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Interactive comment on “On the temporal variability of the OH* emission layer at the mesopause: a study based on SD-WACCM4 and SABER” by S. Kowalewski et al.

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Response to general comments:

The general comments of referee #1 outline two main issues of this study:

1. “[...] the conclusions of the comparison are far from clear. ”

We agree with the general criticism that the conclusions we draw from our comparison are less clear than we would have initially thought. Even though referee #1 agrees that the overall topic is interesting, we realised that despite the recognised efforts we put into this study, a clear focus of our study is missing. We think that this is mainly

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caused by our rather general approach to describe changes in the OH* emission layer based on our model simulations and SABER observations, which we later connect with the impact of the collisional quenching process on the vertical shifts between density-weighted layer heights. Indeed, some of the reported features, in particular the reported daytime OH* features, are rather disconnected from our analysis on the collisional quenching process and may possibly distract the reader from the actual scope of this study.

2. “[...] There is actually very little in terms of direct model/observation comparisons in the paper. ”

We are aware of the potential discrepancies that may exist between the SABER observations and our model results, which make a direct comparison a difficult task. However, since the main scope of this study is on the improvement of our understanding of the physical mechanism that is driving the temporal variability of the vertical shifts between different OH* Meinel bands (revision, line 53-54), we believe that we can still learn something new about the collisional quenching process by our comparison, despite the existing differences between the model and observational data.

Our general approach to address these issues:

We find the referee's general comments very useful and the initial approach of our study was probably a bit too ambitious. In order to improve the conclusion of our study, we decided to narrow down our study to its most essential parts and to make the scope of our study much clearer. This includes:

- Addressing the scope of this study to the question: "How does the temporal variability in the collisional quenching affect the vertical shifts between different OH Meinel bands?" (revision, line 54-56)
- Limiting our study to the equatorial latitudes, where the large amplitude of the diurnal migrating tide gives us the best testing ground to assess the question above. (revision,

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line 61ff)

- Discarding any previous aspects that are not relevant to the assessment of the above mentioned question.

In addition, we also decided to change the title of our study to strengthen our focus on the above mentioned question. We think that this approach helps this study to become much more focused, such that the conclusions will be much clearer to the reader than it was the case in the initial version of our manuscript.

Response to specific comments:

In the following, we address each bullet point in the specific comments separately.

1. "The WACCM model simulations should be repeated"

For our analysis on the diurnal variability we repeated our model calculations for averaging periods that are matching with the SABER yaw cycle (as suggested). Furthermore, we now explicitly focus on the equinox conditions for our study on the diurnal variability because of the associated maximum of the diurnal migrating tide. For the seasonal variability we choose the same local solar times for SABER and SD-WACCM.

"Either the model vertical resolution needs to be increased or the authors need to show that the current resolution is sufficient [...]"

Unfortunately, we are limited to the provided vertical resolution of SABER and SD-WACCM. Following the study of Savigny and Lednyts'kyi (2013) , we therefore use the same method to quantify the peak altitudes (revision line 225). As we show in our later analysis, this method indeed successfully reveals a semi-annual oscillation in the vertical OH(9;5) peak shifts (i.e. for SABER and SD-WACCM), which is confirming our expectation according to the hypothesis on the impact of collisional quenching on the vertical OH(9;5) peak shifts.

"Since WACCM is a model pressure levels, I would also like to know how they derive

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dZ”

Our provided SD-WACCM4 data set includes geopotential heights (GPH) for each vertical output bin (revision line 163). Accordingly, we converted the GPH values to geometric heights to allow for a better comparison with the SABER data.

2. “Better validation of the model needs to be presented [...]”

Because of our emphasis on the diurnal-migrating tide, we discuss the seasonal as well as the diurnal tidal signatures with corresponding references to the literature.

3. All plots are now limited to nighttime conditions (as suggested)

4. “[...] spell out more clearly the quantities being presented”

We improved our explanation of the different quantities to sense relative vertical shifts between the selected OH($\nu=9$) and OH($\nu=5$) profiles. For this task, we now explicitly define the two reference points, which we use to determine the vertical shifts (see definition D.1 and D.2 between line 225 and 235).

