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Interactive comment on "Overview and update of the SPARC Data Initiative: Comparison of stratospheric composition measurements from satellite limb sounders" *by* Michaela I. Hegglin et al.

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This paper is an update to an ambitious effort to assess currently available satellite limb measurements of stratospheric trace gases. The authors are to be applauded for their comprehensive assessment of a number of different data sets produced by different institutions and spanning multiple decades. It is encouraging to see that for several of the species, the use of updated retrievals results in better agreement than in the earlier version of the SPARC DI data set. This data set will no doubt be useful to

the observational and modeling communities for studies of stratospheric composition. I have only a few minor comments and recommendations before it is accepted to ESSD.

We thank the reviewer, Sean Davis, for his assessment of our manuscript and his valuable comments. Please find below our answers in blue.

Page 3, lines 15-20 – This data also contributed to several of the S-RIP chapters/papers, and I think that is worth mentioning here somewhere.

We now have included a reference to S-RIP and cited the relevant publication (Davis et al. ACP 2017; Fujiwara et al., ACP 2017).

Page 9, Lines 1-3 – As I understand it there are multiple MIPAS retrievals from different groups. Could the authors please provide some justification for why they choose the IMK retrieval, and/or provide any information and references concerning known differences between the retrievals?

Yes, the reviewer is correct, there are several MIPAS retrievals available, however, they were not contributed to the SPARC Data Initiative. We added the following sentence to highlight this and to provide a reference instead:

"Several other MIPAS retrieval products are available (see Lossow et al., 2019), however, were not contributed to the SPARC Data Initiative in the required climatological format. Note, the IMK-processor also provides more species than these other processors."

Page 5, line 22 – the reference to appendix table A4 seems quite out of order. Additionally, I don't understand the distinction between the figures and tables in the "appendix" versus the main text. Content-wise, it seems like the material in the appendix belongs in the paper itself and is not really an appendix.

Thank you for catching that the tables were not numbered in chronological order! We have now corrected the problem and moved all appendix tables and figures into the main manuscript.

Page 10, section 2.14 – It would be helpful if the authors mentioned the end date and reason for the end of HIRDLS data.

We added the following information: *"HIRDLS stopped acquiring data on 17 March 2008 due to a chopper failure."*

Page 11, section 2.16 – It looks like the authors are using two different versions of OMPS (based on table A5). Which is the primary one they are considering? Reference to/discussion of the version they are using here would be helpful. Also, I believe there is yet another OMPS-LP retrieval that is not included here (Kramarova et al., 2014). As with the MIPAS discussion it would be helpful to have some insight into the choices the authors have made and justifications for excluding certain products, and what the known major differences are between the retrievals.

Kramarova, N. A., Nash, E. R., Newman, P. A., Bhartia, P. K., McPeters, R. D., Rault, D. F., Seftor, C. J., Xu, P. Q. and Labow, G. J.: Measuring the Antarctic ozone hole with the new Ozone Mapping and Profiler Suite (OMPS), Atmospheric Chemistry and Physics, 14(5), 2353–2361, doi:10.5194/acp-14-2353-2014, 2014.

Indeed, we use OMPS data based on two different retrieval algorithms. We added the following information: *"It should be noted that the OMPS-LP ozone datasets used in the SPARC Data Initiative are based on two different retrieval algorithms, IUP-OMPS (Arosio et al., 2018) and USask-OMPS (Zawada et al., 2018). The main difference between these two products is that the USask is retrieved using a 2D tomographic algorithm and the IUP uses a standard 1D algorithm. Furthermore, the spectral information and associated tangent height ranges are used differently. NASA also produces a stratospheric ozone product from OMPS-LP (Rault and Loughman, 2013) which is not included in the SPARC Data Initiative."*

Rault, D. F., and R. P. Loughman (2013), The OMPS Limb Profiler Environmental Data Record Algorithm Theoretical Basis Document and expected performance, IEEE Trans. Geosci. Rem. Sens., 51, 2505-2527.

Page 11, lines 26-28 – I think the term "climatology" is a confusing term to use to describe this data set. As the authors acknowledge here, a climatology typically refers to some long term mean state. But in this paper, "climatology" is being used to describe a time series. The authors also use the term "climatology" (e.g., "climatological approach") as a stand in for "gridded data set" when contrasting their approach to profile-to-profile coincident comparisons (e.g., sentence starting line 28). I also find this terminology confusing. The data set the authors have produced is a gridded time series data set, and I think it is more accurate to describe it as such.

We agree that the term "climatology" for the monthly zonal mean timeseries can be confusing. We now change this notation throughout the manuscript to "gridded datasets", "mean fields" or "timeseries of monthly zonal mean fields". However, we kept the "climatological validation approach" terminology in order to highlight its difference to the coincident validation approach, since this evaluation approach is based on comparing multi-annual means of the zonal monthly mean fields.

Page 11, starting line 28 – It seems as though one of the main advantages of the approach used here (comparing gridded data sets) is that all data from each sensor are used in the comparison, as opposed to profile-profile comparisons where some profiles simply don't meet the chosen coincidence criteria. I believe this is the reduction in random error the authors are referring to here. However, this benefit must be weighed against the sampling bias (e.g., as addressed in Toohey et al 2013) that is introduced when one grids data. It's not totally obvious how these two factors compete, and some acknowledgement of this balance would be appreciated.

Some more discussion of the influence of the sampling bias has been added in the new Section 2.3.1.

