Comments to:

Surface fluxes of bromoform and dibromomethane over the tropical western Pacific inferred from airborne in situ measurements

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Submitted to ACP

This manuscript explores an important topic regarding the recovery of the stratospheric ozone layer, in particular the emission rates of halogenated very short-lived substances (VSLS) at the surface. These compounds can be rapidly transported into the stratosphere, especially over areas of active, deep convection, and affect stratospheric ozone levels, thereby delaying the recovery process. Very few observations exist in key areas of the world where VSLS emissions and transport rates can be significant to the stratospheric budget of inorganic bromine, so the availability and use of new aircraft data sets constitutes a unique opportunity to test our models and evaluate our inventories.

I will first provide some general comments to the manuscript, followed by some specific ones.

General

1. While the topic is of great relevance, it is unclear what the contribution of the manuscript is, as written. Similar flux calculations were done by previous studies (lines 171-173). Is the contribution of this manuscript related to the methodology used, to the new aircraft data set over the tropical Western Pacific, and/or to the new magnitudes of fluxes obtained in this study?
2. The results presented in the manuscript are based on numerous model assumptions (e.g., lines 215 – 226). Were any sensitivity tests performed on the choice of values used? Are there any references to justify the choice of values used? Paragraphs 4 and 5 in the Introduction highlight how previous studies were based on several (different) assumptions and how those results need to be examined with caution. How can the results from this analysis, along with the assumptions used, be compared against previous studies?
3. The type of correlation between bromoform and dibromomethane is of importance in this analysis. What is the rationale for a linear correlation used in several studies published earlier (e.g., lines 103-105)? Given that the new aircraft data set elucidates a different correlation between the two

compounds, elaborating some more on this topic will highlight one of the new findings from this study.

4. There are several instances of missing punctuation marks such as commas and periods throughout the manuscript.
5. Some of the listed uncertainties are significant (e.g., line 262, line 342). What is the impact of these uncertainties on the conclusions of this study?

Specific

6. Abstract, line 25: An r value of 0.38 does not really qualify as "reasonably consistent" correlation.
7. Abstract, line 36: Which *a priori* inventory was used for the comparison?
8. Introduction, line 47: "The wide range of … lifetimes allows for …"
9. Introduction, line 55: "There is a wide range of…"
10. Data, paragraph 1: Are there any references available for the CAST and CONTRAST instruments?
11. Data, lines 146-149: What are "mean absolute percentage errors"? Which data set is higher? Are the differences uniform with height? How is this metric used in the analysis and how does it impact the results?
12. Data, lines 146-149: The second half of the statement is confusing as stated. WAS refers to the collection method and GC/MS to the analysis technique. Each campaign provided one data set. It might be simpler to state "…between the CAST and CONTRAST instruments", instead.
13. Data, lines 157-158: Is there a reference available to support the statement?
14. Data, lines 163-164: Are the referenced data from NOAA's ground network collected at the surface? Given that this study examines data at higher altitudes as well, are there any model comparisons with data at higher altitudes?
15. Data, line 186-187: Is a 6-month spin-up enough time and seasonally appropriate?
16. Results, line 248: Even with higher a priori ocean fluxes, the model still depletes bromoform much faster between the surface and 2 km than the observations show. Is this a result of chemistry, transport, and/or something else within the model?
17. Results, line 274-278: The right panel of Figure 2 shows that the model's vertical distribution of bromoform is practically the same when run at coarse and fine spatial resolutions. This suggests that sub-grid convection, assuming that the model resolves some events at the finer scale used, does not play a significant role in the modeled vertical profile. Is this result expected for a tracer with a relatively short lifetime and over a region of active, deep convection?
18. Results, line 293-294: How were the 50% and 30% chosen? How sensitive are the results of the analysis to these percentage choices?
19. Figure 1, line 445: Suggest using "15^{o}S – 25^{o}N"
20. Figure 1 and Figure 2: Are the in situ data shown in these figures an average of both aircraft data sets?