

## ***Interactive comment on “Very short-lived bromomethanes measured by the CARIBIC observatory over the North Atlantic, Africa and South-East Asia during 2009–2013” by A. Wisher et al.***

**Anonymous Referee #2**

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This is an interesting, useful, and well done study reporting measurements of short-lived brominated gases in the upper atmosphere along three different ongoing flight tracks that cross a wide range of latitudes and longitudes. Because of the interest in quantifying the contribution of these gases to stratospheric Br and the unique nature of the location of these samples this is an important and valuable contribution. The writing is clear and touches on many of the important issues related to this issue such as measurements, calibration, atmospheric gradients, stratospheric input, etc. The figures provide a good presentation of many different aspects of the measurements.

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As a result I favor publication after consideration of a few fairly minor points that are described below.

Seasonality is mentioned in the text in a couple of places, but it is unclear why this is invoked as a potential means by which differences can be explained as opposed to variations in transport and upwind emission rates. I refer to p. 29957, lines 26–28: is there evidence that suggests seasonal variation in some aspect of sources or concentrations of these gases? It isn't shown or cited here. Also on p. 29963 lines 25–28, the message being conveyed from these statements is unclear. (Note I am not talking about the seasonality discussion of Figure 4, which is clear and useful as is).

On relative abundances of VSLs. P. 29957, line 1–6, is it possible that source variations could contribute some to this anomaly? If so, modify the text. In Section 3.3, it is not well described why the high mixing ratio of CH<sub>2</sub>Br<sub>2</sub> relative to CHBr<sub>3</sub> in the BKK-KUL leg needs such an in depth discussion; inform us why this difference is more interesting than the others. HIPPO-4 data are mentioned here, but the discrepancy in this region is not apparent in that data. Also, local lifetimes in WMO are not representative of any specific atmospheric region and are not likely to be representative of the true loss frequencies in this region of the atmosphere. To make this discussion useful you'll need to consider some realistic values for OH and photolysis in this region.

On the peak in tropics observed during the South African flights (Figure 7): Given the relatively few samples and the fairly high degree of variability in concentrations, it seems a stretch to conclude that these gases (minus CH<sub>2</sub>BrCl) all are higher in the tropics. They give a hint of this, sure, but extratropical NH results do not look very different. Consider some rewording.

On HIPPO data and its use. Multiple groups reported data for CH<sub>2</sub>Br<sub>2</sub> and CHBr<sub>3</sub> from the HIPPO campaigns. Did you use only the data from the Univ. of Miami, or the entire set of results? This needs specification in the text, as a subset of results is likely to provide different means than the entire set even with the data being referenced to a

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common scale. I'm surprised the HIPPO team involved with these mostly unpublished measurements wasn't brought more fully into this analysis; I imagine they could have contributed quite a bit to this paper.

Details: On figures. Overall the figures do a very nice job of displaying the data and making the points being described in the text. It seemed implied from the text that data displayed in Figures 5 and 6 were only from the extratropical tropopause layer; if so, please indicate this in the caption. In the Figure 5 caption, the description of the averaging is unclear. Furthermore, the text relating to the delineation of color in Fig 5 could be improved, what "degree of stratospheric influence" is used to separate the colors (p. 29959, l22)? In the Figure 6 caption, indicate if the tropopause is at  $z_{O3}=0$ .

The WMO reports provide a useful service in compiling and assessing results from the community, so citing them is important. Yet citing them alone leaves out critical information valuable to the reader. Consider describing the results reported in WMO as a compilation of data from multiple sampling mission by many different groups, and possibly also describing (if not also citing) some of the methodologies and results that were compiled in the WMO reports. Consider also that some of the uncertainty in the Br delivered to the stratosphere from VSLs arises from kinetic parameters that have uncertainty (p. 29951, lines 1-5).

Stratospheric ozone depletion is associated with increased anthropogenic trace gas emissions. Consider clarifying the text on p. 29950, lines 20-23 to avoid the interpretation that VSL gases might be a significant contributor to this time-dependent problem.

p. 29957, l. 23, value for CH<sub>2</sub>Br<sub>2</sub> doesn't agree with Table 2 entry.

p. 29959, lines 1-4. I presume you are referring to VSLB in the form of organic Br here, as opposed inorganic Br from the degradation of organic trace gases. This distinction needs to be made for the sentences to be accurate.

Table 1., I agree with the other referee, specify the concentrations for which the stated

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precision is relevant.

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Interactive comment on Atmos. Chem. Phys. Discuss., 13, 29947, 2013.

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