

Interactive
Comment

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

Anonymous Referee #1

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In their paper the authors propose a parametrization of the diurnal variation of OH concentrations. Related parameters are fitted to MLS measurements. I find this paper interesting and useful and thus recommend publication in ACP after consideration of the specific comments listed below, particularly if the problems raised with Eqs. 1 and 3 are satisfactorily solved and an errors can be excluded.

Abstract, l 11: The term “very good agreement” is quite vague and should be replaced by something more quantitative.

p22319 l1ff: What about other OH production reactions like $\text{H} + \text{O}_3$? Isn't this one important at least in the upper mesosphere?

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p22319 Eq.1: This Equation is not quite obvious to me. From R1, R2 and R3, and $[OH]/[HO_2] = c_1$, the equilibrium concentration of OH should be:

$$[OH] = \sqrt{\frac{2k_1[H_2O][O(^1D)] + J_{H_2O}[H_2O]}{k_3/c_1}},$$

and I miss the factor of 2 in Eq.1 of the discussion paper in the term related to R1. Eq 1 does not claim equality but only proportionality. However, since this term cannot be factorized this does not help. If the authors go a step further and apply $[OH]/[H] = c_2$, I get

$$[OH] = \sqrt{\frac{2k_1[H_2O][O(^1D)] + J_{H_2O}[H_2O](1 + \frac{1}{c_2})}{k_3/c_1}}.$$

There is no obvious way (at least I do not see one) to reproduce Eq 1 from this without further assumption. Particularly the multiplier of two cannot be factorized unless $c_2 = 1$ is assumed. The latter assumption, however, which seems essential to me to end up with Eq 1, is in conflict with Fig. 5.28 of Brasseur and Solomon, Aeronomy of the Middle Atmosphere, Springer, 2005: c_2 seems to span orders of magnitude. Since the proportionality postulated in Eq 1 plays a key role in the paper, it is important that (a) possible errors in Eq. 1 are rigorously ruled out, (b) that there are no hidden approximations or assumptions made which are necessary to infer Eq. 1 but that these assumptions are explicitly stated, and (c) that such assumptions are verified. Even in the paper referenced in this context (Canty and Minschwaner, 2002) I did not find a clear hint on this. I may be on the wrong track but at least I can state that Eq.1 is not obvious. Thorough clarification of this issue is necessary to put the paper on solid ground.

p22319 Eq 3: This equation seems to assume that photochemistry is exclusively driven by direct sunlight attenuated by the atmosphere. Scattered light from clouds or Earth's

surface are not considered although Fig. 4.38 in Brasseur and Solomon, *Aeronomy of the Middle Atmosphere*, Springer, 2005, indicates that scattering may indeed play a role when photolysis rates are estimated. Even the fact that the authors claim only proportionality instead of equality does not help because the impact of scattered sunlight depends on the solar zenith angle. An estimate of the error due to neglect of scattered light is needed to justify the simplified approach chosen.

p22321, line 10: This suggests that retrieved OH profiles were averaged. If MLS retrievals are maximum likelihood retrievals (c.f. Rodgers 2000), then it's fine, but if these retrievals are optimal estimates (maximum a posteriori retrievals), the a priori information will be overrepresented in the mean, since the optimal mean is not the average of optimal estimates of single profiles. I am not familiar with the MLS retrieval scheme, but if optimal estimation was used, this issue deserves some discussion.

Figure 4/5/7: The profiles of the beta parameters etc are most useful for the community if provided as numbers. I strongly suggest to provide these data either in a table or an ASCII file as supplementary material (or both). This will help the data to be actually used by the community.

Technical, wording, and other minor issues:

p22319, l17: I suggest a ":" at the end of the line.

p22319, line 21: While certainly everybody should know what the secans function is, I asked five randomly chosen scientists in my lab, and none of them could define the secans function without consulting a book or the internet. I thus recommend to define it in the text. I am sure this will make the article easier accessible for many readers.

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p22320, line 22: Why are the problems mentioned important only at 32 and 21 hPa (and not in between)? To understand this statement correctly, the reader needs to know that these pressure levels refer to the two lowermost altitudes of useful OH data. I suggest to reword “but at the two lowermost altitudes, i.e. 32 and 21 hPa, ...” or something similar.

p22320, line 14/15: The statement that conclusions are presented in the last section is quite obsolete, since conclusions are presented in the last section of almost every scientific paper, particular if it is entitled "Conclusions". Can't the authors summarize the content of this section a little more specifically?

Figures: Another axis with approximate geometric altitudes would be nice.

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