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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "The contribution of oceanic halocarbons to marine and free troposphere air over the tropical West Pacific" *by* S. Fuhlbrügge et al.

Anonymous Referee #1

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This paper describes a rather comprehensive set of measurements performed in the region of the South China and Sulu Seas that were designed to improve our understanding of the fluxes of three short-lived halogenated hydrocarbons from the ocean to the free troposphere. This is an important region for understanding the input of naturally-emitted bromine and iodine to the stratosphere and is woefully under-sampled. Furthermore, the authors have brought many useful resources and ancillary observations to the experiment in addition to just atmospheric mixing ratio measurements to improve our understanding of halocarbon fluxes in this region. Unfortunately, I found the paper very difficult to read and follow. After hours of studying it I was still unsure that the con-

ceptual framework of and conclusions drawn from the simple box-modeling approach were appropriate. I'm concerned with oversimplification of the processes involved. Some of this confusion stems from the language used in the paper. Descriptions often use jargon or short-cut terms that confuse rather than clarify the arguments being presented. Statements are often overly general and imprecise. Confusion is enhanced by a main conclusion stated in the abstract that isn't supported by any portion of the text (line 23): "bromoform in the FT above the region origins [sic] almost entirely from the local South China Sea area", despite numbers in the summary that indicate local contributions to free troposphere $CHBr_3$ of 60%, which to me isn't "almost entirely" (see lines 20-26, p. 17917–is the word "originates" meant?). Perhaps some schematics or diagrams showing the magnitudes of fluxes would help. In short, there is substantial room for improving communication of the simple modeling framework so as to enhance the value of the manuscript to potential future readers.

Other items: Section 2.3, to what degree are conclusions based on the particular air-sea exchange parameterization the authors have chosen (at the exclusion of others)?

Lifetimes: are the simple lifetimes calculated for this region of the globe and season of year? Are they a mean over 24 hrs? How do clouds affect trace gas lifetimes in this regionand might they explain some of the underestimations of calculated mixing ratios (particularly for CH_3I)?

Section 4.1 Line 5-6: mixing ratios are higher afterwards and winds speeds are lower (not higher?). Last paragraph: any discussion of age of air inferred from the ratio of two gases (CH_2Br_2 and $CHBr_3$) seems to require some consideration of the magnitude and variability in the emission ratio. Fortunately, you have measured emissions for both chemicals in this region to provide some information, if one presumes that ratio and variability are appropriate for a much broader region. How variable is their emission ratio and how do the ratios of measured atmospheric mixing ratios compare to this variability? A glance at figure 6d seems to indicate that there is enough variability in their emission ratio in this region of the globe that any discussion of age of air based on the ratio of the ambient mixing ratios of these gases could be not defensible.

Section 4.2 I find it quite surprising and interesting that in this region of supposedly high natural emissions of VSLS the authors suggest that the highest emissions are apparently associated with anthropogenic influences and river outflow. This seems a significant point that I haven't been aware of being made previously. Can the authors add some additional explanation and provide hard evidence from the observations made during this experiment to support this assertion? Do any previous studies support these assertions?

Section 4.3: an indication of the number of comparison measurements and an uncertainty on the values being compared (in the text and in Table 2) is lacking but would be useful. Line 20-24. Regarding the intercomparison, I would think any interpretation of gradients between the free troposphere and the boundary layer should be done with data that are internally consistent so that any potential instrumental influences don't affect the conclusions. In that respect, I don't understand why the mean of the different measurement techniques onboard the aircraft (and that have substantial differences that would seem to be instrumental) is used to compare with the ship-board marine bl results. In a discussion of mean results, sure, mention results from both instruments. But when gradients are being interpreted, it seems only appropriate to use aircraft results that are consistent with those from the ship (good to see that the unbiased result appears in figure 13).

Figures: 6d, I'd like to be able to see the CH2Br2 results, but they are often obscured by other data. Figure 8, consider making the legend more informative by indicating ship, flask, insitu instead of the instrument acronyms. Figure 13, I presume the unadjusted observations from the aircraft are the mean of the two available measurements and the adjust ones are only the data from aircraft flasks? Explicitly stating so would help.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 17887, 2015.