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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "A Tropical West Pacific OH minimum and implications for stratospheric composition" *by* M. Rex et al.

M. Rex et al.

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Dear reviewer,
thank you for reviewing our paper and your helpful comments.

General changes

- We improved the structure and readability of the paper. We restructured the paper by introducing a new section structure, divided into "Measurements", "Modeling" and "Effect on chemical species". The introduction has been expanded considerably by moving paragraphs from later sections at the beginning of the paper. The appendixes have been integrated into the main text, with the ex-

ception of Appendix A, which is quite long and contains details not of interest to everyone. Several paragraphs have been moved to positions where they fit better into the context. A "Conclusions" section has been added. A description of the sections has been added to the introduction. The abstract has been extended. Some more specific changes can be found at the end of this reply under "Additional changes".

Your comments

- **"...broader introduction to the basic climatology and history of the study region..."** and **"...extensive discussion of results in light of previous similar investigations ..."**

  We have considerably extended the discussion of many aspects (e.g. more measurements are discussed or interannual variability is discussed, see also specific answers to other points and to the other reviewers) and added a considerable number of new references. Please understand that we do not want to write a review paper here or to repeat information in detail that has already been published elsewhere. We would like to keep our study focussed.

- **"...broader introduction to the relevant chemistry of atmospheric halogens considered to be important..."**

  We have expanded the introduction to the CH2Br2 calculations considerably (now in Section 5.1) and added several new references. The chemistry which is relevant here is quite simple: The most important species by far are CHBr3 and CH2Br2 (short-lived chlorine species are only a minor contributor to ozone loss, since the abundance of stratospheric chlorine is much larger than that of stratospheric bromine). CHBr3 is mainly destroyed by photolysis, while CH2Br2 is mainly destroyed by reaction with OH, which is the reason why it is treated here. The chemistry of the product gases is complicated and still not well understood, and involves washout and heterogenous reactions. But the amount of product gases can be seen as an upper limit for the amount of bromine that does not reach the stratosphere, since only soluble species that are washed out do not reach the stratosphere in the end.

- **"mere speculation (as, for example, the brief hint at the modification of the stratospheric sulfate layer being sustained by moderate volcanic activity and increasing SO2 emissions in the region and its relation to global climate, i.e., the Solomon et al. paper and WMO study)"**

  We have considerably toned down our introductory remarks on these more speculative potential implications of our main findings and have focussed the paper on the main aspects.

- **"It lacks in comprehensiveness and fails to set the observations in a proper modelling context in comparison with previous field and model studies."**

  We have incorporated a considerably broader discussion on several topics into the paper (see above). The incorporation of the appendices into the main text should also help.

- **"There is no real Conclusions section"**

  We have added a section "Conclusions". Much of the material in the previous "Discussion and Conclusions" section was incorporated in the main body of the paper.

- **"And why are there five appendices? Are these considered to be less important, although they include four critical figures and key details on data sources, measurements, and modelling?"**

  We have reduced the number of appendixes to one and have integrated the other appendixes and figures into the main text.

- **"The abstract mentions halogen emissions from kelp and seaweed farming as potentially important sources for reactive halogens in the stratosphere. Nothing about this statement is further substantiated in the text, not even maps of primary production or chl-a are included."**

  We have removed this statement from the abstract. This is only a minor side aspect of the paper and should not be mentioned in the abstract. Also, we have now added some more information in the introduction to the new section 5.1. We cited two references in the old manuscript which give more details (WMO, 2011 and Martinez-Aviles) and have added several other references that discuss the topic of halogen emission sources in detail (e.g. Liang et al., Warwick et al., Hossaini et al.). We have replaced the two references Quack et al. and Pyle et al. by more appropriate ones. We don't think it makes sense to show maps of primary production or chlorophyll here. We think it is clearly outside the scope of this study to add a detailed study on emissions of halogenated species here. This topic has been covered elsewhere in great detail (see our new references), there are still large uncertainties and it would be a completely new and largely unrelated topic compared to our main topic of OH abundances. We would like to keep the study focussed.

