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Atmospheric Chemistry and Physics Discussions

Interactive comment on "On the dependence of the OH* Meinel emission altitude on vibrational level: SCIAMACHY observations and model simulations" *by* C. von Savigny et al.

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Reply to comments by reviewer 1

We thank reviewer for his/her thorough and helpful comments, and we believe that the revised version has improved significantly. We included essentially all the suggestions made by the reviewer in the revised version of the manuscript. Before addressing the individual points raised we would like to mention that the following aspects were changed / corrected in the revised version of the manuscript:

1. The absolute radiances of the spectra shown in Figures 1 a-c were not correct, but significantly too large. The ones shown in the ACPD version of the manuscript

corresponded to the accumulated (not mean) radiances of all spectra measured in July 2005. This is now corrected.

2. We identified a little (indexing) bug in our current implementation of the OH model (the results published in McDade (1991), McDade and Llewellyn (1987) are not affected), that lead to slightly different vertical shifts. This bug is now corrected, and is the reason why Figs. 6 and 7 look slightly different.

Note that our responses are italicized.

Review 1

In this manuscript, the authors use Envisat/SCIAMACHY observations of the vertical volume emission rate profiles of the OH(3-1), OH(6-2) and OH(8-3) Meinel bands to study the differences in emission peak altitudes between the different OH Meinel bands. This work gives observational evidence that the higher vibrational levels have higher emission peak altitudes, and gives some quantitative information of the peak altitude for different vibrational bands. There is some useful information here, even though some of the insights presented here are already known. In all, the results shown in this paper will be interesting to the middle and upper atmospheric science community. The paper is overall well written and easy to follow. I believe this paper can be published on ACP eventually. However, I think that some work should be done to improve the paper before it is published on ACP. I hence recommend that this paper be accepted for publication on ACP after some revisions are made.

Major comments:

A brief introduction of the satellite should be given. For instance, what is the satellite's orbit. Is it Sun-synchronous polar orbit? What is the height of the satellite orbit. If it is sun-synchronous, what is the local time of the observation of the night glow? This information is important because airglow is strongly modulated by tides [e.g., Ward, GRL, 1999; Marsh et al., JGR, 2006; Xu et al., GRL, 2010]. The peak height of the airglow emission varies with time because of the modulation by tides.

Reply: We agree that these are important pieces of information and we added them to section 2 (SCIAMACHY on Envisat). Envisat is in fact in a sun-synchronous 800 km orbit with a 10:00 a.m. descending node. The night-time observations used in this study are all made at around 10:00 p.m. local time.

The introduction section should be extended to include a comprehensive review of previous research. For the differences of the peak altitudes OH airglow emission rates for different vibrational levels, Makhlouf [JGR, 1995, Figure 4] and the TIMED/SABER observation and theoretical model [Xu et al., JGR, 2012, Figure 1, 2] indicated that v=9 peak at slightly higher altitude than v=1 peak. And Makhlouf [JGR, 1995] pointed out that [OH(v)] peak altitude moves down slightly as the quenching is increased.

Reply: Thank you for pointing this out. Unfortunately we were not aware of these studies. They are now mentioned in the introduction and also in the discussion section of the paper. We also mention in the modeling section that Makhlouf et al. (1995) already emphasized the importance of quenching by atomic oxygen.

It is better that the comparison between the TIMED/SABER observation and the Envisat/SCIAMACHY observations of the OH airglow is added if possible, because the observation periods of the two satellite observations overlap. There are two channels of the SABER's OH airglow observations, one is 2.0um band (OH(9-7)+OH(8-6)), another is 1.6um (OH(5-3)+OH(4-2)). There are obvious differences between the peak altitudes of the two channels airglow observation of the SABER's observation (Figure 1, 2 in Xu et al., 2012).

Reply: We appreciate the reviewer's comment and think that it is a good idea to directly compare the SCIAMACHY results with SABER results. However, we are planning a more comprehensive comparison of the SCIAMACHY, SABER as well as OSIRIS observations related to altitude shifts between OH Meinel bands origination from different vibrational levels that will be published in the near future (McDade et al., 2012; in preparation). We added a brief note on the upcoming follow-up paper by McDade et al. near the end of section 4.

For the model simulation, some sensitivity tests have been made. However, this reviewer thinks they are not enough. Some additional tests should be added in order to address the aim of this work, which is to give quantitative information of the peak altitudes for different vibrational bands. The following tests may be needed:

Reply: Based on the reviewer's comments we extended the modeling part of the paper and carried out the sensitivity tests described in (1), (2) and (3). We added a new section (5.1) to the paper that discusses these sensitivity tests.

(1) The assumption of Eq (5): This paper assumed that the initially produced OH follows a Gaussian altitude profile with a peak height of z0=87 km, and a FWHM of 8 km. In reality, this profile is mainly controlled by O3(z)+H(z). Therefore, sometimes, it is not a Gaussian function, or at least not a symmetrical Gaussian function. The peak altitude changes with latitude and seasons, and also local time induced by tides. Do these changes vary the conclusion? Simple tests, such as changing the peak altitude or FWHM of Gaussian function should be made.

