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Interactive comment on “OH and HO_2 radical chemistry in a midlatitude forest: Measurements and model comparisons” *by* Michelle M. Lew et al.

Anonymous Referee #2

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Review acp-2019-726 OH and HO2 radical chemistry in a midlatitude 1 forest: Measurements and model comparisons M. Lew et al.

The paper describes measurements of OH reactivity, OH and HO2* in a biogenic dominated regime with medium NO concentrations during daytime in the order of a few hundred pptv and compares the measurements with a box model study using four different model schemes. The main findings are that the model describes well the measured HO2* but underestimates the OH concentration. The most likely reason identified is a poor quality of the available NO measurements for the time periode shown. When the model is constrained by NO2 to calculate the NO concentration a better agreement of the model is found but local NO sources, independent of the photolysis of NO2 worsen the agreement as soon as the steady state assumption cannot be made anymore. The

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Discussion paper

paper also shows that the Indiana instrument successfully implemented a chemical scavenging modulation, improving the quality of the OH measurements. The quality of the dataset does not allow a detailed investigation of testing different model schemes, though it is obvious that the LIM1 based recycling reactions do provide a better agreement in the late afternoon when Isoprene is larger. The paper is well written, hPa seems to me preferable than Torr. The paper can be published after minor corrections.

P3 L11 : The statement “The extent of RO2 radical contributions during HO2 measurements in previous campaigns is unclear.“ Is not correct. For HUMPPA 2010 the contribution of a RO2 interference to the HO2* signal had been estimated based on H2O2 in Hens et al 2014, as well as calculated based on the PAA-PAN-HO2 system in Crowley et al. 2018 . Mallik et al. 2017 did model the internal production of OH from RO2 as well as compared it with a NO titration scheme done routinely in ambient air during the CYPHEX 2014 campaign.

P4 L29 : To what extend is double pulsing an issue, considering the volume flow, the expansion of the UV beam due to the white cell and the 10kHz repetition rate?

P4 L29 : please use SI units

P6 L28 : Unclear why the precision of the HO2* measurements is unrelated to the RO2 interference. Why would not the variability in the relative RO2 & HO2 composition translate into a variability in HO2* and therefore in an apparent precision ?

P8 L18: Please be more specific why a constant scaling factor can be used. Hansen et al. 2014 does not describe further the reason for the factor 1.4 beyond speculating about incomplete mixing or issues with the flow speed. Without knowing the fundamental reason for the factor, the application of such seems to be arbitrary.

P9 L27: Not conclusive is a 50% contribution of the background signal between 8:00 and 20:00. In the figure it seems rather in the range of 20%-300% even just for noontime. In any case I am not sure if the fractional description is of much use anyhow as the relationship between ambient OH and chemical background OH is not clear at best. I would drop the discussion about the fractional contribution. You mention it above already that you are using a chemical scavenger method to remove ambient OH for quantification of the non-ambient OH.

P10 L5: Do you observe a correlation of the internal background signal with O3*BVOC as described in Novelli 2017 ?

P10 L12: Novelli 2014 proposed the presence sCI decomposition as reason for the internal OH.

P11 L4: What is the time periode used for calculating the average ? Did you model the non averaged time series and then average the model together with the measurements ?

P11 L14: "However, as seen in Fig. 6, . . ." sentence seems to be reduntant to "If the measured interference was not subtracted from the total OH. . ..“

P11 L27: Is there a NO source close by and to what extend is the assumption of steady state NO/NO2 justified? The floating NO leads to much better model estimates for OH, but seems to deviate as soon as the sun sets. From a model point of view, NO production in the model will follow JNO2, which decay quickly into the night and reduce the OH source from HO2+NO whereas the measured OH is significant different from 0, therefore the question, is there a still active NO source close by ?

P11 & P16 Check spelling of the name, Rohrer

P 16 L25: Mallik et al, 2017 like Mao found a decent agreement of modeled OH and measured OH only when the interference, determined by a chemical modulation technique had been taken into account. I would be careful with a generalization, the instrument by the Leeds and Juelich group seem to be not as much as sensitive to the interference as the PennState/Indiana/Mainz & Lile group. The most striking difference is the use of a multipass cell vs. single beam cell.

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