Dear Editor,

our responses to the reviews are provided below. Due to an adjustment in the abstract of our revised manuscript, a few indicated line numbers in our responses may be shifted by one or two with regard to the revised manuscript version. Thank you for your understanding.

Kind regards,

Stefan Kowalewski (on behalf of all co-authors)

Final Author comments: Anonymous Refereee #1 – acpd-14-C463-2014

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 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 (see 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 comment 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?“ (see 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. (see revision, 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’kyy (2013) , we therefore use the same method to quantify the peak altitudes (see also 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 dZ*”

   Our provided SD-WACCM4 data set includes geopotential heights (GPH) for each vertical output bin (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(v=9) and OH(v=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(v=9) and OH(v=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 to quantify the peak width of the OH(ν) layer (see also 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 anticorrlation 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 by 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 “dZpkweigjhted” is only partially caused by the modulation of atomic oxygen concentrations (e.g. see 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 nomenclatur 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 about how to interpret the word „essentially“.

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

Final Author comments: Anonymous Referee #2 – acpd-14-C482-2014

<u>Response to general comments:</u>

The general comments of referee #2 addresses the following two main issues of our study:

1. “[...] *is not clear which is the aim of the paper* [...] ”

Similar to the general comment from referee #1, the aim of our study is not clear to referee #2. Again, we agree with this comment for the same reasons as mentioned in our response to referee #1. We think that the narrowing down of our study to the assessment of a single question, as described in our response to referee #1, makes the aim of our study much clearer than it was before.

“[...] *references to earlier work on the rocket-borne instruments are missing”*

We now included a reference, which addresses the earlier work on rocket-borne instruments. (see revision, line 38)

2. “[...] *I cannot see any new result from what is already known* .”

The second critical point in the general comments refers to the new aspects of our study, which referee #2 is missing.

The quantitative effect of O2 quenching on the vertical displacement of the OH layers has not been discussed in any other studies so far. Although it is generally known that O2 also contributes to quenching of OH* - as referee #2 certainly correctly points out - its effect on the vertical displacement has - to our best knowledge - not been discussed explicitly, neither in a qualitative, nor in a quantitative way.

Another novel aspect compared to the sensitivity studies presented in von Savigny et al. (2012) is that the OH*(v) profiles modeled in this study are based on H and O3 profiles, i.e. fully consider the best knowledge of the relevant chemical processes on the shape of the OH layer. In contrast, in von Savigny et al. (2012) a Gaussian OH* profile shape was assumed and the model used to estimate the vertical displacements between different OH* layers is based on simplying assumptions.

We agree that the report of a coherent semi-annual oscillation in the vertical displacements between different OH* layers and atomic oxygen is not a new aspect, which has been revealed by our study for the first time. However, this is also not the main aspect of this study.

Despite the previously observed correlation between the relative vertical OH(v) shifts and atomic oxygen concentrations (see Savigny and Lednyts’kyy, 2013), we cannot exclude that other physical mechanisms control the seasonality of the relative vertical OH(v) shifts in addition. Thus, the main emphasis of this study is on the improvement of our understanding of the physical mechanisms that are driving the observed temporal variability in the relative vertical OH(v) shifts.

“[...] *what can we learn from the use of this model*”

Given the possibility of our quenching model to activate and deactivate individual quenching processes, this allows us to study the contribution of each process on the temporal variability of the relative vertical OH(v) shifts at different time scales, which is a novel aspect of our study as stated above.

Moreover, while the study of Savigny and Lednyts'kyy, (2013) was limited to seasonal timescales, the inclusion of the diurnal variability of the relative vertical OH(ν) shifts is another new aspect of our study. From our model approach we find an explanation why the diurnal correlation between vertical OH(ν) shifts and atomic oxygen concentrations is not as evident as for the seasonal timescales, which demonstrates the potential use of our model approach.

"[...] *why do you want to derive the correlations above-mentioned if you already have the model?*"

We agree with referee #2 that all dependencies, which determine the relative OH(ν) shifts , are (at least ideally) included in Eq. 2. of our study. However, coming back to the question about the new aspect of this study, we investigate for the first time the temporal variability by this approach, while the study of von Savigny et al., (2012) does not include the consideration of diurnal or seasonal changes. Thus, it is less our intention, as questioned by referee #2, to validate the SD-WACCM4 model with the observed relative changes in the SABER VER profiles, but to study the relevance of the temporal modulation of the quenching for the vertical shifts between the OH* Meinel bands.

