Atmos. Chem. Phys. Discuss., 12, C5144–C5148, 2012
www.atmos-chem-phys-discuss.net/12/C5144/2012/

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.

C. von Savigny et al.

csavigny@iup.physik.uni-bremen.de

Received and published: 31 July 2012

We thank the reviewer for her/his constructive and helpful comments. 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, 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 2 This is a very well written paper based on data from the SCIAMACHY instrument over the period 2002-2010. This manuscript provides a clear explanation of the methods employed in the data reduction, the survey of previously published data, and the model simulations. The conclusions are well supported by the observations and simulation results. They are a valuable contribution to the field because they are based on a substantive quantity of observational material (2002-2010) and they confirm previous reports (based on relatively small datasets) of the variation of peak emission altitude with OH vibrational band upper state. The paper should be published with only very minor revisions.

Reply: We thank the reviewer for her/his positive and encouraging comments.

2. One of the motivations for the study (Page 5819 line 22) is the difficulty of interpretation of ground-based rotational temperatures due to the possibility that Meinel bands originating from different vibrational levels may have different peak emission altitudes. An important contribution that should be mentioned in this context is the paper by Cosby and Slanger (2007). Based on astronomical sky spectra taken with the echelle spectrograph imager (ESI) on the Keck II telescope on Mauna Kea, Cosby and Slanger (2007) found substantial vibrational population changes occur in all vibrational levels during the course of a night, and the magnitude of these changes varies from night to night.

Reply: Thank you for pointing this out. We have included this reference and a brief statement about it in the introduction.

3. It is important to note that the OH vibrational distribution is not a static equilibrium,

but is instead a balance between the formation of OH(v'= 7-9) and the transfer of population into the lower vibrational levels by radiation and collision (Cosby and Slanger, 2007).

Reply: This aspect is now also mentioned explicitly in the introduction.

4. It may be worthwhile clarifying that the altitude of various OH emissions has been found to vary in a systematic way as a function of local time, latitude, season, and phase of the solar cycle, e.g., Liu and Shepherd (2006). This may be one of the reasons for limiting the data interval to a one month period in Figures 1, 2 and 4, even though a much larger volume of data is available.

Reply: Thanks for pointing this out. We added a brief paragraph to the introduction discussing the know variations of the OH emission altitude with latitude, solar cycle etc.

The main reason for using monthly averages are the low signal-to-noise ratios in individual spectra of the OH(8-3) band. For the OH(3-1) band the signal of individual limb measurements is sufficient to invert them individually. But this is not the case for the (8-3) band.

5. In the context of the use of zonal averages, it may be worth mentioning that significant longitudinal variations in OH infrared emissions have been reported, e.g., Baker et al. (2007) and Gao et al. (2011).

Reply: We added a brief statement to section 3 to cover this aspect.

Minor textual and other typographical corrections

The manuscript is well written with very few typographical errors.

Reply: Thank you

Page 5818, lines 18/19: suggest move the word "well" as indicated in the following: "The model simulations well reproduce the observed vibrational level dependence of

the emission peak altitude well – both qualitatively and . . . ".

Reply: Changed

Page 5820, line 24: lowercase "n" in nadir.

Reply: Changed

Page 5835, line 14/15: suggest " .. model that allows the relative population of the different vibrational levels of OH to be simulated." instead of " .. model that allows simulating the relative population of the different vibrational levels of OH."

Reply: Changed

Page 5844, Fig. 2. Caption: "July 2007" should be "July 2005" for consistency with text (Page 5823, line 22).

Reply: Corrected

References

Cosby, P. C. and T. G. Slanger, OH spectroscopy and chemistry investigated with astronomical sky spectra, Can. J. Phys., 85, 77-99, 2007.

Baker, D. J., Thurgood, B. K., Harrison, W. K., Mlynczak, M. G., and Russell, J. M.: Equatorial enhancement of the nighttime OH mesospheric infrared airglow, Phys. Scr., 75, 615–619,doi:10.1088/0031-8949/75/5/004, 2007.

Gao, H., Xu, J. Y., Chen, G. M., et al. Global distributions of OH and O2 (1.27 micro m) nightglow emissions observed by TIMED satellite, Sci. China Tech. Sci., 54: 447-456, doi: 10.1007/s11431-010-4236-5, 2011.

Liu, G. and Shepherd, G. G.: An empirical model for the altitude of the OH nightglow emission, Geophys. Res. Lett., 33, L09805, doi:10.1029/2005GL025297, 2006.

Reply: We have included all these references in the updated version of the manuscript.