This paper addresses an important issue, namely the global impact of heterogeneous loss of gas-phase radical HO_2. As HO_2 is the major precursor of tropospheric OH and ozone, this heterogeneous loss may significantly impact global OH and ozone budget, as well as CO burden. In contrast to previous studies that are mainly focusing on aerosols, the authors attempt to show that the heterogeneous loss of HO_2 on clouds are also important.

I completely agree with J-F. Muller's comments. As authors explained in their replies, the resulting change on mixing may largely reduce the impact of the heterogeneous loss of HO_2 on clouds. Therefore I would expect significant revision on the global evaluation of CO and ozone. I will comment on those sections in the revised version.

Here I have two more comments that need to be addressed before the revision:

1. The uptake of HO_2 on cloud droplets and aerosols are driven by different aqueous chemistry. Although there is increasing evidence on HO2 uptake by aerosols not yielding H_2O_2 (H_2O instead), no such evidence has been found on HO_2 uptake by cloud droplets. Current understanding is that, this is driven by HO_2 + O_2^- reaction (unless the authors can prove otherwise). As a result, one would expect the production of H_2O_2 from HO_2 uptake by cloud droplets. So my recommendation is to treat aerosol and cloud uptake differently in the model:

for aerosols : HO_2 -> products

for clouds: HO_2 -> 0.5 H_2O_2

The authors must realize that different products of HO_2 uptake make large difference on global OH, as H_2O_2 is a radical reservoir and can photolyze to make OH. With this new treatment on liquid clouds, I expect the impact of HO_2 uptake by cloud droplets to be even smaller.

2. I also have issues with using $\gamma(HO_2) = 0.06$ for clouds. The authors argue that this is from Kolb et al. (2010) for salt solution. But cloud droplets are NOT salt solution. They are much more diluted than aerosols (by three to six orders of magnitude). In fact, I think HO_2 loss to clouds is insensitive to the choice of $\gamma(HO_2)$, as the loss is largely limited by the gas-phase diffusion. Given the large uncertainties on $\gamma(HO_2)$ for clouds, I think the authors should do a few sensitivity tests with different $\gamma(HO_2)$ to assess the impact of HO_2 loss to clouds.

I will be happy to review the revised manuscript after these questions are addressed.