We thank both reviewers for their constructive comments. Our responses are given below in red text.

Response to anonymous referee #2

1. I would recommend the authors to add some discussion about $CHBrCl_2$ and CH_2BrCl, for which very few have been reported.
   We had added few previous studies on $CHBrCl_2$ and CH_2BrCl by Leedham et al., 2013 and Seh-Lin Keng et al. (2013) in to section 3.4 and discussed now mention the less commonly measured compounds in the conclusions.

2. The emission-ratio estimates based on the "chemical decay line" and "dilution line" become reliable only when the data are sufficient in number and variable in degrees of reaction or dilution. The values from the intersection of the two lines in Fig.7 could be taken as "lower-limits" of the emission ratios rather than their best estimates.
   We acknowledge that the values chosen for the intersection are a matter of judgement in plots of this type, and so must be slightly uncertain. We will make this clearer in the text.

3. The authors should refer to the paper by Ziska et al. (ACP, 2013) which has reported global map of $CHBr_3$ and CH_2Br_2.

   We have added new additional references in our result and discussion section.

4. p. 955 line 10-11 "there was no significant correlation between bromocarbons and in situ chlorophyll a". What does this finding suggest for the source of bromocarbons?

   Just because there is high chlorophyll (and potentially high bromocarbons in the seawater) it does not necessarily mean that you would expect high concentrations in the atmosphere directly above. The PESC air measurements therefore say little about the source of bromocarbons in the seawater (seawater measurements would be needed to link the two).

5. p.955 line 20-24 "we note that satellite-derived chlorophyll a (chl-a) products do not always agree well with in situ measurements, particularly in coastal regions of high turbidity, meaning that satellite chl-a may not always be a good proxy for marine productivity." Isn't there any possibility that seaweeds growing in coastal regions caused the difference between satellite-derived chl-a and in situ chl-a? What is the definition of "marine productivity" in this case?

   By marine productivity we specifically mean micro and macro algae in the water. Our measurements suggest that satellites cannot always distinguish between biology (marine productivity) and sediment (turbidity), particularly in coastal zones. There were no major exposed seaweed colonies in the close vicinity of our sampling locations.

6. p.967 line 6 “CHCl3” Misspelling for “CHBr3”?
   We have corrected $CHCl_3$ to $CHBr_3$.

7. p.967 line 13-16 “However, even filtering the satellite-derived chl-a for turbidities of less than 0.5 FTU, did not reveal any significant correlations with halocarbon concentrations (not shown). Similarly, there were no obvious correlations between the halocarbons and turbidity.” The plot of halocarbon vs. satellite-derived chl-a should be helpful for understanding.
   We have added a new plot of halocarbons vs both insitu and satellites-derived chl-a. Please refer to figure 5.

8. p.967 line 16-23 “Although turbidity measurements in the Strait of Malacca (average of 3.3 FTU) were significantly higher than those in the South China Sea (average of 0.3 FTU; Table 1), coinciding with high CHBr3, the turbidity was almost as high close to land near Semporna (average of 2.1 FTU for Stations 24–27), but. . ....” The paragraph needs to be clarified.

   We believe these sentences are already clear, and refer back to our reply to point 4. There is not necessarily a direct relationship between what is in the water and what we measure in the atmosphere immediately above.

9. Table 1 There is an error in the cited values (bottom row). The mean for CH_2Br_2 (1.3) is out of the range (0.2-0.5).
   We have corrected the value 0.2-1.9.

10. Figure numbering is confusing.
    We have corrected the figures number.