Reply to comments: Referee #3

The listed suggestions by referee 3# aims at improving the description of methods as well as the conclusions of our study. In the following, we give a point-by-point reply to the specific suggestions made by referee 3#

>> Line 19-20: What is meant by "changes of the entire OH emission layer width"?

The term „entire OH emission layer width" refers to the OH layer width that is determined by the H+O3 source gases. We further clarified this point in the updated manuscript version (line 20, 525,550)

>> Line 152-153: Whatever Hoffman et al said, nudging does not turn a chemistry climate model into a chemical transport model, "essential" or not.

We agree with referee 3#. Any further discussion about the term „essential" would also be beyond the scope of this paper and has been removed accordingly (new manuscript: line 155-160).

>> Line 176: „We may assume that …" (diurnal evolution contained in zonal variation)

Replaced by „we make the assumption" in the new manuscript version. As outlined, Sect 4.5 addresses the non-migrating tide as a potential error sources in our conversion of latitudes to local solar times with regard to the SABER observations.

>> Line 188-191: „but is the emissivity of a vibrational transition not dependent on temperature there?"

The temperature dependency is considered in the rate constant k_1(T) in our simulations. To clarify this point, we now explicitly mention the temperature dependency in the new manuscript version (see table 2). Therefore, the error in the production rate of OH is related to the departure between the SD-WACCM4 temperatures from reality (new manuscript: line 176,256-258).

>> Line 192-195: „You state here that the Saber atomic oxygen is derived from the OH radiance, as was discussed by reviewer #2. However, you do not discuss how this might affect the applicability of the SABER O for investigating the dependency of properties of the OH layer on atomic oxygen."

We added a discussion about this point in the new manuscript version (line 197 – 211). In principle, atomic oxygen is indeed strongly affecting the production of OH, since it is in steady state with the O3 concentrations and therefore directly impacting the Bates-Nicolet mechanism. Vice versa, if the SABER model is consistent with the reality, this should imply that any correlation between O3 and properties of both VER profiles is real. Furthermore, the consistency between the derived day- and nighttime concentrations of O by Mlynczak et al. (2013) supports the performance of the SABER model, because two completely independent methods are applied for the derivation of day/nighttime O. This should also support our approach to correlate changes in SABER O with vertical shifts between both VER profiles.

>> Line 192-195: "Despite the less pronounced vertical ..." (improving the discussion on both definitions of OH layer altitudes).

We are now discussing the differences between both peak definitions in a more general sense. (new manuscript: line 228-232). In principle, we could think of two layers that are both symmetric for the most simplistic case. Apparently, changing their profile widths would affect the relative profile shifts above the peak positions, but with respect to the weighted peak altitudes, the relative peak shift wouldn't change at all. The situation is different, if we change the symmetry of the layer(s). Due to the vertical profiles of the O and O_2 quenchers and the different radiative lifetimes of OH(v), the collisional quenching is in principle affecting the symmetry of the OH(v) layers. Vice versa, changes in the $H+O_3$ source profiles will affect significantly the OH(v) layer widths, but the overall response will be a mixture of both, i.e. changes in the profile shape and widths. Therefore, the evolution of profile shifts will strongly depend on the method that is used for their determination.

>> Line 219 - "... significantly higher precision ...." Significantly higher compared to what?

We replaced this term with "dynamic range" (line 235)

>> Line 268-269 „you could provide a formula for the weighting. „

see line 312, new manuscript

>> Line 342-348: „I think, and I suggest that you show such a figure and invest some more work in investigating the combined effects of O and O_2 quenching. „

We included the suggested panel in our sensitivity study on the seasonal and diurnal variability of OH profile shifts. (see Fig4e, Fig7e) and agree that this considerably strengthens our conclusion on the role of O_2 quenching for the temporal evolution of OH peak shifts (new Manuscript line 408-422, 518-530, 593, 604). We also notice that the combined effect of O and O_2 quenching is scaling quite similar to the sum of the individual contributions for the diurnal evolution. (new manuscript: line 514)

>> „Figure 5 is hard to discern" - 3 years of SABER data in a row

As proposed, we display averaged SABER values to improve the comparison with our model results (see Fig. 5).

Reply to comments: Referee #2

Referee #2 claims that our approach in utilising WACCM results for our study is still not fully convincing in his opinion, even though, the major issues of the initial manuscript version now seem to be solved in the first revision of our manuscript.

We agree that our study further benefits from a more critical discussion on the suffering of our model approach from "important caveats in the upper mesosphere/lower thermosphere". We added a new section (new manuscript: Sect. 4), with a discussion on relevant departures of the WACCM simulations from reality and how this may affect our analysis. This includes discrepancies in the simulated temperatures, ozone and atomic oxygen concentrations.

With regard to the consistency of our nomenclature, we also decided to change the term "OH(9,5) peak shift" to "OH(9,5) profile shift" in the updated manuscript version, because this should avoid any further confusion with regard to our second altitude definition D.2, that is actually referring above the profile "peaks" of the OH(9) and OH(5) layers.