

Interactive comment on “Direct photolysis of carbonyl compounds dissolved in cloud and fog droplets” by S. A. Epstein et al.

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This work is very interesting and gives insight into the carbonyl compounds that could undergo direct photochemical transformation in the atmospheric aqueous phase. The work is well done and the results relevant to atmospheric chemistry, thus acceptance can be recommended after MINOR TO MODERATE REVISION. Recommendations for improvement are given below.

GENERAL AND SPECIFIC REMARKS

1) I perfectly agree with the authors' strategy to exclude all the compounds the direct photolysis of which cannot possibly be important, to finally come up with a shortlist of the interesting ones. However, when a few compounds (2 to 3) are finally highlighted, I

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wonder if it really makes sense to make a simulation of how their properties could be. To my opinion, it would be much better to make experiments, if possible, to have definite answers (including the measurement of actual absorption spectra). The authors should address this observation in the manuscript, and state why did they decide to make calculations instead of experiments at the very last step (which sounds as a less effective strategy).

2) The authors compared direct photolysis with $^{\circ}\text{OH}$ reaction as potential transformation processes in the atmospheric aqueous phase. This approach is made reasonable by the fact that chromophoric dissolved organic matter in atmospheric waters is poorly photoactive (see Albinet et al., Science of the total Environment 2010, 408, 3367-3373), differently for instance from surface waters where other photoinduced processes can be important. A brief comment over this issue in the manuscript would be useful.

3) Page 10908, top. Note that among the processes that should be taken into account to carry out a proper modelling of the photochemistry in droplets, there are the photoreactions occurring at the air-water interface. Although the interface is thin (but not so thin in smaller droplets), it is a very effective photoreactor and the weight of the interface processes may not be negligible. See for instance Nissenson et al., Atmos. Environ. 2010, 44, 4859-4866. Some hint about interface reactivity should be added in the manuscript.

4) Page 10915, 1st half. To understand what a Nosé-Hoover thermostat is I had to make a net search. It is true that most article readers will have easy Internet access while reading it, but it is strongly advisable to add a brief explanation.

5) Page 10915, bottom. The FTIR instrument was evidently used, but it was not described in the experimental section. Please add the instrument description were relevant (section 2.1?).

6) Page 1096. Is there a reason for the choice (at least, I presume it was a choice by the authors) to have the different groups occupy the terminal positions in the chain?

Please add a brief rationale.

7) Figure 5 is not very clear. Is there something missing? It would be better to explain how were the kOH curves obtained and/or give their equations, and (if possible) to put some reference points related to different compounds. In the present form, the figure looks rather puzzling.

8) Page 10919, lines 25 to the end. The sentence "The lower left. . .by OH" is rather awkward, please check and rephrase.

9) Page 10923, top. Please spell out SZA (solar zenith angle, I presume, but the acronym was not defined before).

10) Page 10923, line 6. "in Ref. in". Awkward phrase, please check.

MINOR ISSUES

a) Page 10907, line 23. Delete "radical" (repetition).

b) Page 10914. Please check "an absorption spectra". Is it rather "an absorption spectrum"?

c) Page 10918, line 23. "slow" should read "low".

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