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Interactive comment on "Biogenic halocarbons from the Peruvian upwelling region as tropospheric halogen source" *by* H. Hepach et al.

Anonymous Referee #1

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General comments This manuscript by Hepach et al. reported their observations on a series of bromo- and iodocarbons from surface ocean as well as subsurface waters in the Peruvian upwelling zone. The authors further investigated into possible sources of the halocarbons and the contribution of organoiodine to stratosphere iodine loading. This manuscript is relatively well written and discussed drivers for the production of the halocarbons and transport of iodinated species, in addition to reporting data. I believe it is a new contribution to the scientific community. The authors also proposed directions for future studies for addressing the halocarbon budgets. I believe this manuscript is suitable for publication in ACP with minor revisions. Please see specific comments suggested below.

Specific comments

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Discussion paper

Page 2 Line 2: delete “halogenated short-chained hydrocarbons” for “halocarbons” is clear enough for this definition.

Page 2 Line 6: should read “Peruvian upwelling zone (or regions)”.

Page 2 Line 23: perhaps use “very short-lived substances (VLS)” following the WMO terminology?

Page 3 Line 16: please clarify this sentence by adding “in seawater” after “for both compounds”.

Page 3 Line 29: should read “the main sinks for both CH2I2 and CH2ClI are. . .”

Page 4 Line 14: replace “the latter two compounds” with “CH2ClI and CH2I2” to avoid confusion.

Page 5 Line 2: it is a bit confusing here, is it “every 3 hours”?

Page 5 Line 6 and 7: were purge efficiency measured for these gases?

Page 5 Line 11: please specified how the gas samples were stored – stainless steel canisters?

Page 5 Line 25: Cyanobacteria may pass through the GF/F filters at the initial filtering (i.e. before the filter pore size decreased as materials accumulated), which may affect the quantification of the cyanobacteria marker pigments. Did the authors estimate such a biomass lost?

Page 5 Lines 28 to Page 6 Lines 1 to 7: The DOM samples were collected from 20cm and the gases were collected from about 6 to 7 m, which were not exactly parallel samples. Some DOM can be recycled relatively fast. In addition, DOM at surface ocean may be degraded via photolysis. I suggest the authors to also report the mix layer depth and possible residence times for the DOM compounds they measured, such that a valid argument can be made about those DOM were well mixed within the mixed layer and hence the depth difference would not affect the data analysis and interpretation.

Page 8 Lines 1 to 15: I suggest move this to before the FLEXPART model simulation.

Page 9 Line 2: change "correlated very well" to "significantly correlated".

Page 11 Line 11: I suggest the authors also include the depth profile of the bromocarbons.

Page 11 Lines 22 to 24: this sentence is a bit confusing, please rephrase.

Page 12 Line 22: Liu et al., 2015 tested a series of carbohydrates, and found that these DOM moieties were not fast reacting substrates for $CHBr_3$, which seems to be consistent with findings in this study. In addition, bromocarbon formations via HOBr reaction are potentially DOM moiety specific. In Liu et al., 2013, no correlations were observed between the bromocarbons and total dissolved organic carbon.

Page 13 Line 12: the bulk DOM may correlate better with total biomass (estimated from TChla).

Page 13 Lines13 to 15: I suggest change "is determined by the phytoplankton species" to "is determined by ecosystem compositions", because DOM contribution is not governed by phytoplankton species alone.

Page 13 Line 21: Please also cite Lin and Manley 2012, who also tested bromocarbon formations using different molecular weight natural DOM as substrates.

Page 13 Lines 24 to 27: The authors depicted possible abiotic sources of HOI and HOBr in Fig 5. I would suggest the authors also put the abiotic sources of HOI and HOBr into this context (see Carpenter et al., 2005).

Page 14 Line 6: I suggest remove "In conclusion" here.

Page 31 Fig 5: The conception model figure is a bit confusing on the CH_3I part via methyltransferase, for it is an intracellular enzyme. Thus the reaction is likely occurring inside the cell. However, figure seems to depict an extracellular reaction.

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