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Atmospheric Measurement Techniques
Discussions

Interactive comment on "Retrieval of daytime mesospheric ozone using OSIRIS observation of $O_2(a^1\Delta_g)$ emission" *by* Anqi Li et al.

Anonymous Referee #2

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Dear Editor, Authors,

As my previous review was more a kind of formal/complete review rather than a quick review, it still prevails.

I have tried to access to the new/current verison but I couldn't. Also, yesterday was the deadline. Hence I am submitting again my previous review.

»» Previous Review ««

The manuscript does fall into the scope of AMT. It deals with a potential new data set of daytime mesospheric O3 and it is therefore very important. Eventually it can be sufficiently sound to be posted in its discussion forum but, in my opinion, not in its current form. I think it needs a substantial revision before it can be posted.

I would recommend to the authors the following actions:

The first part of the paper is well written, clear and well focused. This section, however, still requires some actions as:

1) Include a Table listing all error sources and their estimated values. It is true that in order to verify if the estimated systematic errors are plausible (for example that of the stray-light, particularly near 80 km when the atmospheric signal is very small but radiance for the bright region below might be important) one needs to perform a thorough validation. Hence, I propose to present that in the second part of the paper (see below).

2) To include a proper radiative transfer (i.e., do not assume the optically thin approach) for the 60-70 km region. Although the authors cite Degenstein (1999) (a Thesis work) as a support for the validity of this approach in that region, Mlynczak et al. (2007) state that "Below 70 km absorption of the O2(1D) emission by O2 itself begins to become important and the weak-line retrieval approach becomes invalid." I suggest that a) include full RT below 70 km, b) limit the data to 70 km, or c) estimate (quantitatively) the errors of that approach in the 60-70 km region.

3) If I have understood correctly, the 1.27 μm channel has not been calibrated in flight. It is just assumed that it behaves as other inflight-calibrated channels. I have not seen any error associated with this (lack of) calibration in the manuscript.

2nd part. Section 3.

This section is a mixture of a kind of a soft (descriptive, not rigorous) validation exercise together with some partial description of the behaviour of O3, incomplete from my point of view. In my view, none of the two aspects are shown with sufficiently sound scientific treatment.

For example, the current validation is only descriptive, a kind of hand-waving comparison, side by side figures, e.g., not showing differences, no co-location criteria has been used (or at least not mentioned). Sentences like "The differences between IRI, ACE-FTS and MIPAS in Fig.12 may be explained by their sampling at different SZA and the underlying assumptions in their retrieval techniques." are very vague and little informative. If sampling is a cause, the differences should be looked at by restricting it. etc.

About the second aspect, the study is mainly descriptive and based on partial datasets and considering the O3 number density instead of the O3 vmr. Sentences like: "Thus such a monthly mean profile should be treated with caution since it may not necessarily well represent the spatial and temporal distribution of daytime ozone." again adds little information. If it does not represent the distribution, why then show it? The study on the "Monthly mean ozone" is based on 1 month of one year and of O3 density. I would suggest the authors to refer to other similar studies (e.g. Lopez-Puertas et al. 2018).

My recommendation for this section 3, would be to focus on a thorough, rigorous validation (following the standard guidelines, see some more comments below) and leave aside from this paper the kind of characterisation of O3 features. Maybe the authors would like to consider future papers as, e.g. the overall OSIRIS O3 data sets, or tackle comprehensive works, including or not other datasets, as seasonal/latitudinal variations, or local time and annual variations, etc.

Recommendations on validation:

1) It should be based on collocated data and, whenever possible, the same physical conditions, e.g., based on a coincidence criteria of time, spatial (latitude, longitude) and local time.

2) Compare the appropriate instruments. That is, there is no altitude overlap of IRI wrt SO. Hence I see no reason for including SO data.

- About MIPAS data, I suggest the author include a more appropriate reference, e.g., Lopez-Puertas et al. (2018). MIPAS middle atmosphere data ranges from ∼20 km (not

5 km, Table 1) up to ∼100 km. BTW, I believe the authors have used only DAYTIME MIPAS data (not stated explicitly in the manuscript). Mention which pressure/temperature is used (MSIS?, MIPAS?) to calculate O3 density.

- I would be inclined to not include ACE data. O3 shows a large diurnal variation in the mesosphere and ACE is always measuring at the terminator. Hence it is difficult to distinguish systematic differences inherent to the instruments from those due to the solar illumination. BTW, the authors should state early in the paper that the 1.27 mm emission has a radiative lifetime of approximately 75 min and does not provide a representative measure of ozone until 2–3 h after sunrise (Mlynczak et al., 2013). Has this fact been taken into account in the current comparison? This fact automatically should avoid to compare to ACE sunrise occultations.

- I am really missing a validation against SABER. In particular the O3 derived from the O2 1.27 μm channel. This is an instrument that uses the same technique and would therefore be very valuable.

3) Quantify the differences (of the co-located data) for the different seasons/latitudes/altitudes. That is, as Fig. 11, but including more altitudes/seasons and enough years of retrievals to make the statistic significant.

About the 2nd part, a description of the O3 characteristics should be presented in a different paper and I would recommend the authors to please cite other previous recent works about this.

Other minor points:

In general several figures are very small, particularly those with several panels, e.g. Fig. 3, 5, 9 and 11

Fig. 3 Could you show the solar local time?

a typo: earth -> Earth

Refs. Lopez-Puertas M, García-Comas M, Funke B, et al. MIPAS observations of ozone in the middle atmosphere. Atmos Meas Tech. 2018;11(4):2187-2212. doi:10.5194/amt-11-2187-2018.

Mlynczak MG, Hunt LA, Mast JC, et al. Atomic oxygen in the mesosphere and lower thermosphere derived from SABER: Algorithm theoretical basis and measurement uncertainty. Journal of Geophysical Research. 2013;118(11):5724-5735.

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