Atmospheric Measurement Techniques Discussions

Atmos. Meas. Tech. Discuss., referee comment RC2
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Comment on amt-2021-178

Anonymous Referee #1

Referee comment on "Retrieval algorithm for OClO from TROPOMI (TROPOspheric Monitoring Instrument) by differential optical absorption spectroscopy" by Jānis Puķīte et al., Atmos. Meas. Tech. Discuss., https://doi.org/10.5194/amt-2021-178-RC2, 2021

General Comments:

In this work, a new retrieval algorithm of the slant column densities (SCDs) of OClO is proposed. This algorithm, aimed to be applied to TROPOMI DOAS measurements, takes into account different spectral effects not considered in previous retrievals. A corresponding error analysis of some retrieval settings has been performed. The authors also present a comparison between OClO SCDs, obtained by TROPOMI trough the new algorithm, and from ground-based zenith DOAS measurements at Kiruna and Neumayer stations. The results show a very good agreement with these instruments (especially at Kiruna). The SCDs of OClO obtained in this work have been also compared to preliminary S5p+I OClO products during different periods of the year, showing similar SCD evolution but presenting an offset between both datasets.

All the manuscript (text and figures) is clearly presented. The new concepts and settings introduced in this new algorithm are exhaustively explained (appendixes), as well as the corresponding error analysis and sensitivity studies. I think that the results exposed in this work will be useful for the treatment and analysis of the OClO SCDs obtained by TROPOMI. Thus, I think the the paper should be publish in AMT. However, I think that some questions should be better clarified.

My main concern is that the authors of this work claim that the new algorithm improves the retrieval results, but looking at the comparison between SP5+I and the results of this work, can that be really stated? The new algorithm takes into account several fine effects that, in principle, should improve the OClO SCDs retrieval and decrease the corresponding errors. But, in practise, how can we say that the results of these work are better than those of SP5+I? For high SCDs, results are very similar, and for low SCDs the offset between both datasets cannot be explained. It is true that, as the authors explained, OClO observation is not expected when the temperatures are still warm, as it is observed in the results of this study (Figure 10). Contrarily, SP5+I results show a background level of OClO. But, it can be affirmed that the results of this work are better than those provide by the SP5+I? Did the authors compared the results of both algorithms with independent measurements (as those of Kiruna or Neumayer)? Please, explain better.

Specific Comments:

I would like also to clarify some questions:

- Figure 1: Some days of different periods of the year for both, NH and SH, have been presented. Are those days representative of the corresponding periods? If it is the case, do you think that the bias introduced by SZA at each season could be, at least partially, corrected somehow?

- Page 11, line 225: why for clear sky cases the signal to noise would be lower?

Technical Corrections:

- Page 4, line 102: "Earthshine" instead "Earth-shine", for coherence.
- Page 6, lines 123-124: A wavelength l=379 nm is selected for evaluation. Please, explain briefly why.
- Legend of Figure 1: ".. indicated in the legend on the right.", instead "left plot".
- Page 7, line 161: "In an ideal case,.."
- Page 17, line 316: "(Sect. ??)"
- Page 25, line 476: "depends on both", instead "depends both on".
- Page 25, line 489: "Within the chosen OClO fit window,..".
- Page 25, line 490: "cross section" instead "cross-section", for coherence.