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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Transport of short-lived halocarbons to the stratosphere over the Pacific Ocean" *by* Michal T. Filus et al.

Anonymous Referee #1

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This work builds on the 2014 joint CAST/CONTRAST/ATTREX missions where VSLS (CHBr3, CH2Br2, CH3I) measurements were made in the tropical West Pacific. Here, the NAME model is used to compute back trajectories from the VSLS measurement location/times and to determine the fraction of released particles that crossed the boundary layer in the preceding 12 days. With this information, the authors estimate the influence of the boundary layer on VSLS mixing ratios (during the campaign period) throughout the vertical extent of the TTL and compare model estimates of VSLS to the actual observations.

I find the description of the method reasonably straight forward to follow and the paper interesting. The results are certainly within the scope of ACP and, in addition to describing a method for interpreting measurements, the paper contributes some analysis

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Discussion paper

on differences in measured VSLS concentrations between ATTREX 2013 (W Pacific) and 2014 (E Pacific). My main concerns (outlined below) are on the use of assumed chemical decay times and on some aspects of the manuscript presentation. Both issues could be addressed readily, and I recommend the paper for publication.

(1) The authors use constant chemical decay lifetimes of 15 days and 94 days for CHBr3 and CH2Br2, respectively, based on the boundary layer estimates given by Carpenter et al. (2014). Can the use of a fixed lifetime be justified given that local lifetimes of the above compounds are known to vary substantially between the surface and in the TTL (e.g. Hossaini et al., 2010, Liang et al., 2010)? These references show a much longer TTL CH2Br2 lifetime than 94 days, for example. Accounting for photochemical removal along trajectories is important and the authors should comment on how sensitive their findings (e.g. boundary layer contributions in the TTL) are to the lifetime assumptions.

(2) The presentation of the manuscript could be improved in several places. Specific suggestions are given below. Additionally, throughout the manuscript the authors should consider whether the citations given are the most appropriate to the points made in the text. An example is on Line 50 where the point is that VSLS are emitted from the ocean and have natural sources. Given that, citations to modelling work looking at impacts of iodine/bromine chemistry (Solomon, Vogt, Salawitch, Saiz-Lopez) seem somewhat out of place. More appropriate and recent references would be, for example:

Hepach, H., et al. Biogenic halocarbons from the Peruvian upwelling region as tropospheric halogen source, Atmos. Chem. Phys., 16, 12219-12237, 2016.

Hepach, H., et al. Halocarbon emissions and sources in the equatorial Atlantic Cold Tongue, Biogeosciences, 12, 6369-6387, 2015.

Yang, G. et al. Spatio-temporal variations of sea surface halocarbon concentrations and fluxes from southern Yellow Sea, Biogeochemistry, 37 121(2), 369-388, 2014.

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I suggest the authors thoroughly proof the paper for similar instances and areas where readability could be improved.

Specific comments: -Line 38: The issue of “continued depletion in the lower stratosphere” is debatable. Mid-latitude and tropical ozone in this region is strongly influenced by transport and much of the apparent downward trend reported by Ball et al. appears to be have been reversed in 2017, as shown by Chipperfield et al. (2018). I would encourage the authors to amend this sentence to a more precise one.

Chipperfield, M. P., Dhomse, S., Hossaini, R., Feng, W., Santee, M. L., Weber, M., et al. (2018). On the cause of recent variations in lower stratospheric ozone. Geophysical Research Letters, 45, 5718–5726. https://doi.org/10.1029/2018GL078071

-Line 47: on first appearance spell out the name of these compounds: i.e. methyl iodide (CH3I), bromoform (CHBr3) and dibromoethane (CH2Br2).

-Line 52: Is there a reason why specifically 12 days is chosen? In the Discussion (line 461), it is noted that longer periods are tested but the details are very vague. I would state earlier on in the manuscript that sensitivity tests were performed and be more quantitative on what was found.

-Line 82: “east” — “East”

-In Section 2.1 it would be useful to indicate the altitude limits of the various aircrafts. Related to this, it would help the reader to know how the TTL is being defined up front.

-Line 140: The citation to Jones et al. should probably appear directly after NAME.

-Line 215: Should “Research Flights” have capital letters?

-Line 217: “very short lived brominated substances” could be deleted

-Line 222: Starting a sentence with this number is a bit odd. Consider rewording or spelling out the number.

-Line 223: “TTL” — “the TTL”

-Line 248: “western and central” — “Western and Central”

-Line 387: define MJO

-Figure 1 caption: I recommend reworking as brackets within brackets looks odd here.

-Figure 2 caption: What are the black symbols? Should also indicate if box and whiskers are the same as Figure 1.

-Figure 4: A reduced x-axis scale for each species would improve readability of the data.

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