RESPONSE TO REFEREE #1 COMMENTS

In the current manuscript, Roy et al. have used the MERR2 reanalysis data set to look at the inter annual variability in the Antarctic stratosphere for 2013-2020. They have also used the MLS O3 measurement combined with a chemical transport model REPROBUS to derive the ozone loss inside the Antarctic region for the past 8 years. The authors have investigated the causes of Antarctic polar ozone loss mainly focusing on different stratosphere and Chlorine activation by looking at the observed ClO evolution from MLS. The current study and methods used here are not NEW and most of the results from the current manuscript are understandable, i.e., most conclusions are consistent with previous studies. There is no exciting result or some interesting sciences to be addressed from this work. However, the paper is well written and organized. The message in the current version is clear. The quantification of chemical ozone loss for the Antarctic polar region under different meteorological conditions is still useful for the atmospheric community. The paper is still publishable at ACP after major revision. Detailed comments are seen below:
Thank you very much for your time, review and positive comments on the MS. Please find answers to specific comments below. We do hope that the referee will find the revised version more interesting and recommend a publication very soon.

Content**:** In the abstract/introduction, the authors have mentioned that "ozone depletion episodes can change precipitation". How significant causes the precipitation changes due to ozone depletion? Is there any strong evidence to support it?

Done. Yes, there have been studies that showed that the ozone depletion has caused the shift in the westerly jet in the midlatitudes which would lead to increased rainfall there. For instance, please see Kang et al. (2011), Kang et al. (2013), Thompson and Solomon (2002) and Gillett and Thompson (2003). Since it is mentioned in Abstract, we cannot cite references there. Thank you for understanding.

Line 18 in the first page, it is not correct to say "quantifies the ozone loss.. using satellite measurements" because the authors also use the passive ozone from the REPORUBUS model.

Done. This is rephrased as, using satellite measurements and passive ozone simulations, in lines 73-74,79-81.

Following on 2), there are some inconsistencies in the current version. Line 88 says ozone loss is from MLS satellites, Line has caused some confusion about the "ozone loss". Then Line 87 in Page 3 says "we calculate the ozone loss using the REPROBUS model simulations' '. Then the authors mentioned they have calculated the ozone loss in Line 94 Page 4 "the loss is computed by subtracting the measured ozone from the modeled passive tracer....".

Done. The ozone loss is calculated as follows; The observed ozone from MLS is subtracted from the Passive ozone (i.e. the ozone simulated by switching the chemistry off) simulated by the model. This is called passive method, which is a widely used ozone loss estimation method (e.g. Gautail et al., 2005; Kuttippurath et al., 2013). This is mentioned in lines 73-74,79-81.

Line 92 in Page 4 mentioned that the "passive tracer identical to the ozone was initialized on July 1 of each year and continued until the end of November ", but there is a weird large ozone loss in early July in Figure 4. It looks to me that the REPROBUS has not simulated ozone well compared with MLS data most of the years from 2013-2020. The model simulations are even worse for 2018 and 2022 when looking at the first July ozone loss figures. Can you explain why? Is tracer transport/chemistry or other processes causing these discrepancies for 1 July 2018 and 2020.?

Done. Yes, this problem of initialisation and thus the tracer simulations. Therefore, we have removed tracer simulations up to 10 June 2018 and 20 July 2019 and we have not calculated ozone loss. This is mentioned in lines 160–163, and see the revised Figure 4.

Most of the main papers, the authors claimed that "observe" ozone loss. This is only true if REPROBUS can reproduce the observed MLS ozone during the period, but this is not the case (please see 3)).

Done. In fact, we used the model simulations only for ozone loss estimation, not for model-observation comparisons as it is beyond the scope of this paper. The term "observed ozone loss" is used to differentiate it from the modelled ozone loss, as in the previous studies. This is mentioned in line 73-74, 79-81.

The calculation of PSC areas. There is nowhere to mention what the values of HNO3, H2O etc (and where they are from) are used for the PSC diagnostics based on thermodynamic equilibrium.

Done. The PSC area is calculated assuming a fixed profile of nitric acid with a concentration of 4.97 ppt at 460 K. The concentration of water vapour used was about 5 ppm. Further details are provided on https://ozonewatch.gsfc.nasa.gov/meteorology/temp_2022_MERRA2_SH.html) . These are mentioned in lines 69–70.

