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Interactive comment on "A Global Total Column Ozone Climate Data Record" *by* Greg E. Bodeker et al.

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The paper presents the datasets of total ozone column (TCO), which are created using the data from several satellite measurements. One dataset is an improved version (v3.4) of NIWA-BS TCO dataset, while another is gap-free BS-filled TCO database. The paper describes the methods used in the construction of the datasets, and some evaluations of dataset, including evaluation of ozone trends. These datasets are valuable contributions to the ozone research. The paper is well written. Several minor comments and suggestions for paper improvement are below.

We would like to thank the reviewer for taking the time to review this paper and

for their helpful comments which have led to improvements in the paper.

MAIN COMMENTS

1. The differences between individual datasets are evaluated using zonal mean values. Does this approach work also in presence of polar vortex? Please add a discussion or evaluation.

Yes. Other than for the four TOMS and OMI data sets, where the biases are calculated from comparisons between satellite-based and ground-based measurements at individual sites (but them modelled without zonal structure), corrections for the other data sets are determined through comparisons of zonal means against the corrected TOMS and OMI data. If the zonal sampling of a satellite was biased, e.g., if the satellite only made measurements between say 0°E and 90°E, then its zonal mean could be very biased with respect to the zonal mean from an unbiased satellite-based instrument, especially in the presence of zonal asymmetries as occurs when the polar vortex is pushed off the pole, e.g. by a large planetary wave of zonal wavenumber 1, as is pointed out by the reviewer. But this is seldom, if ever, the case, i.e. the TCO measurements from the satellites we have used are zonally unbiased and the comparisons do not suffer from such effects. Cognisant that this may have not been the case for the more spatially sparse SBUV measurements, we did include an additional source of uncertainty in the calculation of SBUV zonal means, as discussed on pages 9 and 10 of the original paper. We have added material towards the top of Section 4 to this effect, i.e. 'There could be a danger here that in the case of biased longitudinal sampling by one satellite compared to another, that the zonal means would be biased but without these differences arising from any intrinsic biases between the satellite-based measurements. Only the SBUV measurements were sparse and corrections for this potential sampling bias were derived as discussed below.'

2. P.18: “For some applications, there is a need for gap-free TCO fields”. Please indicate these applications.

We have now expanded this sentence to say ‘For some applications, there is a need for gap-free TCO fields, e.g., TCO fields for validating chemistry-climate models which generate TCO fields over the entire globe for each day of the year.’

3. Conservative filling algorithm (Section 9.1). In general, it is easy and advantageous to demonstrate the quality of the filling procedure with artificially created missing data: from a full field some data can be masked and the filling algorithm applied. Then the quality of filling and the quality of uncertainty estimates can be directly evaluated using the true and reconstructed data.

This is a good idea and the same suggestion was made by Referee 1. The results of exactly that sort of evaluation (an additional 10 figures), are provided in the response to Referee 1. We decided not to include all of this additional validation of the filling method in the paper as (1) the paper already has 18 figures and adding another 10 would have been untenable, and (2) because the response to the referee comments accompany the paper, we felt that readers who were interested in a deeper validation of the filling technique could refer to our response to Referee 1.

For the conservative filling, using the data from previous of following day is a dangerous operation, in my opinion. The air masses are moved, thus such interpolation can result in significant errors, especially in regions of high ozone gradients.

We never just use the value from the previous or following day. As stated in the paper, we linearly interpolate between values on day $N+1$ and day $N-1$ to estimate a value for day N. We have found this to be very robust and as can be validated by inspecting a TCO time series for any location on the global. Given the very long

photochemical lifetime of TCO, the time series tend to be rather smooth suggesting that linear interpolation between neighbouring days is an acceptable way to estimate missing data. The assumption that is made is not that ozone is unchanged from one day to the next, but that TCO changes approximately linearly from one day to the next. Essentially this is equivalent to a CFL (Courant–Friedrichs–Lewy) condition for TCO.

