All changes to the manuscript including those requested by the reviewer 1 are marked in Green.

General (Reviewer 1)

The paper is well written, very clear, and provides a new and potentially useful analysis. The deduced trends are broadly consistent with those published previously (e.g. by Weber et al., already cited, or by McKenzie et al., 2019, DOI: https://doi.org/10.1038/s41598-019-48625-z).

This reference is about modelling, local UVI measurements and future projections (World avoided). Figure 3 of McKenzie et al. does show a levelling off of UVI somewhere between 1990 and 2000, which is consistent with Weber et al., and the trends calculated here. The calculated trends are not particularly sensitive to the value of T_A within the range 1992 to 2000. T_A is a signal that models should be able to reproduce if they have volcanic activity, chemistry, and atmospheric dynamics done correctly.

Unfortunately, though, I'm not yet convinced that the results for the latitude-dependent turnaround dates – which are the new finding here - are correct (presuming that the object was to find the turnaround date due to the bottoming out of manmade ODSs, rather than the turnaround due to all sources, including volcanic perturbations). If the revised analysis proves to be valid - after satisfactorily addressing my main concerns below - then the paper will be suitable for publication.

The object was to determine the turnaround from all sources just using the ozone data set. Because of volcanic eruptions, these values of T_A are not necessarily correlated with the bottoming out of ODSs. Models should be able to show the latitude dependent hemispherically asymmetric shape of turnaround dates that are a combination of dynamics and chemistry driven by all sources including volcanic eruptions, if the models are correct. The turnaround dates suggest a dynamics signature. Mt. Pinatubo was in 1991, well before the turnaround dates but within age of air estimates. An explicit statement has been added on page 4.

The calculated trends and $T_A(\theta)$ include the effects of volcanic eruptions such as Mt. Pinatubo in 1991.

The latitude-dependence in the turnaround date is useful new knowledge (though error bars are required) and seems qualitatively consistent -at least in the southern hemisphere - with the latitude dependence in age-of-air (which should be cited**).**

The asymmetry between the Arctic and Antarctic is caused by the lower winter Antarctic temperatures (-80^{O}C) leading to the formation of low altitude clouds containing ice crystals along with the isolating Antarctic polar vortex winds (Solomon et al., 2007). In the spring sunlight the ice releases ODS and depletes ozone to a monthly average of about 155 DU. During the summer, ozone rich air from lower latitudes is able come into the polar latitudes and fill in the ozone layer above Antarctica (monthly average about 300 DU. The Arctic does not routinely have the low temperatures needed for winter ice clouds nor does it have the persistent isolating polar vortex winds because of wave action forced by the land topography The latitude band at 75^{O}N (Fig.1) has the highest amount of monthly average winter ozone 450±25 DU that decreases to 290±20 DU monthly average during the summer that are comparable to mid-latitude values. The result is earlier values of T_A in the NH compared to the SH.

The Age of air CO_2 calculation suggests that ozone will be controlled by the Antarctic low temperatures, heterogeneous chemistry, the persistent polar vortex winds, and the summer mixing of ozone-poor air with SH midlatitude ozone-rich air after the polar vortex winds break down. A discussion has been added on pages 12 and 13 as well as an age of air figure.

But the actual turnaround dates do seem a little early, especially in the northern hemisphere. A useful additional plot would be to compare the delay in turnaround date from some reference, say 1994, with age-of-air in the stratosphere as a function of latitude (as for example in Fig 6 of Waugh et al., 2002, DOI: 10.1029/2000RG000101). With that suggested new figure, it would be clear that the deduced turnaround date for the northern hemisphere is too early – possibly because of Pinatubo's effect.

The value of T_A in the NH is driven by the slowing down of the increase in ODSs (Stratospheric Halogens) early in the 1990's, aerosols from volcanic eruptions (e.g., Mt Pinatubo in 1991), and the fact that the extremely low winter temperatures needed for the buildup of halogens on ice crystals in the Arctic are not persistent. That, plus the lack of sustained polar vortex winds does not lead to late T_A, since the NH mixing of ozone poor air with lower latitude ozone-rich air is not present.

I have added a discussion of the age of air on page 14.

