Review of Herman et al. ***Total Column Trends from the NASA Merged Ozone Time Series 1979 to 2021 Showing Limited Recovery to 1979 Amounts after Declining into the Mid 1990s.***

GENERAL COMMENTS

The submitted manuscript presents an analysis of the 'Merged Ozone Dataset', calculating ozone trends using two different methods for 10 degree wide latitude bands, before and after a latitude-dependent "turnaround" time which the authors identify using Lowess smoothing.

This subject (stratospheric ozone depletion and recovery) is of iconic importance to the atmospheric sciences.

Further, the Merged Ozone Dataset itself has proven to be of enormous value within this field and the dataset has been well utilized for trend studies.

However I believe the current work needs major revision before being suitable for publication.

The study of Weber et al. 2022, (hereafter W2022), referred to on multiple occasions by the present authors - but not sufficiently engaged with in my opinion - may be considered the conventional approach at the current time by the community, and forms one of the foundations for the 2022 WMO/UNEP Ozone Assessment. W2022 includes the MOD as one of the datasets analysed, along with four additional satellite and ground-based records.

The main differences between the analysis of MOD contained in W2022 and the present work are the use by the authors here of the latitudinally dependent turnaround time, and a somewhat different regression model.

In general, researchers experimenting with variations to the most popular approach is a good thing for science, but with it, there does need to be sufficient motivation, explanation and comparison provided along with it, otherwise it offers no value in addition to what has already been done.

In this case I believe the authors need to explain their reasoning much better.

I have revised the paper to strongly emphasize the motivation for a study calculating the turn-around time T_A as a signature that should show up in model calculations. I changed the title to further emphasize the motivation. "Total Column Ozone Trends from the NASA Merged Ozone Time Series 1979 to 2021 Showing Latitude Dependent Ozone Recovery

Dates (1994 to 1998)". The calculated trends are not sensitive to the exact values of T_A and agree with Weber et al., 2022 within error bars.

I like the use of heavy smoothing to enable the low frequency changes in the ozone timeseries to appear more clearly. However, the significance of the "turnaround" times identified by the smoothing is not clear to me. The word "turnaround" can only have meaning in the context of a long-term decline followed by recovery (or vice versa). The authors seem to really mean by "turnaround" the first local minimum since 1979.

The definition of T_A is now clearly stated as the date when long-term ozone decrease stops. Except in Antarctica, there is no turnaround towards 1979 ozone values as of 2022.

As can be seen in Figure 4, for some latitude bands the first minimum does not show much evidence of being a "turnaround" at all, as the identified time point was later followed by relatively large decreases and even oscillations in subsequent years. This is quite different to picking a turnaround year based on EESC, where calculating before-and-after trends is in effect a test of the hypothesis that ozone levels are following ODS concentrations, once other identifiable influences have been accounted for. It is of course always possible to calculate a trend between any two arbitrary years, but what does it tell you?

The paper is not trying to correlate T_A with ODS concentrations, but instead includes whatever volcanic aerosol effects or temperature changes that happen to be present. This is now explicitly included with a figure that removes the seasonal terms for the equatorial region and shows the QBO and volcanic effects. The regressors are similar to Weber et al., 2022 except that Weber attempts to remove volcanic and dynamics effects from El Chichon and Mt. Pinatubo eruptions.

My second major concern is related to this point, which is that the choice of regressors in the regression model here seems quite odd to me, and more appropriate for tropospheric ozone rather than stratospheric. There is no dynamic term of any sort and no term for stratospheric aerosol.

The reviewer is correct. The dynamical term in Weber et al., P(t) is not included. Weber was trying to estimate the effect of just ODS concentrations on the trends. This is not the purpose here.

This lack limits the interpretation of the trends calculated, particularly in the light of Figure 4, and the conclusions offered currently in the manuscript are similarly extremely limited.

The details of the regression are also not properly explained, and the overall ability of the regression model to fit the timeseries in each band is not given, either in total or for the different regressor variables.

I have included a figure showing the residual of the fit to the time series for both the Northern and Southern Hemispheres and have given references for the regressor variables.