The calculation of uncertainties is not described in detail. In particular, it is unclear how the distance from available measurements is taken into account, and this is not seen in Figure 13.

Yes, we have not described the calculation of the uncertainties in the longitudinal linear interpolation in great detail. We were trying to keep the paper as succinct as possible. Calculating the uncertainties on such longitudinally interpolated values is non-trivial. The challenges of estimating the uncertainties on such interpolations are described nicely in Fassò et al. (2020). We have used a rather simple method for estimating the uncertainty on the longitudinal interpolation, as follows:

$$\begin{aligned} \alpha &= (\phi-\phi_1)(\phi_2-\phi_1) \\ T_1 &= (1-\alpha)\times\sigma_1^2 \\ T_2 &= \alpha\times\sigma_2^2 \\ \sigma_{interp} &= \sqrt{T_1+T_2} \end{aligned} \tag{1}$$

where ϕ is the longitude at which the interpolation is being performed, ϕ_1 is the longitude to the west where there is a non-null value, ϕ_2 is the longitude to the east where there is a non-null value, σ_1 is the uncertainty on the non-null value to the west and σ_2 is the uncertainty on the non-null value to the east.
The reason that this inflation of uncertainties in conducting the longitudinal interpolation is not apparent in Figure 13, is that much of the interpolation in Figure 13 is through interpolation between day $N-1$ and day $N+1$ which takes precedent over

the longitudinal interpolation.

4. Machine learning estimated ozone: It is important to assess the quality of this approach in unexpected ozone conditions, for example Arctic ozone hole in 2011 and 2020. For example, data from 2011 can be excluded from the training dataset, and then tried to reproduce. In general, demonstration of ozone hole evolution in the created datasets would be a very interesting and valuable addition to the paper.

We agree and this has been done in some detail in response to Reviewer 1. We have not explicitly done this for 2011 and 2020. In suggesting that some validation can be achieved by withholding, e.g., 2011, from the data set, we believe that the reviewer is seeing this as a neural network-based solution. It is not. Rather, it is a regression approach where the regression model establishes a local (in space and time, where neighbouring years are included in the time window) statistical dependence of TCO on local tropopause height and local potential vorticity at the 550 K surface. We could train the regression model to generate TCO fields for all of 2011 when all TCO fields from 2011 are excluded, but this would not be an appropriate validation since the regression model has been constructed to assume that there will be TCO fields within a few days of the target field which is always the case. We have not done as the reviewer has suggested for 2020 since our database terminates in 2016.

5. Section 10. More details on the regression model would be useful. In particular (a) If possible, repeat the sources of proxies (P.27, L.28) instead of reference to Bodeker et al. (2001),

We did not include details of the El Niño Southern Oscillation (ENSO), solar cycle, Mt. Pinatubo and El Chichón basis functions in this paper as they have been published in Bodeker et al. 2001b and Bodeker et al. 1998, and both papers are publicly available. Including the additional information here is unnecessarily verbose

and would lengthen the already long paper even further.

(b) Please add a note on performance of this global fit when data from some months are missing (in polar night conditions).

It is not clear to us what the reviewer is referring to here as the fit is not global, i.e., equation (18) is applied completely independently at each latitude and longitude. As to the challenges of obtaining trends during the polar night, while the Fourier expansion in the regression model fit coefficient (accounting for trend structure at annual, 6-monthly, and 4-monthly periods) is somewhat robust against data in missing months, there is a danger of over-fitting at high latitudes in winter and obtaining unreliable trends in regions of polar darkness. This is why we exclude trends in these regions in Figures 16 and 17.

Please add the figure with TCO trends after 2000 (in addition to change trends). This will allow direct comparison with other studies.

Yes, this is a good idea and we have added this figure as the new Figure 18.

SPECIFIC COMMENTS

1) Please write direct links to the datasets, not via tinyurl.com, which do not work properly.

Thank you for catching this and we have now included direct links and ensured that the data can be found under the links provided.

