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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.

Anonymous Referee #2

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1. 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.

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.

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).

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.

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).

Minor textual and other typographical corrections

The manuscript is well written with very few typographical errors.

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 . . . ".

ACPD

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Page 5820, line 24: lowercase “n” in nadir.

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.”

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

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 μm) 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.

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