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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Hydroxyl in the stratosphere and mesosphere – Part 1: Diurnal variability" *by* K. Minschwaner et al.

Anonymous Referee #2

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General comments:

The paper deals with a study of the diurnal variability of OH in the stratosphere and mesosphere by using a simple parameterization of the known chemistry and comparing with MLS data of several years.

For this referee is not clearly stated which is the main objective of the paper. Two aspects are mixed together: 1) the use of a simple parameterization for studying the MLS OH diurnal variability, and 2) to confirm/check if MLS measurements of OH diurnal variability are consistent with the known chemistry. It is not clear for this referee if the authors are pursuing the first, the second or both.

About the first point, the study presented seems to me more an student exercise than a research work, and it could done on the basis of the known OH chemistry without the need of MLS data. Even though, if that study is included in the paper, I think it could be done much shorter, stating directly from the beginning the known dependencies of [OH] on the different J's, and avoiding basic explanations of basic radiative transfer on the altitude behavior of J's. That is, if we know that [OH] is proportional to sqrt(a1*J_O3+a2*J_H2O) and since J $\sim$ exp(-Beta sec(SZA)), then [OH] = A1*exp(-Beta1*sec(SZA)) + A2*exp(-Beta2*sec(SZA)), where A1 and Beta1 are related to H2O and J_H2O, and A2 and Beta2 to O3 and J_O3. This expression can even be expanded in many more terms if we consider different wavelengths of J_H2O and J_O3 (the authors consider up to 3 terms when explaining the behavior of Beta). The exercise is, can those terms be represented by a single constant A and Beta? The answer is yes if 1) Beta depends on altitude (to cope not only with H2O and O3 altitude distribution but also with the different altitude behavior of J_H2O and J_O3 (cf. fig. 5) 2) A limited variability in O3 and H2O has to be assumed, and 3) Valid only for a limited range of SZA (SZA<75deg).

A large part of the paper (nearly the whole section 4, from the beginning of the section in p. 22324 and the next two pages) is devoted to explain the altitude behavior of Beta on the basis of the equation above. But I think this is very obvious, and explaining how the J coefficients change with altitude depending on the optical depth are obvious too. This is basic radiative transfer. This section could be reduced to just the two last paragraphs in Sec. 4 (p. 22327).

Also, it is important to highlight its limitations: small variability in H2O and O3 and SZA<75.

The second objective seems to be the analysis OH MLS diurnal variability. From Figs. 4 and 7 it seems that MLS data can be well explained with the currently established chemistry. However, again, this is limited to the measurements where the variability of H2O and O3 is limited and also a limited range of SZAs. If this is a main objective of the paper, I think that other conditions (H2O, O3 concentrations and SZA interval)

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should also be explored and analyzed in the paper.

Overall, I think that the paper, in its current form, contains a large fraction which is only an academic interest, i.e., well known. I do not recommend its publication in its current form. I would suggest to focus it more on the two topics of research which, in my opinion, are most interesting: 1) Methodologically: to establish a simple parameterization of OH diurnal variability and validate it again experimental (MLS) data, and 2) check if currently established OH chemistry is confirmed by MLS data, exploring the most extended range of conditions (i.e., O3 and H2O variability and SZA range) as possible.

Specific important comments:

1. p. 22324, first lines. The agreement between SLIMCAT and MLS data in Fig. 4 is not so good between 3 and 1 hPa. For steep changes, the eye might gives a wrong interpretation. The differences there are about 0.07, which is nearly 40%. It seems there is an altitude shift between MLS and SLIMCAT beta profiles.

2. Same applies to the first par. in Sec. 4. Please quantify. Also remember that SZA are limited to <75 deg and it is well established that most radiative transfer problems are more likely to arise at high SZAs.

3. Sec. 5. Conclusions. lines 9-11, p. 22328. If we start from the point that OH is controlled by H2O and O3 photodissociations, this assumption is trivial. I can't see anything new with this conclusion.

Minor comments and Technical issues:

1. P. 22319, ll. 9-12. Contrary to the statement of the other referee's comments, I think that Eq. (1) is correct, i.e., no need to include the factor of 2 in the H2O term. I would, however, make more clear that when the authors refer to the photochemical equilibrium for HOx, we also have to assume photochemical equilibrium AMONG the HOX species, i.e., H, HO2 and OH, because that is what allows to express [OH], [HO2] (and [H]) as factors of [HOx], e.g., [OH] = a*[HOx], [HO2] = b*[HOx], and [H] = c*[HOx].

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2. p. 22321, line 5, Talking about the 0.15 hPa level as the stratopause region might possibly be OK, but certainly not saying that the stratopause is located at ~64 km. I was confused, and so might be some students. The stratopause is seldom located so high (except in the "unusual" NH polar winters in the recent years of 2004, 2006, and 2009. I would say "low mesosphere" instead of stratopause. The same applies to line 3 in p. 22323.

3. p. 22321, line 12. " ... may spread over ...". I think the authors should be more categoric here. Since they have the data they can state whether or not the individual profiles spread over a large range of latitude, longitude and local time.

4. p. 22323lines 16-17. Nevertheless ... It should be emphasized here the assumptions considered: limited variabilities of O3 and H2O, and SZA<75 deg. Hence the capture of the OH variability to within +/-5% is only valid under those assumptions, not overall.

5. p. 22324, l. 2-5. I am not sure if the limited variability of O3 and H2O assumed for MLS data was taken into account for SLIMCAT data. Maybe this variability is much smaller in the model and then does not apply. It would be useful to be clarified.

6. p. 22327, ll. 3-6, Since the derived beta=0.45 is smaller than 0.5, it seems more likely that some losses are missed instead of productions, isn't it?

7. Sometimes the authors refer to "oxygen", e.g. line 2 in p. 22326. Although it is very obvious that they refer to "molecular oxygen", it would not harm, thinking mainly in students, to mention that explicitly.

8. Fig. 2, left panel. The blue '+' signs are hardly seen in the printed copy I have.

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 22317, 2010.