Referee 1 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:**).

This manuscript is well-written. I have only minor comments on this.

1. The paper should be better motivated, rather than saying MLS has long-term data and was devised to track ozone and associated chemistry from space. I agree this, but there are also MLS-led studies on the same topic, although for different time periods.

   Reply: We have added a few sentences to further motivate and distinguish this work. Please see the revised manuscript (LL89-109, highlighted portions of text):

   "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."

   Also, our use of vortex-focused PDFs for both MLS and model (sampled like the MLS data) is a novel approach for Antarctic ozone studies, so we have emphasized this as follows (LL100-102): "; the use of these detailed PDFs for weekly data analyses from both MLS and similarly sampled model simulations is a novel approach for Antarctic ozone studies."

   The sentence before last in the Introduction gives additional new motivation, now clarified as follows (yellow highlights): "To further motivate this work and distinguish it from past results, we also discuss another metric, namely trends in Antarctic ozone loss over the period from 2005 to 2023 computed using the Match technique…".

   With the ozone depletion and recovery context provided in the first two paragraphs (and in the associated references), along with the text additions we point to here, we believe that the motivation for this work has been spelled out sufficiently; we bring forth additional data, novel analyses, and careful data/model comparisons regarding the detection of and the uncertainties associated with Antarctic ozone trends and the Antarctic ozone recovery process. It is unfortunate that the very valuable MLS data will not continue for many more years; having a good dataset for careful analyses is indeed part of this work's motivation.

2. The model and measurement differences have to be discussed by comparing with similar studies in the past. There are several CTM simulations and measurement comparisons. Please check whether your analysis is consistent or not in terms of the differences or chlorine activation.

   Reply: Direct comparisons of various trend studies are complicated by the diversity of methods they employ, their analysis of different time periods and regions, and the lack of detailed error bars in some publications. We have added some discussion in Sect. 3.2.1 regarding our primary ozone trend results and comparisons versus a few recent studies (although there are some noted differences in approach, periods, or methods), namely those by Kessenich et al. (2023), Wang et al. (2025), which was already partly discussed in the Conclusion section (with this text now moved to Sect. 3.2.1), and He et al. (2026). See the revised highlighted text at LL467-505, with some text moved there from its previous location in the Conclusions. Finally, we acknowledge in the Conclusions [LL599-602] that there is more work to be done: "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."

3. The slow deactivation is a known issue. ClO and HCl comparisons would show this. However, what about PSCs? Are these well represented in the model? How good is the PSC simulation in the model?

   Reply: We see no reason to add a lot more details to the model description in the manuscript, as there are already references describing this complex topic (as mentioned in the current manuscript). This reply is therefore really intended for clarifications to the referee.

   In the model description section, we state the following: "the polar heterogeneous chemistry module that is important for this work is discussed in Wegner et al. (2013), Solomon et al. (2015), Wilka et al. (2021), and Weimer et al. (2023)."

   ***Additional details for referee 1*:** A useful schematic diagram of this module is represented in Figure 1 of Solomon et al. (2015). This heterogeneous chemistry approach is an "equilibrium state for particle composition" where Polar Stratospheric Cloud processes (type, composition, surface area, and reactivity) are derived at every model timestep. The PSC formation does not consider the history of the air parcel. Observations by the space-borne lidar (CALIOP) indicate that most PSCs consist of a mixture of Supercooled Ternary Solution (STS) droplets and low particle number density Nitric Acid Trihydrate particles (NAT) (Pitts et al., 2009, 2011). These large NAT particles are responsible for denitrifying the stratosphere (e.g., Fahey et al., 2001). The STS particles are mainly responsible for halogen activation and therefore ozone depletion. In Wegner et al. (2013), the WACCM PSC representation was adjusted such that it would be consistent with the STS and NAT evolution observed by CALIOP during winter/spring. This was accomplished by only allowing a fraction of total available HNO_3 to freeze to NAT and the remaining part to form STS. This approach leads to more gradual denitrification, but this denitrification reaches the same extent as simulated in the vortex core at the end of the Antarctic winter. A mixture of both PSC types is present throughout the winter, in agreement with observations. The model gas-phase HNO_3 abundance during this period was also compared to both Aura-MLS and MIPAS observations. The consistent representation of PSC type I particles and subsequent HNO_3 (gas) abundance between model and observations gives a representative ozone depletion (see Wegner et al., 2013). This agreement between model and observations of ozone depletion only holds if the temperature distribution in the model is within ~2K of the observed temperatures. The use of data assimilation temperatures (i.e., MERRA2) meets this requirement. In WACCM, there are also Type II PSCs (ice particles) which can cause dehydration of the lower stratosphere by sedimentation. Type II PSCs can also cause halogen activation; however, this process is less important than that of STS and/or NAT. The condensed phase H_2O distribution is derived in the microphysics component of the underlying climate model. The representation of this process and the impact on gas-phase H_2O is well represented in the stratosphere (again, from comparisons to satellite observations, e.g., Aura-MLS). Finally, the latest PSC heterogeneous rate data from laboratory studies are used.

Additional references (not in main text) for the above clarification details:

Fahey, D. W., et al. (2001), The detection of large HNO_3-containing particles in the winter Arctic stratosphere, Science, 291, 1026–1031, doi:10.1126/science.1057265.

Pitts, M. C., L. R. Poole, and L. W. Thomason (2009), CALIPSO polar stratospheric cloud observations: Second-generation detection algorithm and composition discrimination, Atmos. Chem. Phys., 9, 7577–7589.

Pitts, M. C., L. R. Poole, A. Dörnbrack, and L. W. Thomason (2011), The 2009–2010 Arctic polar stratospheric cloud season: A CALIPSO perspective, Atmos. Chem. Phys., 11, 2161–2177, doi:10.5194/acp-11-2161-2011.

4. Vortex edge definition and estimation are not clear. Please elaborate.

Reply: The first paragraph of Sect. 3.1 now provides more details regarding the vortex edge definition used here. The revised text reads as follows [LL223-231]: "The Antarctic vortex edge is defined using a height-dependent threshold of scaled potential vorticity (Lawrence et al., 2018). Specifically, the Antarctic vortex edge at each level is determined from the climatological average over April through November of the maximum PV gradient as a function of equivalent latitude (the latitude that would enclose the same area between it and the pole as a given PV contour, Butchart and Remsberg, 1986). A constant value at each level is used because applying any widely used vortex edge identification method (including those based on PV gradients such as this one) on a daily basis commonly leads to large spurious variations from day to day that may compromise comparisons of different dates and seasons (e.g., Manney et al., 2007; Lawrence and Manney, 2018; Lawrence et al., 2018)."

5. Line 251 [now L270]: Is there any reason for this difference?

   Reply: This comment refers to the following sentence: "The differences between WACCM and WACCM-fix-EESC trends are largest at 68.1 hPa and lose significance in this analysis by 14.7 hPa." The reason has to do with the only difference between these two runs, namely, the variable versus fixed ODS emission schemes, and no significant ozone trend is expected in the fixed ODS case. In fact, this is already explained two sentences earlier, as follows (LL267-268): "However, the trends obtained from the WACCM-fix-EESC simulation are consistent with a trend value of zero, as one might expect from such a fixed-EESC scenario." The non-fixed scenario leads to generally positive trends in ozone (as part of the ozone recovery process). We believe that this is clear enough (no changes were made).