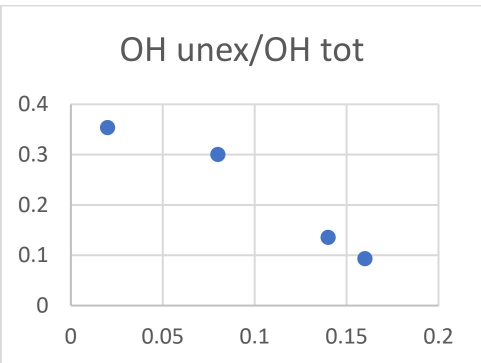
The authors have done a lot of work to improve the paper. My main concerns were on the quality of the data set, because in my opinion some major data were missing:

- Total OH reactivity: in the revised version, such measurements have appeared for a short period during the campaign. These data show a good agreement with the model, so I am really wondering why these data have not been shown in the original version of the manuscript. Anyway, these data allow now to get a better confidence in the data treatment concerning the OH losses.
- HO_2 measurements where done at very high NO concentrations for HO_2 conversion in order to use the HO_2 signal as reference cell for stabilizing the laser wavelength. In the revised version, the calibration of HO_2 with increasing added NO concentration has been discussed, and indeed under the “low” NO conditions the HO_2 conversion rate is up to 95%, while under the “high” NO conditions the conversion rate is over 100%, showing that RO_2 interference plays a role in both conditions. However, no RO_2 measurements are available from this campaign, and therefore the correction of the HO_2* signal can only be done based on modelled RO_2 concentrations and supposed RO_2 interference yields. It is not really clear to me, which RO_2 interference yields have been used for correction: did the authors measure it themselves or did they use data from Fuchs et al? This increases the uncertainty on the HO_2 concentration, and thus on the OH data as well.
- The OH concentration is underestimated by the model at low NO concentrations, and this disagreement has even increased since the HO_2 concentration has been corrected for RO_2 interference. The mysterious X species has been added to the model, and its concentration has been adjusted to bring into better agreement model and measurement. There is now some discussion on the concentration of this species and comparison with earlier studies. However, I am still regretting that the pre-injector system has not been used in this campaign to fully exclude any interference in the OH measurements. The argument that the PKU-LIF instrument has been proven free from interference in earlier campaigns, thus demonstrating the accuracy of the PKU-LIF system, does not fully convince me neither: looking for example to the Wangdu data, where a pre-injector has been used, some unexplained OH has been detected. There is a table in the paper giving the unexplained OH concentration together with NO and total OH concentrations. If one plots this unexplained OH (divided by total OH in order to normalize to overall photochemical activity) as a function of NO concentration, one gets a clear increase of the unexplained OH with decreasing NO:



In the new version, the authors at least mention that there have been reports on interferences in OH measurements when sampled air contains ozone, alkenes and BVOCs. They do not mention that also an interference has been detected in FAGE instruments due to ROOOH, the product of the reaction of RO_2 with OH (Atmos. Chem. Phys. 2019, 19, 349-362). In a very recent Science paper by Berndt et al. (Hydrotrioxide (ROOOH) formation in the atmosphere.

Science 2022, 376, 979-982) it is experimentally proven that ROOOH species have lifetimes of up to several hours, and that in low NO environments up to 1% of isoprene can be transformed to various ROOOH species. Even if RO_2 concentrations have not been measured during this campaign, the reaction of RO2 + OH should be added to the mechanism and the sum of modelled ROOOH concentrations should be compared against the modelled underestimation of OH concentration.

Finally, the authors have done a good job in improving the paper as good as possible, however, the data set (even if now somehow completed with a few days of OH lifetime measurements) is still lacking information to allow drawing solid conclusions and obtain new knowledge on atmospheric chemistry. I still have doubts that it is meeting the standard of ACP.