

## ***Interactive comment on “Drivers of diel and regional variations of halocarbon emissions from the tropical North East Atlantic” by H. Hepach et al.***

**Anonymous Referee #2**

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Overall, the field observations presented in this study contribute a valuable dataset of simultaneous atmospheric and surface ocean halocarbon concentrations, and resulting sea-to-air flux estimates, in a potentially important source region for these gases. Observations of these gases (particularly simultaneous air and seawater measurements) remain sparse, so this type of study is crucial for improving our understanding of the natural occurrence and distribution of marine halocarbons.

One general criticism of this manuscript is that in some instances the language used can be a bit difficult to follow. While I do not think that there is a major problem in this respect, I would encourage the authors to look carefully at the writing style, with a view

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to improving the read-ability of the manuscript. Some specific examples of this are highlighted in the individual comments below.

Detailed comments:

Page 19702 L14: In the sentence beginning “Atmospheric mixing rations” – presumably “rations” should be “ratios”?

Page 19702 L14-16 – Reword this sentence for clarity – particularly the part “. . . and 1.8, 12.8, respectively 2.2 ppt at a Cape Verdean coast. . .”.

Page 19703 L15-17: The sentence “Phytoplankton produces these trace gases as well and especially upwelling regions where cold, nutrient rich water is brought up to the sea surface contains large amounts of these compounds” would be much clearer if re-worded as follows: “These halocarbons are also produced by phytoplankton, and as such elevated concentrations of these compounds are often observed in upwelling regions, where cold, nutrient rich water is brought up to the sea surface”.

Page 19704 L8: “The tropical Mauritanian upwelling is an example for a recently intensified. . .” – change to “. . .an example of a recently intensified. . .”.

Page 19704 L25: Change “Regional” to “Regional”.

Page 19705 L26: Change “were” to “where”.

Page 19706 L19-20: Please provide details of the standards used for calibration - e.g. concentrations and number of points on calibration curve.

Page 19706 L23-24: The air samples in canisters were analyzed for halocarbons within one month of sampling - can the authors comment on the stability of the halocarbons in these canisters over this time period?

Page 19707 L5-6: It is stated here that the pigments were analyzed from seawater samples taken from ships’ underway system, whereas the halocarbon samples were taken from the ships moon pool. Does this mean that the seawater samples for phyto-

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plankton pigment analysis were taken from a different water supply to the halocarbon samples? If so, why was this the case, and what are the implications for comparisons between pigments and halocarbons (if any)?

Page 19707 L9: Does “waters” refer to the HPLC manufacturer? If so, this should be capitalized, as in “Waters”.

Page 19709 L14-15: Suggest re-wording the sentence beginning “The beginning ceasing of the upwelling. . .”.

Page 197010 L4-5: The authors should be clear that the average CH3I concentration reported for open ocean water is in fact determined based on measurements at only 2 stations. Furthermore, from table 2 it seems that the mean and range of CH3I concentrations at S2 are very similar to the concentrations observed at the coastal stations, and as such the reported open ocean average concentration appears to be somewhat skewed by the measurements at the S1 station.

Page 19710 L4: Rephrase the sentence “Oceanic CH3I was with 2.4 pmolL<sup>-1</sup> on average higher at the open ocean stations S1 and S2 than at coastal stations S3-S6 with 1.8 pmolL<sup>-1</sup>”. Page 19710 L5-7: Rephrase the sentence beginning “While maximum mean (max-min) oceanic CH3I. . .”

Page 19711 L25-27: Can the authors offer any explanation / interpretation of the measurements at the S5 station, where low oceanic CH3I coincided with high atmospheric mixing ratios, which gave rise to a net deposition flux of CH3I from the air to sea?

Page 19712 L3-4: Is there any significant difference in the mean CH3I fluxes from open ocean vs coastal sites? I would assume that the uncertainty in these values would be at least 10%, which would imply that the mean open ocean and coastal fluxes are essentially equivalent.

Page 19713 L10: Change “were” to “where”.

Page 19713 L15-18: This sentence requires re-wording, as it is difficult to follow in the  
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current form.

Page 19714 L2-3: Reword this sentence. Take care that the word “respectively” is used correctly.

Page 19716 L13: The quoted upper limit value for the CH3I seawater concentrations reported by Jones et al. 2010 appears to be too high. Jones et al report CH3I concentrations in seawater in this region of up to 26.1 pmolL<sup>-1</sup>.

Page 19718 L4-7: As part of the comparison of CH3I fluxes derived in this work and in previous studies, the authors should clearly state that the spatial resolution of the measurements presented here is relatively limited compared to both the Jones et al 2010 and Richter and Wallace 2004 studies. All three of these studies indicate that there is a great deal of variability in the CH3I flux within this particular ocean region (e.g for this study -1.7-941.6 pmol m<sup>-2</sup> h<sup>-1</sup>), so perhaps it is not entirely surprising that the mean flux derived from observations at 2-4 discrete measurement sites differs from the mean flux derived from the more spatially resolved surveys? To this end, it may be more insightful to also compare the total range of fluxes determined from this study and the other studies, rather than just the mean values.

Page 19718 L12-14: This sentence needs re-wording.

Page 19719 L27: Suggest replacing the expression “could be a hint for” with “may indicate” or “may imply”, or similar.

Page 19720 L8: Correlation coefficients of wind speed and sea-air flux are given in Table 4, not Table 5.

Page 19720 L21: Regarding comparisons of CHBr3 fluxes derived from this study and those of Carpenter et al 2009 – see comment above regarding comparisons of CH3I fluxes with those of other studies.

Page 19723 L1-9: Can the authors offer a clearer explanation for the overestimation in the atmospheric concentrations of all three halocarbons at S5?

Page 19723 L25: Delete “regionally” from this sentence.

Page 19724 L15: This sentence is a little confusing - please re-phrase.

Page 19725 L22-23: The statement “The regional oceanic CHBr<sub>3</sub> and CH<sub>2</sub>Br<sub>2</sub> distributions and emissions were a result of biological production. . .” seems a bit strong. The observations in this study may suggest that this was the case, but the authors cannot be 100% certain, and so this conclusion should be less strongly worded.

A final comment - some of the figures could be improved by increasing font size (Fig 2) and changing data markers such that when several datasets are displayed on the same plot the separate datasets can be easily identified (Fig 3).

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