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Comment

## ***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.***

### **Anonymous Referee #1**

Received and published: 10 March 2014

Mohd Nadzir et al. present new measurements of  $\text{CHBr}_3$ ,  $\text{CH}_2\text{Br}_2$ ,  $\text{CHCl}_2\text{Br}$ ,  $\text{CHClBr}_2$ ,  $\text{CH}_2\text{BrCl}$  from the Strait of Malacca, the South China and Sulu-Sulawesi Seas during June–July 2009. They also examined the correlations among individual bromocarbon species and their ratios, as well as the correlation between bromocarbons and chlorophyll *a* to examine and quantify sources of bromocarbons. The results are interesting, and data from a rather under-examined region are presented. Therefore, I recommend the paper to be published after my following comments are addressed.

My major concern for recommending the paper to be published is, in its current form, the paper is just written poorly and certainly not of publication quality. I have spotted

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numerous grammatical errors. And in many places, the wordings are either redundant or awkward. In many occasions, the use of certain words is just not accurate for a scientific peer-reviewed article, and therefore greatly impairs the clarity and quality of this paper. There are also sloppy careless mistakes. For example, of all 7 figures, 5 of them are referenced with the wrong figure numbers in the text (see below). I have included my commented PDF manuscript as suggestions for re-wording, etc. However, I strongly encourage the help from a native English-speaker, or one of the co-authors, to help read through the final manuscript carefully before re-submission.

Minor Comments: 1. Page 956, line 25-26: Isn't the western Pacific a part of the tropical region? 2. Page 956, line 27-29: Maximum precipitation doesn't necessary mean deep convection, if it is the precipitation related to large-scale ascent? Please clarify. 3. Page 957, line 29: "a short period in 2008" – be more specific. How short? A couple of weeks, months, etc? 4. Page 958, line 19: Change "Experimental" to "Observations" or "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. Please explain. 6. Page 962, lines 17-18: "Figure 2 also shows the anthropogenic tracer C2Cl4". I don't find C2Cl4 on Figure 2. 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. 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. 9. Page 965, line22: Figure 7 should be Figure 3. 10. Page 965-966: It would be good to mark the geographic locations, e.g. South Java Sea, and Sipaden Island, on Figure 3, since these are not familiar names to the majority of the readers.

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11. Page 966, line 19: Fig 5 should be Fig. 4. 12. Page 966, lines 22-29. Please explain what SeaWiFS turbidity indicates. It might not be apparent to every reader. 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. 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? 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. 16. Page 967, line 12: “In this context, satellite-derived chl a may potentially be more relevant than in situ measurements”. Why? Please explain. 17. Page 968, line 5: 24 days should 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. 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? 20. Page 969, line 13. Figure 4b should be figure 7b. 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). 22. Page 971, line 25: It is good to stick with the same lifetime for CHBr3 (26 days) throughout the text. 23. Page 972, line 2-3. It is exaggerating to call it “reasonable agreement” if your estimate is almost

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30% higher than the upper limit estimate from Pyle et al. (2011). 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. 25. Table 1. It would be good to include in table 1 the number of samples taken at each site. 26. Figure 1: Colorbars are too small and units are missing. Please make them more visible. 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.

I didn't list the editorial comments, but they are available in the commented pdf file (supplement).

Please also note the supplement to this comment:

<http://www.atmos-chem-phys-discuss.net/14/C368/2014/acpd-14-C368-2014-supplement.pdf>

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