Age of air AoA is a measure of how long a parcel of air resides in the stratosphere after it leaves the troposphere (Linz et al., 2016; Ploeger et al, 2021). A comparison of T_A with AoA estimates from the relatively inert tracer gas CO_2 (Fig. 10) for the altitude range near the ozone maximum (approximately 20 km) vs latitude (based on Waugh and Hall, 2002, their Fig. 6a and Ploeger et al, 2021 their Fig. 10a) shows near symmetry between the hemispheres with the shortest AoA in the equatorial region. The turnaround dates T_A in Fig. 5 are also symmetric in the equatorial zone corresponding the upwelling Brewer Dobson circulation and the smaller AoA. This suggests that the combined effects of chemistry and dynamics on ozone amounts are similar between ±25^{O}. The precursors to ODS are also lifted into the equatorial stratosphere and transported towards the polar regions (Newman et al., 2004; 2007) where they can be photo dissociated into ODS in proportion to the longer AoA. Ozone at higher latitudes, NH and SH, with longer AoA, will be dependent on ozone and ODS photochemistry and especially the different dynamics and chemistry in the Arctic and Antarctic regions.

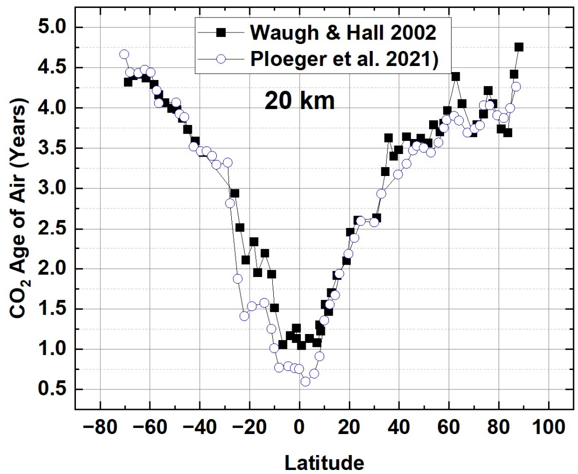


Fig. 8 Age of air derived from CO_2 data (Waugh and Hall, 2002; Ploeger et al., 2021)

My main issues with the present version are:

1. It's hard to envisage why the turnaround dates should precede the date that equivalent chlorine (EESC) reaches a maximum in the stratosphere. According to the most recent Ozone Assessment, EESC reached a peak in the stratosphere at mid-latitudes in 1998 –in reasonable agreement with the deduced turnaround dates at southern latitudes, but 4 years later than deduced here at northern latitudes, where ozone was affected by the eruption of Mt. Pinatubo.

T_A includes volcanic eruptions such as Mt. Pinatubo

That raises the following questions.

1. Is the merged data set of satellite data alone suitable for trend analysis such as described in the manuscript. I know at least one of the authors has claimed in the past that they should not be, which is why merged data sets normalized to Dobson values were developed (e.g., by Bodeker). Please explain what has changed that now enables you to use the satellite data directly. Please also include the resulting error in ozone trend from that source. I see a comparison with MLS gives confidence for the period since 2005, but what about the 27 year period before that?

The satellite ozone data used in the MOD data set were reprocessed with

an improved and consistent calibration applied to the entire series. Richard McPeters and others no longer recommend normalization to the Dobson network (private communication, 16 June 2023). Further, the Dobson network is not corrected for temperature sensitivity of the ozone cross sections and is very sparse in the SH and none over oceans.

2. Are the deduced turnaround dates influenced by aerosols from the 1991 eruption of Mount Pinatubo, which led to significant reductions in ozone in the northern hemisphere for a couple of years after the eruption?
   Yes they are.

3. For example, if those years are omitted in Figure 4, it would appear that ozone has continued to decline more or less monotonically at 55N. An additional sensitivity analysis is required to look at the effects on the final results of omitting that period of data (e.g., all data from 1992 and 1993). Alternatively, you could try even larger values of 'f' to better remove short term effects. Additionally, I would suggest including aerosol impulses – possibly latitude-dependent- from volcanic eruptions as new basis functions in the analysis, as used previously by Liley et al (see Fig 2 in https://doi.org/10.1029/1999JD901157).There's a nice depiction of these shown in Q13 of the Twenty Questions and Answers document that accompanies the most recent ozone assessment (available from https://www.csl.noaa.gov/assessments/ozone/2022/). That depiction shows that Pinatubo effects continue until after your deduced turnaround dates in the northern hemisphere, and peak EESC peaking much later, as does the minimum ozone in the lowest panel. My guess is that the steps described above will make a difference to the northern hemisphere turnaround dates, but not the southern hemisphere where Pinatubo's effects were much smaller.

   T_A is supposed to include all effects, including volcanic injection of SO_2, so that models can judge whether they have the dynamics, chemistry, and aerosol effects properly incorporated using ozone as a tracer.

