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Interactive comment

# *Interactive comment on* "Measurement report: Regional trends of stratospheric ozone evaluated using the MErged GRIdded Dataset of Ozone Profiles (MEGRIDOP)" *by* Viktoria F. Sofieva et al.

**Anonymous Referee #2**

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The manuscript presents a new dataset (MEGRIDOP) of global stratospheric ozone vertical distribution based on the merging of various limb and occultation satellite measurements (GOMOS, SCIAMACHY, MIPAS, OSIRIS, OMPS, MLS). This dataset and its merging methodology are very similar to those SAGE–CCI–OMPS presented in Sofieva et al. (2017), except that while MLS data are included, SAGE II and ACE-FTS observations are not considered. In contrast to SAGE-CCI-OMPS, the new dataset is resolved in longitude with horizontal bins of 10°x20° in latitude and longitude. The MEGRIDOP dataset is then used for evaluating trends of stratospheric ozone as a function of longitude, latitude and altitude. The paper is well documented and provides an important contribution to the study of ozone trends in the stratosphere. It is thus suit-



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able for publication in ACP, provided that following comments and recommendations are considered.

Major comments

1. The description of the merging methodology is based on the Sofieva et al. (2017) study published in ACP, itself using the results of Sofieva et al (2014) in AMT. Since the number of individual satellite datasets differs from that of SAGE-CCI-OMPS it would be interesting to see how this change in the use of satellite observations impacts the resulted latitudinal ozone fields. Such comparison could be presented in an appendix.

2. Discussion of uncertainty of the gridded monthly means from individual instruments needs improvement. It is heavily based on previous studies by the author team. Its presentation in this article is not completely self-explanatory, even if some of the equations used in the previous articles are provided here. As an example, in section 3.1, authors mention the characterization of the non-uniformity of the sampling pattern by the inhomogeneity measure H, which is a combination of asymmetry A and entropy E. But they do not precise how H is considered in the uncertainty of the averaged data and why the main contribution to H is Htime. Also, how the standard error of the mean compares with the rms of each measurement profile uncertainty? For a better understanding of uncertainty of the gridded monthly means from individual instruments, it would be useful to provide maps of H for some of the merged instruments (contrasting e.g. occultation and limb sounding instruments).

3. Evaluation of deseasonalized anomalies: for all instruments the seasonal cycle is estimated using the 2005-2011, while the 2012-2018 period is used for OMPS. This is understandable because the ENVISAT based instruments stopped in 2012. But since OMPS anomalies are adjusted to the median of anomalies from other instruments and that can impact of ozone trends, more precision of this offset as a function of altitude and latitude should be given.

4. OSIRIS and SCIAMACHY dominate the start of the record, while MLS and adjusted

OMPS dominate end of the record. How this shift in dominating instruments can impact trend as a function of altitude and latitude/longitude? As an example, Fig. 5 shows an overestimation of ozone anomalies by SCIAMACHY compared to OSIRIS and MLS. Discussion on this issue is lacking in the manuscript.

5. Section 3.3: Some discussion on possible correlation between the datasets should be provided, especially since OMPS anomalies are adjusted using the other measurements anomalies. This could affect the error bars. In equation 5, how is evaluated ïĄşïĄĎ,imed when there is an even number of measurements? Also, from the example in appendix of Sofieva et al., 2017, the final uncertainty can vary from one bin to the next, depending on the availability of data since the median is used, in particular after the stop of ENVISAT based measurements. This is illustrated in Fig. 7 that shows a decreased of uncertainty in the lower stratosphere at the end of the record in the bottom panel of the figure. How does this affect ozone trends? Even if uncertainties are not taken into account in the trend model, larger variability of data in the lower stratosphere linked to OSIRIS should affect trend results in this region. A discussion of the validity of trend results below 20 km should thus be included in the article.

6. An independent validation of the MEGRIDOP reconstructed ozone dataset (section 4) based on e.g. ground-based or other satellite instruments is lacking. Validation using ozone sondes (up to 25 – 30km) as well as SAGE II (up to 2004) or more recent ISS/SAGE III data would be an asset for the study.

7. The latest compilation of stratospheric ozone trends from Petropavlovkikh et al. (2019) emphasises the lack of significant ozone trends in the lower stratosphere, pointing to a potential discrepancy with results from CCI models, although not significant at 2 sigma level. Other publications have also addressed ozone trends in the lower stratosphere (Ball et al., 2018; 2019; Wargan et al., 2018). Considering the importance of this issue, a dedicated paragraph addressing ozone trends in the lower stratosphere should be added. Such a discussion could include quantification of ozone trends in the lower stratosphere in the SH high latitudes, in order to eventually confirm ozone

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recovery in this region (e.g. Salomon et al., 2016).

8. Figure 10 and 12 show different trend results from the MEGRIDOP dataset at 35 km, with more pronounced positive ozone trends in the tropics in the period 2004 – 2018 compared to the period 2003 – 2018. Such a sensitivity to the starting year is interesting. Can the authors comment on that? Also on the non-significant decrease of ozone over Siberia at 20, 25 and 35 km. The asymmetry of trends between the Northern and Southern hemispheres at 20 and 25 km deserves also some discussion.

References

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Solomon, S., Ivy, D. J., Kinnison, D., Mills, M. J., Neely, R. R. and Schmidt, A.: Emergence of healing in the Antarctic ozone layer, Science (80)., 353(6296), 269–274, doi:10.1126/science.aae0061, 2016.

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Specific comments

L75: It is not clear why the authors use MLS temperatures for conversion to ozone number density but ERA-Interim data for altitude-pressure conversion. Did the author check sensitivity of the results using ERA-Interim data for number density? ERA-Interim data stop in August 2019. They are now replaced by ERA-5. Is there a prospect to use ERA-5 for extending the MEGRIDOP dataset to 2020 and beyond?

L103: Fig 1 as well as Fig. 2 and all similar color figures lack axis titles.

L137: The use of deseasonalized anomalies enables the removing of biases if sampling patterns do not change over time. Is it true? Can the authors comment on this?

L148: Fig. 5 lacks the median curve.

L157: In equation 3, the term ïĄši is missing (using error propagation). The term “relative” should be added to uncertainty.

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