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Atmospheric Measurement Techniques Discussions

Interactive comment on "Geophysical validation and long-term consistency between GOME-2/MetOp-A total ozone column and measurements from the sensors GOME/ERS-2, SCIAMACHY/ENVISAT and OMI/Aura" *by* M. E. Koukouli et al.

Anonymous Referee #1

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General

This paper focuses on a global-scale comparison between total ozone column (TOC) derived from GOME-2/MetOp-A and TOC data inferred from other three satellite sensor, using as reference Dobson/Brewer TOC measurements. The authors show the satellite-ground-based differences as a function of several geophysical parameters (latitude, SZA, and cloud parameters). Although this manuscript presents some interesting results, my major concern is that it contains only a vague description and very little in-depth discussion. Thus, some major revisions are needed before final publication in AMT.

Major comments

1. Section 2 is called “Data sets and methodology”, but I could not find any methodology. In my opinion, it is necessary to include a subsection explaining how was performed the comparison. For instance, were only direct-sun Dobson/Brewer measurements used in the comparison?, do the authors work with daily TOC ground-based averages or averages around satellite overpass?. According to table 1, the satellite footprint ground pixel size of the instruments are very different, thus, which was the spatial collocation criteria used in the comparison exercise?. All this questions should be answered.

2. In subsection 3.1.2, the authors must explain the possible causes of the clear seasonality shown in the evolution of the satellite-Dobson differences for all satellite algorithms except OMI_TOMS (Fig. 2). Is this behaviour related to the “obvious SZA dependency” seen in Fig. 4 (second row left)?. In my opinion, the authors should relate these two plots.

3. Brewer and Dobson instruments are calibrated using the Bass and Paur ozone cross-sections at a fixed temperature while the satellites use BDM. This must be commented in the text. Do the authors think that temperature dependence of cross sections could partially explain some of the differences reported in the manuscript?.

4. This reviewer would like know why for the Brewer comparisons (Fg.4 second row right) the plot is “a lot more homogeneous” than for Dobson comparison.

5. The analysis of the dependence on CTP and cloud fraction does not exit. Only a brief description of the plots is given (page 3033). A discussion of these results is

need.

6. Previous validation papers are only briefly commented in the text. Although they qualitatively agree with the present results, they are not really compared. The authors should add to section 3, a new subsection with a detailed comparison between their results and the results of previous studies.

Specific comments

- Page 3021. Line 20. GDP must be defined here.

- Page 3022. I think that lines 12-13 and 24-26 refer to the same issue. Please rewrite lines 12-13 using the information given in lines 24-26.

- Page 3025. Lines 2-4. A brief explanation about this decreasing trend should be given here.

- Page 3026. Lines 25-26. This information should be moved to Page 3023, line 15.

- Page 3027. Lines 15-16. The Brewer retrieval algorithm derives the total ozone column from four wavelengths, not five. An additional wavelength is used to retrieve SO2.

Interactive comment on Atmos. Meas. Tech. Discuss., 5, 3019, 2012.