

We thank reviewer 1 for making their review and for highlighting a series of problems that need to be addressed. We thank reviewer 1 for taking the time to make an extensive and thorough review. Based on the general comments of both reviewers, we felt it was necessary to restructure the paper in a major way. Below we give a summary of each of the reviewer's arguments and then we provide a general outline of these major changes to each general comment separately.

We have responded in to each of the points raised. Modifications made to the article are shown in bold.

We address each of the reviewer's points in turn in the same order they are listed, and we will refer directly to their numbering system.

1.i. The reviewer has highlighted the apparent inconsistency between the model results and general statements for bromoform in the boundary layer and convective outflow. This is much the same issue raised by reviewer 2, and we have therefore reproduced some of the responses given to reviewer 2 below. In short, however, we create a new exposition of the results that shows that the ratios between the boundary layer mixing ratios and convective outflow mixing ratios are reasonable, and that the mixing ratios in the convective outflow and boundary layer compare well to those observed by the aircraft. We acknowledge that this was not very clear in the previous manuscript version.

Overall, we feel these criticisms are all symptoms of the same problem: we presented all of the available observations whether they had direct relevance to the convective cloud we that focused on or not. This had the consequence of making the paper very long, and it made a lot of our explanations overly intricate. In light of this we have removed many observations from the paper, and we have re-structured the paper to highlight how the observations support our conclusions. With regard to the key comparisons for CHBr_3 (we have made some of the comparisons clearer. We describe these changes in more details below:

- We cut down figure 13 (now figure 12) and highlighted the observations made in the convective cloud and those outside of it. Consequently, we were more easily able to highlight the agreement between the model and the observations in the outflow plume.
- We have removed all of the observations made during Flight A since this flight did not observe convective activity and the observations were made in the boundary layer in a region that was not dynamically connected to the region near Flight B.
- We have now structured the results and discussion according to the specific observations that we use to support different components of the case study, e.g., marine boundary layer observations, convective outflow observations, meteorological observations supporting general circulation, and meteorological observations relating to the studied convective cloud. This now makes the exposition of the results clearer.
- In particular, we highlight more clearly the marine boundary layer observations from the second local boat cruise and how they support our conclusions regarding the entrainment of air enriched with CHBr_3 . These observations are now discussed in a specific section and the data are added to Table 4. We explain how they support our conclusions regarding the entrainment of CHBr_3 enriched air into the convective system.

- All of the CHBr₃ observations aside from those made during Flight B are now shown only in either Table 4 or 5. The temporal variability of the other comparisons are not directly relevant to our conclusions.
- We have moved the discussion of the emissions to a new section, and we have reduced length of the explanation and have both simplified and clarified it.

Here is a reproduction of the new figure comparing CHBr₃ during Flight B:

