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Interactive comment on "Global nighttime atomic oxygen abundances from resampled GOMOS hydroxyl airglow measurements in the mesopause region" *by* Qiuyu Chen et al.

Anonymous Referee #3

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The paper presents a new dataset of atomic oxygen measurements in the MLT region. Atomic oxygen is a key species for understanding this atmospheric region and its current measurements all show, probably associated to its variability, very large uncertainties. Therefore, a new measurement database is very important.

I have found the paper very clearly written, with an extensive background, and the methodology and measurements description very well covered and written. It is very comprehensive since it covers from the retrieval to its analysis and validation, including a comparison with several atomic oxygen datasets.

I therefore recommend the paper for publication in ACP. I am listing, though, some

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comments below which the authors might want to consider for improving the paper.

Probably my major comment is one that I have already given in a similar paper. The authors named the retrieved atomic oxygen as "GOMOS-OH" O, when it is actually additionally based on the external measurements of three key atmospheric quantities: O3, temperature and pressure (density). The authors clearly state this in the introduction and also present an error analysis of these parameters in the retrieved O but, in my opinion, it should also be mentioned in the abstract and in the conclusion. In this way the reader would have a more clear idea of the derived data.

Other comments, in order of appearance in the manuscript (not in order of importance) are listed below.

Title: Is really need the word "resampled"?

Page 1, l. 13. For the sake of clarity I would write " ... by the photolysis of molecular oxygen and of ozone..."

Page 5. Interval of wavelength used. The authors justify the limited spectral interval because of problems at longer wavelengths. They state errors of about 12% at these wavelengths. However, from the spectrum shown in Fig. 3, which looks very good at wavelengths of 935-955 nm, and given that 12% is not really large compared to the overall error of about 20%, I wonder if it would not have been useful to include the wider spectral interval. If not, could this be considered as an additional complexity (potential source of error) of retrieving O from the OH Meinel bands?

Page 6. Figure caption. Last two lines. Could the authors clarify (give some more details or even an equation) about how the measurement noise of the mean spectra (those used as measurements in the retrieval) were computed? In particular, the mention "integrating", over which quantity? "residual noise standard deviation" residual of what? "spectrum"? I believe they where averaging the spectra in the month/latitude bins, correct? I have no doubt the authors are doing correctly but it would be useful a

more detailed description for the readers. Also, please consider moving it to the body text.

Page 7, line 3. "total"? Do they want to say something additional to "the removal of OH(v=8) by O"?

Also, I do not understand why adjusting ONLY this rate for "adjusting the OH(v=8) populations to be consistent with laboratory measurements? Why not, for example, adjusting other rates as, e.g., that with O2? I might be wrong but this seems to me like as ad hoc adjustment with not much justification. Are these uncertainties included in the model error budget?

How this rate (OH(v=8) +O) compares to that used/derived by Sharma et al and Panka et al.? In general, could the authors comment on the similarities/differences of the rates affecting OH(8) with those used in other O retrievals from OH? In the conclusion section they mention the possibilities of the discrepancies of this O dataset with other databases possibly caused by differences in the collisional rates. I have no doubt of that but the discussion suggested would be very beneficial to support such conclusion.

The measurements from Oliva et al. are not laboratory measurements but ground-based nadir atmospheric observations, aren't they?

Page 7, line 15. "... the a priori information from the real atmospheric state.."? Please clarify: the "real" atmospheric O is used as a priori? This does not make sense. Please be more specific, which O is used as a priori? (I understand from the following text that a priori has a negligible effect but this should be clarified).

lines 16-17. Which kind (order) of Tikhonov regularization? "Noise is minimized...", which is then the vertical resolution of the retrieved O? This is clearly discussed below, but if you talk about the "noise" at this point you should also mention the vertical resolution.

Page 10, lines 6-7. At which altitude does the discussion in the paragraph above these

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lines refer to? At the O peak near 95 km? Note that the opposite behaviour is observed (larger values near the equator) in Fig. 8 for, e.g., moths 3, 4, 10 and 11, at an altitude near 90 km. The reader might be confused, as I was.

Which is the mean local time for each of the plots? Does it change with latitude?

Page 12, line 3. "... solar cycles", caption of Fig. 10, Table 2 and the whole discussion of the solar cycle. The study about the solar cycle(s) has been done assuming that the latest GOMOS measurements analysed, December of 2011, coincides with he maximum of solar cycle 24. However, this is not fully correct, see https://www.swpc.noaa.gov/products/solar-cycle-progression. This shows that the maximum can be placed somewhere between the 2nd half of 2012 or more likely near the end of 2014. Hence, I suggest that all the discussion, including the two suggested (soalrmax and soalrmin amplitudes) be revised accordingly. The data presented do not really cover a full solar cycle (see https://www.swpc.noaa.gov/news/solar-cycle-24-status-and-solar-cycle-25-upcoming-forecast).

Page 14, line 7. Although mentioned earlier it is very useful to give here the references to the O databases.

Lines 20-21. I would remove the last sentence: "The O ... shown in Fig. 13b". First, because it is not discussed here but later, and also because it additionally contains another O-retrieved profile which is discussed in a different paragraph.

I would exchange the order of Figs. 13 and 14.

Fig. 13 is very important as it compares the three O- retrievals. However, for the community, it is more interesting to provide a figure of a GLOBAL comparison of the already published SCIAMACHY OH(9-6) O dataset by Zhu and Kaufmann (2018) with he current dataset. E.g. similar to Fig. 14 but for SCIAMACHY OH(9–6) band instead of SCIAMACHY OH(8-4).

Page 14, line 29. Typo SCIAMACHY

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