In Figure 4, please also include those blue and red curves for latitude 55 (it would be instructive to see these plots for other latitudes as well). By including the lower latitudes, the reader can better understand what the authors are getting at in line 177. Please also state the range of years over which the "slight downturn" applies. Since 2016? **Now in the text.**

As shown later, the apparent downturns in the Lowess(0.3) fit to Ω_{MOD} after 2010 are not yet statistically significant in trend estimates from Ω_{MOD} as an indicator of long-term ozone decrease.

Figure 5. The turnaround dates will possibly (probably?) be revised after the analysis suggested above, which will affect the subsequent trend analysis. Please also include error bars in the figure. **Since volcanic eruptions are included, the turnaround dates have not changed. The error in estimating T_A is 0.5 years as now stated in the caption.**

Minor points

Line 57. Start the sentence with "The beginnings of ozone recovery were …" (Or use the word "slowdown" instead of "recovery". I don't think that a slowdown in the rate of depletion can correctly be described as a recovery). **I have changed it to "end of ozone decrease".**

Line 104. Should that be high "latitude" (rather than "altitude")? **Altitude is correct This referring to diurnal variations that occur at 40 km and above.**

Line 137. Please state the period of each of these QBO terms.

The two QBO terms are orthogonal functions with approximately a 90 degree phase difference resembling sine and cosine functions, but based on noisy data. The period for both is a 28-to-29-month QBO cycle There is also an average 11.3-year solar cycle.

Line 156, …:but ignore …' (no 's', as refers to a plural term, integrals)

Changed to ignore (Thank you)

Line 160. If it is valid to say so, you could add something along the lines of the following to give context. "Over the total 4-decade period since 1979, the maximum annual ozone reduction was approximately 13% at 70S, and smaller elsewhere. For example, the reduction was approximately 4% 45S, and 3% at 45N."

Based on the first derivatives (Fig. 5) of Lowess(0.3) for O3(t), the maximum annual rate of reduction occurs in 1980 in the NH and SH except for 65N in 1992 where the rate of loss is -8.75%/Year. The following has been added on pages 12 to 13:

The delayed (1997) Southern Hemisphere mid and high latitude values of T_A are caused by coupling to the increasing Antarctic spring ozone loss after 1979 until a recovery starting in about 1998-2000 (Solomon et al., 2016). The mid and high latitude, from 35^{O}S to 65^{O}S, delay is caused by the summer mixing of ozone poor air from the Antarctic region with SH midlatitude ozone-rich air once the polar vortex winds break down in November-December.

Antarctic ozone loss is driven by sustained low temperatures enabling the formation of thin ice clouds before Spring UV sunlight starts destroying ozone through heterogeneous chemistry on ice crystals within the isolating polar vortex wind region (Solomon et al., 2007; 2016). Smaller but significant ozone losses occurred in the Artic region caused by occasional low temperatures and ODSs. However, the Arctic region does not have sustained low temperatures that form winter ice clouds, nor does it have long duration isolating polar vortex winds (Solomon et al., 2007) needed to form an ozone hole region. The NH T_A is earlier than the 1997 minimum in stratospheric halogens (Weber et al., 2022; Newman et al., 2007). Note that T_A is not the time of the start of recovery, but rather the time for the end of rapid ozone decrease.

Before the SH T_A, total column ozone decreased at a rate of P_D= -10.9±3.6% at 77.5^{O}S and -8.0±1.1% per decade at 65^{O}S, during the period from 1979 to 1997 with smaller decreases from 55^{O}S to 25^{O}S (Fig. 7a). After the turnaround period T_A, ozone at 65^{O}S increased at P_D = 1.6±1.4%/decade based on the MLR method. After T_A, most other latitudes (Fig. 7b) show stationary ozone amounts within 2σ. In the NH the decreases were smaller than in the SH before T_A because of the absence of an Arctic ozone hole region. At 77.5^{O}N was P_D = -5.6±4%/decade and at 65^{O}N P_D = -4.4±0.35 %/decade.

The asymmetry between the Arctic and Antarctic is caused by the lower winter Antarctic temperatures (-80^{O}C) leading to the formation of low altitude clouds containing ice crystals along with the isolating Antarctic polar vortex winds (Solomon et al., 2007). In the spring sunlight the ice releases ODS and depletes ozone to a monthly average of about 155 DU. During the summer, ozone rich air from lower latitudes is able come into the polar latitudes and fill in the ozone layer above Antarctica (monthly average about 300 DU. The Arctic does not routinely have the low temperatures needed for winter ice clouds nor does it have the persistent isolating polar vortex winds because of wave action forced by the land topography The latitude band at 75^{O}N (Fig.1) has the highest amount of monthly average winter ozone 450±25 DU that decreases to 290±20 DU monthly average during the summer that are comparable to mid-latitude values. The result is earlier values of T_A in the NH compared to the SH.

