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.

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

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. 3), the reference list is a wide spectrum of authors related to the shared variation of atmospheric constituents. It is advisable that the authors here need a transmission amount of data from the references, which will provide some help for the results of their study. The authors are correct in their assumptions and board with a correct way, made a sensitivity study on the different values of the rate coefficient (ClO+HO2/CH3-CHO) through a 1D model to assess that the optimum index rate the one from Volchikov et al. (2009). I just acknowledge, in it is true to the赫尔曼, 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 when the comparison within the context, lots of them were already published. I believe, this study does not contribute as much as possible, and at the end, it could help fit the results. The model results are very interesting regarding the value of k2 (Fig. 3) but the conclusions 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 JARWEX/AMT variations are used as a basis for the model results, which is somewhat surprising. The model was not to offer correction which is a sound approach. To my knowledge, there are never before from such comprehensive comparison. In addition, the kinetic study also gives some indication for preference on which value to use for the reaction rate coefficients of HO2 formation. However, the problem with the 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 partly standard.
- The main part of the paper consists of a lengthy description of the simulations and differences drawn from individual results, which reader could also deduce a lot from looking at the figures.
- The paper presents a large data set, but it does not provide any new insights in atmospheric processes or their description.
- The kinetic study is nice but only confirm a similar result from an earlier study for another altitude.

In summary, in my opinion the main value of the paper lies in the comparison of the large number of observations and their through processing and comparison. I think that a good job was done on the aspect of the study, and I am sure that the figures presented will be of interest for people working on atmospheric chlorite and hydrogen chemistry. However, due to the descriptive nature of the paper and the comparison of the results, it is not clear that the paper contributes enough to the field. Nevertheless, I suggest to re-submit this manuscript, which is more oriented towards presentation of data. Should the authors decide to submit a second version of the manuscript, they will have to improve much of the text; for describing what it 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.
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