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

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


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


$^1$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. 2), the references present a wide spectrum of analyses related to the shared variation of atmospheric constituents. I believe that the authors have used a tremendous amount of data from international sources and that the paper presents a model that is compared against observations and also against a model. It is clear and consistent, and the methodology is well communicated. However, it would have been helpful to have a more detailed discussion of the model parameters used, especially those related to the stratospheric chlorine reservoir. The model is explained and described and it would be easier to assess the model's performance if more information about the model's assumptions and limitations were provided. I am not sure if the authors have considered the possibility of using a different chlorine reservoir, which is a valid approach. To my knowledge, there is no single approach that can model the stratospheric chlorine reservoir. In addition, the kinetic model does not give a clear indication of its performance on what I refer to as the reaction rate coefficients of HOCl formation. However, the problem with this paper is that there is only very little information about the data used for its derivation. I encourage the authors to report more on this topic to provide a more complete picture of the atmosphere's processes and their description.

The kinetic study is nice but only confirms a similar result from an earlier study of another altitude. In summary, in my opinion the main value of the paper lies in the description of the large number of observations and their thorough processing and comparison. I think that a good job was done on the analysis of the data and I am sure that the figures presented will be of interest for people working on atmospheric chlorine and hydrogen chemistry. However, due to the descriptive nature of the paper and the limited number of data points, the model results are not as strong as they could be. I suggest that the authors consider submitting a revised version of the manuscript that will have to reanalyze much of the data and present a clearer picture of what one can learn from the data and the comparisons performed in this study.

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 Table 2 detailed variations are useful for understanding the results. However, it is not clear how the authors derived the offset correction which is a valid approach. To my knowledge, there is a single approach that can model the stratospheric chlorine reservoir. In addition, the kinetic model does not give a clear indication of its performance on what I refer to as the reaction rate coefficients of HOCl formation. However, the problem with this paper is that there is only very little information about the data used for its derivation. I encourage the authors to report more on this topic to provide a more complete picture of the atmosphere's processes and their description.

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Fig. 2.