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Interactive comment on "Stratospheric and mesospheric HO_2 observations from the Aura Microwave Limb Sounder" *by* L. Millán et al.

Anonymous Referee #1

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General comments:

This manuscript presents algorithm and results for a new offline HO2 retrieval from MLS/Aura limb observations. The main difference between this new retrieval and the MLS standard retrieval is that the retrieval is not applied to individual MLS limb measurements, but to zonally averaged data. The resulting noise reduction allows covering a significantly enhanced altitude range and a wider latitude range. I find the paper in general well written and relatively easy to follow. The paper is in my opinion suited for publication in ACP, but I ask the authors to consider the comments given below. My main criticism concerns two aspects:

a) The model-measurement comparison presented in section 4.4 does not really allow

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any conclusions to be drawn, as far as I can tell. Therefore, one may question the necessity of this section. In my opinion the section should at least be improved to better describe the assumptions made for the 2 model scenarios (see specific comment below) and by adding a more detailed discussion of the implications of the comparisons performed.

b) Figure 6 shows a comparison between MLS offline and FIRS-2 balloon HO2 observations for the 1 to 10 hPa pressure range. The MLS profile is a daytime/nighttime average, despite the fact that earlier in the paper it was stated that the retrievals between 1 and 10 hPa are affected by systematic biases. For this reason the daytime measurements presented for this pressure range are differences between daytime and nighttime measurements. If there are known biases, a comparison of daytime/nighttime averages to FIRS data does not appear to be a valid comparison.

Specific comments:

Page 22907, line 10: "in the Lyman-alpha and the Schumann-Runge bands“

This phrase implies that the Lyman-alpha signature is also a band, which is not the case.

Page 22907, line 22: “a problem known as the HOx dilemma”

From the following description of differences between observations and model simulations it’s not fully clear what the “HOx dilemma” is. Is it the low bias of OH measurements compared to model simulations reported by Summers et al. (1997) or is it the general disagreement between models and measurements, with the latter being sometimes higher and sometimes lower compared to the models?

Page 22908, line 2: “lower that the values” -> “lower than the values”

Page 22908, line 11: “Furthermore, models have consistently under-predicted the amounts of O3 at such altitudes, an issue known as the O3 deficit problem”

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Some of the references cited are 3 decades old. The most recent one is already 10 years old. I'm wondering, whether more recent studies find better agreement between modelled and observed O3?

Page 22909, line 17: "It covers between 82S and 82N" -> "It covers latitudes between 82S and 82N"

Page 22910, line 13: "in (Livesey et al., 2006)" -> "in Livesey et al. (2006)"

Page 22910, line 19: "indecipherable" -> "indistinguishable" ?

Page 22910, line 21: "with a 10deg latitude typical precision"

I'm not entirely sure what you mean here. Probably the typical precision for measurements zonally averaged and binned in 10 deg latitude bins? I suggest stating this more explicitly.

Page 22911, line 20 "with day-night differences used as a measure of daytime HO2 for pressures between 10 and 1 hPa where the nighttime values exhibit non-zero values indicative of biases."

I'm wondering, whether this special treatment between 1 and 10 hPa leads to discontinuities at the 1hPa level? It would be good to provide a quantitative estimate on the jump or discontinuity at 1hPa – or an upper threshold. In Figure 2 such a discontinuity is not visible, but this may just be because of the finite width of the vmr bins.

Page 22913, line 6: "this retrieval"

Suggest to replace this by "the retrieval presented in this study", to avoid confusion with the standard retrieval, which is also mentioned in the previous sentence.

Page 22913, line 22: ""truth" model atmosphere" -> ""true" model atmosphere" ?

Page 22914, line 13: "The MLS HO2 profiles is a 20 deg latitude bin"

This statement is certainly not correct, a profile is not a latitude bin. Suggest replacing

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by, e.g.: “The MLS HO2 profile corresponds to a 20 deg latitude bin”

Page 22916, line 4: “within half an hour of the MLS measurements”

You mean half an hour in terms of local time, not UT, right? I suggest mentioning this explicitly.

