

Interactive comment on “Bromocarbons in the tropical coastal and open ocean atmosphere during the Prime Expedition Scientific Cruise 2009 (PESC 09)” by M. S. Mohd Nadzir et al.

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We thank both reviewers for their constructive comments. Our responses are given below: 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).

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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 CHBr₃ 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 CHBr₃ 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 CHBr₃ 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 CHBr₃ can change with time.

6. Page 962, lines 17-18: “Figure 2 also shows the anthropogenic tracer C₂Cl₄”. I don't find C₂Cl₄ on Figure 2.

C₂Cl₄ 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

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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
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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 cor-

relation, with the highest mixing ratios of CHBr₃ and CH₂Br₂ 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 CHBr₃ and CH₂Br₂ 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 CHBr₃ and CH₂Br₂, 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 CHCl₂Br, r=0.7 for CHClBr₂), it is known that different seaweed types can release bromocarbons in different proportions (Leedham et al., (2013; Keng et al. 2013). We now

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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 CH₂Br₂ emission of 6.4 Gg/yr, we obtain a regional CHBr₃ 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 CHBr₃ 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.

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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.

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/14/C2607/2014/acpd-14-C2607-2014-supplement.pdf>

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 953, 2014.