An analysis of ozone trends prior to the start of reliable satellite data in late 1978 showed that the annual rate of ozone loss (%/Year) increased after 1978 (Staehelin et al., 2001). Based on the first derivatives of the data in Fig. 5, the maximum annual rate of ozone reduction occurred in 1979 and 1980 in the NH and SH (Fig. 8) except for 65^{O}N in 1992 where the rate of loss is -8.75%/Year. The loss rates range from -20.6 %/Year at 75^{O}S to 2.39 %/Year at 5^{O}N. An interesting feature occurred for 35^{O}N to 75^{O}N where the loss rate is almost constant between 8%/Year and 10%/Year compared to the larger SH loss rates caused by the presence of the springtime Antarctic ozone hole.

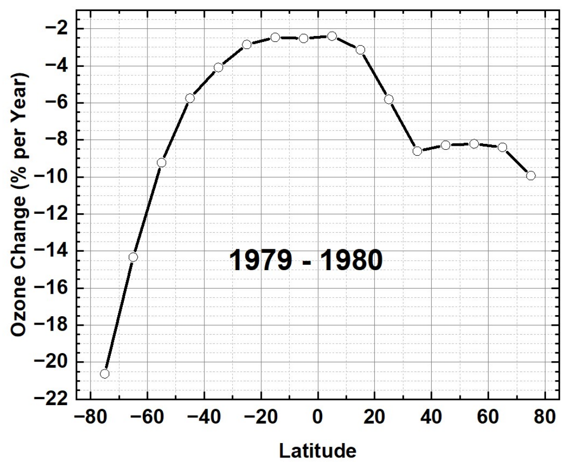


Fig. 8 The percent change in ozone per year in 1979 or 1980

Line 172. By “harder to see”, I presume you mean “less precise”. Please include error bars on your determinations of T_{A}., as this is the key new parameter that comes out of this work. **The error in determining T_A is 0.5 years. The phrase, “harder to see” has been removed**.

Line 172-173. I disagree with this statement. I agree that effects of the smaller El Chicon eruption are smoothed, but I can still clearly see what looks like a Pinatubo effect at 45N and 55N. (and at other northern latitudes in Fig 5).

I have changed it to “The Lowess(0.3) degree of smoothing removes most of the short-term effects on ozone such as volcanic eruptions from El Chichon (1982) and Mt. Pinatubo (1991), both well before the earliest estimated turnaround time T_A in 1994.”

Line 181. After “sharp downturn”, add the words “after around 2010”. Also, add a note that in Fig 5, the range of ozone differs markedly between rows. **After 2010 added**.

Added: Note that the ozone scale varies for each latitude.

Line 214. Change "led" to "leads" because that still happens (it may be best to change order of sentences too). **Now "leads"**

Line 203. There is no need to include Tables 1 and 2 because (as stated) the information there is the same as in Figures 6 and 7. The Tables could be included as supplementary data. Accordingly, remove or modify the sentence starting on line 203.

Table 1 is now embedded in the figure and Table 2 (now Table 1) is kept because the values in the figure, especially the error bars are hard to read.

Line 222. I'd suggest a slight rewording, as follows: "However, computing the trends from either the MLR or annual average methods shows that the small decline from 15 to 65N is not significant at the 2s level (1.5 ± 2% per decade)". I note that at no latitude shown is the change significant over this period.

The sentence has been changed, "The Lowess(0.3) plots in Fig. 5 suggest that Ω_{MOD} has been declining since approximately 2010 from 5^{O}S to 65^{O}N but still increasing from 45^{O}S to 65^{O}S (Fig. 8). However, computing the trends from either the MLR or annual average methods suggest that the decline in ozone from 5^{O}S to 65^{O}N is not significant at the 2σ level over the period 2010 – 2021."

Line 224. Fig 9. Please clarify whether the trend is over an 11-year period (as implied by the legend), or a 12-year period (as stated in the caption). **2010-2021**

Line 233. Can you say anything comparable about the period prior to 2005 (see main point above). **You must be referring to the apparent upturn in O3. I do not know why that might have occurred.**

Figure 6. Clarify punctuation in the caption. (a) ….., (b) …. **Clarified**

General Comments: (Reviewer 2)

The authors use total column ozone data to determine the specific date at which the zonally averaged ozone stopped declining (referred to as $T_A(\theta)$), which holds significance for atmospheric models. Subsequently, the trends of column ozone were calculated using MLR and linear regression, both before and after $T_A(\theta)$. The findings indicate that there has been only a minor recovery in the Southern Hemisphere towards the ozone levels observed in 1979, with virtually no recovery in the Northern Hemisphere, except for the Antarctic region. While these results present new insights, the robustness and interpretation of the findings require further reinforcement. Thus, significant revisions are necessary before considering the publication of this article.

