Atmos. Chem. Phys. Discuss., 14, C482–C485, 2014
www.atmos-chem-phys-discuss.net/14/C482/2014/

Atmospheric Chemistry and Physics Discussions

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14, C482–C485, 2014

Interactive Comment

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 #2

Received and published: 14 March 2014

General comments This work aims at investigating the diurnal and seasonal responses of the OH*(v) emission to changes in the concentrations of atomic and molecular oxygen. In particular, they try to find correlations between the altitude and extension of the emission layers of different OH*(v) vibrational levels with the atomic and molecular oxygen concentrations and their diurnal and seasonal variability. The authors have performed a very detailed and complex work but it is not clear which is the aim of the paper and I cannot see any new result from what is already known. The authors review and describe very well the work so far carried out on OH*(v) emission (although references to earlier work on the rocket-borne instruments are missing), and mention the previ-

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Interactive Discussion

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ous work carried out has on the correlation between the difference in the altitude of the peaks of different OH*(v) layers and atomic oxygen concentrations, i.e. (von Savigny et al., 2012; von Savigny and Lednyts'kyy, 2013). I understand that they try to go further by searching for correlation(s) between those vertical displacements with O and O2 concentrations. This would seem right to me if they would use instruments data. However, they take model simulations (WACCM plus an OH*(v) model) and look for those correlations in the model-produced data. (They also use marginally SABER data but, I discuss below, it does not make sense to me). Hence what do you expect to learn from the use of the model? Maybe to validate the model? But then you would not need to develop such an intricate analysis for that purpose; just look direct at the OH emissions layers themselves and compare with measurements. On the other hand, why do you want to derive the correlations above-mentioned if you already have the model? That is, if the model is correct it includes all information, including all dependencies between all parameters (see Eq. 2). If one needs to study a particular relationship just use the model output and correlate them with the inputs. You have all the information there and control everything. In addition to that, as the authors themselves found, the pursued correlation, even using the well defined model, does not always show up, because the OH layers are not only controlled by O and O2 but also by O3 and H. Furthermore, it is well known that temperatures in WACMM are too warm in the mesosphere (Smith, 2012) and that the mesospheric O3, which largely control OH through H+O3, is known to be significantly less than measured by recent instrument (Smith et al., 2013). Hence, even if the relationship would appear, since WACMM is rather different from the "reality" (at least temperature, ozone and even the meridional circulation -the altitude of the strong poleward flow in WACCM is at too low- (Smith et al., 2011), the results would be questionable. About SABER, one should mention that nighttime O is "derived" (not directly measured) from the OH*(v) emission itself (mainly from v=9). The procedure is to retrieve H first and then, using the O3 measured from the 9.6 μm channel, and assuming O3 is in photochemical equilibrium with O, to derive the latter. That is, O is an inferred quantity that needs an OH*(v) model for its derivation. The conclusions

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that would be derived from SABER OH*(v) and O, by using the OH model of this work would be questionable because: 1) both OH*(v) models should have to be consistent (or identical) and 2) even though there will be using a circular argument.

I have a number of less important comments throughout the manuscript, which I think are not worthy to be mentioned at this point. For example, the manuscript describes and illustrate with figures several features of the OH*(v) layer which are well known, as the authors recognized. They should not be included in a research paper.

Let me, however, give my opinion on the conclusions.

The first 3 bullets: l. 10 and ff. on page 1263: "This model approach ... confirm them". The model will "confirm" them if compared against new measurements. All you can say is that the model "reproduces" (or a synonymous) previous observed features. I cannot see new results here unless you want to "validate" the models. In that case it should be confronted with measurements, although, apparently, this has been done already (von Savigny et al., 2012); von Savigny and Lednyts'kyy, 2013).

Next two bullets indicate description of WACMM results. Is it that the objective of the paper? Maybe it is but is just do not correspond to the major aim described in the abstract and introduction. Next paragraphs: lines 15-18. " ... fail to find a significant correlation between the vertical OH(9)/OH(5) displacements and the effective quenching with either O or O2". Not surprising because of the O3 and H variability. But, do you need to find such a correlation if you already have the full dependency, i.e., Eq. (2)?

p. 1264 line 21-23, "In addition, our study reveals that a similar oscillation also exists for the absolute O and O2 concentrations at OH* layer altitudes". As said above, this is just telling us that WACCM includes those variations appropriately and that introducing them into Eq.(2), the resulting OH*(v) (or vertical displacements) also reflect them. What do we learn here? We already know (see Eq. 2) that the quenching process by O and O2 are important. Not sure if I'm missing something here.

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Lines 24-27. "While previous studies ... the effect of O quenching to the OH* profile, the systematic impact of O2 quenching on the vertical OH* structure is, to our knowledge, a new aspect in terms of the processes that are driving the semi-annual response in OH* airglow ..." I am surprised by this assertion. Definitely this is not a new aspect. 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 (see, for instance, Adler-Golden (1997) and the references there in. The authors cite this work.

Sentence starting at line 5 on page 1265 makes a similar assertion as discussed above.

Overall, although it is a well presented study, it is very detailed and there is a hard work behind, I cannot see a clear outcome or anything in this work, nor on the new results about the OH*(v) layers or validation of the model. Hence, I would not recommend the paper for publication in ACP.

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