"[...] *WACMM is rather different from the "reality* [...]"

As discussed in our response to referee #1, we are aware of the existing discrepancies between the model and the observational world. Even though the listed references by referee #2 all refer to the precedent WACCM version 3.5, we cannot exclude that the discussed issues are still present in the updated WACCM version 4.0. This indeed makes a direct comparison between our modeled OH(ν) profiles and observed SABER VER profiles difficult. Nonetheless, we find that both, our model and SABER results, still reveal a consistent picture on the role of the temporal modulation of the collisional quenching with the observed vertical OH(ν) peak shifts. In addition, we included the reference to the work of Lu et al. (2012) in line 280, which gives us some confidence on the simulation of the diurnal-migrating tide, even though its magnitude is slightly underestimated.

"*About SABER, one should mention that nighttime O is "derived" (not directly measured)* "

With regard to the SABER O profiles, we mentioned their derivation in line 175 (old manuscript version) as well as the description provided by Mlynczak et al. (2013).

"[...] *the manuscript describes and illustrate with figures several features of the OH*(v) layer which are well known* [...] *They should not be included in a research paper*."

We agree that some of our previous figures are not essential for the main emphasis of our study on the collisional quenching process. However, we still kept those figures in our revised manuscript that we use to explain our methodology (e.g. vertical OH(ν) nighttime profiles). In particular with regard to the mentioned concerns with the SD-WACCM4 model data, we think that we should show that a tidal response is indeed present in the quenching fields.

Response to the specific comments:

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

1. "*All you can say is that the model "reproduces" (or a synonymous) previous observed features.* "

   Indeed, we fully agree that the term "confirm" is less appropriate for the reproduction of some general OH(ν) features with our simulations and has been removed accordingly.

2. “*The fact that O2 is the dominant quencher of OH*(v), either single or multi-quantum, it is a very well established result long time ago* ”

   As discussed in our response to the second general question above, we do not question that the efficiency of the O2 quenching was well established before, but this is also not our point as explained above. It is worth noting that the semi-annual modulation of the OH(v) profile weighted O2 concentrations is in phase with the O concentrations, which again is an interesting new aspect with regard to the temporal impact of the collisional quenching process. As mentioned before, it is not the “static” effect of the collisional quenching we are interested in, but the temporal evolution in the quenching species. Again, the ability of our model approach to switch individual quenching processes on and off allows us to differentiate between the impact of both quenching species. This again demonstrates, what we can learn from our model approach, because it suggests that collisional O quenching is not the only responsible mechanism that is driving the seasonality in OH(v) peak shifts (which we cannot simply tell from observations only).

3. “*I cannot see a clear outcome […] nor on the new results about the OH*(v) layers or validation of the model.*“

   The last comment addresses the missing outcome of our study as well as the previously mentioned missing new aspects. We think that our major revisions should make these two points much clearer now. Just to summarise the new aspects of our study:

- To our knowledge, this is the first sensitivity study that investigates the temporal modulation of the collisional quenching process with regard to the seasonal as well as the diurnal evolution of vertical shifts between the OH* Meinel bands. In addition to the pure observational studies, our approach enables us to explicitly study the effect of the temporal modulation of the two most effective quenching species. Furthermore, we are not aware of any study, that has explicitly investigated the impact of the diurnal modulation in the collisional quenching process with respect to the vertical shifts between the OH* Meinel bands.

  With regard to the new findings we should outline:

- This study gives new insights to the relevance of the temporal modulation of the O and O2 quenching species for the vertical shifts between the OH* Meinel bands. For the seasonal impact of the collisional quenching, our model simulations indicate that the changes of O cannot entirely explain the observed semi-annual oscillation, which according to our knowledge is a new aspect that has not been considered before. Vice versa, the rather insignificant diurnal response to the collisional O and O2 quenching is another new result. Both findings, i.e. with regard to the seasonal and diurnal variability, are consistent between our model study and SABER observations, thus, our sensitivity study helps us to interpret the previously observed correlations in a much more sophisticated way.