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Atmospheric Chemistry and Physics Discussions

Interactive comment on "BrO and Br_y profiles over the Western Pacific: Relevance of Inorganic Bromine Sources and a Br_y Minimum in the Aged Tropical Tropopause Layer" *by* Theodore K. Koenig et al.

Anonymous Referee #2

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In this manuscript, several airborne DOAS BrO profile measurements over the Western Pacific are reported which were taken during the CONTRAST field campaign. Using a box model constrained by the DOAS BrO data and additional measurements taken on board the aircraft, Bry profiles and bromine partitioning are derived. The Bry profiles are then discussed with respect to altitude and characteristics of the respective atmospheric layer, compared to 3d model output and to previous results.

The manuscript is overall well written although in some places hard to read. The topic of the study is of atmospheric relevance and fits well into the scope of ACP. The mea-

surements presented as well as the discussion and interpretation provided are interesting and thought provoking and I therefore recommend this manuscript for publication in ACP. I do however have some concerns and suggestions which the authors should address before the paper can be accepted for final publication.

General comments:

- An important limitation of the paper (which the authors acknowledge) is that out of the Bry budget, only BrO was measured. None of the other gas-phase bromine species were measured, nor any aerosol phase bromine. Aerosols are only constrained via aerosol surface area but the authors don't seem to trust these measurements. Washout is another unknown in this system. As a result, the inferred Bry profiles must have considerable uncertainty which the authors address by two sensitivity runs of their box model. However, in the discussion (and also title, abstract and summary), the Bry profile often is treated as if it had been measured. In my opinion, the authors should
  - consider changing the title into something like "BrO profiles over the Western Pacific: Implications for Bry and . . ."
  - make clear throughout the discussion that the Bry is inferred, not measured
  - add a discussion of the uncertainties in the Bry profiles (I'm not sure what the error bars in Fig. 2 represent but they seem rather small to me considering the uncertainty of the BrO measurement and allowing for some uncertainty in the partitioning)
  - explain how the confidence limits quoted in the manuscript were derived
- acknowledge that to some degree the comparison of the Bry from the 3d-models with the Bry profiles from their work is a model-model intercomparison, not a validation with measurements itemize

- One of the main results of the manuscript is detection of a BrO / Bry minimum in the "aged TTL". This is nicely visible in Fig. 7 and also Fig. 6 but not in the BrO profiles in Fig. 2, which give very little indication of a maximum between 12.5 and 13.5 km. I assume that this differences results from inclusion of RF15 and RF06. This raises the question of how representative the profiles in Fig. 6 or Fig. 2 are. This is of particular relevance considering that the CIMS BrO measurement do not appear to support the C-shape finding. Please comment.
- Another important finding is the relevance of a sea salt aerosol bromine source to reproduce the observed BrO profiles. While I agree with this conclusion for the lowest altitudes, I do not see "strong evidence" from the measurements or the comparison to the models for such a link in the upper FT. I therefore suggest to remove this statement or at least to make it less bold.
- More details need to be given on which measurements exactly were used to constrain the box model. In Table 1, several quantities listed are measured by multiple instruments. Is IO from the DOAS instrument really used to constrain the model, and if so, which values did you observe? Which measurements are included in the GV data called "water"?
- The authors define several abbreviations and use them extensively throughout the text which in places makes for difficult reading. I'd suggest using only standard abbreviations, in particular in the summary.
- I think that it would be helpful for the readers to have a brief outline of what is presented in the manuscript and why at the beginning of the manuscript, for example at the end of the introduction

Minor comments:

- P2,l2: profiles is found => profiles are found

- P2,l6: Cl not defined
- P2,l7: TTL not defined
- P3,l26: suggest to rephrase: "microwave radiometry has . . ."
- P4,l16: whcih => which
- P4, l23: "and is actively" – rephrase (grammar)
- P4,l25: "above geopotential horizontal" – is geopotential horizontal something different than just horizontal?
- P5, l1: "Low altitude . . ." duplication
- P5, l5: "in one of two ways optimal..:" Something missing here (colon?)
- P5,l24: What is the advantage of orthogonalising some of the cross-sections to the first coefficients of the polynomial?
- P5,l27: mention that non-linearity is from wavelength alignment only
- P8,l28: "An additional . . ." – what is the benefit of mentioning an additional sensitivity study without reporting any results?
- P9, l30: organic compound => organic compounds
- P10,l8: emissions six => emissions of six
- P11, l3: at last => at least
- P12, l4: with altitudes => with altitude
- P13, l15: "Relative to the aged TTL is characterized. . ." – something missing?
- P14, l25: "The impacts . . . is" => The impacts . . . are
- P14,l28 and l30: What do you mean by "convoluted"?
- P16, l5: ". . . is most sensitive in the . . ." – sensitive to what?
- P18,l28: ". . . ae more consistent" => is more consistent
- P19,l10: during same period => during the same period +enditemize

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