2) P.4: A map showing locations of Brewer & Dobson stations would be useful.

We have decided not to do this for two reasons:

1. It would add yet another figure to the paper.
2. As a nice interactive map of all WOUDC sites is provided at https://woudc.org/data/stations/index.php.

We felt that an additional figure of all the measurement sites is unwarranted and any reader will find the information (easily available) on the well maintained and updated WOUDC website. We have added a pointer to the this URL in the paper towards the beginning of Section 3.

2)Also a statement on compatibility/similarity of Dobson and Brewer data and their quality would be useful.

We have added the following sentence to the paper 'We assumed in all cases that the Dobson and Brewer spectrophotometer data submitted to the WOUDC were the highest quality data available, that all possible corrections to improve the quality of the data had been made prior to submission, and that the measurements from these two networks were unbiased with respect to each other. Assumed uncertainties on the measurements from these two networks are presented below'. We felt in unnecessary to provide a more detailed discussion of the uniformity of the data either within or between these two measurement networks as many other papers have conducted such analyses, e.g. Bojkov et al. (1986), Bojkov et al. (1990), Zerefos et al. (1992), Bokjov et al. (1995).

3) P.4, L1: Please clarify what you mean by "a higher quality data set": a higher spatial resolution?

We have replaced this sentence with 'While TOMS and OMI data are provided in gridded data files, the original overpass data, unlike the gridded data set where several measurements from different times through the day might be averaged within a grid cell, provide location-specific measurements that are more suitable for comparison with the ground-based measurement networks, i.e. the overpass measurements are specific to a latitude, longitude and date/time.' to be more explicit in what we had previously meant when we had only said 'a higher quality data set'.

4) P.5, around Eq.(2): Since you use integer numbers for f, please indicate also the units of t. It is also worth to note that the choice Nf,b=0 corresponds to the assumption of a constant drift.

We have added just after equation (1) that time is measured in decimal years. The choice of $N_{F,\beta} = 0$ is not that the drift is constant but that the drift is seasonally independent. A sentence has been added to the paper to clarify this.

5) P.7, Fig.3 caption: "Regions shaded in grey" –should be "in black"?

Thank you and we have corrected this error.

6) Table 2. Please indicate that all uncertainties presented in the table are averaged/typical values, which can be different from the uncertainties for a particular location and time.

As suggested by the reviewer, we have amended the table caption to 'Typical uncertainties on the source data sets as reported in the references provided and as used in the construction of the TCO databases. Note that the uncertainty on any particular measurement may differ from the typical values quoted in this table'. We have also removed the links from Table 2 and included the links as references.

7) P.9 L 2: should be sigma_Delta (big Delta)?

The reviewer is correct and we have changed the small delta to a big delta.

8) P.9, Eq.(6), sigma_i should be squared

Thank you for spotting this mistake, which we have now corrected.

9) P.10, L.5. Some data are on 1x1 deg grid. Please clarify how the re-gridding is done.

Where source data were provided at $1^\circ \times 1^\circ$ resolution, bilinear interpolation was used to resample the data to $1.25^\circ \times 1^\circ$ resolution. A sentence to this effect has been added early in Section 2.

10) Fig.7. It would be useful to indicate also standard deviation of differences.

We agree and have made a new version of Figure 7.

11) Figures 9 -11: please use smaller color limits.

If by ‘use smaller color limits’ the reviewer means that we should reduce the colour range on each plot, we prefer not to for two reasons: (1) We would like to capture the extreme values and (2) we would like to use the same color range on each figure to make them comparable.

12) Eq.9, LHS: comma instead of “-“

This has been corrected.

13) P.23, lines 22-26: The description is not clear, perhaps, an illustration (can be put in the Supplement) would be useful.

This paper is not accompanying by a Supplement and we were reluctant to now add a Supplement just to accommodate this single additional figure. Rather than adding a figure to the paper, we have described in greater detail how the A_{proxy} values are calculated.

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