

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 #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
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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. (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 simplifying 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(ν) 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(ν) 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(ν) 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(ν) 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 [...]"

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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

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features.”

Indeed, we fully agree that the term “confirm” is less appropriate for the reproduction of some general $\text{OH}(\nu)$ features with our simulations and has been removed accordingly.

2. “The fact that O_2 is the dominant quencher of $\text{OH}^*(\nu)$, 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 O_2 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 $\text{OH}(\nu)$ profile weighted O_2 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 $\text{OH}(\nu)$ peak shifts (which we cannot simply tell from observations only).

3. “I cannot see a clear outcome [. . .] nor on the new results about the $\text{OH}^*(\nu)$ 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

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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 O_2 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 O_2 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.

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