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°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°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°. 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.

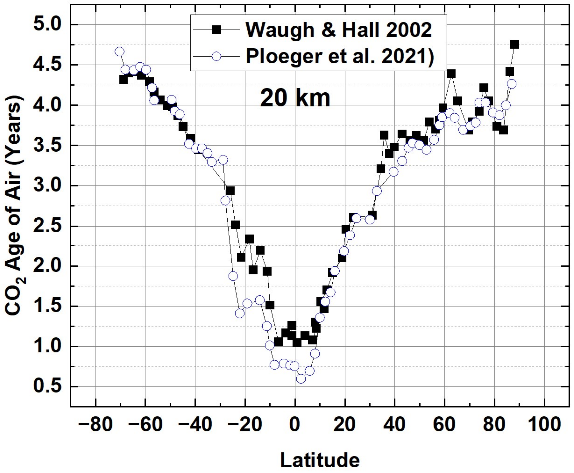

Fig. 8 Age of air derived from CO_2 data (Waugh and Hall, 2002; Ploeger et al., 2021)

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.

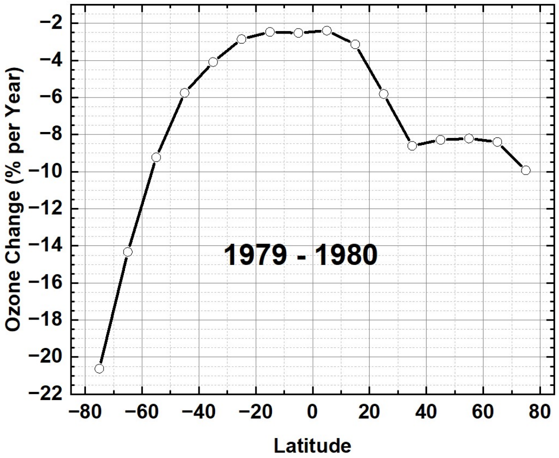

Fig. 8 The percent change in ozone per year in 1979 or 1980

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**