

Interactive comment on “Resolving the mesospheric nighttime 4.3 μm emission puzzle: New model calculations improve agreement with SABER observations” by Peter A. Panka et al.

Anonymous Referee #2

Received and published: 19 December 2016

This study investigates the impact of the additional vibrational energy transfer channel $\text{OH}^* \rightarrow \text{O}(^1\text{D}) \rightarrow \text{N}_2(v=1) \rightarrow \text{CO}_2(v_3)$, as recently proposed by theoretical and laboratory studies, on CO_2 4.3 μm nighttime radiances by means of NLTE radiative transfer simulations in comparison with SABER Channel 7 measurements. The authors show that the inclusion of the proposed new mechanism improves noticeably the agreement between simulated and observed radiances.

The manuscript is generally well written and the topic is of high interest because of its implication for the analysis of measured 4.3 μm radiances (and potentially for the retrieval of CO_2 nighttime densities in the MLT). However, I have one major comment which needs to be addressed: The authors state on p3, line 27 (and also p5, line 26)

C1

that OH is taken from WACCM results. However, what is required here is $\text{OH}^*(v=1-9)$, likely not available from WACCM simulations. Further, the text in Section 2.2 suggests that $\text{OH}^*(v=1-9)$ populations have been calculated based on kinetic rates provided in Table 1. This means that the relevant input for such calculations is H, O_3 , and O (the latter taken from SABER, as stated in Section 2.1). Have the required H and O_3 profiles been taken from WACCM? If this was the case, it might be possible that OH^* excitation is underestimated since WACCM O_3 was shown to be in tendency lower (by about a factor of 2) than observations (Smith et al., 2014). Given the importance of the actual amount of excited OH on the simulated 4.3 μm radiances, this point needs to be clarified.

Further, since SABER provides also independent measurements of OH^* (Channel 8 and 9), a direct validation of the calculated OH^* densities is feasible (as done in the López-Puertas et al., 2004 study) and should be undertaken.

Minor comments:

Table 1, footnote b: There seems to be a typo in f_v for $v=8$. According to Adler-Golden (1997) this factor should read 2.7 (instead of 7)

Smith, A. K., M. López-Puertas, B. Funke, M. García-Comas, M. G. Mlynczak, and L. A. Holt (2015), Nighttime ozone variability in the high latitude winter mesosphere, *J. Geophys. Res. Atmos.*, 119, 13,547–13,564, doi:10.1002/2014JD021987.

Interactive comment on Atmos. Chem. Phys. Discuss., doi:10.5194/acp-2016-766, 2016.

C2