

Interactive comment on “Using airborne observations to improve estimates of short-lived halocarbon emissions during summer from Southern Ocean” by E. Asher et al.

Anonymous Referee #1

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This study of airborne observations of halogenated VOCs (HVOCs) represents a valuable addition to the knowledge of these compounds over the Southern Ocean, where few data exist. The study confirms the current view that the main sources of CHBr_3 and CH_2Br_2 are biological, and that CH_3I has both biological and non-biological sources. The authors have put forward a novel concept of using enrichment ratios of HVOCs to O_2 to infer the contribution or otherwise of ocean biological sources, and propose a new function to estimate non-biological emission fluxes of CH_3I . The dataset has been used to evaluate the CAM-Chem HVOC emission scheme at high latitudes in the Southern Hemisphere. The take home message/s from this evaluation are rather opaque – they could do with being put in context. E.g., do they infer that fluxes from these regions

are poorly known, or problems with the models mixing /convection schemes special to these latitudes, or issues with photo-oxidation rates?.

In terms of presentation, the paper has a number of typographical and other errors, listed below, and needs a thorough reading (I doubt I captured all of them).

However overall, I think this manuscript presents sufficiently novel results to be suitable for publication, once these matters have been attended to.

Specific comments:

Ln 26 onwards. The regional enrichment ratios should be put in context here - there is no explanation as their relevance.

Ln 50-52 “Indeed, HVOCs may be among the most important sources of inorganic bromine to the whole atmosphere (Murphy et al.,in review).”

This is not conventional wisdom and thus quite a bold statement. Are the authors confident that the Murphy et al paper will be published soon?

Lns 60-61. The anthropogenic sources of CH₃Br have changed over time and now are dominated by quarantine and pre-shipment (QPS) applications (not controlled by the Montreal Protocol). Please stick to the most recent information from WMO 2018 (and update the reference).

Lns 110-117. The last paragraph of the introduction would benefit from an introduction to the concept of enrichment ratios of HVOCs to O₂, which feature prominently in the abstract.

Lns 204-218. The fact that the polyhalogenated bromocarbons are likely co-emitted is not new – there are numerous papers that show this, and the discussion could elaborate on those a bit more. What is also missing from this paragraph is a discussion of macroalgal sources of these compounds, although this is presumably not relevant for the Antarctic.

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Lns 213-214 “For instance, Huges et al. (2013) also report distinct seawater slopes between CH_2Br_2 to CHBr_3 , when chl a was increasing.” It is not clear what is meant by this. Please rephrase.

Lns 312- 313. “In both regions, the model under predicts CH_3I above the MBL, which may indicate slower observed photochemical loss than the model predicts.” Has this been found in other CAM-Chem studies – e.g. is it a general result? If not, could a different source emission distribution (i.e. more homogeneous source) explain these results?

Ln 468 onwards. There is no mention in Moore and Zarifou 1994 nor Richter and Wallace 2004 as far as I can see on the influence of iron availability – do the authors mean iodide availability?!

Lns 901 onwards (Table 1). Note that units should be $\text{pmol m}^{-2} \text{hr}^{-1}$ (not m^2). Please state whether the values given for the observations are means or medians. It would be also be good to include their ranges.

Typos:

Ln 82. atmospheric Ln 213. “oppose” should be “opposed” Ln 213. “Huges” should be “Hughes” Ln 242 : “HOVCs” Ln 469. “Zafarou” should be “Zafariou” Ln 980. “includind” LN 1015. “fluxed”

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