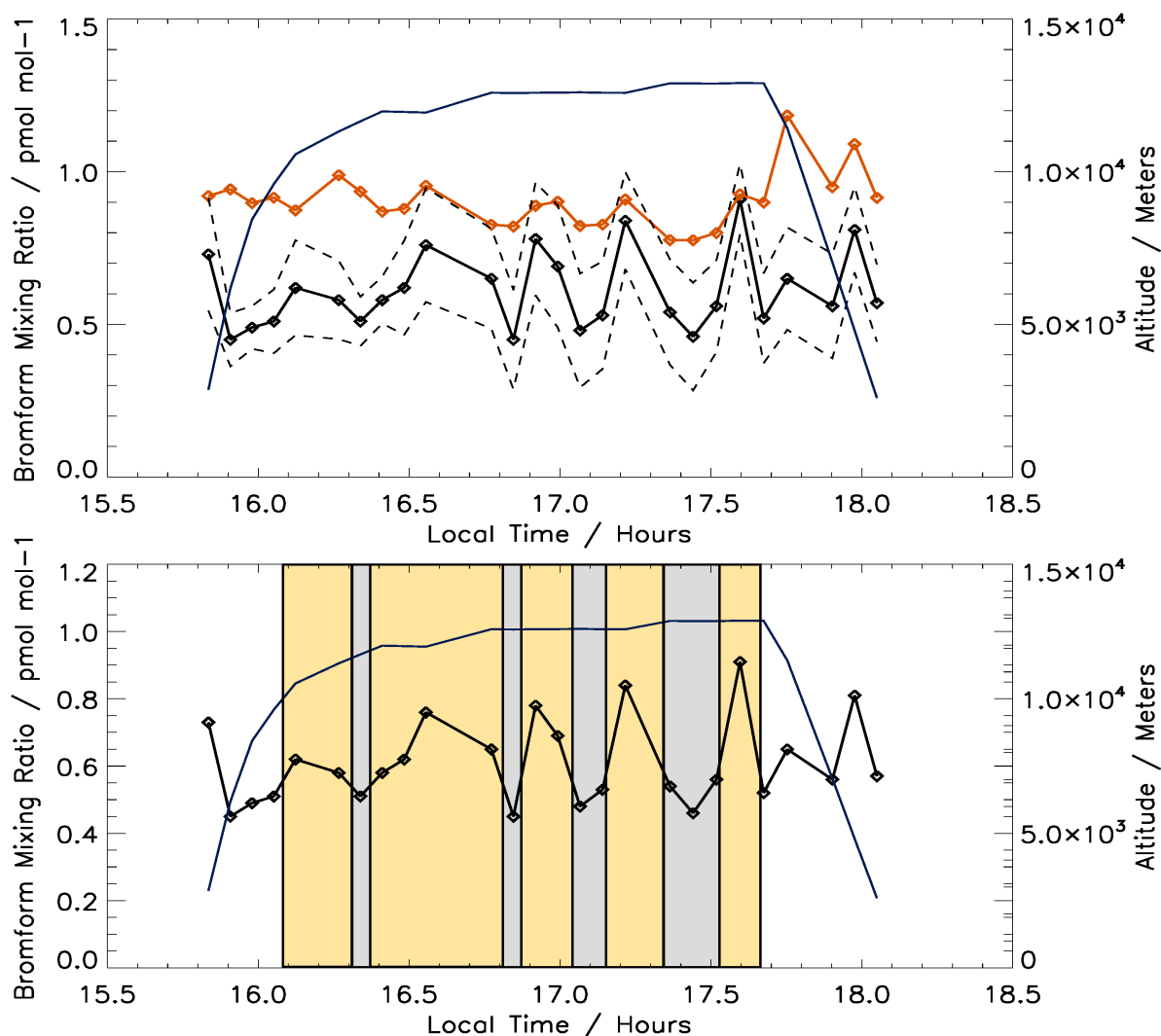


Figure 13. (a) This plot shows observed (black solid line) CHBr_3 and its accompanying 1σ error range (black dashed) in comparison with the simulated CHBr_3 (orange line) along the offset flight track. (b) The solid line shows the observed CHBr_3 variability both within the outflow plume (yellow shaded) and outside of it (grey shaded). The aircraft altitude is represented by the blue solid line in both figures. In addition to the precision of the observations, there is also an uncertainty due to the accuracy to which the standard is known that is equivalent to 4.5%.

We need to make several clarifications:

- The error bounds are equivalent to 1 sigma. The reviewer questioned this in their comments. We changed the paper to reflect this.
- This plot includes a more recent version of the GHOST-GCMS CHBr_3 data. We regret that have changed the data subsequent to submitting it to ACPD, but this was due to revised post-

processing of the GHOST-GCMS data. We are assured that this is the final version of the data. The new data has improved the model to observation comparison.

In addition to the uncertainties shown in the plot there is an additional uncertainty due to the accuracy to which the standard is known. This uncertainty would act to scale all of the values either up down in unison. We have made this clear in the plot legends and text.

1 ii The reviewer raised concerns that our attempted validation of the OH climatology in the model is probably not applicable to the OH within the convective column and outflow plume. They argue that the OH within these two portions of the convective cloud could be atypical compared to the climatology that we discuss. The Falcon did not carry an instrument capable of measuring OH, and, regardless, it would have been very dangerous to sample directly in the convective column. Given that we can't directly validate the OH within either the column or the outflow, how does this issue affect the interpretation of our results and our conclusions? The reviewer has highlighted the impact this uncertainty may have on the lifetime of CHBr₃. However, we do not think that this uncertainty will strongly affect the model's representation of its oxidation via OH within the column because the transport time from the boundary layer to the upper troposphere is fast (~ 1 hour) compared to the lifetime of CHBr₃ with respect to OH even under the extreme case suggested by the reviewer. Likewise, for our examination of the outflow plume approximately one hour downwind from the point of detrainment, the timescales are likely too short for errors in the representation of OH to be problematic. Of course, we think it is necessary to talk about these uncertainties, and we have therefore added the following text to the discussion of OH:

Due to the lack of OH observations onboard the Falcon we therefore perform a comparison between the model's simulated OH and the various reported climatologies and observations over this region as in Marécal et al. (2012). We compare the simulated temporally and spatially averaged vertical profiles to the available observations (Tan et al., 2001) and simulated and accepted climatologies (Spivakovsky et al., 2000). The model simulates very similar vertical profiles (not shown) and we therefore conclude that the quality of the OH simulation at the regional scale is sufficient. The vertical profiles are also similar to those reported in Marécal et al., (2012), which were themselves reasonable. This means that the background oxidation of CHBr₃ by OH is probably well represented in the model. One limitation of this attempted validation, however, is that it is probably not relevant when photochemical conditions differ strongly from the climatology. For example, within the convective column where photolysis rates are lower, and where certain OH source gases (e.g. H₂O₂) will be efficiently removed by washout. However, the uncertainties in the OH representation will only affect HO_x chemistry occurring on short timescales, and it will not affect the oxidation of CHBr₃ within the convective column or outflow to a significant degree because the transport times are sufficiently short, i.e., 1 to 2 hours, compared to the CHBr₃ lifetime, i.e., several days at the least.