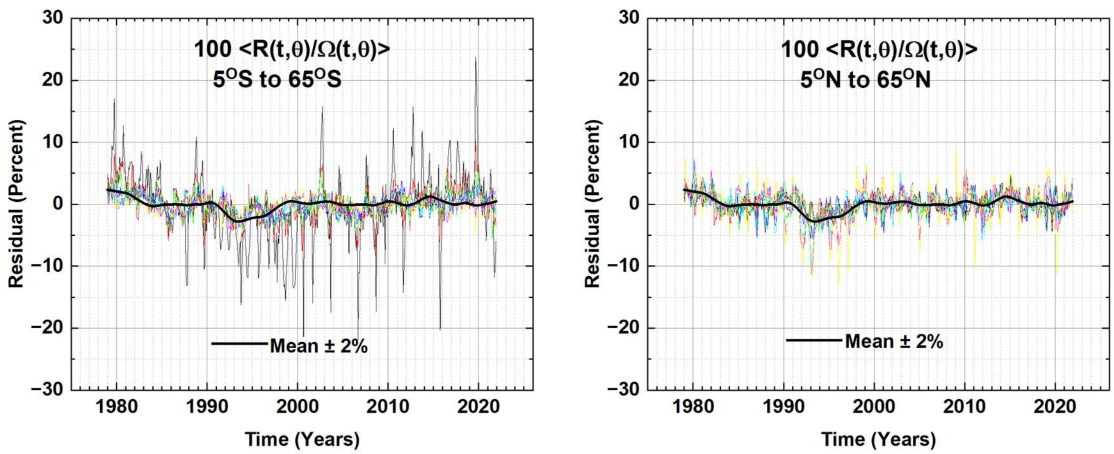


Fig. 2 The latitude average residual term from Eq. 1 in percent $100 <R(t,q_i)/W_{MOD}(t,q_i)>$ The black line is the Lowess(0.1) fit (Cleveland, 1979) to the R(t,q) with an average error estimate of ±2%. The light-colored lines are each latitude's R(t,q) in a hemisphere $0^{O} < \theta < 65^{O}$.

Further, I note, with a small number of exceptions, there is a general lack of engagement with the literature from the last ten years or more which is disappointing.

Specific Comments

Lines 37-67 Most of the introduction talks about the Antarctic ozone hole, which is not the focus of the rest of the work. The introduction barely mentions ozone depletion in the mid-latitudes or tropics, which is the main focus of the rest of the work.

Some text has been added discussing midlatitudes

Line 43 In fact, most of the activation takes place on liquid droplets, eg Tritscher et al.

Comment and reference added

Line 71 Dameris and Baldwin 2012 is nicely written but there has been a lot of work on this subject since then. **I added additional references.**

Line 95-96 This is quite a hand-waving comment, and doesn't explain why the spring build-up is smaller in the SH than the NH for example.

Line 99 Is that true? What about the TOMS/OMI series? (Perhaps it can't be called continuous). **TOMS/OMI are not continuous and were not included in the standard MOD time series. Also, TOMS and OMI are different types of instruments from each other.**

Line 124 I would not say MLR is necessarily 'Fourier based', just that your implementation is. **I agree. Text modified.**

Line 131 What is the meaning of 'generalized' here? **I removed the word generalized. It has no meaning here.**

Lines 139-141 You should give the source of the data you are using for the proxies.

The data sources for QBO and F10.7 are now given.

The F10.7 cm solar flux monthly time series is used for the Solar(t) proxy https://lasp.colorado.edu/lisird/data/noaa_radio_flux , first and second leading EOF QBO monthly time series proxies $QBO_1(t)$ and $QBO_2(t)$ are used for the QBO component (Wallace et al., 1993), and Nino 3.4 (Oldenborgh *et al* 2021) is used for ENSO(t) (https://www.ncei.noaa.gov/access/monitoring/enso/sst). $QBO_1(t)$ and $QBO_2(t)$ are nearly orthogonal (correlation coefficient approximately zero) oscillating time series based on data with approximately a 2.3-year periodicity.

Line 142 I assume the five coefficients rather than seven is as a result of p in equation (2) only running from 1 to 2 rather than 1 to 3? **Yes**

Line 148 When you say "the linear deseasonalized trend" do you mean the constant term, b0? **, $B(\theta_i)$ is the trend coefficient. One coefficient for each latitude band. I corrected the typing error $B(\theta_i,t) \rightarrow B(\theta_i)$**

Lines 148-149 As mentioned earlier, you should give some indication of how successful the regression model is in being able to account for both short-term and long-term variations in ozone, preferably breaking down the influence of each of the proxy terms.

I have supplied a new figure showing that the Residual R(t) is near zero +/- 2% (See Fig. 2 above).