Reply: We fully agree with the reviewer that the shape and altitude of the OH emission is quite variable, and we performed the tests as suggested by the reviewer. We changed the mean emission altitude in steps of 3 km between 84 km and 90 km, and the FWHM of the emission peak in 2 km steps between 4 km and 12 km. In general we find that the vertical spacing between the different vibrational levels increases with FWHM of the initially produced OH, as expected. This is now discussed in the paper. Changing the mean altitude, while leaving FWHM constant is also associated with changes in vertical shifts between emissions from different vibrational levels. This effect is mainly caused by changing quenching by O, as O-quenching is the main cause for the vertical splitting, and because the O concentration changes rapidly below the [O] peak which occurs above 90 km. This implies that OH emissions occurring at lower *altitudes – assuming that the O profile is unchanged – will be associated with smaller vertical shifts between emissions from different vibrational levels. The results are included and discussed in the paper. We also would like to mention, that a full treatment of the problem requires modeling the [OH] profile produced by H and O3. Our simple approach using the Gaussian profile was just intended to illustrate that the vertical shifts observed in the SCIAMACHY data can be qualitatively and semi-quantitatively reproduced.*

(2) Vertical concentration profiles of O, O2 and N2 are taken from the MSIS-E90 climatology in this paper. Does the profile of O influence the results? A simple test should be done.

Reply: The reviewer is right that the assumed atomic oxygen profile will affect the model simulations. We performed a test and scaled the entire atomic oxygen profiles with several factors between 0.1 and 10. The results are also included and discussed in section 5.1 of the revised version of the paper.

(3) Some tests of important photochemical reaction parameters, such as, the quenching rate of O2, the O-quenching rate, and A(9), should be done using the new parameters given in recent papers [Smith et al., JGR, 2010; Xu et al., JGR, 2012].

Reply: Based on the reviewer's suggestions we performed different new tests. We ran the model using the Smith et al. (2010) O-quenching rate in combination with the correction factor (beta = 1.293) derived in Xu et al. (2012), as well as the Xu-correction of the O2 quenching rate. The overall result is, that a reduction of the O-quenching rate (i.e. using the Smith et al. value instead of the Adler-Golden value) – or alternatively of the O concentrations – will lead to a reduced vertical shifts between emissions from different vibrational levels. An additional effect is that the vertical shifts below the emission peak become – in relation to the shifts above the emission peak – smaller. This behavior is consistent with SCIAMACHY observations (see Fig. 2), and directly addresses the last point raised by reviewer 3. The results are now included

and discussed in detail in the paper.

(4) The measurements are used to establish a relationship between the emitting vibrational level and the altitude of the peak emission. However, they only show one transition per vibrational level. For example, Figures 2-4 show (8-3) but not (8-7), (8-6), etc. Are the peak altitudes all the same? This appears to be assumed in the model (Figure 6, right panel), since all the emissions from a given vibrational level are combined.

Reply: We believe that the peak altitudes for, e.g. the (8-7), (8-6), (8-3) bands should all be the same, except perhaps for a second order effect that may occur, if the vibrational population is not entirely thermalized.

The v' to v" band volume emission rates can be written as:

V(v',v",h) = A(v',v",h) x n(v',h) where A(v',v",h) is the total band transition probability for the rotational temperature at altitude h, and n(v',h) is the number density of vibrational level v' and altitude h.

So, assuming rotational thermalization (or near thermalization), the volume emission rate profiles for bands (8-7), (8-6), (8-3), as given by the above equation, will only differ in magnitude and not in altitude distribution. The altitude distribution is solely driven by the n(v',h) term which is common for all v" levels.

Minor comments:

Line 8-10 in page 5822: "The retrieval is based on the assumption that the atmosphere can be approximated by a set of 10 homogeneously emitting layers of 3.3 km thickness ranging from about 73 km up to 103 km." should be " .. 70 km up to 103 km" or " .. 73 km up to 106 km".

Reply: Thanks for catching this. The statement was not entirely correct. It has been changed to: ".. emitting layers of 3.3 km thickness with center altitudes ranging from about 73 km up to 103 km."

References:

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Marsh, D. R., A. K. Smith, M. G. Mlynczak, and J. M. Russell III (2006), SABER observations of the OH Meinel airglow variability near the mesopause, J. Geophys. Res., 111, A10S05, doi:10.1029/2005JA011451.

Ward, W. E. (1999), A simple model of diurnal variations in the mesospheric oxygen nightglow, Geophys. Res. Lett., 26, 3565–3568, doi:10.1029/1999GL003661.

Smith, A. K., D. R. Marsh, M. G. Mlynczak, and J. C. Mast (2010), Temporal variations of atomic oxygen in the upper mesosphere from SABER, J. Geophys. Res., 115, D18309, doi:10.1029/2009JD013434.

Xu, J., H. Gao, A. K. Smith, and Y. Zhu (2012), Using TIMED/SABER nightglow observations to investigate hydroxyl emission mechanisms in the mesopause region, J. Geophys. Res., 117, D02301, doi:10.1029/2011JD016342.

Xu, J., A. K. Smith, G. Jiang, H. Gao, Y. Wei, M. G. Mlynczak, and J. M. Russell III (2010), Strong longitudinal variations in the OH nightglow, Geophys. Res. Lett., 37, L21801, doi:10.1029/2010GL043972.

Reply: We have included all of these references – except the Xu et al. (2010) paper – in the revised version of the manuscript. We did omit the Xu et al. (2010) paper, because it does not deal with variations in OH emission altitude.

Interactive comment on Atmos. Chem. Phys. Discuss., 12, 5817, 2012.