We thank both reviewers for their constructive comments. Our responses are given below in red text.
Response to anonymous referee #1

1. Page 956, line 25-26: Isn't the western Pacific a part of the tropical region?
   We agree that this, and the following (referee point 2), sentence could be clearer and have therefore re-written them as follows: Importantly, such convective transport appears to be particularly strong over the western Pacific (Gettelman et al., 2002; Fueglistaler et al., 2004).

2. Page 956, line 27-29: Maximum precipitation doesn't necessary mean deep convection, if it is the precipitation related to large-scale ascent?
   See response to point 1.

3. Page 957, line 29: "a short period in 2008" – be more specific. How short? A couple of weeks, months, etc?
   Yes, we now note in the text that the Pyle et al measurements cover 3 weeks.

4. Page 958, line 19: Change "Experimental" to "Observations" or "Measurements"?
   We have changed 'Experimental" to "Measurements'.

5. Page 960, line 12-14: "we believe the concentration of $CHBr_3$ in the working standard has declined by approximately 40% over the period October 2008–September 2012". I am not an expert on measurements, and I am confused by what is the underlying implications if the NOAA working standard for $CHBr_3$ declines by 40% between October 2008 – September 2012.
   There are no underlying implications from the drift seen in the working standard (which, incidentally, is not a standard supplied by NOAA). We simply note that there has been a 40% decline in the concentration of $CHBr_3$ in this particular aluminium tank and that we have time corrected our data to account for this. Other groups using this type of tank should be aware that concentrations of $CHBr_3$ can change with time.

6. Page 962, lines 17-18: "Figure 2 also shows the anthropogenic tracer $C_2Cl_4$". I don't find $C_2Cl_4$ on Figure 2.

   $C_2Cl_4$ should not have been in Figure 2, and the reference to it has been removed.

7. Page 963, lines 17-24. I suggest delete this part as I don't see the importance of this discussion in this paper. All five targeted bromocarbons are predominantly of biogenic oceanic origin, therefore the anthropogenic sources are irrelevant.

   We have deleted the following section. 'SM is one of the world's busiest shipping lanes, connecting the Indian Ocean to the Pacific (Tan et al., 2006). With the close proximity of Singapore and the numerous oil and gas platforms in the area, it is also possible that, for some halocarbons, 20 concentrations here might be strongly influenced by anthropogenic sources. For example, Yokouchi et al. (1997) measured anthropogenic short-lived C2Cl4 increased near Singapore and the affects suggested to be very local. However, of the anthropogenic compounds measured (CFCs, halons and C2Cl4) none were enhanced in samples 2–4."

8. Page 965, lines 2-8: I don't understand what the authors are trying to say here or may be this paragraph is just poorly written. It is more straight-forward and clear if you explain things using one-to-one quantitative comparison, i.e. the mean concentrations, standard deviation, and range from PESC-09 vs. those from Yokouchi et al., 1997, 2005, Pyle et al., 2011.

   We have rephrased the paragraph, and now direct the reader to Table 1 which makes the requested one-to-one quantitative comparisons.

9. Page 965, line22: Figure 7 should be Figure 3.

   We have corrected the figure number.

10. Page 965-966: It would be good to mark the geographic locations, e.g. South Java Sea, and Sipadan Island, on Figure 3, since these are not familiar names to the majority of the readers.

    We have added South Java Sea to figure 3 and more geographical details, including Sipadan Island, in figure 1.

11. Page 966, line 19: Fig 5 should be Fig. 4.

    We have corrected the Figure number.

12. Page 966, lines 22-29. Please explain what SeaWiFS turbidity indicates. It might not be apparent to every reader.

    SeaWiFS generates a measure of chl-a, not turbidity. The turbidity was only measured by the *in situ* CTD instrument. We will now make this explicit in the text.

13. Page 966, line 28-29. I am exactly sure what the authors mean here "the points that fall above this line . . .". Do you mean the points that do not follow the positive linear regression line, then these are better categorized as samples with MODIS-measured chl-*a* concentrations above 1 mg $m^{-3}$.
    Yes, we agreed this text could be clearer. We will now follow the referee's suggestion and write 'The points that do not follow the positive linear regression line are characterised by satellite chl-a concentrations greater than 1 mg $m^{-3}$, and turbidities greater than 0.5 FTU, implying that the satellite sensors over-estimate chl-*a* under such conditions. Indeed the *in situ* measurements showed that ship-board measurements of chl-*a* are lower than those made in the open ocean.'

14. Page 966-967. In section 2.2, the authors discussed the substantial difference in the temporal and spatial scales between the monthly-averaged and 9km x 9km averaged satellite data and in situ measurements. In figure 4, please clarify if you used the monthly averaged satellite data or the 8-day averaged data. If the monthly data were used, please at least comment on how the above mentioned "substantial difference" may impact the comparison in Figure 4 – in other words, with the limitation of the

spatial and temporal coverage, is such a comparison meaningful to draw any conclusions?
We used monthly data for June and July 2008. We think this is clear in the text but have also made this explicit in the caption for figure 4. In addition, we have added some text to the end of this paragraph to remind the reader of the uncertainties discussed in section 2.2.