1 iii Rainfall is too strong by a factor of five in the worst cases in the model compared to radar.

Due to an error on our part, we included the wrong figure of the simulated precipitation in the original manuscript that was from a different simulation. In fact, the simulated precipitation in the right figure shows far less discrepancy:

“During the most intense period of rainfall in the simulation, this discrepancy in intensity is by up to a factor of ~1.5 (65 mm hr⁻¹ compared to 45 mm hr⁻¹), but this only occurs within a limited time period during Flight B”.

In addition, we do acknowledge these issues in the conclusion and indicate how they might affect the results and conclusions. Indeed, we use as the basis to argue for more study of modeling of convection and rainfall in this region.

2. Reviewer 1 argues that the objective addressing CCM and CTM assumptions is discussed in only an incomplete and insufficient manner. We thank the reviewer for pointing out this problem because it is important for us to clearly address this objective in the paper. In responding to the criticisms from reviewer 2 about how simulations in the troposphere related to the stratosphere we dealt, in part, with this concern. We now clearly relate our tropospheric simulations to tropospheric processes that indirectly affect transport of Br_y to the stratosphere. We also explain in the response why it is not possible to make a more direct connection between our simulation results and the complete transport pathway to the stratosphere. We have reproduced some of the same arguments below as were given to reviewer 2.

We first need to start by explaining why we think the reviewer made this recommendation. We believe it is because we did not explain our real objectives clearly enough or place enough emphasis on addressing the assumptions describing tropospheric processes that are present within CTMs and CCMs. As far as any link to the stratosphere goes, we actually only wanted to be focused on tropospheric processes and then indirectly refer to estimates of the stratospheric Br_y where they control the vertical transport of bromine. We realize that this objective was inadequately addressed, and that we had placed too much emphasis on impacts on the stratosphere.

Given that the previous version of the manuscript lacks sufficient discussion of the implications for CTMs and CCMs we have modified it in the abstract, introduction, aims, discussion (in an entirely new section focused on these issues, 4.3), and conclusions to solve this problem. Note too that these improvements were also recommended by reviewer 2. This point relates directly to the main objectives of this work, so we have rewritten the objectives and include the new text below:

- **To understand the chemistry and transport of CHBr₃ and its PGs, and to estimate their chemical budget in the troposphere within an observed convective system.**
- **To discuss how these key processes relate to assumptions regarding the surface emissions, tropospheric chemistry, and transport of CHBr₃ and its PGs within existing CTMs and CCMs.**

Now, for clarity, we would also like to briefly address what this work did not seek to address and cannot. Due to the regional domain that we use, and the short timescale of the simulation (three days), we cannot simulate the transport and chemistry of VSLS and its PGs above the LZRH or make any estimates of strat-Br_yVSLS.

3. Reviewer 1 has indicated that we did not explain in enough detail how the reactions of Br + HCHO reactions and BrO+NO₂ could affect the subsequent vertical transport of Br_y. We agree with the reviewer that we could expand on this discussion and provide more detail. The reviewer also requested that we quantify the importance of these reactions.

We have added some further discussion on this and we make it clear how these reactions affect the resulting solubility of Br_y and its consequent washout. We have not, however, included any calculations of the fluxes due to these reactions because the fluxes were too spatially and temporally variant to be able to provide any sort of overview as to their contribution. A more dedicated study would be needed to test the effects of these reactions under more idealized conditions. As it is, the interpretation of these chemistry results is complicated by the intricacies of the chaotic low-level meteorology at the base of the convective system.

The reviewer has made a series of minor comments. We deal with these issues below:

- The reviewer recommended that we add a citation to Aschmann and Sinnhuber, 2013. We have done this.
- The reviewer has suggested that Section 2 is out of place and that it would be better placed at the beginning of section 4. We actually considered this possibility in the original manuscript. However, there are certain details that were necessary to be explained in section 2 prior to our description of the model setup. Without the details of the case study, it is not possible to adequately justify the specifics of the model setup. We therefore wish to keep section 2 in place.
- The reviewer asked to give some more description of the chemical scheme in the paper. We have now done this.
- P20622, L15: Add “,” after “its transport”. We modified the text accordingly.
- P20626, L12: Add “,” after “qualitatively well”. After the re-structuring of the paper, this sentence no longer exists in this form.
- P20626, L17: Change “time” to “temporal”. We modified the text as the reviewer recommended.
- The reviewer pointed out that section 4.1 was too long. We have reorganised the paper, and now the discussions of the meteorology are more streamlined and focused.
- The reviewer correctly pointed out that we made an error in the citation of Aschmann et al., 2011. We have corrected all instances of this error.
- The reviewer carefully noted that in fact Aschmann et al. set constant VSLs at the base of the tropopause rather than in the boundary layer. We have corrected this error too.