

Interactive comment on “OH and HO₂ radical chemistry in a midlatitude forest: Measurements and model comparisons” by Michelle M. Lew et al.

Anonymous Referee #1

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This study focuses on the analysis of the OH and HO₂* radicals concentrations and total OH reactivity during the IRRONIC field campaign. The campaign was performed in a forested area characterized by high isoprene emissions and low NO concentrations. Measured radicals, which include a possibly interference-free OH radical measurement, are compared with two mechanisms (RACM2 and MCM) both with and without isomerization reactions for isoprene-RO₂ as described within the LIM1 mechanism.

The paper is well written and the data are adequately presented. Though, the analysis of the results and the discussion of the findings is too limited and in the current status this reviewer is not sure it is enough for publication on ACP. Following are some general comments which could help improving the discussion.

One of the problems of this study is the lack of NO data for a large fraction of the
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campaign. The authors overcome the issue by using the measured diurnal average when no NO data is available. I do not think this is a very good approach. Indeed, a much better solution is to constrain the model to the ozone and NO₂ concentrations and jNO₂ values and let the model calculate the NO. This is shown only for one model run (MCM 331) but should be done for all the models. Also, the modeled NO concentration should be compared with the measured one to see how well the model is able to reproduce it. This would allow for a better confidence in the models output also for days where no NO measurements are available.

It would be good to focus on the days when the measurements are complete and try and understand why there is still a discrepancy between modeled and measured OH radicals even after the inclusion of the LIM1 mechanisms. On those days it could be good to perform an experimental budget if possible. I can understand it could be difficult as the HO₂* radical measurement is affected by an interference from RO₂ radicals but a % of this interference is given based on laboratory studies so it should be possible to remove it. This would allow for an additional way to assess whether there is a discrepancy between the included sources of OH radicals and the total OH radical production.

Both RACM2 and MCM mechanisms are used in this study but there is no discussion about why both are used and, based on the results, which one is able to better reproduce the measured data and why. As both are used extensively within the community a better analysis of the differences between the two should be given. Also, both are implemented with the LIM1 mechanism. Is this done in the same way or are there differences? What is the reason behind the large differences in the modeled HO₂* concentrations between the two mechanisms?

The total OH reactivity measurement shows that, overall, when the contribution from modelled OVOCs is included, the budget is closed. Though, this is not true for some days when still a certain fraction of OH reactivity is unexplained. It would be good to look if there were differences between these days and the ones where the OH reactivity

could be explained. . . different wind directions, different VOCs distribution, different meteorological conditions, etc. Does this missing reactivity correlate with the days where the measured OH concentration is larger than the modelled one?

In general, often it is written the agreement is good or there is better agreement. . . but there is no value reported of a ratio of model to measurement or correlation coefficient so it is not possible to really assess the correctness of these statements.

Specific comments:

Abstract: It would be good to have, in addition to percentages, also the mean concentrations of radicals and OH reactivity and what low NO_x means.

Page 2, Line 12 to 17. No mention of the recent campaigns performed in China (Tan et al., 2017; Tan et al., 2018; Tan et al., 2019).

Page 5, Line 11. Is it pure NO injected to convert HO₂ to OH radicals? Or why is the interference from RO₂ radicals so large? Would not it make sense to reduce the NO further to reduce the interference?

Page 9, Line 1. Is the LIM or the LIM1 included in both RACM2 and MCM 3.3.1?

Page 10, Line 34. What does it mean that measurements on the 21-22 July focused on the HO₂* thus OH measurements were not available? The instrument should measure OH and HO₂ radicals in parallel or? How is stopping the OH measurement going to improve the measurement of HO₂* radicals?

Page 16, Line 9. What does “similar OH” stands for?

Figure 5. Colors of the models are different between OH and HO₂* panels.

Figure 12. Suggest to have consistency of the colors within the upper and lower panels.

References:

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