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Interactive comment on "Modeling global impacts of heterogeneous loss of HO_2 on cloud droplets, ice particles and aerosols" *by* V. Huijnen et al.

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This modeling study is very interesting, as it draws attention to an issue, the HO2 uptake by clouds, which was overlooked in past studies. However, I'm concerned by a modelling assumption which might lead to a large overestimation of its impact. As noted by the authors, clouds are present in only a limited fraction of every grid cell, and subgrid-scale processes must be addressed in some way. The assumption made here of full mixing between the cloudy and non-cloudy parts is a convenient one, but I'm afraid it is not justified.

The authors mentions "an assessment of the HO2 chemical budget under cloudy conditions" indicating that HO2 heterogeneous loss is "relevant, not dominant in the overall

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chemistry budget", but no detail is provided. That the global-scale HO2 loss through clouds is small compared to the total HO2 losses (Table 7) doesn't mean that heterogeneous loss does not strongly deplete HO2 inside the clouds. Among the HO2 losses of Table 7, the reactions of HO2 with NO or O3 are no sink for the HOx(=OH+HO2) family, contrary to HO2 uptake, and have therefore a much lower impact on HO2 levels than the global fluxes might suggest (since [HO2] $>>$ [OH] in the troposphere). Remembering that clouds occupy only a small fraction of the tropospheric volume, the fact that HO2 uptake by clouds is found to deplete gridcell-averaged HO2 concentrations by up to about 30% according to the model calculations (Fig. 4) implies that its impact must be much stronger on HO2 levels inside the clouds. Note that the characteristic time for mixing cloudy and non-cloudy air (between 15 minutes and 5 hours, Lelieveld, 1990) is of course much longer that the characteristic time of HO2 uptake inside the clouds (typically less than one minute using Eqs. (1) and (2) with M=0.1·10^{-6} g cm^{-3}, r_e=5-15 μm, γ=0.06).

I have another minor question related to Eq. (3) which relates the effective droplet radius to the liquid water path (LWP). Using ECMWF data, the application of this equation leads to unrealistically high values, much higher than the usually accepted range (5–20 μm). Could the authors provide more details and possibly show some distribution of the radius (or provide an average value)? In the same vein, application of Eqs. (4) and (5) (with the factor of 10 on A_{ice} as discussed in the text) seems to lead to quite large values (450 μm) of the effective radius of ice particles. Is that justified?

Lelieveld, J., The role of clouds in tropospheric photochemistry, Ph D. Dissertation, University of Utrecht, The Netherlands, 1990.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 8575, 2014.