Atmos. Chem. Phys. Discuss., 10, C4736–C4740, 2010
www.atmos-chem-phys-discuss.net/10/C4736/2010/
© Author(s) 2010. This work is distributed under
the Creative Commons Attribute 3.0 License.

Atmospheric Chemistry and Physics Discussions

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

M. Weber (Referee)

weber@uni-bremen.de

Received and published: 2 July 2010

1 Introduction

This paper reports on a new assimilated data set for column ozone spanning 30 years coinciding with the period of continuous satellite observations. A unique aspect is that nearly all available satellite data sets are assimilated, for some instruments data sets from different retrievals are even used. The first part of this paper deals with the correction of the various satellite data sets with respect to ground data in order to create a "bias free" merged satellite data record. This merged data set is then assimilated in the KNMI assimilation model. Overall this is an interesting paper. Since I am not a modeler, the most novel and interesting part is the evaluation and corrections of the various satellite data sets that is valuable for assessing the quality of the different

satellite data. The description of data assimilation and discussion of model results is rather brief and not very detailed.

One of the major shortcomings of this paper is that the selection of figures appear somewhat arbitrary and in many cases are not sufficiently discussed in the main text. In my opinion this paper needs some major revisions and should be peer-reviewed again.

2 Figures

Figure 1 shows monthly mean anomalies between two satellite data sets and De Bilt ground station. The differences in the seasonal behavior of the anomalies for the two data sets is not discussed at all in the main text. The question arises if the two predictors used in the correction ("effective ozone temperature" and "SZA") may have a similar seasonal behavior and may not be independent. Is it possible that depending on the phase of the seasonal behavior in the anomalies, the effective ozone temperature or SZA become the more dominant predictor?

Figure 2 provides a good summary of the performance of the individual data set with respect to the ground data, but only comparisons to the de Bilt station is shown. It would be nice to see similar comparisons in the polar region, tropics, and SH mid-latitude. One could show anomalies for all overpasses within a given zonal band. This figure could be improved by showing anomalies with respect to ground data rather than the MSR level 2 (or corrected satellite data). A second figure could show then differences between the corrected satellite data (MSR level 2) and ozone temperature corrected ground data. This would document how well the corrections work for each of the data sets.

Figures 4 and 5 are described in the main text in two sentences and they do not provide any value to this study apart for (colorful) advertising of the assimilated data set.

ACPD

10, C4736–C4740, 2010

Terms like ozone mass deficit is not explained nor the trend in the ozone mass deficit explained. I strongly recommend to drop both figures and substitute them with additional figures as discussed above.

Figures 7 and 8 show observation minus forecast (OmF) and observation minus analysis (OmA) as a function of various parameters. The authors should explain in more detail in the main text what are the separate roles of OmF and OmA in the "quality control". Why do you need both? Also exactly define what is "analysis" (model value at +0h just before assimilating observations?) and what is "forecast" (+12h, +24h?). It would be helpful to properly define OmA and OmF and their different roles in the *quality control* in the beginning of Section 5.

3 Other major issues

p. 11402, l.11: Here and other places the term *stratospheric temperature* or *effective temperature* is used. It is suggested to use a single term like *effective ozone temperature* or simply *ozone temperature* to make it clear which temperature is considered here.

p. 11404, l. 28: *Multiple level 2 data sets from the same instrument are sometimes used since their errors are not highly correlated.* I do not understand what is meant here. Later in the text it is shown that the correlation between different retrievals are on the order of 0.5 (p. 11418, l.6), which means there is significant correlation. Please clarify. What values do the *estimated correlation coefficients* (p. 11418, l. 8) between data sets from different instruments have. (This important since these numbers are used in the error covariance matrix as described in Section 4). Is the calculated correlation coefficient between different instruments much lower than those between different data sets from a single instrument? Another question comes then in mind: do instruments for which only one data set is available get less weight in the data assimila-

ACPD

10, C4736–C4740, 2010

tion than instruments with multiple data sets available or does the *estimated correlation coefficients* causes a more appropriate weighting.

p. 11406, l. 19. A better source for discussion of differences between ground instruments can be found in Staehelin et al., 2003 (Staehelin, J., Kerr, J., Evans, R., and Vanicek, K.: Comparison of total ozone measurements of Dobson and Brewer spectrophotometers and recommended transfer functions, Tech. Rep., WMO, World Meteorological Organization Global Atmosphere Watch (WMO-GAW) Report 149, 2003.)

p. 11407, l. 18. Why are different maximum collocation radii for overpasses are allowed for each satellite data set (50-200 km) to determine overpasses?

p. 11409, l. 8. Table 5 is mentioned here before Table 2. Change numbering of Tables.

p. 11409, l. 20. It should be mentioned here that the standard algorithms for Brewers as well as Dobsons assume a fixed effective ozone temperature (-46°C). This explains the offset used in Eq. 1 (l. 27) (please add an eq. number).

p. 11409, l. 27. Are the values 0.0013 and 46.3 in this Eq. are derived by fitting all 21 stations having simultaneous Brewers and Dobsons, or are these numbers from the literature. Please clarify.

p. 11412. There are two type of corrections applied to the satellite data. In a first step all predictors and individual station biases are fitted (Table 2), but for the data to be used in the data assimilation the corrections using only significant predictors and a single bias for all stations is applied (Table 3). It is not explained why the second correction (with less degrees of freedom) is favored for the data assimilation input.

p. 11412, l. 12: Do you have an explanation why OMDOA3 and OMTO3 have opposite temperature dependence in the anomalies?

p. 11412, l. 27: Please explain here why RMS3 is higher than RMS1 as shown in Table 2, I would have expected them to be equal (satellite data before corrections) or RMS3 should be even lower than RMS1 when using a single bias correction in the former. In

ACPD

10, C4736–C4740, 2010

CC BY

general it is noticeable that the RMS after corrections is in general only slightly lower than before corrections (see comment by other reviewer). Would the improvement in RMS more significant when looking at monthly mean anomalies rather than daily anomalies due to large day-to-day variability dominating the RMS?

p. 11415, l. 9: Some data were already rejected as part of the merging and correction procedure described earlier. Please describe what additional screening is explicitly done during data assimilation.

Fig. 1: Please state in caption if satellite minus ground or ground minus satellite is shown.

Fig. 2: Indicate units of y-axes (DU).

4 Minor issues

An annotated file with additional suggestions for improving texting/spelling is provided.

Please also note the supplement to this comment:
http://www.atmos-chem-phys-discuss.net/10/C4736/2010/acpd-10-C4736-2010-supplement.pdf

---

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 11401, 2010.