Lines 155-157 I don't feel you have explained the second method adequately. I take it to mean you are simply fitting a linear trend to the annual ozone averages – is that right? What is the motivation of the second method? **Yes, fitting a linear trend to the annual averages from 1979 to T_A and from T_A to 2021. The motivation was to compare with the MLR method for overall trends, and then to apply the annual averages to the polar regions where you have extended night. The MLR method as implemented here does not work well in the polar regions where you have latitude dependent extended winter night. The MLR method is good for isolating the various contributions, which Is not the case for the annual average method.**

Lines 163-165 The text implies to me you are considering this range of turnaround times to represent the ozone response to EESC changes – if not, how can you talk about "***the"*** "turnaround"? I would like to see your thinking much better explained here.

In the revised manuscript I specifically say that T_A is the approximate date of cessation of decrease in O_3 including the effects of ODS, dynamic, and volcanic eruptions.

Lines 173 I don't think it's true that even your heaviest smoothing removes the effect of Mt Pinatubo, it still seems evident at both 45 N and 55N in Fig 3.

The smoothing does not remove all the effect or Mt Pinatubo and is not intended to remove the effect. The smoothing locates the actual atmospheric ozone minimum in the time range 1990 to 2000.

Lines 174-176 Do you have any basis for making this statement? That statement is incorrect. **Changed to: "The Lowess(0.3) degree of smoothing removes most short-term ozone oscillations but not the effects of volcanic eruptions from El Chichon (1982) and Mt. Pinatubo (1991), both well before the earliest estimated turnaround time T_A in 1994."**

Line 177 You don't show 5 S and 5 N in Fig 3 though.

A new figure (Fig. 4) has been added.

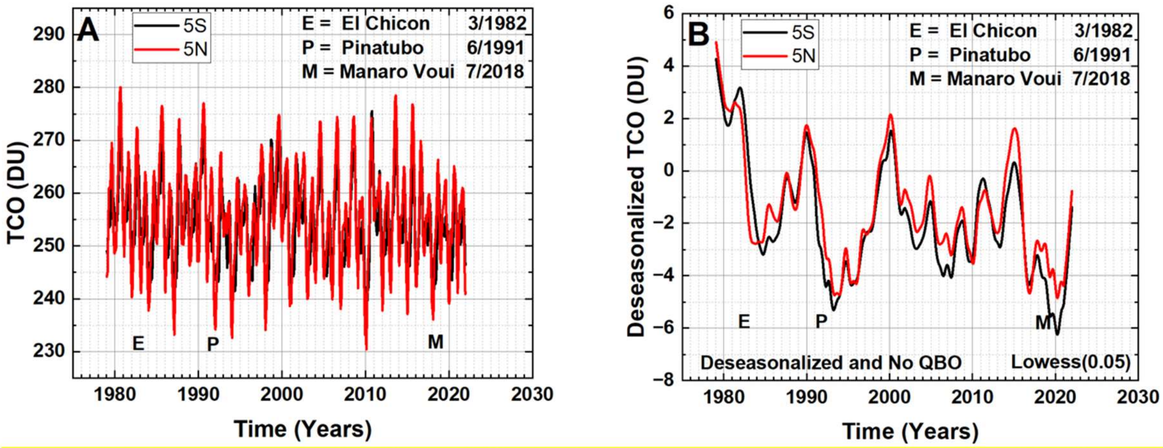


Fig. 4. A. TCO time series for $\theta = 5^{O}N$ and $5^{O}S$. B. The deseasonalized TCO time series for $\theta = 5^{O}N$ and $5^{O}S$ with QBO effects subtracted (Eq. 1). The resulting time series shows peaks approximately every 2.3 years. The approximate dates are shown of volcanic eruptions that injected large amounts of SO_2 into the stratosphere leading to minima approximately 1 year later.

Line 192 Why does the turnaround year vary across this range of latitudes then? (ie it's markedly earlier at the equator). **I do not have the modelling capability to verify this result. However, it is likely that the result follows from the age of air that is a minimum at the equator (see Figure 11 in the revised text). The age of air is a minimum at the equator and allows the effects of changes in the Brewer-Dobson dynamics and decreasing ODS from lower altitudes to get to the stratosphere sooner.**

Lines 194-195 Do you have any basis for making this statement?

