

Interactive  
Comment

## ***Interactive comment on “Deriving an atmospheric budget of total organic bromine using airborne in-situ measurements from the Western Pacific during SHIVA” by S. Sala et al.***

**Anonymous Referee #2**

Received and published: 30 April 2014

General comments:

This paper presents new airborne measurements from organic bromine sampled above the Malaysian coast and the South China Sea during the SHIVA campaign 2011; published within the SHIVA ACPD special issue. This paper contains relevant new data which are suitable for the scope of ACP. The scientific results are presented in a clear and structured way; the use of English is appropriate. However, I am a bit disappointed by the lack of considering relevant new work, by the context of the background/introduction as well as by the discussion and conclusions of the paper. Overall it became a bit boring data presentation, although an exciting unique data set for the

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Malaysian part of the tropical West Pacific exists! Here, the authors could have worked more thoroughly, searching through the new available publications (since WMO, 2011) as well as cross-linking their own work with other publication within the SHIVA consortium/ special issue also to get more inspired. A list of critical details is given below. Due the amount and kind of corrections the paper may be publishable after a careful revision.

Critical details:

The title should be changed as the SHIVA aircraft campaign did not cover the whole “tropical” Western Pacific but only Malaysian/South China Sea coastal and open waters! To be able to state that the “tropical” Western Pacific was investigated the authors would have to add more available and published data for that area including the HIPPO aircraft campaign (e.g. Tegtmeier et al., 2013 ACP SHIVA special issue), NOAA/ESRL ground station measurements (e.g. Hossaini et al., 2013 ACP SHIVA special issue) as well as the TransBrom ship campaign (Brinckmann et al., 2012; Tegtmeier et al., 2012).

In principle the authors tend to often exclusively cite Montzka et al (2011) neglecting the original as well as newer publications since WMO (2011). They do, however, cite their own new papers Brinckmann et al. 2012 and Wisher et al. 2013. But then they neglect to cite their own open ocean tropical West Pacific VLSLS measurements from Brinckmann et al (2012). Here a more thorough literature search and critical data comparison are needed, see the detailed comments below.

I strongly suggest adding a map for the flight tracks to get an idea how spread out the SHIVA FALCON flights really were. This should be then Figure 1.

Abstract:

P. 4958, lines 15-16: Rewrite the sentence “...could be a major course of brominated...”! If you want to refer to the “source” of VLSLS you need to cite the production of VLSLS in the ocean! I guess you mean here elevated atmospheric abundances around NE

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Borneo published by Pyle et al (2011).

P. 4958, Line 22: “while being slightly higher” is a contradiction to the statement in line 15 “major source” > rewrite!

Introduction:

I miss the background for the different oceanic sources of the different brominated VLS, see comment below. Add this to the intro.

P. 4959, lines 6-13: Rewrite this passage. I disagree with the message here that SG observations “...of VLS have so far not been able to explain the amount of bromine derived in the stratosphere...” The three refs given here are not representing the current knowledge and substantial other references are missing for the VLS part (line 8) e.g. Hossaini et al 2010; Liang et al 2010; Aschmann et al 2009; Tegtmeier et al 2012; Ziska et al. 2013; Hossaini et al. 2013...

P. 4959, lines 10-13: Rewrite the whole sentence; this is not reflecting the current state of science here. We do have more insights of spatial and temporal variability of VLS given the extensive fieldwork by ship and coastal measurements and observations based climatologies by e.g. Yokouchi et al; Carpenter et al., Pyle et al, 2011; Fuhlbrügge et al 2013 ACP SHIVA special issue; as well as Palmer and Reason 2009; Ziska et al 2013 for the observed based climatologies among others!

P. 4959, lines 25, 27 and 29: Add specific references here for the PG papers: Hossaini et al 2010, Liang et al 2010, Tegtmeier et al 2012. Here, no references are given at all! And please do not only refer to Montzka et al 2011 as a sum up for all available references. You need to give credit to the original publications as well as to new papers published after WMO (2011).

P. 4960, L. 4: “these emission”. You were not talking about any emission before at all. In fact you are totally neglecting oceanic sources of VLS and their emission! This should be added to the introduction as well. This is an important SHIVA outcome!

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P. 4960, L. 13-15: The “tropical” West Pacific is expected to be the most important source region...” The sentence is not correct, please rewrite. The referred publications analysed only the Tropical Tropopause Layer thus “the atmosphere above the tropical West Pacific”. In Aschmann et al 2009 they simulated VLSL transport from the upper troposphere into the stratosphere. None of these studies looked at the oceanic sources of the VLSL from the ocean surface to the stratosphere. However, unique data exist for the tropical West Pacific and are already published by e.g. Butler et al 2006 JGR, Tegtmeier et al 2012 ACP, Krüger and Quack, 2013 ACP; HIPPO aircraft campaign (e.g. Tegtmeier et al 2013, ACP SHIVA special issue) and should be cited here as well. Given that the SHIVA campaign included also a ship expedition, which was directly linked with the FALCON aircraft measurements, I totally miss the link between the aircraft and the ship measurements within this paper. You also need to cite other SHIVA relevant results for the VLSL sources in the coastal and open waters of the Malaysian waters/ South China Sea here (Leedham et al 2013 ACP SHIVA special issue; Fuhlbrügge et al in preparation), which are important for the interpretation of your own VLSL air measurements.

