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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Multi sensor reanalysis of total ozone" *by* R. J. van der A et al.

Anonymous Referee #1

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The paper describes an ozone re-analysis for the period 1978-2008. The focus of the paper is put on the construction of a coherent satellite observation data set from 14 different retrievals products. This achieved by comparing the satellite data against ground based observations of the WOUDC network. A statistical model has been build to correct the individual satellite retrievals based on predictors such as viewing and solar zenith angle, time (for trend correction) and effective temperature. The reduction in RMS by this correction seems to be small. The corrected data set was assimilated by the TM5-CTM using a Kalman Filter technique. The assimilation system performed well as shown in an OmA and OmF analysis

The paper is technical and the thorough description of the satellite retrieval correction process and the findings about the properties of the satellite data are the main strength of the paper. The time series showing the biases of the satellite retrievals against observations for De Bilt (Figure 2) is an interesting piece of information which could be

made more general by showing the bias time series also for other areas.

A shortcoming of the paper is that the examination of the MSR re-analysis data set is not sufficient in its present form. The paper does not include an evaluation of the MSR data set with independent data. The inter-annual variability and trends of the data sets, arguably the motivation for the whole effort, are hardly discussed, nor is the data set compared with alternative long-term ozone analyses. Only a few figures - although as such quite interesting as the one showing the Antarctic ozone deficit - are included but not sufficiently discussed.

It would be good to get a better balance between the description of the constructions of the MSR data set and its scientific value. For instance section 2.2 and 2.3 could be made much shorter because they contain too much details which could be omitted or referred to in the literature.

The conclusion section remains a bit vague. It should contain more about the results (satellite biases, correction model , assimilation performance) and should possibly be extended to the evaluation, trends and variability in the MSR. The more general statements of the potential usefulness of the data set would be better omitted or placed in the introduction.

Finally, I would like to encourage the Authors to draw a more optimistic conclusion as "As the true amount of ozone in the atmosphere is not known, it is not possible to draw conclusions from this work about the quality of an individual dataset." I think the paper has shown that with a well chosen reference data set one can learn more about the properties of the individual data sets.

More specific comments:

The data set is called a multi-sensor-reanalysis although it includes not just different sensors but also different retrievals from the same sensor. What was the motivation for the choice of the retrievals and how are possible correlations in the retrievals from the

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same sensor treated in the assimilation scheme?

How does the MSR data set differ from other long-term data sets such as CATO (Brunner et al. 2006), ERA40 (Dethof and Holm, 2004) or, more recently, Kiesewetter et al, 2010(JGR115, D10307) in terms of bias correction and satellite data usage. What is the novelty aspect of MSR?

The effect of the correction is shown in a reduction of RMS in Table 2 and 3. It would be interesting to get a picture (similar to Figure 1 or 2) of the corrected time error time series. Could the seasonal variation of the differences be corrected?

The reduction in RMS (RMS1 vs. RMS2 or RMS3 vs. RMS4) by the correction scheme seems to be very small. It is mostly smaller than 5 %. Does this justify the whole effort of the correction scheme? Would it be a valid conclusion that the chosen correction scheme did not significantly reduce the RMS of the differences? Could the RMS reduction be bigger with a different approach?

The use of the offset is unclear. Is this the actual total bias between the satellite retrieval and the ground-based observation or a site specific value? It would be worthwhile to more information in the paper. Does the offset contribute to RMS? Why is RMS3 (Table3) higher than RMS1 (table 2). I would assume it should be the same RMS before the correction.

Given the apparently small impact of the correction, has it been tested what would happen if the un-corrected retrievals were presented to the assimilation system. Small biases might be tolerated by the assimilation apparatus and the analysis would perhaps not differ much from MSR. A check with independent data could help to identify which of the analyses would be better.

As the bias correction was important, have other theoretical assumption such as the Gaussianity of the increments be tested before the assimilation.

For the interpretation of the OmF and OmA statistics it should be made clear how long

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ago the forecast was updated with the previous analysis. This should be put in relation to the ozone lifetime.

It is common practise to spell out acronyms of the sensors, institutions such ESA or NASA or RMS at the first ocurrence.

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