The T_A delay to 1997 for latitudes $35^{O}S$ – $65^{O}S$ follows the delayed recovery of ozone depletion within the Spring Antarctic Ozone Hole (Solomon, 1990; Stone et al., 2021, their Fig. 3; Bodeker and Kremser, 2021, their Figs. 6 and 9) and backfilling (air exchange with lower latitude ozone-rich air) during the summer months after the polar vortex winds break down in October – November.

Lines 198-200 This would only be true if the model is driven by observed dynamics though, and includes all relevant factors such as stratospheric aerosol, wouldn't it? How do you know it isn't unforced variability?

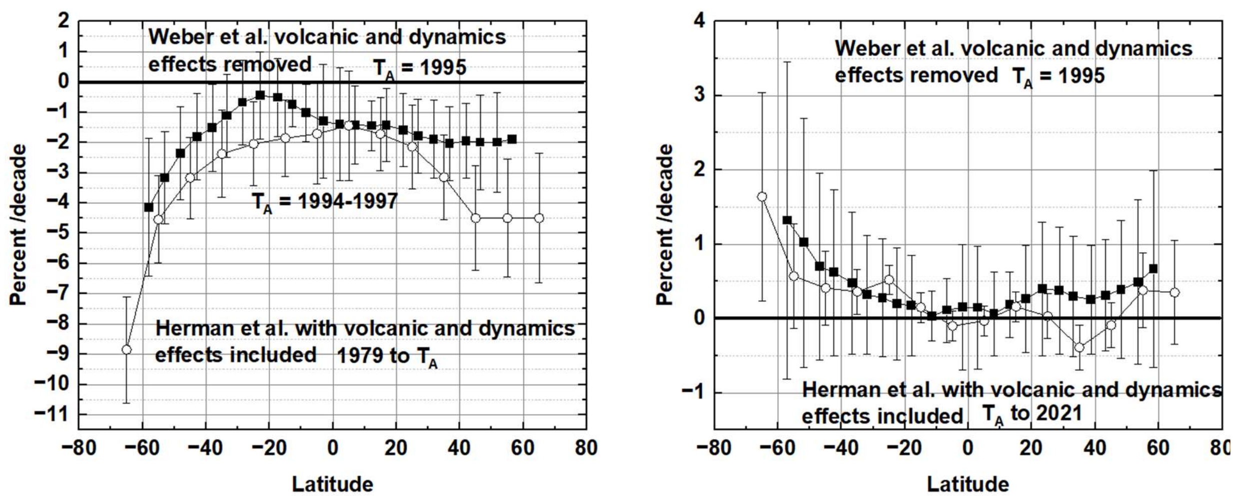
I added volcanic eruptions to this sentence, which is one of the sources of stratospheric aerosols. This is the point of this paper. T_A includes all effects. Models should be able to see a similar asymmetric shape.

Line 205 – In Weber et al. 2022, their Figure 3 shows that the pre- and post- trends (after regressors have been accounted for) are approximately in a ratio of 3:1 as expected from EESC – this doesn't seem to be so for your calculated trends.

From their Figure 3 "The dashed pink lines are the post-ODS peak trends as expected from the 1 : 3 ratio (corresponding to changes in the stratospheric halogen), applied to the median time series' trends before 1996." The trends that are calculated in Figure 8 and Table 1 are not just from stratospheric ODS.

Lines 214-215 Do you mean, mixing of ozone-poor air from the vortex into the midlatitudes? **That paragraph no longer is in the paper. However, a new extended paragraph talks about air exchange.**

Lines 235-251 I would like to see some discussion of the meaning of your results in the conclusion, and comparison with other work. **The main result is T_A as a function of latitude, which is not discussed elsewhere. A secondary result is the small turndown on O_3 after 2010, which is also not discussed anywhere that I could find. The trends are different because I do not remove the effects of volcanoes and dynamics. However, the trends in Fig. 8 and Table 1 are within the mutual error bars with Weber's Fig.3. The biggest difference is before T_A for Lat > 40. I have added some text to the summary about the different trend calculation.**



A Comparison of Herman et al. (open circles) with Weber et al., 2022 (black squares). This figure is not in the paper.

Lines 253-274 You should discuss the fact that you're comparing total ozone with stratospheric ozone and why you presumably don't think changes in tropospheric ozone over this period need to be considered.

Line 272 The discrepancy in the tropics looks like it might well be real – what do you have to say about that?

