Referee 2 report with author replies

We thank this referee for the comments and suggestions; our point-by-point answers and clarifications are listed below (after **Reply:**).

Review of "Trends in measured and modeled lower stratospheric Antarctic vortex ozone over the past two decades" by L. Froidevaux et al., 2026

This paper takes a deep dive into the long-term behavior of the winter/spring Antarctic lower-stratospheric ozone using MLS data and specified dynamics chemistry model simulations. The analysis focuses on ozone and related species and diagnostics within the polar vortex rather than using zonal averages. This is one of the things I really like about this work as this approach clearly isolates processes responsible for heterogeneous ozone depletion from those occurring within the mid-latitudinal air mass. **Perhaps the authors' motivation for a vortex-centered analysis could be mentioned in the introduction.** The paper also looks at the trends in winter/spring chemical ozone change derived from satellite data.

The results are consistent with the recovery of Antarctic ozone, reported in the 2018 and 2022 Ozone Assessments and represent a nice refinement of the existing literature. It is a good and interesting paper, certainly worthy of prompt publication. I only have a few minor comments and suggestions, which should be easy to address. I wish this work came out a little sooner and could be included in the 2026 Ozone Assessment!

Great work!
Kris Wargan

Perhaps the authors' motivation for a vortex-centered analysis could be mentioned in the introduction.
Reply: Yes. In answer also to a comment by Referee 1, we have added a few sentences to further motivate and distinguish this work. Please see the Introduction in the revised manuscript (LL89-99), which reads as follows:
"Two decades of high-quality MLS data provide a record of sufficient length for trend analysis. Moreover, we select abundance profiles obtained within the Antarctic vortex. Such a vortex-centered analysis takes into account variations in the dynamical characteristics (e.g., size, shape, and position) of the Antarctic vortex and thus better isolates the effects of chemical depletion in the ozone hole than the use of geographic latitude bins, which inevitably mix inside-vortex and midlatitude air." See also LL99-101, and LL104-106.

Technical and editorial comments

L51. "Slow" compared to what? Presumably to the rate of increase in the second half of the 20th century(?).
Reply: Yes, this was meant to be implied but not made clear enough; however, this is a long sentence already, so we just deleted the word "slow".

L56. Do you need “slowly” here? It seems subjective.
Reply: OK, we deleted “slowly”.

L63. The WMO 2022 citation presumably refers to Chapter 4 of the 2022 Assessment, which was previously (and later) cited as Chipperfield, Santee et al. (2022) in line 42.
Reply: Yes, we have now specified the Chipperfield, Santee et al. (2022) chapter.

L68. Again, WMO 2022 and Chipperfield and Santee (2022) are cited in the same line. Do you really mean to distinguish between the full report and the chapter? Perhaps it would make sense to just cite WMO (2022) consistently for the purpose of this paper. I don’t feel strongly about this either way; just pointing it out.
Reply: In this case, we now point to the “latter reference”, which is the specific chapter reference. More generally, in the revised manuscript, we have specified the chapter, unless there is a more general topic, or something covered in more than one chapter.

Section 2.1. Since MLS version 6 is already available, how about a quick note saying that v5 is very similar to it as far as the species considered here go?
Reply: Done; we have now added such a sentence at the end of Section 2.1 (LL138-141).

L135 [now L150]. It seems strange that the explanation of how the second simulation differs from the first is given without having described the first one. In fact, the first simulation doesn’t seem to be explained at all.
Reply: We have now moved the 3 relevant scenario sentences that were mentioned later in this section to the part right before the mention of this 2nd simulation (LL145-150), which only differs from the main simulation in terms of the halogen sources (ODS) scheme (variable versus fixed). This now flows better and should avoid the concern mentioned here. These sentences are as follows: “The first model scenario used here is based on historical forcings (and recent updates) from the Climate Model Intercomparison Project – Phase 6 (CMIP6; Meinshausen et al., 2017). The forcings include greenhouse gases (CH_4, N_2O, and CO_2) and organic halogens. After 2014, the greenhouse gas and organic halogen inputs follow the CMIP6 SSP5-8.5 scenario that projects inputs beyond 2014 (O’Neill et al., 2016; Riahi et al., 2017; Meinshausen et al., 2020).”