- **"And what about seaweed farming? Any data from the study area or a survey of worldwide seaweed farming growth and related halogen emission estimates?"**

  We removed any mention of this from the abstract. It was unfortunate that we had mentioned this in our original abstract, which led to some misunderstanding. This is really only a side remark in the paper. We think this issue is clearly outside the scope of the paper and would like to refer to the relevant citations here, see above.

- **"Likewise, no tabulated data on moderate volcanic activities and anthro-**

pogenic SO2 emission trends in the East Asia/West Pacific region are provided."

This would be beyond the scope of this study. The new abstract should make it much clearer that this study is about the ozone and OH minima in the tropical West Pacific. Some discussion of the potential implications for halogenated species or sulfur is included, but these are not central for the paper.

- **"I am also missing a thorough analysis in relation to the warm pool climatology of the region, and in particular its potential relation to ENSO (2009 was an El Nino year, although anomalous SST were not reaching the study area (see, e.g., Kim et al.). The related large-scale atmospheric circulation patterns developing in ENSO causing advection of low O3, low OH air masses to the western Pacific mentioned in the paper should be thoroughly investigated! Is this a persistent feature throughout each year, or occurring seasonally, or just in relation with ENSO events, etc.?"**

  Figure A4 (now Figure 5) shows that the position of the minimum of the ozone column follows the eastward shift of the warm pool during El Nino conditions to some extent, but that the minimum is very stable. The stability was also shortly discussed in the manuscript on page 28873, lines 9–11. We have now added some more discussion in the text in Section 2.3 (new manuscript).

  It was said in the old manuscript that interannual variability for the density distributions is limited (page 28880, line 2). The corresponding part has now been moved into the main text. We have added figures that show the interannual variability of the density distributions of Figs. 4d and e (Figs. 7 and 8) and have added a discussion in the text. The main features, as a clear maximum between 10 S–10 N and 120 and 180 W are very robust.

- **"One previous model study comprising a large data set from multiple NASA field experiments over the Pacific, specifically PEM-West and PEM-Tropics,**

also including direct airborne measurements of OH in this region, should certainly be referenced and discussed for comparison of model results: Liang et al."

  We have cited and discussed the Liang study now in the introduction to the new section 5.1. A direct comparison of our more conceptual model studies with the results of Liang et al. is not possible without access to the Liang model data. E.g. we use values relative to the boundary mixing ratios here, and Liang uses absolute values.

  A discussion of the OH measurements carried out during the PEM-Tropics B campaign has been added. We have cited Tan et al. (2001) as a reference. The reference Liang et al. does neither show nor discuss measurements of OH.

- **Figure 4**: The figure is not reproduced in the correct size in the "printer-friendly" version of the manuscript. In our original manuscript, this was a big panel covering a complete page. This will be corrected in the final version.

- **Figure A2**: We have removed the Figure and the corresponding paragraph. A very similar Figure is shown in Ridder et al. (2012, Fig. 5, see citation in the paper). Instead, we added discussion on the FTIR measurements to the new section 2.1 and added discussion on the good agreement between model, FTIR and ozone sondes visible in Fig. 5 of Ridder et al. to the new Section 3.

Additional changes

- Changed "Much of our understanding of transport of short-lived species into the stratosphere is based on studies that assume fixed uniform lifetimes" to "Some important studies were based on fixed uniform lifetimes of OH in the past".

- We rephrased section 2.1 to discuss more ozone measurements and to discuss the CEPEX measurements in more detail. In particular, we added a reference to the Appendix, where we propose that there is a low bias in the CEPEX measurements compared to our measurements. We added discussion on additional ozone sonde measurements (Fujiwara et al., Takashima et al.).

- We have split Figure 4 (old manuscript) into two Figures. These are the Figures 9 and 10 in the new manuscript.
- Figure A4 (now Figure 5) was blurry. A new version is included in the new manuscript.
- A new Figure 6 showing OH profiles from the model run and discussion in the text comparing these profiles to the PEM-Tropics B measurements has been added.
- In the description of the back trajectories, the information that the trajectories were started in January was missing and has been added.
- Added discussion of OH modeling uncertainties.

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 28869, 2013.