

***Interactive comment on* “The contribution of oceanic halocarbons to marine and free troposphere air over the tropical West Pacific” by S. Fuhlbrügge et al.**

Anonymous Referee #2

Received and published: 7 August 2015

Review of:

The contribution of oceanic halocarbons to marine and free troposphere air over the tropical West Pacific by Fuhlbrügge et al.

This paper presents seawater and atmospheric measurements of CHBr_3 , CH_2Br_2 and CH_3I obtained during the ship cruise and aircraft campaign of the SHIVA project. Samples were obtained around Borneo in November–December 2011 and are used here to derive ocean emission fluxes in the region and, with the aid of transport modelling, the contribution of the above gases to the free troposphere. Given the likely importance of

C5711

this region for the transport of air masses to the stratosphere, and given that the region is poorly sampled (in terms of VSLs), this paper is a useful addition to the literature, providing a good synthesis of the SHIVA measurements. I have no major objections to the method and therefore recommend the paper for publication.

My main concern with the paper is that I found it extremely difficult to read and somewhat convoluted in many parts. I encourage the authors to carefully check the manuscript for places where the text could be streamlined to improve readability and where the main messages could be distilled to avoid them being diluted by so many numbers and detail. I had to stop listing technical corrections given the sheer enormity of the task. I also encourage the authors to check that the most appropriate citations are given throughout the manuscript. Citing older classic papers is fine (and good) but in many places the discussion would be better served by citing the most up-to-date literature in addition.

For example, in the introduction numbers are given for the “mean atmospheric lifetime” of CHBr_3 , CH_2Br_2 and CH_3I . I don’t understand why the authors refer to such old papers here (Ko et al. 2003 and Solomon et al. 1994) when the most recent and comprehensive evaluation of the lifetimes of these compounds is given in the 2014 WMO O3 Assessment (Chapter 1: Carpenter and Reimann et al.). The concept of a mean atmospheric lifetime for VSLs can be somewhat problematic. I suggest quoting the tropical MBL local lifetime and range from the 2014 report.

Abstract:

Line 10: The sentence beginning “Elevated oceanic concentrations..” is long. Consider using the word “respectively” in the second half; i.e. change to “...with high corresponding oceanic emissions of 1486, 405, and 433, respectively, characterize..”

Line 24: Change “origins” to “originates”

Introduction:

C5712

Line 2: Change “the atmospheric ozone” to “atmospheric ozone”

Add “e.g.” before citations to Solomon (1999) and Saiz-Lopez and von Glasow (2012).

Line 4: Change “via photochemical and heterogeneous reaction cycles from” to “following the photochemical breakdown of”.

Line 14: Change “the halogenated very short lived substances” to “these halogenated very short-lived substances”.

Line 16: “Climate change could strongly affect marine biota. . . “. Are there not other recent papers that also suggest this? For example: Hughes, C., et al. (2012), ‘Climate-induced change in biogenic bromine emissions from the Antarctic marine biosphere’, *Global Biogeochem. Cycles*.

Line 23: Change “methyl iodide” to “CH₃I”. Generally, why bother keep using the full names bromoform, dibromomethane and methyl iodide throughout the text once they have been defined? Check full text and at least be consistent.

Line 25: The sentence beginning “Significantly lower. . .” should be amended. Why talk about just model runs looking at the impact of bromine when the previous discussion was iodine-focused. Numerous recent papers from the Saiz-Lopez group have examined the impact of bromine and iodine on tropospheric O₃ (e.g. Saiz-Lopez et al. 2014, iodine chemistry in the troposphere and its effect on ozone, *ACP*, 2014). As we are talking about VSLS in this paper, an appropriate citation would also be to Hossaini et al. (2015, *Nature Geoscience*) who examined their impact on UTLS O₃. Please include these additional citations and add “e.g.” before the list.

Line 17: The sentence beginning “The goal of SHIVA” is odd. What that really the main objective? Consider “was to combine observations of VSLS and models to better understand the processes contributing to ozone loss in the stratosphere and how such factors could respond to climate change.” This seems more accurate to me.

Data and Methods:

C5713

Why does the sub-sub section “2.1.2 Aircraft campaigns” come under the SHIVA SONNE subsection? Could Section 2.1 simply be named something like “Overview of ship cruise and aircraft campaign”? Given the length of these sections, I don’t think the subsections are needed.

Section 4.2:

Line 20: Is it possible to comment more on the possible different sources for CH₃I compared to the bromocarbons here?

Line 27 on river run: This point needs expanding. Why would the bromocarbons be elevated due to river run and how is the influence of river run detected?

Discussion:

Line 10: WMO (2015) should be WMO (2014)

Interactive comment on *Atmos. Chem. Phys. Discuss.*, 15, 17887, 2015.

C5714