All changes to the manuscript including those requested by the reviewers are marked in Green or Yellow (Reviewer #2).

Specific Comments:

Lines 83-86: The two trend research methods have distinct study areas, and it is important to explain why the MLR method might be affected by the polar night, potentially due to the solar cycle. Additionally, it might be more appropriate to include this discussion about the different study areas and the potential impact of the polar night on the MLR method within the introduction section of the methodology.

Inclusion of the polar night region introduces extra frequency components that are not always physical, especially near the Arctic and Antarctic circles. This could have been considered in the generalized MLR method with additional terms of varying periods depending on latitude for latitudes greater than 70 degrees. The annual average method does not have these complications.

The MLR method (Eqns. 1 and 2) are not applied poleward of the Arctic and Antarctic circles where latitude dependent extended winter night periods occur. Additional latitude dependent terms of varying periods would be needed for latitudes greater than 70^{O}. The annual average method does not have these complications.

Lines 124-129: It would be better to provide more description for the Fourier-based MLR to clarify its difference from the generalized multivariate linear regression (MLR) discussed in the next paragraph.

The MLR method is the generalized multivariate linear regression (MLR) discussed below. I have modified the sentence on page 4:

2) Fourier time series decomposition or generalized multivariate linear regression MLR (Ziemke et al., 2019) discussed below.

Fig. 2: In addition to the difference in the latitude range studied by the two methods, it is worth noting that they also differ in terms of latitude intervals.

Use of slightly different latitude intervals only in Figure 2 was done so that the error bars could be easily discerned between the two methods (larger for annual averages). The trend results are the same if identical intervals had been used in Fig. 2. In subsequent figures, identical intervals are used. Figure 2 is not physical as stated in the text, since it assumes a linear trend when in fact the time series is non-linear. It is just a mathematical exercise comparing the two methods and showing the difference in calculated trends even when making the erroneous linear assumption.

Although Fig. 3 demonstrates the fitting effects of different Lowess values (e.g., 0.05, 0.1, 0.3), it is necessary to provide a clear explanation as to why Lowess=0.3 was chosen as the optimal value in the final analysis.

Lowess(0.3) was chosen as the preferred value since it was the smallest value (f) that produced smooth curves with unique zero crossing dates in its derivative. Estimates for T_A have now been made for f = 0.1, which produce different noisy results in the derivative requiring averaging leading to an uncertainty estimate of 0.5 years. The uncertainty is now stated in the paper and in Fig. 6. Note: New figures have been added, Fig. 3, discussing volcanic effects.

The result mentioned in lines 176-177 lacks an accompanying visual display.

I added a small section and figure on the volcanic influence on ozone at the equator.

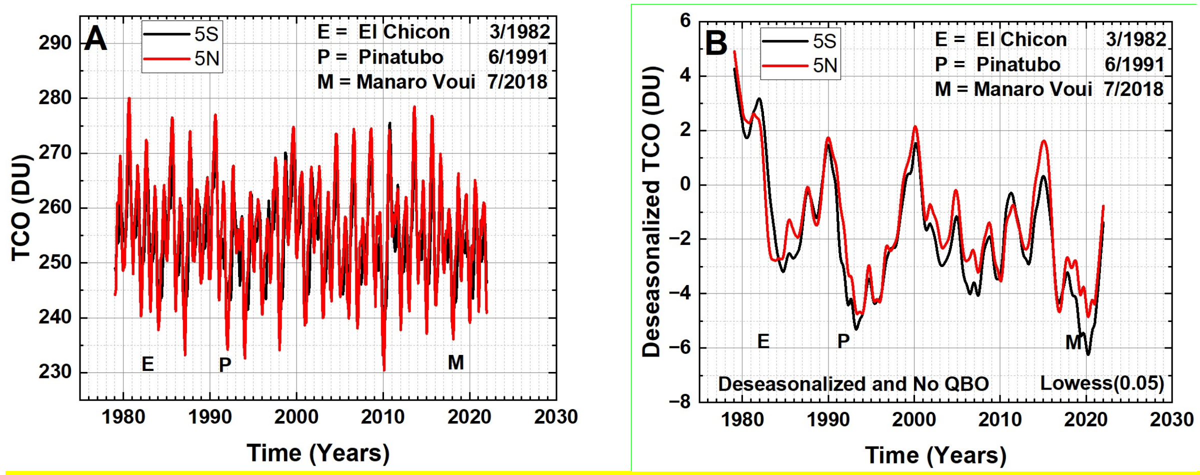


Fig. 3. A. TCO time series for $\theta = 5^{O}$N and 5^{O}S. B. The deseasonalized TCO time series for $\theta = 5^{O}$N and 5^{O}S with QBO effects subtracted (Eq. 1). The approximate dates are shown of volcanic eruptions that injected large amounts of SO_2 into the stratosphere leading to minima approximately 1 year later.

