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Atmospheric Chemistry and Physics Discussions

Interactive comment on "NDACC UV-visible total ozone measurements: improved retrieval and comparison with correlative satellite and ground-based observations" *by* F. Hendrick et al.

Anonymous Referee #2

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Review of the paper "NDACC UV-visible total ozone measurements: Improved retrieval and comparison with correlative satellite and ground-based observations" by F. Hendrick et al.

In this paper, F. Hendrick and colleagues report on a new set of recommendations for the retrieval of ozone total columns from NDACC zenith-sky UV/vis observations. The recommendations are described, the error budget is discussed and the new settings are applied to a large set of SAOZ observations which is then compared in detail to satellite ozone measurements and also some Brewer and Dobson observations. The paper is well written, the analysis is thorough in many aspects and the results are

interesting for people working in the stratospheric ozone field. I therefore recommend publication of this manuscript.

However, I also do have some concerns about this paper as discussed below. The authors need to address these points in detail and change the manuscript accordingly before it can be accepted for publication.

1) In my opinion, this paper would probably be better suited for AMT(D) as it reports on retrieval techniques and validation but does not really contain any new information on atmospheric composition or atmospheric processes.

2) In the title and in several places in the paper, reference is made to the NDACC UV-visible observations. However, the analysis and comparisons shown are limited to SAOZ instruments, which are an important part of the NDACC UV-visible network but not identical to it. In particular, the comparisons between V2 and V1 of the SAOZ analysis should not be equated with a comparison of the old and the new NDACC data analysis. I'd recommend making this difference more clear in the text and also in the title of the paper.

3) The new NDACC recommendations have two parts – one for the retrieval of the ozone slant columns, the second for the airmass factors. While the latter part is discussed in detail, the first part is only briefly mentioned and the discussion, in particular with respect to uncertainties is much less convincing.

First of all, I think it is absolutely necessary to indicate what the V1 retrieval settings were, and how they relate to the settings used in previous papers applying NDACC values for satellite validation. The changes from V1 to V2 discussed in the text are interpreted as AMF changes only – does this imply that the other settings remained unchanged? And if other settings have changed as well, wouldn't it make sense to investigate what the relative importance of these changes (cross-sections, wavelength-window, Ring parametrisation) are?

Second, the uncertainty in the slant column is estimated by assessing the uncertainty in O3 cross-section and the variance in results of three different fitting codes on the same spectra. This is in my opinion not the full story – I would hope that least squares retrievals using the same settings on the same data should provide the same results within some limits, but this does not tell me the uncertainty in the slant column. There is uncertainty introduced from the measurements (noise, slit-function, straylight, temperature drift, etc.) and also from the analysis (choice of fitting window, cross-sections, polynomial, etc.). Together, this will be significantly more than the 1% cited in the paper, and I'm sure the authors will agree that if I put two different NDACC UV-vis instruments side by side and then compare the results, they will not agree within 1 %.

I therefore think that the error discussion for the slant columns needs to be revised and extended and the estimates need to be more realistic. Also, I don't think Fig. 2 is adding any information, in particular as nothing is said on what the different scenarios were for which results are shown.

4) The changes in the AMFs proposed in this manuscript are relatively large and show a significant seasonality. The arguments given for the use of a seasonal and latitudinal climatology of ozone profiles are convincing and I believe the new AMFs are more realistic than the constant values used before. However, these problems have been noted and discussed before e.g. in work by Lambert et al., and I'm surprised that these previous results are not mentioned more in the current manuscript.

I'm also surprised by the large change for Jungfraujoch (nearly 10% or 30 DU in winter) – is that because of the altitude of the station, and why has it not be noticed and corrected before as there is plenty of other ozone measurements available at this site?

Another surprising result are the AMFs for Bauru – I think there is no good reason for the large scatter in AMF values observed at this tropical site and would see this as indication for a problem in the LUT used.

5) After the initial comparison of SAOZ and satellite retrieved O3 columns, the authors

proceed to discuss and correct for a temperature dependence in the difference between satellite and SAOZ results. The final result shows less seasonality and better overall agreement between the two datasets. While I'm convinced that the analysis shows a valid point (the not fully corrected fortemperature dependence of the UV absorption of ozone used in the satellite data), I'm a bit worried by this approach for several reasons:

a) The authors take the variations between the seasonalities in the differences to different satellite retrievals as confirmation for the absence of a seasonal bias in the SAOZ data. I don't think this is a valid conclusion – in a comparison of two (or more) data sets, one always has to accept the possibility that all of them are off.

b) In the analysis, the difference between satellite and SAOZ is correlated with temperature, and then a correction is applied. What would have happened, if the authors had applied the same approach to SAOZ V1 data? I assume that the final results would have looked very similar, only that the correction terms would have been larger. I do believe that SAOZ V2 is better than V1 but the authors seem to take this analysis as proof that there is no seasonal bias in the SAOZ data, and I don't think this conclusion can be drawn from the data.

c) I'm concerned by the overall approach to see good consistency between SAOZ and satellite data after T-correction as validation of the new retrieval settings. While this is certainly a nice result, the SAOZ data are often used as validation data set for the satellite retrievals, and therefore should not themselves be "validated" by comparison to satellite data. The comparison to Dobson and Brewer is much more relevant in this context, as would have been comparison to sonde data.

I recommend that this part of the paper is formulated a bit more cautious making clear which data set is validating which and which statements are firm conclusions and which are just plausible.

6) The impact of tropospheric ozone needs more attention. Tropospheric ozone has several possible effects – it can enhance the observed signal, in particular in the pres-

ence of clouds, fog or snow; it can affect the comparison of satellite and ground-based observations as they have different sensitivities to the troposphere and it can change the real AMF if the true tropospheric column is different from the climatological one. In fact, the authors mention the ghost column added to the satellite observations in the presence of clouds, but at twilight, the climatological tropospheric ozone used in the AMF calculations has a quite similar role in the ground-based observations.

As a side note, it is also worthwhile to consider the risk of a circular argument when the same ozone climatology is used in the ground-based observations and in the OMI observations which are then used to derive the tropospheric column by subtracting the MLS columns. Consistency between measurements using the same assumptions does not necessarily imply that they are correct. In the case shown in the paper, the excellent agreement with ozone sondes at OHP is of course independent confirmation for the tropospheric ozone columns derived.

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