As stated in the Appendix, "The question addressed here is not the absolute agreement between MOD TCO and the MLS mostly stratospheric ozone column, but rather if there is a systematic drift between the two data sets after 2016." Since each time series has been deseasonalized the long-term mean value of each would be zero unless there was drift in one of the instruments or real change in the tropospheric ozone amount. The accuracy of MOD is given as ±3DU so that the changes in Figs. A1 to A3 are within that limit. However, there appears to be a systematic 2σ' drift that suggests a change in 2020 tropospheric ozone that might be associated with the COVID-19 economic slowdown. By 2021, the difference is mostly gone except at the equator where it is 1DU.

Figure A3 has been changed and the following text added.

Since both MOD and MLS time series were deseasonalized, the mean values would be zero unless there were changes in tropospheric ozone or instrument calibration drift. The differences are summarized in Fig. A3 along with the 2σ', (σ' = standard deviation from the mean) error bars estimated from the average of each deseasonalized time series. In 2020 there appears to be a systematic change in <MOD – MLS> that may be a reduction in tropospheric ozone amount caused by the economic slowdown associated with COVID-19. The systematic change mostly recovered in 2021 except for -1DU near the equator (-5^{O}S to 15^{O}N).

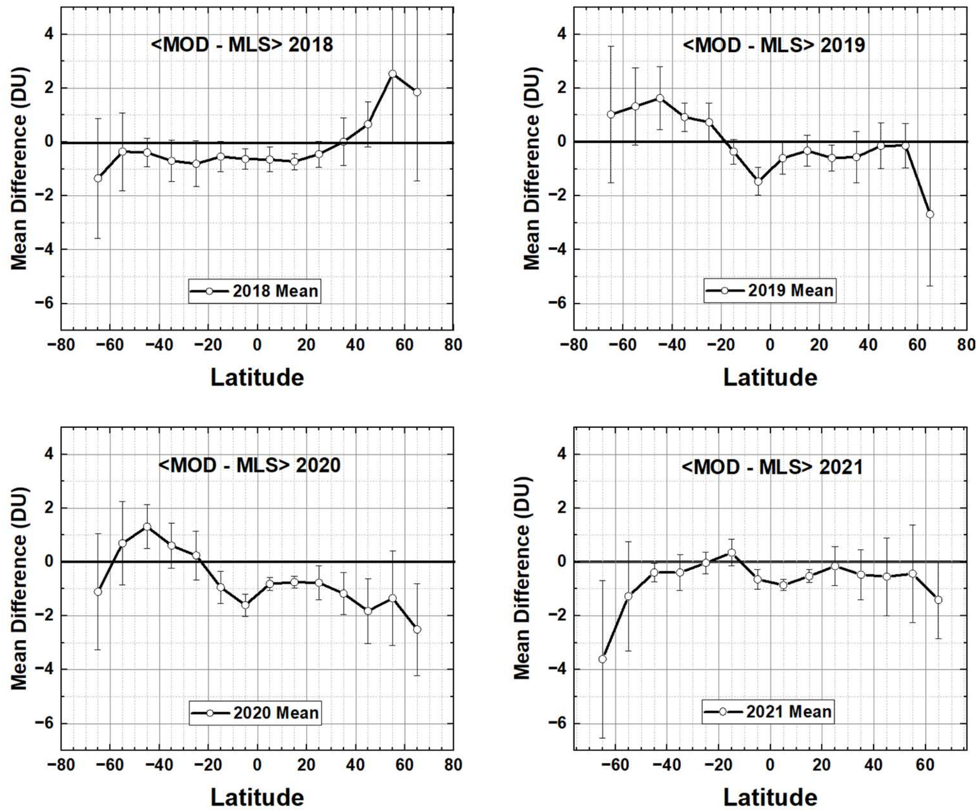


Fig. A3 Annual average <MOD – MLS> for the years 2018 to 2021. Error bars are 2σ′, where σ′ = standard error of the mean estimated from the average of the deseasonalized time series for each year shown in Figs. A1 and A2.

References (**These have been added**)

Tritscher, I., Pitts, M. C., Poole, L. R., Alexander, S. P., Cairo, F., Chipperfield, M. P., et al. (2021). Polar stratospheric clouds: Satellite observations, processes, and role in ozone depletion. *Reviews of Geophysics*, 59, e2020RG000702. https://doi.org/10.1029/2020RG000702

World Meteorological Organization (WMO), Scientific Assessment of Ozone Depletion: 2022, GAW Report No. 278, 509 pp., WMO, Geneva, 2022.