Some volcanos inject significant amounts of SO_2 into the lower stratosphere leading to the formation of aerosols that reduce UV light and the production of ozone, especially in the equatorial region. Figure 3A shows the $\Omega_{MOD}(t,\theta=5)$ time series for TCO in which volcanic SO_2 injection effects from El Chicon March 1982, Mt. Pinatubo June 1991, and Manaro Voui July 2018 are not obvious. After removal of both deseasonalized and the QBO effects from Eq. 1, the reduced ozone effects from three volcanic eruptions, El Chichon, Mt. Pinatubo, and Manaro Voui are shown in Fig. 3B.

Fig. 4-5: Fig. 4 shows decrease in TCO after 2010 in North Hemisphere, and the authors indicated that “the apparent downturn in the Lowess(0.3) fit to MOD after 2010 is not yet statistically significant as an indicator of long-term decrease”. However, do the “Turnaround dates” (Fig. 5) calculated based on Fig. 4 in the North Hemisphere make sense statistically?

The values of T_A are statistically significant with the ±0.5 years uncertainty. The decrease in TCO at the end of the record is not long enough for the trend to be statistically significant. If it continues at the present rate of decrease for a few more years, the trend will be statistically significant.

Fig. 5: The reason for the near symmetry in the early turnaround dates of the Brewer-Dobson ozone upwelling region (±25°) warrants further investigation. It is important to consider that there is considerably more longitudinal asymmetry in topography, land, and ocean distribution in the Northern Hemisphere (NH) compared to the Southern Hemisphere (SH). Consequently, the planetary wave drag may differ between the two hemispheres, which could contribute to the observed differences in ozone recovery patterns.

I agree that the differing topography is a contributing factor to hemispheric asymmetry especially the effect of NH topography preventing the formation of a persistent Arctic vortex wind. The delayed Antarctic ozone hole recovery and the mixing of mid-latitude ozone rich air with the Antarctic ozone poor air is also part of the delay in the SH. Researchers using models that include the topographic drag effect along with volcanic eruptions and all the atmospheric chemistry and dynamics should be able to see this asymmetry.

In lines 194-195, it is mentioned that the Spring Antarctic Ozone Hole and polar vortex winds led to a delay in high latitudes in the Southern Hemisphere (SH) until 1997. However, it is important to note that these phenomena should occur every year. Therefore, additional evidence, such as models or observations, is required to support the author's claim and provide a more robust explanation for the observed delay in high SH latitudes until 1997.

The following has been added on page 10: 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 (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.

This paper's conclusions are not entirely consistent with those of Weber et al. (2022), despite utilizing similar data and methods. To explain the differences between the two studies, further analysis and investigation are needed. Possible factors contributing to the disparities could include variations in the data preprocessing techniques, differences in model configurations, or the incorporation of additional variables in one study compared to the other. A thorough comparison and evaluation of these factors may shed light on the discrepancies observed between the two studies.

The trends in this paper and Weber et al. are consistent within the error bars (their Fig. 3). However, Weber et al. included specific Pinatubo and El Chicon terms in their MLR method, which was not done here since we wanted to include volcanic effects not just ODSs. This leads to differences in the calculated trends. The trend calculations are only weakly dependent on T_A as noted by Weber on their page 6849. Weber et al. uses a fixed T_A (1995) with their trend figures (Fig. 2) suggesting a considerable uncertainty in defining T_A. The importance of latitude dependent T_A is for models to be able to reproduce the shape of the hemispherical asymmetry, including volcanic effects, while maintaining the equatorial symmetry associated with the Brewer-Dobson circulation.

Technical Comments

The abbreviation "TCO" should be defined and explained in line 53 rather than line 64.

Done

The legend of Figure 4 (e.g., 35°N) should be revised. **Fixed**

In lines 216-218, the text presentation and punctuation should be adjusted for clarity and accuracy. **Fixed**

Reply

Reviewer No. 3

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.

