

Interactive comment on “Isotopic constraints on the role of hypohalous acids in sulfate aerosol formation in the remote marine boundary layer” by Qianjie Chen et al.

A.-L. Norman (Referee)

alnorman@ucalgary.ca

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This paper represents a very important contribution to the literature on sulfur dioxide oxidation in the remote marine boundary layer and has large implications for contributions to the global sulfate budget from marine biogenic sulfate sources (oxidation of dimethyl sulfide (DMS)) and the role of halogens. It demonstrates for the first time, measurements that constrain the proportion and amount of HOX necessary to be consistent with previously proposed halogen oxidation mechanisms for DMS by von Glasow. It also shows latitudinal bands where the role of HOX may be more important.

I have just a few other comments with respect to the arguments in the manuscript:

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Discussion paper



1. First, in paragraph 230, the fractional contributions are discussed rather than the concentration. Later in the manuscript, the authors address the concentration but it may be worth mentioning the rationale for describing fractional contribution rather than amount here. 2. Second, dark OH reactions (from nitrogen reactions on aerosol surfaces, e.g. Fuchs et al., 2013 doi:10.1038/ngeo1964) has recently been described as an important night-time oxidation pathway that is typically not considered in chemistry and aerosol models. What would the implications be here and can it be ignored? 3. Third, how realistic is it that S(IV) + HOX results in no pH change in clouds (paragraph 420) and although this is the treatment used in the model, what are the difficulties associated with changing both cloud pH and the fraction of S(IV) + HOX at the same time?

Otherwise the manuscript is in excellent shape and is appropriate for publication. The tables, figures and captions are all appropriate and sufficiently descriptive and the appropriate literature has been cited.

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