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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.

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Dear Martin Manning,
thank you for reviewing our paper and your very 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”.

Specific comments

- Page 28870, line 25: Thanks for the additional references. We have added both references to the text.
- Page 28871, line 9: We have followed all your recommendations here: The reference to Naik et al. has been added. A sentence referring to the systematic differences has been added. In addition we have considerably extended the introduction by moving several paragraphs from later sections into the introduction. That includes the paragraph about the West Pacific as entry point for stratospheric air and the reference to Fueglistaler et al.
- Page 28871, line 10: We cleaned up the structure of the paper to achieve a clearer separation between the different topics. There are now several new sections and subsections for measurements, modeling and chemistry. We have included most of the appendixes in the main text now.
- Page 28871, line 20: We moved discussion about the entry region into the stratosphere to the introduction.
- Page 28872, line 1: Only one appendix is left in the new version, which avoids the ambiguity.

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- Page 28872, line 9–10: The Appendix is Appendix E. The Appendix has been integrated into the main text now.
- Page 28872, line 11–15: Unfortunately it is not easily possible to be more quantitative here or to make a quantitative assessment about the relative roles of loss in the marine boundary layer versus loss in the free troposphere. This would require a cloud resolving modelling study with interactive tropospheric chemistry, which is far beyond the scope of the current paper. We use the words "most likely explanation" to make clear that this statement is not based on such a rigorous model study.
- Page 28872, line 16–28: The Appendix has been integrated into the main text just below this paragraph. We hope that resolves the issue.
- Page 28872, line 28: Actually the key point we want to make here is that the low OH was actually seen in a measurement at upper tropospheric levels. Otherwise the discussion of OH in our paper is purely based on model results and we find it important to support that by this observation. We do only have the data that is shown in the Figure – this is the only occasion where the ER-2 has probed a patch of air that originated from the "OH minimum" area. Potential mixing of this patch of air with air from higher latitudes could only increase OH. Therefore our purely qualitative point about very low OH in this air mass is robust.
- Page 28873, line 1–11: We have reorganized the whole paper. This is hopefully much clearer now.
- Page 28873, line 12–13: Added this information.
- Page 28874, line 6–17: There is a misunderstanding here: The ATLAS run used here includes no chemistry, although ATLAS has a (stratospheric) chemistry module. The results of the chemistry run from GEOS-Chem are interpolated on the

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ATLAS trajectories here. This important information was missing in Appendix D and has now been added. There are two reasons for this approach: 1) A Lagrangian model like ATLAS is better suited to calculations of air mass origin. This is easily done with back trajectories, but difficult in an Eulerian model like GEOS-Chem. 2) ATLAS contains no detailed tropospheric chemistry scheme, so we have to use GEOS-Chem here.

We have moved the text of Appendix D, which clarifies the approach, into this section. Additionally, we have added some more information on how the 57% value was calculated.

- Page 28874, line 12–17: This was not intended to imply that advection of OH is a significant factor. We hope that the changes described in the last comment clarify what we have done. Nevertheless, the statement is correct as written here: It is not said that OH is advected, but that other species which ascend into the stratosphere, like CH2Br2 or SO2, will encounter these OH values (no matter how these OH values are maintained).

- Page 28874, line 13: Changed.

- Page 28874, line 21: The model was explained in Appendix E, but unfortunately the reference to the appendix was missing here. We have now restructured the text and introduced a new section which aggregates all text dealing with the CH2Br2 and SO2 results. The text of Appendix E has also been moved into this section, so that the model is explained close to this paragraph. We hope that clarifies what we have done. The simple box model was specifically written to model CH2Br2 and only contains two reactions.

- Page 28875, line 8–12: We have restructured the text and there is now a separate subsection for the SO2 results. We have changed “our findings” to “model calculations” to avoid misunderstandings. These changes hopefully clarify things.

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- Page 28875, line 11–12: Changed as suggested (also in the abstract, which includes the same sentence).
- Page 28876, line 1–8: This paragraph deals with the conditions after volcanic eruptions, while the paragraph before deals with the background conditions without volcanic eruptions. We do not think that we are repeating points here, the subjects of the paragraphs seem sufficiently different to us.
- Page 28876, line 9–14: Thank you for this suggestion. We have added a final statement along this line.
- Appendixes: We have moved the text of the appendixes into the main text (except for Appendix A).
- Appendix A: –
- Appendix B: Added a remark that there is some seasonal variation in OH abundances. The measurements from Wennberg et al. are from the same campaign, but the paper show only data below 50 degree solar zenith angle, which excludes the measurements that show very low OH.
- Appendix C: We have added a reference to the Data User's Guide of TES. The TES data of Fig. A4 are mentioned in the main text (Page 28873, lines 9–11), but the only given reference there is the Appendix. We have now moved the Appendix into the main text and replaced the reference to the Appendix by a reference to the Figure. That should make it much clearer.
- Page 28879, line 21: We have changed that to "vertical motion". The vertical coordinate is pressure at the surface and potential temperature above the tropopause and slowly transforms between these coordinates inbetween. Likewise, the "wind" matching these coordinates transforms from pure vertical winds at the surface to pure heating rates at the tropopause.

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- Appendix E: –

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.
- We have removed Figure A2 and the corresponding paragraph and moved the discussion into the main text.

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- Added discussion on interannual variability and relation to ENSO, including two figures.

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