

## ***Interactive comment on “Modelling the Inorganic Bromine Partitioning in the Tropical Tropopause over the Pacific Ocean” by Maria A. Navarro et al.***

### **Anonymous Referee #2**

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Review of: Modelling the Inorganic Bromine Partitioning in the Tropical Tropopause over the Pacific Ocean by Navarro et al.

General Remarks: This paper uses global model stimulations to examine the inorganic bromine (Bry) budget of the TTL, building on the work of Navarro et al. (2015). In that work, the authors (a) presented measured (and modelled) vertical profiles of brominated very short-lived substances (VSLS), such as  $\text{CHBr}_3$  and  $\text{CH}_2\text{Br}_2$ , from recent NASA ATTREX flights, and (b) used a model that reproduces the observations well (CAM-Chem), to estimate the contribution of VSLS to Bry in the TTL (highlighting the significance of that contribution).

In the present work, the same approach is adopted as above, though the focus is more on understanding the modelled Bry speciation in the TTL, the Bry diurnal cycle, and

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differences between the West and East Pacific (where the ATTREX missions sampled). The model results from this work show that BrO and Br are the most abundant daytime species, while BrCl and BrNO<sub>3</sub> are more important at night. The authors also discuss differences in modelled Bry partitioning between the West and East Pacific, and briefly the sensitivity to heterogeneous processes on ice.

Overall, this paper is an interesting case study that provides an (incremental) advance on our understanding of Bry partitioning in the TTL over the Pacific. In the absence of new BrO measurement data being included in the manuscript, this advance is somewhat subtle when viewed alongside the modelling study of Fernandez et al. (2014, ACP) that also used CAM-Chem to look at TTL bromine partitioning, in some detail. I have outlined three major areas below that should be addressed before publication.

Major Comments: 1. The authors should ensure that the Introduction clearly sets out which of the broad model findings have come before, in order to help determine what the main motivation and purpose of this paper is. For example, the model results on zones where the Br/BrO ratio is  $>1$  in the UTLS are interesting, though have been discussed previously by Fernandez et al. (2014, ACP) and Saiz-Lopez and Fernandez (2016, GRL). The same can be said about the analysis of the Bry diurnal cycle and Bry speciation in the TTL, and their sensitivity to heterogeneous processes. Is the advance here that this is simply a CAM-Chem case study for the ATTREX campaign period? If so, that is fine, but the measurements of BrO (and NO<sub>2</sub>) from ATTREX would very much strengthen the paper and help corroborate the modelled fields. In the first paragraph of Results and Discussion, it is noted that “BrO and NO<sub>2</sub> measurements from the ATTREX mission were still under examination by the time of this analysis”. Is this still the case? It strikes me that it is quite odd that these data are not included here.

2. The most novel aspect of this work is the examination of differences between the W and E Pacific. The discussion of chlorine could be improved in this regard. If differences in Cly between the two regions can impact local Bry partitioning, some discussion on how well constrained the actual Cly simulation over the WP (average up to 84

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ppt Cly in daylight) and EP (up to 181 ppt Cly in daylight) is needed. At the very least some more details of the chlorine simulation could be given. More broadly, I would suggest that the title of the paper should reflect that the emphasis of the paper is on the differences between W and E Pacific.

3. The writing is quite awkward in many places and the paper would benefit from a very thorough check/read through. In addition, although the paper is compact, it would benefit from some sub-headings, particularly in the Results and Discussion section (e.g. Model-measurement O<sub>3</sub> comparison, Diurnal cycle in Bry partitioning, or something similar).

Other points:

\*Abstract\* Sometimes the “E” in Eastern (Pacific) and the “W” in Western (Pacific) are capitalized and sometimes they are not. Please be consistent throughout manuscript (including in figures and captions).

\*Introduction\* P1, L33: “Bry” is defined early on in the manuscript but “inorganic bromine” is used in numerous places after that. I suggest changing the latter to the former where appropriate throughout the manuscript.

P3, L2: Struck me that introducing the “proposed tropical ring of atomic bromine” here is odd. Could you not make mention of these papers earlier in the Introduction?

P3, L3: “section 3” — “Section 3”

\*Methods\* I would separate out Methods into 2.1 Observations and 2.2 Modelling. This section seems quite unstructured in its present form. The Modelling section needs more details as to the ozone precursor emissions that were used in CAM-Chem + some brief information of the chlorine simulation.

P3, L12: Has “very short-lived organic substances” not already been defined as VS-Lorg? Also, as the focus of this work is on VSLS, it would be better if some of the gases listed (e.g. CHBr<sub>3</sub> etc.) are actually referred to earlier in the Introduction (maybe

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around line 31).

P3, L17: The two sentences beginning “At the tropopause level” seems out of place. Is this not motivation/background for the present study and should it not appear in the Introduction?

P4, L4: CAM-Chem has already been defined.

P4, L21: “for both, the WP and EP respectively” — “for both the WP and EP”.

P5, L7, Sentence beginning “During ATTREX” to the end of the paragraph. This text describes the different sampling times/paths of the observations/flights and would be better placed in Section 2. In the current location it disrupts the flow of results. Similarly, consider moving Figure 3 to the Measurements section.

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