

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 3:

We thank the reviewer for his/her encouraging and helpful comments on our manuscript, particularly for the last comment related to the asymmetry of the shifts between the upper and the lower part of the OH emission layer. This aspect is now explicitly several times in sections 4 and 5 of the paper. Before addressing the individual points raised by the reviewer 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, C5176

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.

Review 3

This paper reports on differences between the vertical emission rate profiles of hydroxyl nightglow emissions from different vibrational levels derived from Envisat/SCIAMACHY limb-scan observations of several bands. The study provides detailed observational evidence, a review and comparisons to previous rocket results and a model study of hydroxyl photochemistry to support the conclusion that higher vibrational level peak at higher altitude, by about 0.5km per vibrational level. The paper is clearly and concisely written, contains appropriate references (with a notable exception of the very recent work by Xu et al. 2012), and the case is convincingly argued that these differences be taken into account when comparing ground based observations from different vibrational bands. I recommend the paper be published after consideration is given to the comments, corrections and suggestions below.

Page 5818 line 9 - omit comma

Reply: Changed

Page 5820 line 24 - nadir

Reply: Changed

Page 5822 line 8-9 – “the atmosphere can be approximated by a set of 10 homogeneously emitting layers” is a little confusing, suggest “the hydroxy layer can be approximated by : : :” Also, height steps 11 and 12 are used on the following page.

Reply: First point: changed. Second point: Yes, the spectra taken at tangent height steps 11 and 12 are used for background correction, but they are not used for the inversion.

Page 5823 line 12 – How does the atomic oxygen emission at 844.6nm and the N2 band emissions get handled in the spectral average over the OH(6-2) band to yield the y vector?.

Reply: These emissions can be neglected in our study, because we only deal with night-time observations. Due to the fact that only low-latitude observations are used, no twilight contamination is expected. Moreover, auroral contamination is not possible. Looking at the 2nd panel of Fig. 1 (OH(6-2) band) shows that there may be a small signature near 844.6 nm, but compared to the OH(6-2) P-branch lines it is negligible, at least for the purpose of this study.

Page 5823 line 22 – July 2005 compared to July 2007 in Fig 2 caption

Reply: The year in the caption of Fig. 2 was wrong and corrected to '2005'. Thanks for catching that.

Page 5823 line 26 – is generally higher than

Reply: Changed

Section 4.1 comparisons – it is appropriate that reference and comparison be made to the recent TIMED/SABER observational and modelling work of Xu et al. [JGR, 117, D02301, 2012). This work may also have some bearing on the reaction rate coefficients and their temperature dependence used in section 5.

Reply: We were not aware of the paper by Xu. It is now cited and briefly discussed in section 4.1. Regarding the impact of the Xu et al. (2012) findings on the modeling results see also our response to review 1 and the new section 5.1.

Page 5826 line 23 and Table 2 – Another separator is required (after the Evans et al.

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entries) in Table 2 to distinguish the 8 cases.

Reply: Changed, thanks for catching this!

Page 5827 line 28- suggest “do not allow accurate determination of the altitude shift”

Reply: Changed

Page 5828 first para – please provide details of the bands observed and altitude differences identified by OSIRIS and SABER measurements (Xu et al. 2012 ref appropriate here)

Reply: We followed the reviewer's suggestion and added more specific information on the SABER and OSIRIS results. We also added a reference to Xu et al., 2012.

Page 5829 line 13 – “e.g.” not required.

Reply: Changed

Page 5832 line 19 – omit “with”

Reply: Changed

Page 5832 last para and page 5833 – From observation of the measured emission rate profiles in Fig 2 it is apparent that the altitude differences between vibrational bands are larger on the topside of the layer than on the bottomside. The model results in Figs 6 and 7 with and without atomic oxygen quenching suggest that atomic oxygen quenching is important on the topside of the layer (topside is Fig 6-like), but less so on the bottom of the layer (bottomside is Fig 7-like). Can the model reproduce this behaviour atomic with appropriate modification of atomic oxygen altitude profile and/or rate coefficients? Reference and comparison to Xu et al. 2012 should be made.

Reply: This is a very good point, and it is now addressed in detail in the paper. The SCIAMACHY observations indeed indicate – as the reviewer pointed out – that the vertical shifts are larger above the emission rate peak than below the peak. Because

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of the high sensitivity of the vertical shifts to O-quenching a natural explanation for the vertical variation of the vertical shifts is related to the [O] profile, which assumes a maximum above 90 km, and exhibits rapidly decreasing concentrations with decreasing altitude below 90 km.

The fixing of the indexing bug mentioned at the beginning of this response to the reviewer's comments already caused a change of the vertical shifts towards larger shifts above and smaller shifts below the emission peak (see Fig. 6 of the revised version). Using the Smith et al. (2010) value of the O-quenching rate constant (including the correction factor determined by Xu et al. (2012)) increases the asymmetry in vertical shifts between altitudes above the emission peak and below, as the reviewer suggests. This is now explicitly discussed in section 5.1 of the paper, that also includes a description of the effect of scaling the entire [O] profile and changing the mean altitude and width of the initially produced OH profile. The overall conclusion is that considering the improved agreement between observations and model results using the Xu et al. (2012) O-quenching rate, the rate assumed by Adler-Golden (1997) is probably too large.*

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