Line 46 in Page 2, it would be better to specific the region either altitude/regions more specifically for "significant recovery trends in the ozone"

Done. In the lower stratosphere, mentioned in lines 46–47.

Line 49 in page 2, what is the value of "the positive ozone trends"? Based on the current version, have authors also compared the ozone loss over the period of 2013-2020 the period 2001-2017? This would be interesting to know if they have also seen something "A reduction in the saturation of ozone loss" over the period 2013-2017, may be similar or even significant smaller ozone loss rate over 2013-2020 compared to 2001-2017, this will make it robust to say "confirming the positive ozone trends"

Done. We have not presented the trends in ozone here, as it is completely a different topic. Instead, we are looking at the interannual variability of ozone loss and chlorine activation. Also, please note that 8 years of data is not enough for trend analysis and also to make robust statistics. On the other hand, we have compared the ozone loss for all winters using the same criterion in **Figure 5**. Please find the discussion in lines 192–222. A new section (lines 212–222 and **new Table 1** is also added now.

Line 60 in Page 60, why only choose these three years? Better to add other examples here.

Done. Please find the corrected statement in line **141**

Lines 89-90. It looks that REPROBUS is forced by ECMWF operational analyses, which has not nudged the satellite/in-situ observations. Please note that ECMWF operational analysis has changed resolution to 137 vertical levels from 2013, not sure why the paper cites Dee etal. (2011) which is mainly for the description of ERA-Interim reanalysis. Of course, this will have some changes in the model simulations if the authors using the simulation forced by ERA5 (as an example).

Done. Sorry for the mistake. We have rephrased this. Please find it in lines 70-73.

Methods. Since the loss calculation is based on the equivalent latitude (Line 95 in page 4), the authors still use the geographic averaged latitude to do other calculations (for example, temperature, PSC etc..). I would suggest the authors use the same criteria to re-make the figures.

Done. Please note that these types of analyses are mostly performed for polar cap temperature, winds, etc, which is why we have presented the analysis this way. These are also needed to check major and minor warming criteria, as we presented in lines 100–103. In addition, this is also needed to compare with previous studies (e.g. lines 102–103). Therefore, we have kept the original analyses for Figures 1 and 2. However, we respect the referee's comment and we have done the temperature and wind analysis inside the vortex and it is presented in Figure S1 and related text in lines 136–142.

some results should be carefully made, there seems some results mentioned by the authors are not consistent with what has been shown in Figures. For example Line 108 in Page 4, but I can still see the lowest temperature for 2015 occurs in the early September, not in August in the top panel of Figure 1.

Done. This is corrected in lines 104–105.

Why use "growth of temperature", then "descend", "descent". They are improper used for the temperature.

Done. This is made consistent and no such words are used now in the MS.

Line 153, "the vortex lasted the longest in 2015", but looking at Figure1, it seems to me "2020" has the long-lasting cold polar vortex.

Done. Yes, this is rephrased and corrected in lines 96–98, 104–105.

Sometimes there is no explanation for the results shown. For example, Line 171 in Page 6, what causes the still large ozone loss in the upper stratosphere? The authors claimed "The loss is less than 1.4 ppmv in the upper stratosphere in all years".

Done. This is explained in lines 157–158.

For the ozone loss, the authors only look at the ozone loss in different ways (sometimes, they gave largest ozone loss values using different altitudes or averaged different regions or periods). I would suggest the authors add the partical column ozone loss, then make one table to list the partial column ozone loss, peak ozone loss, averaged ozone loss etc, which should make the readers understand the key ozone loss results from 2013-2020.

Done. Please note that Figure 5 is made for the comparison for different winters as it shows the amount of PSC, ClO and ozone loss computed using the same criterion for all winters. However, as suggested, we have also made the partial column loss comparison. Please see the **new Table 1** and related text lines 212–222.

Again, there is inconsistency in the text and the caption of Figures. For example, Line 206 (mean of the ClO values...." and Figure 5 "peak ClO measurements". For Figure 5, there is no explanation why 2015 has the largest PSC areas than other years, which is very hard to see from all the figures including Figures 1 and 2.

Done. These are corrected and explained now in lines 192–193, 197–198. Thank you.

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