“What exactly is dZpk+HWHM and what is it supposed to represent, what advantage does it have over dZpkweighted?”

The relative vertical shifts are now defined in Eq.(3) and Eq.(4). Given these references, we think that our terminology (including the signs) is now much clearer. The same also applies for the reason, why we decided to use two different reference points to study the vertical changes in the OH($\nu=9$) and OH($\nu=5$) profiles as explained from line 213 to 218 in our revision. This paragraph should also answer the question “Why shift the peak by its half maximum.”

“Why mix HWHM and FWHM in the paper?”

We agree that the mixing of the terms HWHM and FWHM should be avoided. However, both terms are used in a different context. While we use HWHM to define a point above the OH(ν) profile peak according to our definition D.2, the FWHM term is used

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to quantify the peak width of the OH(ν) layer (revision, line 362). Initially, we used the term “+0.5 FWHM” to define our reference point above peak altitude, however, due to the asymmetry of the OH(ν) profiles, it is more appropriate to speak in terms of the HWHM according to our definition D.2.

5. “[...] atomic oxygen decreasing with time, but dZpkweighted increasing. This is opposite to that shown in von Savigny and Lednyts’kyy (2013) ”

It is important to mention that the anticorrelation between both quantities in our model study refers to the diurnal variation, whereas the study of von Savigny and Lednyts’kyy (2013) refers to the seasonal variation. Indeed, this is one of the important new findings of our study that the diurnal correlation between both quantities cannot be explained by the process of collisional quenching. This picture is consistent within our SABER and SD-WACCM4 based analysis and does not contradict the more important role of the collisional quenching process for longer (i.e. seasonal) timescales. Another essential outcome of our sensitivity analysis is that even the seasonal variability of “dZpkweighted” is only partially caused by the modulation of atomic oxygen concentrations (e.g. revision, line 347) without contradicting the observed correlation. We think that this is a very important aspect of our study.

6. We improved the consistency of our nomenclature for the hydroxyl radical

7. “SD-WACCM is not a chemical transport model [...]”

We agree that SD-WACCM is not a chemical transport model and corrected the expression in the abstract. According to the study of Hoffmann et al. (2012), they describe that the nudging of the GEOS-5 data “essentially” turns the SD-WACCM4 model to a chemistry climate model. Following their paper, we adapted the same expression in the SD-WACCM4 section of our manuscript, even though one might debate on how to interpret the word “essentially”.

“[...] if you are to compare SABER VER to SD-WACCM you should probably show

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temperatures agree as well. ”

We also agree that a direct comparison between SD-WACCM temperatures and measured SABER temperatures would aid to better understand the absolute differences between both datasets. However, given the main focus of our study on the physical mechanism, which is driving the temporal variability of the OH Meinel bands, our SD-WACCM and SABER based studies produce at least consistent results. Of course, one could choose a more optimised model approach, but we hope that in the frame of this study, we could already give some interesting answers to the above stated question.

“If there are problems, why would they appear only at daytime?”

Even though, we excluded the daytime features in our revised version, the mentioned issue about the observed daytime features is just limited to a narrow mid-litudinal band, therefore, these features should not interfere with our analysis on the equatorial regions. The rather cautious consideration of these features is more referring to the difficulty in validating these daytime features from observations. Thus, we cannot actually exclude that these are real existing features (as they show some systematic characteristics), but as mentioned before, the assessment of this question would already exceed the scope of our study.

8. “SABER observations now extend for over a decade, but the authors concentrate on just 13 months.”

Our initially downloaded SABER files contained atomic oxygen profiles that were filled with Not-a-Number values before the beginning of our displayed time series. However, we do not expect any drastic changes by expanding our nearly 3 years time series (not 13 months) to an even longer period. The revealed seasonality furthermore agrees with the reported results from Savigny and Lednyts'kyy (2013), thus, for the scope of this study, we think that we would not gain any new insights by expanding this time series

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