

Review for "The MIPAS HOCl climatology" by T. von Clarmann et al.

Anonymous Reviewer

1 General Comments

The authors present a valuable data set derived from MIPAS-ENVISAT HOCl limb measurements in the mid-infrared spectral region covering the time domain from June 2002 to March 2004. Results from an up-to-date chemical climate model are also presented. The paper is based on earlier HOCl papers by the same group focussing on global distributions of HOCl for short temporal episodes (von Clarmann, 2006) and on the the Antarctic winter vortex HOCl chemistry (von Clarmann et al., 2009a).

While the data set well deserves publication the present state of the paper is not suitable for publication before major revisions have been made.

The first and main criticism is a missing or much to short discussion of the model-measurement intercomparison that shows several interesting features. Here the authors do not even attempt to give possible reasons for the major model overestimation of HOCl in the 30-40 km altitude regime for polar night conditions. This is a very interesting feature that has not been explained and is just mentioned in a few sentences. A total of eight figures showing measured and modelled HOCl distributions and evolutions is dealt with in a short paragraph of intercomparison which for my taste is not at all sufficient for publication in a peer-reviewed journal.

Secondly, the data and model features are not really presented in an effective manner so the reader can easily grasp the details. Several figures do not focus on the features they are supposed to present but standard global plots are used instead. In the introduction the JPL recommendation of 2006 is given as the current recommendation although the reaction in question has been updated in the 2009 issue. Several other points are raised in the detailed comments below.

In general the data and model results do have the potential for a solid paper but substantial effort is needed to discuss the features in an adequate way and to present the argumentation and results to the reader.

At some places the paper suffers from Germanisms.

2 Major Detailed Comments

p.20794, l.6: The abstract should be more quantitative in places: Formulations such as "at lower altitudes" and "in the lower stratosphere" are both used leaving the reader unsure about the altitude regime.

p.20795, l.12: Sander 2006 is NOT the current recommendation. For ClO+HO₂ it has been superseded by the 2009 update Sander et al. 2009 (JPL Publication 09-31).

p.20796, l.13: For the intercomparison of absolute HOCl mixing ratios with model results precision is not really the relevant error estimate. Accuracy of the measurements must be stated for this and used throughout the intercomparison process.

p.20797, l.6: The quality of agreement must be quantified in terms of the measurement error and it should be stated which trends have been used in the intercomparison. An intercomparison with the earlier publication from the same group (v.Clarmann et al. 2006) based on an earlier data version should be presented or it should be mentioned that the data compare very well.

p.20797, l.:8. The data set should be made publicly available. A download location should be given.

p.20797, l.:8. A whole bunch of improvements is listed here but none is explained in detail. This should be done or a proper reference must be given.

p.20799, l.:16. Since the altitude of the peak mixing ratios is lower during daytime this should be a downward shift (NOT upward) during nighttime, right?

p.20800, l.13: Which ensemble of data in terms of space and time does this standard deviation refer to? This must be clearly stated.

p.20800, l.15: The statement concerning high standard deviations in the polar night regime must be weakened since other potential causes have not been ruled out.

p.20801, l.5: While the model data are prominently presented along with the MIPAS HOCl data the paragraph does only present an extremely short discussion on the model-measurement comparison. The improvement of the modeled peak HOCl mixing ratios by using the Stimpfle (1979) rate constant is just mentioned but not shown in any way. A figure showing the effect on the global distribution would have been very worthwhile. Else just a few facts are mentioned and no single attempt is made to explain the strong model overestimation of HOCl for polar night conditions, which is the most obvious feature in several of the plots presented.

p.20801, l.22: The behaviour discussed in the paragraph is not in any way obvious from Fig.8 that gives zonal mean HOCl over all latitudes (see comments below concerning the figure). In the way presented the arguments of the authors (although sensible) are not comprehensible to the reader.

Figs.2,3,5,6: The plots are much too small in the size given in the discussion paper. They should be enlarged and reorganized in a 2x2 scheme which provides better intercomparison between measurement and model. Also, since the color bars are identical one per figure will be sufficient. A geometric altitude axis to the right might also help some readers.

Fig.2: The vast overestimation of HOCl inside the polar vortices by the model is obvious but not discussed in the text at all.

Fig.5: The major part of the figure does not add any significant new information as compared to Fig.2. The plot should be reduced to the regimes that are discussed in the text and enlarged.

Figs.5,6: What is the reason for the unnaturally steep vortex boundary gradients in the winter antarctic model data? These look like artifacts?

Fig.6: Regarding that Fig.7 shows the features discussed in the text much better Fig. 6 could be skipped completely.

Fig.8: The figure should be changed in a way to focus on the antarctic region discussed in the appropriate paragraph in the text. At the first glance the figure seems incompatible with Fig.7 which as well shows the Sept. 2002 and 2003 averages over the antarctic region (although different color scales are used). This must be checked.

Appendix A: Not reviewed, due to other obligations.

3 Minor Comments

p.20794, l.2: Use "period" instead of "episode"

p.20794, l.22: Is Solomon et al. 1986 really a good reference for mid-lat. O₃ loss? Why not use a WMO-report?

p.20794, l.25: Occasional measurements exist ...

p.20795, l.5 and in general: Be more specific: ... MIPAS onboard ENVISAT ... (MIPAS-E was used in earlier papers)

p.20796, l.9: ... differences constraint

p.20796, l.18: The Figure deserves better attention than being introduced in parentheses at the end of the sentence.

p.20799, l.3: It should be checked whether there is no better reference than a Ph.D. thesis of obviously German language which is of limited use for the community.

p.20799, l.11: ... maximum ...

p.20799, l.12: (appr. 35-43 km), respectively.

p.20799, l.14 and following: Figures should not be introduced in parentheses.

p.20799, l.25: Delete one "of"

p.20800, l.8: ... to warm ...

p.20801, l.5: ...maximum ...

p.20802, l.17: ...confirmed...

Fig.1: The title on top of the figure is superfluous.