHO but not for isoprene with respect to its emissions. Does this have a significant bearing on the model application here?

31601, it would aid the interpretation of Fig 4 to discuss the differences between AM3 and UWCM in terms of the isoprene chemistry implemented in each. To what degree could the discrepancies between the two in Fig 4 reflect mechanistic chemical differences? Or is it just the effect of steady-state versus non-steady-state model frameworks?

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 31587, 2015.