

Interactive comment on “Photoinduced oxidation of sea salt halides by aromatic ketones: a source of halogenated radicals” by A. Jammoul et al.

C. George

christian.george@ircelyon.univ-lyon1.fr

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Referee 3:

We thank the reviewer for the comments and suggestions. We took all comments into consideration and give below our corrections and/or answers to them.

I do find the $E_{\text{Cl/Cl}^-}$ of 2.6 V as given in Table 1 too high. There are lower values frequently used in literature - See the Stanbury-review. Why do the authors cite only one value which is at the very upper end of all published data ?

The oxidation potential of (Cl-/Cl) is higher than the one of (Br-/Br) or (I-/I), then the

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free energy of electron transfer with chlorine anion is less negative. Even, if we use a smaller value as in Stanbury's review or in the handbook of photochemistry (1.66 v), the ΔG will be close to zero and therefore the electron transfer mechanism is almost ineffective. Nevertheless the values in Table 1 will be corrected.

The section 3.4. on page 7692 is not fully clear to me. What is the bottom line of the studies on gas phase products ? Could that be important ? Are further experiments required ? Which ones ?

The section 3.4 shows that a fraction of the halogenated species produced by the suggested photosensitized reaction may indeed go from the aqueous to the gaseous phase. It is not obvious that this might be the case (previously to the experiments) as degassing might be prevented by the reaction between the halogenated radicals and the benzophenone. Clearly more experiments are required with techniques able to specifically measure gas phase halogens (radicals or molecules) at low levels. However, this is currently beyond our experimental capabilities.

All minor suggestions will be considered and corrected in the revised manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 7681, 2009.

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