LL234–235 [now LL252–253]. Can this be verified, e.g., by comparing MLS and WACCM N_2O? Rather than transport, can the representation of gas-phase chemistry in WACCM be a factor?
Reply: A model underestimate of the (multi-year average) downward transport would bring down too little ozone to altitudes below the ozone peak in the upper region mentioned, and this deficit would show up as a negative difference, as shown for the WACCM – MLS plot in Fig. 3b. A similar plot for vortex MLS N_2O compared to WACCM (not shown in the manuscript) shows an excess in the modeled N_2O versus MLS for this general region/time. N_2O decreases with altitude, so both comparisons are broadly consistent with a model underestimate (on average) of the downward transport. Given the good overall current knowledge of polar ozone-related gas-phase chemistry, we do not see how an issue with the chemical representation could contribute much; furthermore, such an issue would not necessarily explain both N_2O and O_3 comparisons/differences.

We have added the following clarification (yellow highlights) regarding N_2O [LL251–255 in the revised manuscript]: “The upper region is where the WACCM values are smaller than the MLS abundances, possibly because the multi-year average downward transport velocities are too small in the model; this conjecture seems to be qualitatively confirmed by a similar plot (not shown here) for N_2O, indicating that WACCM N_2O values are larger than MLS N_2O in the relevant pressure range.”

LL320-332 [now LL342–344]. Why not just use the actual QBO and solar cycle as proxies?
Reply: For our analyses, we have used weekly-averaged data over many years, which makes it inconvenient and difficult to use monthly-averaged proxies. Furthermore, yearly periodic functions enabled us to readily change to (or add) a 5-year or 8-year cycle, for example, to check on other types of “decadal” variability.

LL347-348 [now LL367-368]. It’s not clear to me what this proxy is exactly. “Year-to-year variability” is vague. The caption of Figure 9 confuses things further with “a scaled version of the variability in the $VPSC_{sun}$ proxy”. If *VPSCsun* is already defined as variability, the caption suggests that we’re looking at the variability of that variability. I believe what you mean here is simply $VPSC_{sun}$ and the word “variability” is used too many times. Perhaps I’ve misunderstood something.
Reply: The original quantities for this proxy term (Fig. 8) are an estimated PSC fractional area of the SH hemisphere (added over the vertical coordinate for a volume-like quantity) and vortex area in sunlight (also, as a fractional area of the SH hemisphere). We have now specified “(expressed as a fraction of the SH hemisphere area)” in the text, and we now say, “This product …is used as a proxy term” here, rather than “variability”. However, see more details in the response below as well, as the year-to-year change in the proxy is what is fitted to the ozone changes; this is (generally speaking) variability.

LL365-366 [now L384]. A few questions here. The text says “Variability associated with the lower stratospheric $VPSC_{sun}$”; again, variability of what? Or is it just $VPSC_{sun}$ itself? The discussion and the shape of the lines in Fig. 9 suggest that it’s simply $VPSC_{sun}$ but in that case why is it in the units of ppmv? The caption says that it is a scaled version of it. Scaled by what?
Reply: It is the relative changes in this proxy variable from year to year that could drive Antarctic ozone changes from year to year (for any given winter or spring week), and it is this relative change that is fitted for. The proxy units are expressed as a fractional volume (integrated fractional area in the lower region of the vortex), which is really unitless, times the fractional sunlit vortex area (also unitless). This proxy variable is scaled to fit within the Figure 9 bounds; so the multiplicative scaling is an average of the ozone abundances divided by the average of the unitless proxy.