15. Page 967, lines 4-6. Fig 6 should be fig 5. Here the authors say "plots of halocarbons vs. in situ chl a show no correlation with satellite's chl a". I am confused since the satellite's chl-a are not shown here at all? Did the authors calculate the correlation coefficients? Please include the r values.

    We corrected the figure 6 to figure 5. We had modified the paragraph with 'Plots of bromocarbons versus satellite's chl-*a* concentration (figure 5) shows a positive correlation, with the highest mixing ratios of $CHBr_3$ and $CH_2Br_2$ associated with above average chl-*a* values (> 5 $mgm^{-3}$) for MODIS and SeaWiFS satellites. Both satellites show for example, R>0.6 ($p<0.01$) for $CHBr_3$ and $CH_2Br_2$ but other species shows weak correlation with for both satellites. *In-situ* chl-*a* (not shown) show negative correlation for all bromocarbons species with R=-0.26 ($p>0.01$) and R=-0.21 ($p>0.01$) for $CHBr_3$ and $CH_2Br_2$, respectively'.

16. Page 967, line 12: "In this context, satellite-derived chl a may potentially be more relevant than in situ measurements". Why? Please explain.

    We have added the following explanation; The above finding is not necessarily surprising even if phytoplankton are a source of such gases, since a connection between bromocarbons measured in the marine atmospheric boundary layer and sub-surface biology may be dependent on other factors including wind speed. Furthermore, the observed halocarbon concentrations might originate over a wide geographic area and are not necessarily driven solely by localised emissions. In this context, satellite-derived chl-*a*, also providing information from a wider area, may potentially be more relevant than *in situ* measurements.

17. Page 968, line 5: 24 days should 26 days.
    We have corrected 24 days to 26 days.

18. Page 968, line 18-21. Figure 3 should be figure 6. Figure 4a and b should be figure 7a and b. Also the legend in figure 6 says it is a "log-log plot" which it is now. Please correct.
    We have corrected all comments in the text; Figure 3 to figure 6, Figure 4a and b to Figure 7a and b.

19. Page 968, line 21. Why the correlation between CHBr3 and CHBrCl2 is much lower than the other two? Any explanation on what this may indicate?

    While it is not entirely clear to us as to why the correlation should be lower (r=0.5 for $CHCl_2Br$, r=0.7 for $CHClBr_2$), it is known that different seaweed types can release bromocarbons in different proportions (Leedham et al., (2013; Keng et al. 2013). We now note that the progressively weaker correlation as the degree of chlorination increases is in fact largely similar to the emission patterns observed in these two studies.

20. Page 969, line 13. Figure 4b should be figure 7b.
    We have corrected the Figure number.

21. Page 969, line 25. The use of Warwick et al (2006) CH2Br2 emission number as a reference is problematic. The global emission estimate of CH2Br2 from Warwick et al., (2006) ( 113 Gg/yr) has been suggested to be too high according to Liang et al., (2010), Ordonez-2012, Zidka-2013, Hossaini et al. (2013) (62-67 Gg/yr).

    We agree, and now use an updated version of the Warwick inventory, in which emissions are halved. The global total of 57 Gg/yr is consistent with the more recent work of other authors. In this revised case, with a Southeast Asian $CH_2Br_2$ emission of 6.4 Gg/yr, we obtain a regional $CHBr_3$ emission of 32 Gg/yr. As noted below, this new value is consistent with other recent studies focussed on the region.

22. Page 971, line 25: It is good to stick with the same lifetime for CHBr3 (26 days) throughout the text.

    We have corrected the lifetime for $CHBr_3$ to 26 days throughout the text.

23. Page 972, line 2-3. It is exaggerating to call it "reasonable agreement" if your estimate is almost 30% higher than the upper limit estimate from Pyle et al. (2011).

    Our new estimate (see reply to point 21) is in reasonable agreement with the estimates of both Pyle et al. (2011) and Ashfold et al. (2014).

24. Page 972, lines 6-15. Please be more accurate in summarizing the conclusions. According to Figure 4, there is a nice correlation between the satellite data and in situ measurements for low turbidity samples, but not for turbidities > 0.5 FTU.

    We are little confused by this comment, and we believe what we have written is accurate.

25. Table 1. It would be good to include in table 1 the number of samples taken at each site.

    We have added the number of samples taken to table 1

26. Figure 1: Color bars are too small and units are missing. Please make them more visible.

    We have increased size of the colour bars and added the units.

27. Figure 4. It is hard to separate the MODIS symbols from the SeaWiFS symbols for chl-a > 0.5 FTU category. Make them bigger or change the symbols.

We have increased the size of the symbols in figure 4.

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 “$CHCl_3$” Misspelling for “$CHBr_3$”?
   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.