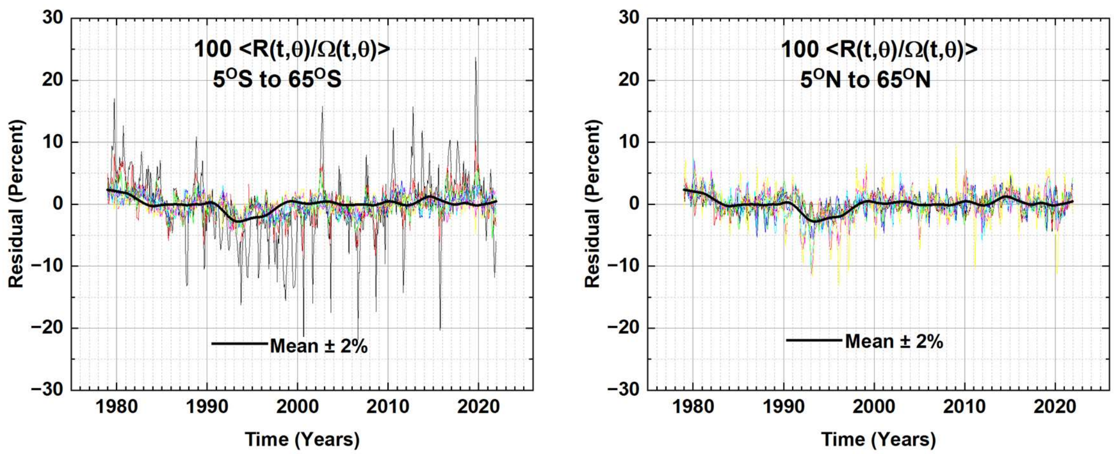


Fig. 2 The latitude average residual term from Eq. 1 in percent $100 <R(t,q_i)/W_{MOD}(t,q_i)>$ The black line is the Lowess(0.1) fit (Cleveland, 1979) to the R(t,q) with an average error estimate of ±2%. The light-colored lines are each latitude's R(t,q) in a hemisphere $0^{O} < \theta < 65^{O}$.

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.

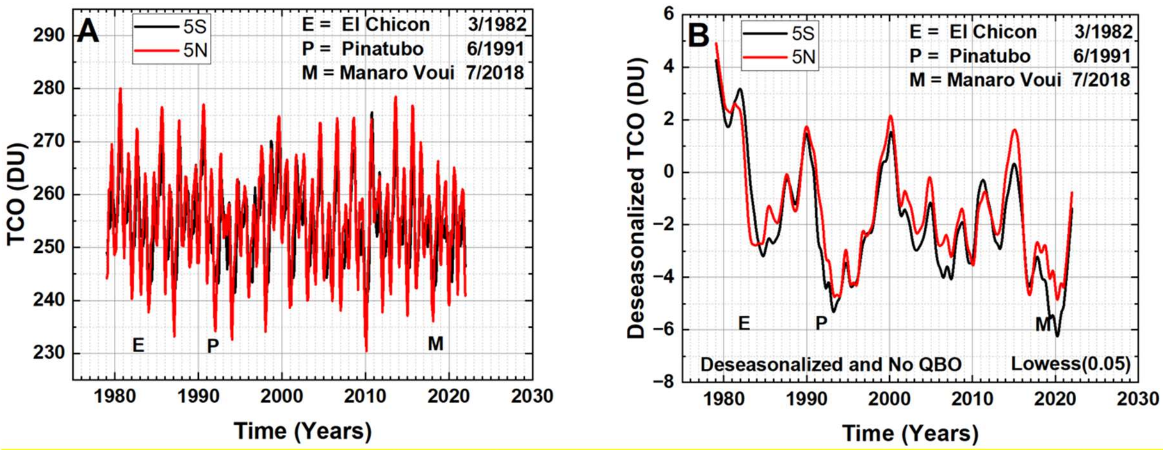


Fig. 4. A. TCO time series for θ = 5^{O}N and 5^{O}S. B. The deseasonalized TCO time series for θ = 5^{O}N and 5^{O}S with QBO effects subtracted (Eq. 1). The resulting time series shows peaks approximately every 2.3 years. The approximate dates are shown of volcanic eruptions that injected large amounts 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? How do you know it isn't unforced variability?