Page 22917, line 5: ’a strong zonal latitudinal gradient’

I don’t quite understand what you mean by ’zonal latitudinal gradient’. Please clarify.

Page 22917, same sentence: ’gradient from the summer pole towards the winter pole’

The “gradient” generally points from low values to high values, i.e. if you speak of the gradient’s direction (and not just the fact that there is a gradient), there’s a gradient from the winter (NH) pole to the summer (SH) pole and not vice versa. I suggest omitting the statement on the direction of the gradient and just state that there is a gradient.

Page 22917, line 11: ’zonal latitudinal gradient’

Please change (see comment above)

Page 22917, line 20: ‘eg.’ -> ‘e.g.’

Page 22918, line 23: ‘as well as a strong zonal latitudinal gradient from the summer to the winter pole’

See comments above

Page 22918, line 26: ‘the H available is the one generated at sunlit latitudes, transported at high altitudes poleward, where it descends and reacts with O2 at night.’

I suggestion mentioning explicitly that this applies to the winter, i.e., the northern hemisphere in this case.

Page 22918, line 27: ‘Pickett et al. (2006)’ -> ‘(Pickett et al., 2006)’

Page 22919, line 17: ‘which adds a constraint to MLS OH to mostly ..’

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I don't understand what this means. What kind of 'constraint' is that. Does it simply mean that you use MLS OH profiles? Please clarify.

Section 4.4: I think this section is the weakest part of the paper, because the implications of the model-measurement comparisons are not clear. If there are no conclusions to be drawn from this comparison, one may question, why section 4.4 is necessary at all. If this section remains in the paper, the implications of the differences between the model runs need to be explained better and in more detail, in my opinion. What exactly do we learn from the fact that the agreement to measured HO2 is improved if OH is also taken from MLS measurements? Can robust conclusions be drawn if the uncertainties of the MLS data products are considered?

Page 22920, line 12: 'These offline HO2 has' -> 'This offline HO2 dataset has'

Page 22920, line 19: 'from 10 to 0.0032' -> 'from 10 to 0.0032 hPa'

Next line: 'from 1 to 0.0032' -> 'from 0 to 0.0032 hPa'

Page 22921, line 3: 'in the low side' -> 'on the low side' ?

Page 22921, line 11: 'as much as 60% but probably' -> 'as much as 60%, which is probably' ?

Figure 4, caption, line 1/2: 'daily, weekly .. yearly 10 deg latitude bin'

I find this phrase odd, because the latitude bin is not a 'daily, weekly etc.' latitude bin

Figure 5, caption, line 3: 'Magenta lines'

There are no magenta lines on my screen (nor on the printout). To me it looks more like violet.

Figure 6, caption, line 2: 'The MLS data correspond to the daytime-nighttime average of the 15 to the 25 September ..'

Earlier in the paper you wrote about possible biases affecting both daytime and night-

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time measurements at altitudes below the 1 hPa level. Because of the bias you reported differences between daytime and nighttime HO2 between 1 and 10 hPa. For the FIRS-1 comparison you use the daytime-nighttime average, which leads to the conclusion that the comparison shown in Fig. 6 is not a valid comparison. Can you use FIRS daytime measurements only to compare to your bias-corrected daytime measurements? This issue needs to be addressed.

Figure 6, caption, line 5: Suggest changing 'The differences shown are' to 'The differences shown in the bottom panels are'

Figure 8, caption, line 2: 'shown on April' -> 'shown for April'

Figure 10: this figure also shows daytime data, right? This should be mentioned in the caption.

Figure 12: the dashed lines in row 2 and 3 are barely visible.

Figure 12, caption, line 5: 'has been use to' -> 'has been used to'

Baron et al. (2009) reference: All last names end with a 'k'. There's something wrong. Also the second 'Urban' should be 'Murtagh', right?

Kikuchi et al. reference, line 3: Is 'Susukik' correct? This should probably read 'Suzuki' ?

Snow et al. (2005) reference: 'Mcclintock' -> 'McClintock'

Interactive comment on Atmos. Chem. Phys. Discuss., 14, 22905, 2014.