Page 12, line 16 – What do the authors mean by hybrid log-linear here? Do you mean interpolating the log VMR linearly in altitude, or interpolating the VMR linearly in log pressure? I'm guessing the latter, but please clarify.

"Hybrid log-linear" refers to interpolating VMR linearly in log pressure, as correctly guessed by the reviewer. We now added this explanation for clarification.

Page 12 lines 16-20 – It appears as though the authors are using the most convenient method for converting to VMR on a pressure grid for each individual data set. I don't mean to belittle this approach because it would be a rather Herculean task to use a common data source for all the different instruments. And even then some of the retrievals may use p/T in their retrieval "upstream" of what is available to the public. Nevertheless, I think it is important to recognize that this grid conversion using different ancillary data as a possible source of uncertainty. I am not aware of any work that has attempted to quantify this source of uncertainty, but any additional discussion or references related to this issue would be very helpful.

This is a valid comment and we have added the following text to the manuscript:

It should be noted, that using different ancillary data for the grid- and unit-conversions will introduce an additional source of uncertainty, which has not been quantified here. Any known problems in the ancillary temperature/pressure data that were used to convert measured species from their native to VMR/pressure grids have been fixed by an updated retrieval algorithm or minimized with empirical corrections. For example, problems in the older SAGE II (v6.2) temperature/pressure auxiliary files, mainly in the tropics above 2 hPa, were empirically corrected (Froidevaux et al., 2015) before being incorporated in the original SPARC Data Initiative (see SPARC, 2017). The anomalous temperature problem in SAGE II (v6.2) has been fixed in the latest V7 retrieval, which is used in the updated SPARC Data Initiative dataset and this manuscript. Both SAGE III/ISS (v5.1) and SAGE II (v7) data were also updated to remove/minimize the effects of altitude registration errors in the auxiliary temperature profiles (Wang et al., 2020).

Page 14, paragraph line 17 – 22 – This paragraph doesn't make any sense and should probably be removed. It is addressing some evaluation that is not shown in the paper, and doesn't really even explain what the result is from this evaluation.

We now have removed this paragraph and rewritten the previous paragraph accordingly.

Page 15, line 25 – spectroscopical -> spectroscopic

Corrected, thank you.

Page 15, line 30 – considerable -> considerably

Corrected.

Page 16, line 22 – The Wang et al paper is now published

Reference is now updated.

Page 16, lines 22-24 – The altitude registration problem is easily corrected, as outlined in the appendix of Wang et al 2020. The authors should implement this correction.

We have now updated both SAGE III/ISS (V5.1) and SAGE II (v7.0) data versions to address the altitude registration problem in the auxiliary temperature/pressure data.

Page 17, line 31 – "also slightly" -> "also has slightly"

Corrected.

Page 21, line 25 – "mechanism" -> "mechanisms"

Corrected.

Page 22, line 5 – I think you mean "time" here instead of "date"

Deleted.

Page 24, line 19 – this paragraph ends abruptly. Can you say something about how this compares to SPARC 2017, as is done for the other species?

We added the following text:*'In comparison to earlier evaluations (SPARC, 2017), the updated nitrogen data sets show a slightly improved agreement. In particular the*

scaled ACE-FTS data sets agree better with the other time series in terms of absolute bias and seasonal cycle.'

Page 23, line 23-25 – This is a run on sentence.

We changed this sentence as follows:

In general, we expect increasing NOy values during the dynamically quiescent spring and summer, and this is observed by ACE-FTS and MIPAS. In the NH, the NOy maximum is observed in boreal autumn by all three instruments. In the SH spring, Odin shows a secondary maximum that is less pronounced than in the NH, but this provides for a better agreement with the other two datasets. For ACE-FTS, the too low NOx values in the SH and NH boreal winter cancel out with the too high HNO3 values, resulting in an overall good NOy agreement with MIPAS.

Table A5 – as previously mentioned, Wang et al. paper has been published now.

Corrected.

Table A6 – should cite Davis et al. for the SAGE III/ISS water vapor

Davis, S. M., Damadeo, R., Flittner, D., Rosenlof, K. H., Park, M., Randel, W. J., et al. (2020). Validation of SAGE III/ISS solar water vapor data with correlative satellite and balloon-borne measurements. Journal of Geophysical Research: Atmospheres, 125, e2020JD033803. https://doi.org/10.1029/2020JD033803

Thank you, added.

Data versions questions:

In general, it is preferable to use the newest data set from each satellite. There is a new Aura MLS version 5 data set, which I assume will become the widely adopted version of the data to use. Could this be included in the data set? Similarly, there is a new ACE-FTS version (4.1) that is the recommended version. Also, which version of MAESTRO data is being used here? It says "31" in the table, which I assume refers to

v3.1. But there are several sub-versions of 3.1 (eg, 3.11, 3.12, : : :). The latest version is 3.13 – is that what is being used?

We agree with the reviewer that it would be preferable to use the latest data versions. However, we rely on the expert advice of the instrument PIs and use the data versions that they are most comfortable sharing. This is why we use version 3.6 for ACE-FTS and version 31 for ACE-MAESTRO (note this is the official versioning number for H2O, which is different from ozone). These data versions have generally undergone considerable validation efforts. Note that Aura-MLS version 5, while available to the public, is just finishing its reprocessing and has not yet been re-evaluated by the MLS team, or rushed to processing into the format of the SPARC Data Initiative. At some point, the data versions have to be "frozen", and a considerable amount of work is needed to redo all the comparisons shown here; moreover, most comparisons will not be affected significantly.

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