

Interactive comment on “Diurnal variation of stratospheric HOCl, ClO and HO₂ at the equator: comparison of 1-D model calculations with measurements of satellite instruments” by M. Khosravi et al.

Anonymous Referee #1

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Review of the paper entitled " Diurnal variation of stratospheric... " by Khosravi et al.

The paper entitled " Diurnal variation of stratospheric... " by Khosravi and colleagues shows a series of comparisons of space-borne measurements and results from a one-dimensional model related to the diurnal variations of several stratospheric constituents in the equatorial band (20S-20N): HOCl, HCl, ClO and HO₂. Measurements from MIPAS, MLS, ODIN, ACE and SMILES are shown for the space-borne sensors whilst a 1D model is used for comparison in conditions and locations of the observations. Results are presented at three different altitudes: 35, 45 and 55 km. Periods under consider-

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ation cover several years but do not necessarily overlap regarding the measured data sets. Observations and model outputs tend to agree when considering both the diurnal cycle amplitude and the absolute mixing ratio, confirming that the gas phase chemistry implying the above mentioned species is well understood.

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 k₁ (ClO+HO₂->HOCl+O₂) through a 1D model to assess that the optimum value was the one from Nicklaisen 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 HO₂ 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.

1. Major points

a. Too vague. The comparison exercise is in my opinion too vague, whilst the presentation of the data is too lengthy. The comparison exercise needs much more quantification, giving more insights in absolute and relative values. This means reducing/avoiding

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the too numerous occurrences of “generally agree well”, “agree reasonably well”, “quite well” in the core of the text, in the abstract and in the conclusion.

b. Figures. The Figures 3-6 are the corner stones of the study and would require enlarging the y-axis on each individual plot in order to actually highlight the diurnal cycle of the constituents as measured/modelled by different sensors/model. One of the caveats of using so many data is that it is almost impossible to detect for instance the model curve on these Figures since it is hidden by the noisier satellite curves. Why not only showing the debiased diurnal cycles (Figs. 5-6) and adding a Table listing the biases between all the data sets? In general, showing offsets/biases will give more insights in the presented analysis (see e.g. section 4.1).

c. Vertical Resolution. It is mentioned that “the model results have been smoothed using a 5-km moving average for the 35 km (...).” This is difficult to understand since a rigorous comparison can be performed by using the averaging kernels of the different sensors to be applied to the model profiles. Furthermore, a moving average will tend to smear out the measurement sensitivity at a considered altitude although the actual averaging kernels in a limb-viewing geometry are well peaked at the tangent altitude. This may considerably affect some of the diurnal variation cycles, e.g. ClO.

d. Cly trends. Another critical problem of the study that considers chlorine compounds is that the time evolution of Cly, as it is stated in the text, is decreasing since 2000. So, the comparisons of ClO, HCl and HOCl, from different sensors averaged over different periods not necessarily overlapping produce a natural bias, independent of the instrumental bias. I have not clearly understood whether the model runs were performed over all the periods under investigation or only over one single period. To me, the model run should be performed over the whole period so that comparisons between model and sensors are not affected by this trend issue.

2. Minor points.

Title. “HCl” does not appear in the title. Why?

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Stratosphere. Note that the layer at 55 km is in the mesosphere. So the title (and the content) of the manuscript will need to be modified.

21070/2: “have” instead of “has”

21072/8: “altitude grid” no “s”

21073/15: not sure “LT” was defined before and add “10:00 LT”

21076/10: what is a “standard error”? You mean a “standard-deviation error”?

21076/24: “between” is missing after “offset”

21079/26: “well” is missing after “reasonably”

21080/6: “amplitude”, “u” is missing

21080/20: “The ACE-FTS (...).” What is the actual number of measurements?

21080/21: The sentence “This could cause” is rather difficult to understand. This needs clarifications.

21080/ What are the conclusions of the section 4.1?

21085/ What are the conclusions of the section 4.2?

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