Line 4960, Lines: 22-25: Not correct, please rewrite! Hossaini et al 2013 carried out chemical transport model simulations using different VLSL climatologies as input and compared these model simulations with NOAA/ESRL ground stations measurements.

P. 4968-4969, L 18-4: How in detail was the BL height calculated or derived? This is totally unclear! How was it done for the radiosondes launched onboard of the ship in contrast to the aircraft data? There must be technical differences? Where were the radiosonde measurements carried out and where the aircraft data (new Fig. 1)? The statement citing Roedel reference does not fit for the convective active West Pacific, better delete! Since 2012 there is no IFM-GEOMAR anymore, now called GEOMAR.

P. 4970, L. 4: What is with VLSL data between 450m and 2 km? You totally neglect them?

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P. 4971, L. 10-13: Exclusion of the high VLSL abundances for the flight 20111119a, but why? Could there not be high oceanic sources at the coast between Miri and Kuching? Please cross-link with oceanic measurements taken from the SHIVA campaign as well! Use the advantage of being a member of a multidisciplinary EU project to deliver more insights in this interesting field of science! In general, I believe, high priority should be given for a special issue journal to carefully cross link between different publications.

Conclusions:

P. 4985, L. 1-2 But why are the values higher than for Montzka et al 2011? Which areas did the Montzka et al/WMO 2010 data cover? What is different? Were any tropical West Pacific coastal and open data included? No, just recently the HIPPO aircraft and now ATTREX aircraft data are carried out for the tropical West Pacific.

P. 4985, L. 20: rewrite: “This shows that the West Pacific may be a preferred...”. Cut out “may” as we do have evidence for this (see my comments above) and ex-change “West-Pacific” with “Malaysian coastal and open waters” (see also my title comment above).

P. 4985, L. 23-26: Rewrite as there are a lot of new papers with VLSL measurements and modelling together showing clear evidence for the tropical West Pacific being the source region for oceanic VLSL into the stratosphere e.g. Warwick et al 2006; Tegtmeyer et al 2012/2013, Hossaini et al. 2013.

Table 6: Compare with other new publications. There are a lot of new VLSL data for the MBL available! Cite and use other available tropical West Pacific data from Pyle et al 2011, Brinckmann et al 2012, Ziska et al 2013, Hossaini et al 2013! Do not stick to the “older” WMO (2011) data.

Table 8: Here you compare coastal and open Malaysian waters, an area with extensive convective activity, with VLSL measurements outside of the tropical West Pacific area! Clarify this in the corresponding text of the ms. This may explain the large differences

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between the two.

Figures 2, 4, 5: The outliers for 20111119a may not be measurement problems, but may be due to elevated oceanic sources. Please check with oceanic measurements carried out within the SHIVA campaign and add this to the discussion of the paper.

Minor and technical comments:

Whole ms: Three different usages of boundary layer abbreviations: PBL, MBL, BL, please synchronize.

Whole ms: Montzka and Reiman et al 2011 should be exchanged with Montzka et al 2011.

P. 4958 Line 18: “ all four halons” unclear which one you refer to here! Be specific.

P. 4959, Line 4 and P 4958 Line 13/14: Use the same abbreviation for VSLS.

P. 4959, L. 16 Add “oceanic upwelling regions” as well.

P. 4959, L. 19: unclear, correlates with what?

P. 4960, L. 5: double use of “change”, rewrite.

P. 4961, line 20: November-December is not “fall” anymore! Either write early winter or write out the month.

P. 4964, l 2: Unclear, what is the sampling interval now? Every  $\sim 7$  min or every 4.3 min?

P. 4965, L. 12: Unclear to me, what is the sampling interval for WASP? Every 20 to 180 s?

P. 4971, L. 21-22: Flight 20111119b was also probing convective outflow!

P. 4973, L. 21-23: High atmospheric VSLS abundances due to high oceanic sources?

References:

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Newell and Gould-Stewart: something is wrong with the reference details.

Oram et al 1995: write capital letters for HCFC.

Table 1; MBL stands for?

Table 4 and 6: Shift the PBL data to the left and UT/ MBL to the right as described in the table caption.

Table 7: If possible compare also with HIPPO aircraft data! Clarify that Wisher et al 2013 is eq. CARIBIC data!

Table 10: “of an individual substances” Sg or PI?

Table 11: Which WMO data? Reference is missing here.

Figure 2: Blue bars are not visible.

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Interactive comment on Atmos. Chem. Phys. Discuss., 14, 4957, 2014.

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