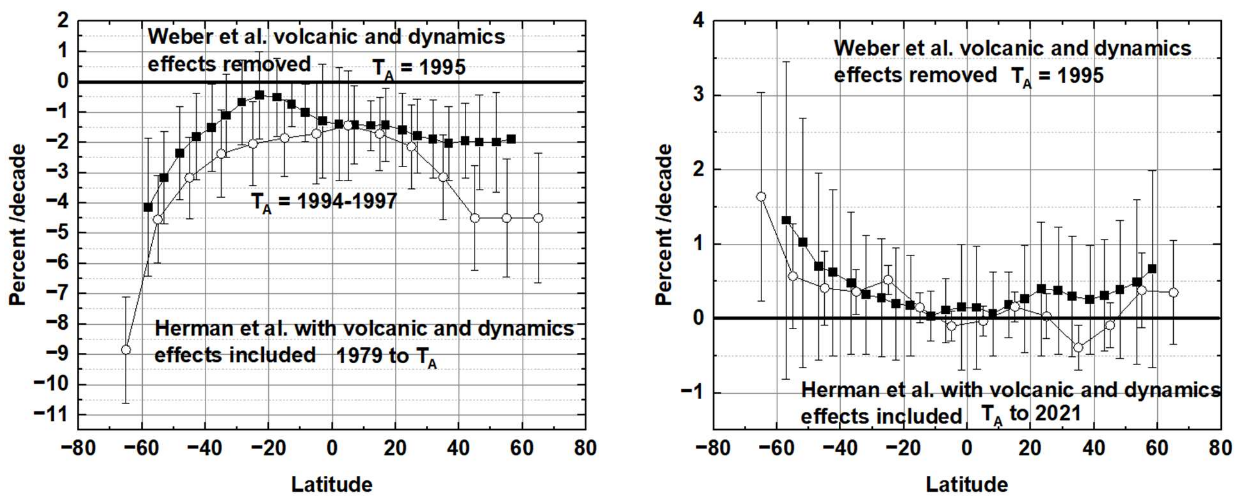
I added volcanic eruptions to this sentence, which is one of the sources of stratospheric aerosols. This is the point of this paper. T_A includes all effects. Models should be able to see a similar asymmetric shape.

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).

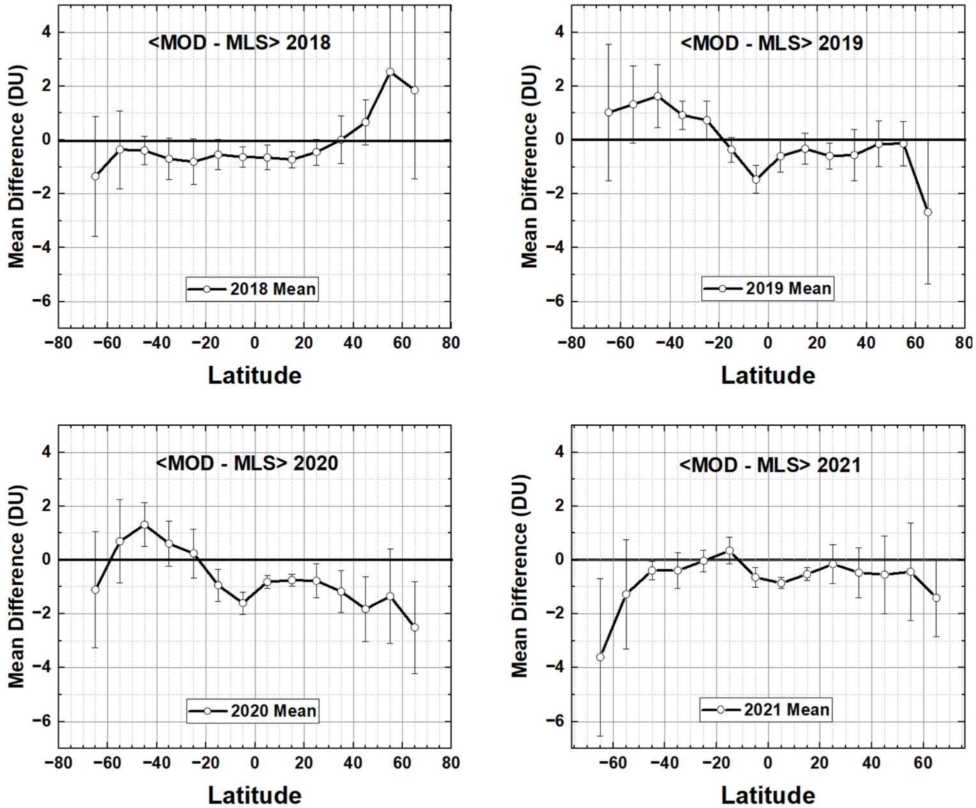


Fig. A3 Annual average <MOD – MLS> for the years 2018 to 2021. Error bars are 2σ', where σ' = standard error of the mean estimated from the average of the deseasonalized time series for each year shown in Figs. A1 and A2.

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.