The content of the manuscript is to develop a new approach to calculate rate constants of specific chemical reactions from satellite observations. In this case Kuribayashi et al. calculate a new rate constant for the reaction ClO+HO2 -> HOCl+O2 (R1) using the satellite observations from SMILES. The authors assume that purity regions exist in the atmosphere in which for ClO and HOCl only reaction R1 take place, i.e. no other reactions with contribution of ClO or HOCl are present. In this case it is possible to calculate a new rate constant for reaction R1 with a method described in this manuscript.

General Comments

I find the possibility to observe ClO, HO_2 and HOCl simultaneously is very impressing and admire the idea to calculate a new rate constant for reaction R1. Maybe I am wrong but I am very skeptical if the assumed pure conditions in the mentioned region of 0.28 hPa and 20-40°S during night exist at any time. I think that HOCl is only involved in Reaction 1, but for ClO this is probably not the case. At least in our chemistry climate model EMAC (Joeckel et al., 2006) we have for the mentioned altitude and time interval besides the reaction R1 for ClO loss, also the reactions $ClO+NO_2$ -> $ClNO_3$; ClO+BrO -> Br+OClO (or -> BrCl or -> Br + Cl) and $ClO+CH_3O_2$ -> HO_2 + Cl + HCHO. Also the production of ClO through the reaction Cl + O_3 -> ClO + O_2 takes place during the night. Maybe this is wrong in our model but I am very skeptical. Are there any observations of NO_2, $ClNO_3$, BrO, BrCl, OClO, CH_3O_2 or HCHO in this region? Such observations could maybe support (or not) the assumption of pure conditions and I would ask you to integrate some of this observations into the paper.

I am also not convinced from the evaluation of the pure conditions by equations 6 and 9. Why you use in both equations the variation of the observed ClO and HO2 (in k/2*(…) x t_{m+1}-t_m) and not in one of the equation the variation of HOCl? Please use also the HOCl observation in one of these equations.

Another question for me is the use of a box model for the definition of the relevant area with pure conditions (defined in the altitude 0.28 hPa). Does this box model reproduce the atmospheric conditions regarding temperature or transport? How is the photolysis integrated? Why you don't use here a Chemistry Transport Model with (for example) the meteorology of ERA-Interim? I mean the definition of the relevant area with pure conditions is very essential for the subsequent calculations. At least you could mention which assumption regarding the temperature and transport you have made for the box model calculations.

I don´t see any explanations for the calculation of the temperature dependency of the rate constants (Figure 7). How do you calculate the rate constants for different temperatures? Do you have observed the total temperature range from 200-300 K at 0.28°hPa (that cannot be) or do you use other altitudes? If so, do you have than the same "purity" conditions in these altitudes? Please explain this in more detail in the paper.

A last point is your error analysis. If I understand this correct you choose your region with the best agreement of Δk (only results with Δk < 0.1), skipping all other values,

assume that the variation in k is due to the measurements errors of SMILES and calculate the standard deviation for this measurements error. I think this is much too simple. First it is very critical to narrow first your results and then to calculate the standard deviation (I mean you can also determine delta k < 0.01 and then your error is probably even smaller) and second there are much more uncertainties in your calculation as only the measurement errors (see also comments of Review 1 by D.G. Johnson).

Overall, I think that the manuscript would benefit from a careful re-reading, perhaps by a native English speaker. There are still many linguistic mistakes in the text.

Because of the mentioned points I can only recommend this manuscript for publication after major revisions. But I would be very happy if you will make these corrections and it would be possible to accept this manuscript for publication, because I find it would be very useful if the possibility exist to deviate reaction constants directly from observations. But it has to be made with the same scientific correctness as in the laboratory experiments.

Specific comments:

Page 12798, line 6-7: This is very general, why do you use than only the altitude 0.28 hPa?

12798, 9: I would write "between the HOCl production rate and ClO loss rate".

12799, 13: Why extra polar region?

12800, 8-9: Which laboratory experiment do you have performed?

12800, 21: I would say depletion instead of destruction and calculated instead exploited.

12801, 1: Again Calculation instead of Exploitation

12801, 10: Is there a reference for the box model? What temperatures you use for the levels? What transport is assumed?

12802, 5: Please skip "due to the lower air pressure".

12804, 8: Is 18:30 not relative? I mean, the sunset changes from February to April in 20°S to 40°S at least one hour.

12805, 8: Do a reference exist for the midpoint method scheme?

12808, 23: This formulation please change in the total paper: Instead [x 10^{-12} cm³/molecule s] write x 10^{-12} cm^3 molecule^{-1} s^{-1} (without brackets).

12809, 6-7: This is in my opinion only a too simple assumption.

12810, 2-3: That is true, but I also think it is also very difficult to find such a condition in the atmosphere. Currently I am not sure if you have found this condition.

12810, 20-22 and 12811, 20-25: You don´t explain these measurements in high pressure conditions (1,5 Torr) in the manuscript. If you have made these calculations than please explain these in more detail.