Atmos. Chem. Phys. Discuss., 12, C12647–C12649, 2013
www.atmos-chem-phys-discuss.net/12/C12647/2013/

Atmospheric Chemistry and Physics Discussions

Interactive comment on "Diurnal variation of stratospheric HOCl, ClO and HO_2 at the equator: comparison of 1-D model calculations with measurements of satellite instruments" *by* M. Khosravi et al.

M. Khosravi et al.

maryam.khosravi@chalmers.se

Received and published: 13 February 2013

The reply letter to reviewers are attached as a pdf file.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 21065, 2012.

Manuscript prepared for Atmos. Chem. Phys. Discuss.
with version 3.5 of the LaTeX class copernicus_discussions.cls.
Date: 13 February 2013

Diurnal variation of stratospheric and mesospheric HOCl, ClO and HO_2 at the equator: comparison of 1-D model calculations with measurements of satellite instruments

M. Khosravi[1], P. Baron[2], J. Urban[1], L. Froidevaux[3], A. I. Jonsson[4], Y. Kasai[2,5], K. Kuribayashi[2,5], C. Mitsuda[6], D. P. Murtagh[1], H. Sagawa[2], M. L. Santee[3], T. O. Sato[2,5], M. Shiotani[7], M. Suzuki[8], T. von Clarmann[9], K. A. Walker[4], and S. Wang[3]

[1]Department of Earth and Space Sciences, Chalmers University of Technology
[2]National Institute of Information and Communications Technology, Tokyo, Japan

Fig. 1.

Reply letter

General remarks / reviewer 1:

The paper is very well written (English and structure), some of the Figures are of good quality (e.g. Fig. 7), the references present a wide spectrum of analyses related to the diurnal variation of stratospheric constituents. It is obvious that the authors have used a tremendous amount of data from different origins, different wavelengths, different vertical resolutions, different time frames, and have averaged and binned them in a correct way, made a sensitivity study on the different values of the rate coefficient kl ($ClO+HO_2$->$HOCl+O_2$) through a 1-D model to assess that the optimum value was the one from Nickolaisen et al. (2000). I can acknowledge, as it is state in the abstract, that all the data sets considered in the study "generally agree" and that the "gas phase chemistry implying the above mentioned species is well understood based on latest recommendations of reaction rate constants". But it is not clear to me whether this paper can be published in a journal like ACP since the amount of scientific new results is very weak. More than half of the manuscript presents the satellite data base and shows the comparisons within the sensors, lots of them were already published before (e.g. MIPAS), but others are presented as the first validation of HO_2 measurements from ODIN. A journal like AMT would better fit this part. The model results are very interesting regarding the value of k1 (Fig. 7) but the conclusions again were already published elsewhere. Consequently I cannot propose the manuscript to go a step further in the ACP journal but recommend some issues listed below to be carefully treated before sending it to another journal.

General remarks / reviewer 2:

The paper is clearly structured and overall well written. It presents a large data set and a detailed comparison between different observations and model results. The SMILES diurnal variations are used as a transfer standard for comparisons between instruments with different observation times and for offset correction which is a sound approach. To my knowledge, there has never before been such comprehensive comparison. In addition, the kinetic study also gives clear indication for preferences on which value to use for the reaction rate coefficients of HOCl formation. However, the problem with this paper is, that there is only very little new which the reader can learn from it:

• Most of the satellite data used have already been presented before

• The model used is pretty standard

• The main part of the paper consists of a lengthy description of the similarities and differences between individual results which a reader could also deduce just from looking at the figures

• The comparison between model and measurements is again very descriptive and does not provide any new insights on atmospheric processes or their description

• The kinetic study is nice but only confirms a similar result from an earlier study for another altitude

In summary, in my opinion the main value of the paper lies in the compilation of the large number of observations and their thorough processing and comparison. I think that a good job was done on this aspect of the study and I'm sure that the figures presented will be of interest for people working on stratospheric chlorine and hydrogen chemistry. However, due to the descriptive nature of the paper and the lack of really new results, I'm reluctant to recommend it for publication in ACP and would rather suggest to re-submit to another journal which is more oriented towards presentation of data. Should the authors decide to submit a revised version of the manuscript, they will have to remove much of the text just describing what is in the plots. Instead, they will have to make a convincing point of what one can learn from the data and the comparisons performed in this study.

Fig. 2.