The text now reads “year-to-year relative changes in the lower stratospheric $VPSC_{sun}$ proxy”. Regarding the scaling, we have added the following in the Fig. 9 caption. “; the scaling (using the average ozone value over the years divided by the average relative change in the proxy) is applied here to show the relative changes in this proxy from year to year on the same scale as the ozone ranges in each panel.”

LL390-394 [now LL409-416]. Presumably some of that variability in transport is implicitly included in $VPSC_{sun}$. The latter is derived from vortex temperature and influenced by vortex size (vortex area exposed to sunlight). Can you briefly comment on that?

Reply: Yes, we agree with this, although the detailed relative influence of chemistry versus transport is not necessarily easy to cleanly separate in terms of proxies. We have added the following in the text: "Some transport-related variability is included in the $VPSC_{sun}$ proxy through the influence of temperatures and vortex area. However, we focus on winter/early spring periods when chlorine and bromine chemical effects are at their strongest over the well-isolated lower stratospheric Antarctic vortex."

LL445-456 [now LL467-468]. The mid-to-late October trends are negative (although not statistically significant) above the 50-hPa level. This brings to mind Fig 3 in Kessenich et al. (2023) and Fig. 4 in He et al. (2026). This may be worth mentioning and discussing briefly because the latter two papers looked at zonal averages (60–90S), while your analysis considers vortex-averaged ozone. Note that Extended Data Fig. 8 in Wang et al. (2025) does something similar (for 2005–2018) but their vortex-average trends are mostly positive (https://www.nature.com/articles/s41586-025-08640-9/figures/12). Granted the latter study omits the large ozone holes in 2020–2023. I feel that some discussion of your result in the context of these other studies could be illuminating.

Reply: We appreciate the pointers to He et al. (2026) and the other reference-specific comments mentioned here. Direct comparisons of various trend studies are complicated by the use of different periods, methods, and regions, as well as the lack of error bars in some publications. We have provided more discussion in Sect. 3.2.1. See the revised text at LL467-505 (with some text moved there from its previous location in the Conclusions, for better flow). As a result of the movement of text from the Conclusions, Figs. S5 through S9 had to be renumbered (original Fig. S9 became new Fig. S5, with subsequent figures renumbered accordingly).

Also, we acknowledge the need for further work on more detailed comparisons or assessments of Antarctic trend results in the Conclusions [LL599-602]: "Other recent Antarctic trend results (see Sect. 3.2.1) show broad agreement with the positive September trends obtained here for pressures larger than 50 hPa; however, more detailed investigations into some of the larger differences are beyond the scope of this work."

L463 [now L512]. "AAO index variation" seems like an unnecessarily complicated way to say "AAO".

Reply: We now specify the "winter/spring average AAO index changes" [as a function of year], as this variation is what we tried to account for in the analyses.

L466 [now L515]. Related to the comment above, is "proxy related to AAO variability" just AAO? Or is it something derived from AAO?

Reply: It is the year-to-year changes in AAO index, as clarified above; we have decided to simplify the wording here to "an AAO proxy".

Section 3.2.2. This is me being nit-picky about double negatives. Figures in this section show ozone loss having negative values. That may be confusing: strictly speaking, negative ozone loss would mean ozone gain, wouldn't it? I would prefer calling it "chemical ozone change" or something like that but I leave it to the authors to decide.

Reply: We did use the term "chemical changes" at L547, and we have now changed the x-axis labels in Fig. 18 to "Chemical O_3 Change" (with a negative value representing a loss).

