

## 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 midlatitudinal 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

### Technical and editorial comments

L51. “Slow” compared to what? Presumably to the rate of increase in the second half of the 20<sup>th</sup> century(?).

L56. Do you need “slowly” here? It seems subjective.

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.

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.

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?

L135. 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.

LL234–235. Can this be verified, e.g., by comparing MLS and WACCM N<sub>2</sub>O? Rather than transport, can the representation of gas-phase chemistry in WACCM be a factor?

LL320-332. Why not just use the actual QBO and solar cycle as proxies?

LL347-348. 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<sub>sun</sub> proxy**". If VPSC<sub>sun</sub> 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<sub>sun</sub> and the word "variability" is used too many times. Perhaps I've misunderstood something.

LL365-366. A few questions here. The text says "*Variability associated with the lower stratospheric VPSC<sub>sun</sub>*"; again, variability of what? Or is it just VPSC<sub>sun</sub> itself? The discussion and the shape of the lines in Fig. 9 suggest that it's simply VPSC<sub>sun</sub> 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?

LL390-394. Presumably some of that variability in transport is implicitly included in VPSC<sub>sun</sub>. The latter is derived from vortex temperature and influenced by the vortex size (vortex area exposed to sunlight). Can you briefly comment on that?

LL445-456. 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.

L463. "AAO index variation" seems like an unnecessarily complicated way to say "AAO".

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

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.

L510+ 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?

LL524-525. What ODSs does the other simulation use? Again, only WACCM-fix (control) is described in the paper.

L577. Why “slow”? Slower than expected? Slow compared to the 20<sup>th</sup> century decline? It’s a subjective assessment unless defined. I suggest just deleting this adjective.

L586. Please delete the extra “however”.

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>.

LL605–609. 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.

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).

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.

### **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]