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Discussions

# *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.

**Anonymous Referee #1**

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General comment:

This paper aims to compare modelled and observed variability in the OH Meinel emission layer near the mesospause. A large part of the paper is devoted to examining the difference in density-weighted layer heights between two OH Meinel vibrational states and a correlation with the atomic oxygen density, as observed by von Savigny and Lednyts'kyy (2013). Clearly there is a lot of work that went into this study, but the conclusions of the comparison are far from clear. This is illustrated by the following line taken from the paper: "As with our model results, the comparison between peak shifts and O concentrations hardly reveal any consistent relationship. The same also applies



for the mid-latitudinal example in Fig. 9b and d, which is suggesting again that the diurnal variability of the vertical peak shifts is mainly driven by the diurnal variability of the H and O3 profiles." There is actually very little in terms of direct model/observation comparisons in the paper. The reasons given are that the model is not output at a cadence where they can compare to observations, might be contaminated with non-migrating tides, does not cover a complete yaw cycle, etc. Which raises the question, should this be split into two papers?, since comparisons may or may not be valid. I am also concerned about using a model and observations with coarse vertical resolution to detect sensitivity of small differences in layer heights. So, while I find the topic interesting, I think the paper is not suitable in it's present state for acceptance to ACP. Below are some suggestions for improving the paper, which I hope the authors find useful.

Specific comments:

- The WACCM model simulations should be repeated with output that can be directly compared to SABER. Ideally, at the observation locations, or at a temporal frequency that yaw-cycle averages based on SABER local time sampling can be assembled. Figure 1 shows the coarse vertical resolution of the model ($\sim$3 km), but the study differences the heights of the profiles, which can be as little as 500 m. Either the model vertical resolution needs to be increased or the authors need to show that the current resolution is sufficient to accurately determine these differences. Since WACCM is a model pressure levels, I would also like to know how they derive dZ

- Better validation of the model needs to be presented. More evidences (via references) that the mesopause variability is reasonable. Comparisons with SABER 1.6 and 2.0 emission rates need to be shown if we are to trust the layer peak estimates.

- If the comparisons and modeling are to be limited to nighttime, figures should be restricted to that. If I understand the definition of peak altitude (see next point), then it is clear that the peak altitude of the daytime profiles with two peaks makes little sense - e.g., a "peak altitude" of 80 km at noon at the equator (Figure 2), where the density is at a minimum.

- The paper needs to spell out more clearly the quantities being presented (dZ9,5, dZpkweighted, dZpk+HWHM). Also, it is never mentioned what the sign of these quantities means - positive means which layer is above which? The terminology is confusing, for example the "peak altitude" is not the altitude of the density peak as one might expect but rather the density weighted layer altitude. Note, the discussion is often of shifts in layers but inspection of Figure 1 shows that the bottom of the layer is stationary, and that rather the thickness is varying with vibrational level - layers become thinner with decreasing vibrational level. What exactly is dZpk+HWHM and what is it supposed to represent, what advantage does it have over dZpkweighted? Why shift the peak by its half maximum. Why mix HWHM and FWHM in the paper?

- Figure 9 shows equatorial atomic oxygen decreasing with time, but dZpkweighted increasing. This is opposite to that shown in von Savigny and Lednyts'kyy (2013). Given the general comments in the paper and this are the authors now showing the previous result is not robust and the influence of O variability is minor at best?

- OH* is shorthand and is better replaced with either OH(v) or OH Meinel. Please be consistent, sometimes it's OH*, others 'vibrationally excited OH*', others OH(v'), or OH(v) or just OH (R4).

- SD-WACCM is not a chemical transport model in the domain of the modeling done here. GEOS data does not cover the mesopause. The dynamics near the mesopause may or may not reflect those observed by SABER. Since SABER measures temperatures, if you are to compare SABER VER to SD-WACCM you should probably show temperatures agree as well. It may be that they don't agree, but that could aid in understanding model/observation differences. Comments such as "However, we cannot exclude that these localised high daytime concentrations might represent a model feature due to the free-running mode of SD-WACCM4 above 60 km" do not inspire confidence. If there are problems, why would they appear only at daytime?



- SABER observations now extend for over a decade, but the authors concentrate on just 13 months. Since there are no direct comparisons between the model and observations, it seems an unnecessary restriction and leaves the reading wondering how representative these results are considering the year-to-year variability observed in the diurnal tide.

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 1239, 2014.