L510+ [now L559+]. Statistical significance is a somewhat arbitrary concept (e.g.https://www.nature.com/articles/d41586-019-00857-9). Isn't there something to be said for the fact that the majority of the trends in maximum loss (as well as the 19 September ones) below 600 K are positive? Would the layer-integrated trend be positive and statistically significant?
Reply: One should neither over-interpret nor under-interpret statistical significance, as noted in the above reference… and in this case, most of the trend results (9 points out of 12 below 600 K) are consistent with a zero trend, whereas only 3 points are consistent with a positive trend. Most points show a small positive "tendency", rather than a negative one, but this is not a very robust detection. A layer-integrated trend would also be consistent with a zero trend, using the 2σ criterion. The interannual variabilities that confound the individual trend determinations are not uncorrelated. These results should mainly "speak for themselves." Our concluding statements (LL634-640) try to give both sides of the story: "While the Match results show some reductions in seasonal lower stratospheric Antarctic chemical ozone loss over the 2005 to 2023 period, consistent with a positive trend (based on simple linear regression) of order 5–15 ppbv yr^{-1}, we obtain statistical significance (as defined in this work) for only a limited subset of cases in mid-to-late September. However, many of the lower stratospheric Match results are consistent with a zero trend; there is also a large spread in these trend results as a function of height (potential temperature) inside the vortex."

LL524-525 [now L573-574]. What ODSs does the other simulation use? Again, only WACCM-fix (control) is described in the paper.
Reply: Since we made some simple changes to clarify this point for (original) L135 above, and this is just a concluding section reviewing what was done, we feel that there is no need to restart any explanations having to do with simulation differences here (furthermore it would add unnecessary length). Therefore, no changes were made here in response to this comment.

L577 [Now L620]. Why "slow"? Slower than expected? Slow compared to the 20th century decline? It's a subjective assessment unless defined. I suggest just deleting this adjective.
Reply: Yes, it has been deleted.

L586 [now L629]. Please delete the extra "however".
Reply: Yes, done.

LL594-595. Nonetheless, the trend is consistent with recovery. Statistical significance is a somewhat arbitrary concept, e.g. https://www.nature.com/articles/d41586-019-00857-9.
Reply: We do agree with the above article's cautionary words against either over-interpretation or under-interpretation of "statistical significance". Given that many of the Match results are also actually consistent with a zero trend, while some are consistent with a positive trend (given our definition of significance), we think that stating the results as follows should make the point that it is not a strongly consistent result with altitude. The yellow highlights below (and in the annotated manuscript) are additions to the original text (see LL637-640).
"…we obtain statistical significance (as defined in this work) for only a limited subset of cases in mid-to-late September. However, many of the lower stratospheric Match results are consistent with a zero trend; there is also a large spread in these trend results as a function of height (potential temperature) inside the vortex."

LL605–609 [now LL649-653]. I greatly appreciate this remark. While STRIVE has been selected, it's not up in orbit yet and, as a community, we need to keep pushing for more satellite measurements.
Reply: Agreed.

Figure 4 caption. I'm not sure what a "mean PDF" is. I don't believe this is actually an average of PDFs. Also, please state what those PDFs are of (MLS ozone mixing ratio).
Reply: We have added more details in the first part of this caption, as follows: "Figure 4. (a) A histogram of all MLS lower-stratospheric vortex O_3 observations from the week of September 9–15 for 2004–2023, normalized separately for each level, representing the underlying PDF. (b) PDF (histogram) trends shown are the slopes from linear fits within each bin to values from histograms prepared as in (a), but separately for each year."

Figure 8. If the thick lines are obtained by integrating the thin ones up to a given day, how can they ever show a decrease in September/October (panels a, c, d, and f)? For a cumulative quantity to decrease the integrand would have to be negative, which it isn't.
Reply: We corrected the caption to "time-averaged" rather than "cumulative"; the thick lines are averages up to a given day (starting on April 1), as opposed to a straight summation, and such averages do show a decrease late in the season. The text referring to this quantity was also changed (L373) to "time-averaged (mean from April 1 to date)".

Additional references
He, H., Chipperfield, M. P., Dhomse, S. S., Feng, W., Chang, S., Li, Y., Weber, M., and Heddell, S.: Analysis of Antarctic ozone trends from 1979 to 2023, EGUsphere [preprint], https://doi.org/10.5194/egusphere-2026-560, 2026. [Accepted]
Reply: This reference has been added to the manuscript; see the comment above, where this reference is now discussed.