

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.

Sean Davis (Referee)

sean.m.davis@noaa.gov

Received and published: 23 December 2020

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

C1

have only a few minor comments and recommendations before it is accepted to ESSD.

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.

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?

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.

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

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., Sefstor, 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.

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

C2

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.

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.

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.

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.

Page 14, paragraph line 17 – 22 – This paragraph doesn’t make any sense and should

C3

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.

Page 15, line 25 – spectroscopical -> spectroscopic

Page 15, line 30 – considerable -> considerably

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

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.

Page 17, line 31 – “also slightly” -> “also has slightly”

Page 21, line 25 – “mechanism” -> “mechanisms”

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

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?

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

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

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 balloonborne measurements. *Journal of Geophysical Research: Atmospheres*, 125, e2020JD033803. <https://doi.org/10.1029/2020JD033803>

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

C4

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?

Interactive comment on Earth Syst. Sci. Data Discuss., <https://doi.org/10.5194/essd-2020-342>, 2020.