

## ***Interactive comment on “Oxidation of low-molecular weight organic compounds in cloud droplets: global impact on tropospheric oxidants” by Simon Rosanka et al.***

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This submission reminds me of discussions which happened in the very early 1990s on the role of liquid water clouds on tropospheric ozone. Somehow, I thought, atmospheric multiphase chemistry moved on in the 30 years since then but this can hardly be seen from the perspective of this submission. It would be great to widen the scope here at least to a certain extent.

Wouldn't it be useful to compare the cloud effects on ozone for a simpler and a more advanced chemical aqueous phase scheme even beyond the 150 rxn schemes for which Tost et al. (2007) and Jöckel et al. (2016) are cited? Jöckel et al. cites SCAV

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(Tost et al., 2006a, 2007a, 2010) - here we turn in a circle, however incomplete as Tost et al. 2010 is not mentioned by Rosanka et al. Why not?

I would really appreciate a reactions table with all reactions and rate constants which are being used and their respective sources.

What developments have been seen in the field which are not implemented in the applied aqueous chemistry schemes and, if so, why not?

What if the limitation in the aqueous mechanism on small compounds artificially changes the effects expected to be observed with a broader arsenal of cloudwater organics? Would the findings of the paper still hold?

I rate the switching of the Fenton reaction as fatal. I cannot understand why this is done. Fenton is one of the most important OH sources. As a judgement of the aqueous scheme: How do your OH / HO<sub>2</sub> concentrations compare to state-of-the-art models and measurements? The paper tells how Fe valuse could be assessed and there are other ways on top of this - this has been done already.

I appreciate the comments of Jos Lelieveld on the earlier days of aqueous phase modelling. It would be desirable to include all available information to give an accurate look back.

Overall, I feel the paper needs more work.

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