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Atmospheric Chemistry and Physics Discussions

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

Anonymous Referee #2

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General

Some severe shortcomings of this paper have been identified and addressed in the public discussion in ACPD already and I feel that the authors should address all of them, point by point, in a revised manuscript as the suggested changes have the potential to strongly change the overall outcome of the paper.

It is difficult to judge the results of the manuscript as it has been submitted because the current findings are going to change.

I am therefore addressing issues which are I expect to remain unchanged.

Despite the shortcomings of the current version of the paper, it could have its merits

which might lie in a better representation of aqueous chemistry - cleary, to neglect H2O2 formation is not realistic. Then again, the current version is considering this in a very indirect way (see below)

Overall, the paper demands drastic revision followed by re-evaluation.

Details

Page 9, line 13: The stated assumption in the work of Mao that no H2O2 is being formed is really a shortcoming of that study and in this respect, the current study should be more advanced from what is implemented in aqueous phase chemistry (see below).

P9, l 23ff: The chosen uptake coefficients appear to be reasonably chosen.

P10, l4ff: It would be very good if the author could actually implement H2O2 formation and study the effects of this implementation. The stated argument that a low value chose for the uptake coefficient for HO2 'in some way' accounts for the in-situ formation of H2O2 might be tru but it is very difficult to judge how good this ver indirect representation is. I would like to suggest to treat this point clearly and explicitly in a revised version.

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