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

Anonymous Referee #2

This paper about “Transport of short-lived halocarbons to the stratosphere over the Pacific Ocean” by Michal T. Filus et al. reports about transport of VSLS above the West Pacific using the Lagrangian model NAME and new aircraft observations from the joint CAST, CONTRAST, ATREX campaign in Jan-Mar 2014. The authors use an improved NAME version which includes a convection scheme. This methodology has been applied to many VSLS transport studies before and is a common procedure in the community. However, as the authors investigate the VSLS transport from the boundary layer to the stratosphere comparing it with a new aircraft campaign and a further developed model version of NAME I believe it can fulfil the criteria to be published in ACP after carefully revising the paper including better specifying the new perspective of your study, the state of the art and background in this field and a thorough discussion of the study uncertainties.

We thank both reviewers for their constructive comments. In our opinion, these have resulted in an improved manuscript.

See my specific comments below.

I) What is really new in your study? To use a Langrangian dispersal model including a convection scheme is nothing new in this field. Next there were several studies including the VSLS contribution to the stratosphere for CAST/CONTRAST/ATREX. Thus, I suggest to think carefully about what is different and thus really new compared to example (i) the old NAME VSLS studies, ii) the FLEXPART model and VSLS studies (including a convection scheme) and iii) compared to other VSLS CAST/CONTRAST/ATREX studies, see Wales et al 2018 JGR. This new perspective should be clearer addressed in the introduction and could be added to the discussion of your results.

The main new aspects of this study are:

(a) the validation and use of an improved convection scheme for use with the NAME trajectory model. The previous scheme was reasonable for convection at mid-latitudes but was far too weak to represent the stronger tropical convection. Comparison with the extensive CH_3I measurements made in this campaign provides good support for its use in modelling transport in tropical convective systems.
(b) The old convective scheme was used in the earlier study by Ashfold et al (2012) using the East Pacific measurements, so the new scheme represents a considerable improvement which found reasonable agreement only up to and including the level of maximum convective outflow.
(c) We have extended the approach used by Ashfold et al (2012) so that VSLS mixing ratios can be assigned to contributions from the boundary layer and from the ‘background’ TTL.
(d) The FLEXPART studies focussed on transport up to the level of maximum convective outflow during the SHIVA campaign based in Malaysian Borneo and had a less complete set of measurements to compare with. The surface concentrations and strength of convection over the South China Sea are different to those over the West Pacific in Jan-Mar.
(e) The conclusions of the Wales analysis are based on the Eulerian 3D CAM-chem-SD model while ours are based purely on a trajectory-based approach. The agreement is good.
(f) We compare results from 2 years (2013 and 2014)

We have changed the introduction a bit to lay the groundwork for a summary of these points in the Summary and Discussion.

Line 85: "using a new Lagrangian methodology" I suggest deleting "new" as it is not a new method.

This has now been deleted and has been replaced by 'updated' in several places. The 'new' aspects of the overall methodology we were referring to were (a) it is a measurement-based way of the quantifying boundary layer and background contributions to brominated VSLS budget in the TTL; and (ii) using and testing with CH3I the improved parameterisation for deep convection developed in the NAME model).

II) What is the state of the art in this research field? Here it seems to me that you are mainly referring to new recent studies and did not go back to the original literature. One example is the citation of the oceanic source of VSLS where you mainly cite VSLS modelling studies, which should be original biogeochemical oceanographic articles such as e.g. Carpenter et al., 1999; Moore and Zafiriou, 1994; Quack and Wallace, 2003 among others. Be aware of the different VSLS components which have different oceanic sources and thus will request different articles to cite. Overall, I suggest to carefully going through all references again citing also the specific original work instead of large selections of recent, maybe randomly chosen, papers.

We have improved the discussion on the state of the art in the introduction and changed some of the references.

III) Discuss the uncertainties of your VSLS transport calculations:

What is the uncertainty due to the model and meteorology used, transport processes (e.g. BL vs convection scheme), using constant VSLS life times? (see Hossaini et al 2010, Fuhlbrügge et al 2016). How good is the "Meteorological Office's Unified Model" meteorology compared to the actual observed meteorology? Here, I refer to observed convection events and winds. How much does the use of this specific meteorology fields affect your results?

The uncertainty is likely to be dominated by the errors in the convection. The boundary layer dispersion scheme is likely to be unimportant as we only track the parcels back until they reach within 1km of the surface. Also the winds from the Unified Model (UM) are expected to be accurate, partly because they are from analyses rather than from forecasts, but also because the UM is among the best operational forecast models – see e.g. https://apps.ecmwf.int/wmolcdnv/ . [It is hard to quantify the errors though, because the analysis is, by definition, our best estimate of the truth, obtained by assimilating a range of observations which themselves have errors. Indeed the analysed winds are often used as the benchmark against which to assess forecasts.]

Convection is difficult to predict well, especially with a large scale global model where the convection is sub grid scale. Fig 5 in Geosci. Model Dev. vol. 12, p. 1909 (2019) shows climatological cloud over the Pacific warm pool from the global UM compared with Calipso satellite data. This shows reasonable predictions, although with the convection not being quite deep enough. This is consistent with the comparison between model and aircraft data. We expect the errors for individual convective events to be significant, but the upper troposphere concentrations will depend on a number of convective events and we are considering a range of flights and measurements locations, which we hope makes the conclusions on general behaviour robust. Again the consistency between model and aircraft data supports this. One could attempt a more detailed estimate of errors by using data from a range of models and from ensemble prediction systems, but that would be another project.

We have added some discussion of these issues to the Summary and Discussion section.

-Btw, what kind of model is it (operational, assimilation or?)

We used operational analyses from the UK Meteorological Office in this study. This has been clarified in the text. Operational forecasts were used during the campaign to assist with planning (Harris et al., BAMS, 2017), but are not considered here.

If I understand it correctly you use constant VSLS lifetimes. Is this appropriate (see Hossaini et al 2010, Liang et al 2010) and what would you expect the results to be using vertical varying lifetimes? I assume you cannot change and add new runs anymore, but you should add a clear and thorough discussion here at least!

Please see response to reviewer 1.

How different are your NAME results compared to other transport model studies? (e.g. Fig. 3)?

A comparison of our results with those from Wales et al (2018) has been added at the end of Section 5. There were existing references to Navarro (2015) which included a comparison with the WACCM model and to Butler et al (updated to 2018). Feng et al (2018) is relevant and uses the same observations, but focuses on ocean-atmosphere fluxes so is not comparable. We are not aware of other papers. References to studies of regions outside the Western Pacific are made elsewhere (e.g. Tegtmeier et al 2012, 2013 and Fuhlbrügge et al 2016.

Figures and text: Thoroughly revise your figures quality. Often the labelling is too small and unreadable on my print out. How about adding a line to your profiles?

The figure quality has been improved as suggested, We prefer not to add a line to the plots of the vertical profiles as we think the information is easier for the reader to grasp without it. We are happy to consider further suggestions.

The main text and references still need revision and editorial help (typos).

We have gone through the main text and references carefully.