We thank the reviewer for careful reading of the manuscript and helpful suggestions that lead to an improvement of the text. Here we reproduce referee's comments in full in italic and show our replies. Similarly, in the manuscript we use bold font to clearly denote the changed text.

I have one major comment which needs to be addressed: The authors state on p3, line 27 (and also p5, line 26) that OH is taken from WACCM results. However, what is required here is OH∗(v=1-9), likely not available from WACCM simulations. Further, the text in Section 2.2 suggests that 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, O3 , and O (the latter taken from SABER, as stated in Section 2.1). Have the required H and O3 profiles been taken from WACCM? If this was the case, it might be possible that OH∗ excitation is underestimated since WACCM O3 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 μ radiances, this point needs to be clarified.

We have really missed to specify in the text where from H and O3 was taken for our calculations. We now added to the paper text that both H and O3 were taken from SABER retrievals.

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.

We have addressed this comment in our replies to the report of referee 2. We reproduce it here.

The referee is raising a good point that we considered in our consistency checks by evaluating the effect on the OH(v) population for the two highest vibrational levels. We found very good agreement (within a few percent) between the number density determined for OH(8+9) from our test data sets and the SABER channel 8 observations. The absolute number density for OH(8+9) can be directly determined from the SABER Channel 8 radiances (Mast et al., 2013) and is therefore a rigorous consistency check. The extracted OH(8+9) absolute number density does not depend on any previous model result (only the radiance profile and the almost equal Einstein emission coefficients A97 and A86 are needed). Our ultimate goal is to develop an updated model that handles simultaneously all the SABER OH(v) and CO2 emission channels, but this is clearly out of the scope of this report. The goal of our study was to estimate, with the help of model calculations that are based on reliable inputs, the effect of the recently discovered “indirect” pumping mechanism of N2(v) at nighttime. The latter was suggested and then experimentally confirmed by Sharma et al, 2015 and Kalogerakis et al, 2016, respectively. We believe we demonstrated the importance of this new mechanism and made significant contributions that complement the previous studies. Nevertheless, as we make clear in our conclusion statements, there is plenty of room for further research.

Minor comments:

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

The value 2.7 appears only once in the paper *Adler-Golden (1997)*, namely in the Table 1, column 2, for the reaction OH(v=2)+O2. The footnote b in our table refers, however, to the values taken from

column 3 of same Table 1 of *Adler-Golden (1997)* for the reaction OH(v)+N2, where the value 7 corresponds to v=8.