Review of "Quantifying the contribution of transport to Antarctic springtime ozone column variability" by H.E. Kessenich et al. (2025).

This paper seeks to identify drivers of interannual variability of Antarctic ozone in October. Elaborating on the results of Kessenich et al. (2023), the authors link the variability of polar cap TCO with the intensity of mesospheric descent. The paper introduces a novel diagnostic of the latter, the mesospheric parcel altitude (MPA) based on tracking the daily evolution of the polar cap CO. It is shown that MPA is highly correlated with the TCO in October.

The paper is well-written with clear figures and comprehensive captions. MPA is certainly an interesting new metric of mesospheric/stratospheric descent. While the use of CO as a transport tracer of mesospheric air into the polar vortex has a long history, it is good to look at things from a different perspective. I find the discussion of the descending layered pattern of high-low-high ozone especially interesting, although a bit confusing at times, at least to me (see my general comment concerning horizontal vs. vertical transport). However, I do have several serious concerns related to the main message of the paper and some of the methods used. These are delineated in my general comments below. My main criticism is that this paper appears to single out year-to-year fluctuations in mesospheric transport as the primary driver of interannual variability of October TCO but never really demonstrates that this is the case. The analysis is based on correlations between metrics like MPA and TCO. But don't these correlations arise from the fact that both diagnostics (and several others) are correlated with wave activity, which is a primary driver of TCO (as is well established), and not from a causal relationship between mesospheric descent and TCO? It's entirely possible that I grossly misunderstood this point of the paper. I will be happy to be corrected.

Much of it may simply be a matter of rephrasing parts of the manuscript, but since the problem is with what appears to be the main message of this work, I think it's fair to say that I'm asking for major revisions.

General comments

1. The introduction states: "*We seek to formalize the link identified in Kessenich et al. (2023), ultimately hypothesising that October TCO outcomes are dominated by descent-modulated horizontal transport effects*." The way I read it, the claim is that variability in ozone transport in the middle-to-upper portion of the vortex (mesospheric descent reaches down to 25 km / 20 hPa or so) drives interannual variability of TCO in October. I don't think this is actually demonstrated in the paper and I don't believe it is correct. I've tried to estimate how much of the overall year-to-year TCO variability can be attributed to variability of ozone in the part of the vortex affected by

mesospheric descent. I did a quick calculation based on M2-SCREAM, which is basically MLS ozone data. The standard deviation of the 2004–2024 October mean polar cap partial ozone column between 26 hPa and the top of the atmosphere is only 5.43 DU, with the maximum range of ~20 DU. In contrast, for the polar cap TCO, these numbers are as high as 24 DU (the paper finds it to be 35 DU, line 352 of the manuscript) and 112 DU, respectively. Furthermore, the correlation between October TCO and the 26 hPa–TOA partial column is only about 0.55. If this is correct then only a fraction of TCO variability can be "blamed on" what's happening in the mid-to-upper polar stratosphere. This makes sense because of the relatively low air density at those altitudes compared to the lower stratosphere. So why is MPA such a good predictor of TCO? It is known that the main driver of variability of ozone hole sizes and ozone mass deficit is wave activity and polar temperature (also linked to wave activity) (Newman et al., 2004; Huck et al., 2005, already cited, among many other publications including several Ozone Assessments back to the 2006 one). I understand that many of those studies did not focus specifically on October but the point is that wave activity affects the vortex temperature, diabatic descent within the vortex (and ozone resupply), the vortex size and stability, all of which impact the amount of springtime ozone over the high latitudes throughout the stratosphere. Your results show that MPA is highly correlated with wave activity. This certainly makes MPA a good predictor of October TCO but does not establish a causal relationship. I would like to see some clarification here.

2. **Averaging**. First, the procedure for calculating the inner-vortex ozone profile seems very complicated. Why is it done this way and why does it work? How do you define inner vortex? Why not use a dynamical definition of the polar vortex instead, e.g. based on reanalysis PV (Manney et al., 1994; Lawrence et al., 2018)? Other formulations also exist (Nash et al. 1996). Note that PV-based approaches can be refined to distinguish between the vortex core and the outer vortex. It seems circular to use an ozone-based definition of the vortex to estimate vortex ozone. Second, I'm not sure if I understand why CO is averaged zonally instead of within the vortex. Section 2.1 states: "*as stratospheric CO concentrations are very low beyond the edge of the polar vortex, we use a simple high-latitude (75°S–82°S) zonal average of daily CO observations*." But the fact that CO concentrations are near zero outside the vortex is all the more reason to average CO within the vortex if we want to use it as a diagnostic of air descent. When averaged zonally, any dilution from extra-vortex air will result in a large decrease in CO that has nothing to do with the descent but, instead, is a consequence of sharp gradients across the vortex edge. It is also inconsistent with the way "inner vortex" ozone is averaged. Again, it's possible that I misunderstood something.

3. **Transport**. When seeking to explain the three-layered pattern of the “ozone corridor” the authors disentangle chemistry from transport, and conclude, based on model simulations, that the pattern and its interannual variability arise from **horizontal** transport and its variability. But the “horizontal” part is never explained. Polar ozone has significant vertical gradients, which are acted upon by diabatic descent. Why is it assumed that only horizontal transport variability is important?

Specific comments

L114. To quantify the “severity of ozone depletion” one would need to estimate chemical loss throughout the stratosphere. This is not what is done here. Or is the term “depletion” used in a different sense?

LL113-116. Polar cap TCO is indeed one of the standard metrics of Antarctic ozone. Other metrics are the ozone hole area and ozone mass deficit (OMD), and these diagnostics are more directly related to ozone holes as opposed to overall high-latitude ozone. Is TCO sufficiently highly correlated with area and OMD to be used as the sole diagnostic here (see, e.g., See Wang et al. (2025) especially their discussion around Extended Data figures 8 and 8)? Would the classification in Table 1 look the same if the other metrics were used?

L128. There are several ways of doing specified dynamics (various flavors of nudging, replay, etc.). How exactly is it done in this case? Is there a reference for it?

L130. Why not all the way to 2024?

L149. No need to define MERRA-2 again (already defined in LL123-124).

L177. Consistent with what?

L182-183. Why? Number density is not a conserved quantity.

Figure 4. The red circle in panel (a) is barely visible against the red shading.

L231. Why not vertical transport? October ozone maximum over Antarctica is at about 6–7 hPa. At least the lower maximum in Fig. 4 could arise from diabatic descent.

L250. I’m not sure if I see that. To me loss due to NOx reaches down to the lower of the two maxima in O_3 tendency while production stops closer to the upper one.

L278.How is the “inner boundary of the stratospheric polar vortex” defined? Also, again why does it have to be horizontal transport? I’m not saying that it isn’t, but it would be good to see some explanation.

L284. This is the only place where the term “outer boundary” is used. Similarly to “inner boundary” and “inner vortex”, it is never defined. Please, provide those definitions in the methods section.

LL343-349. This is related to my general comment #1. The way I read it Figure 8a, it shows that most of the TCO difference between Weak and Strong/Regular descent years comes from the lower stratosphere. This makes sense because years when the vortex is relatively undisturbed due to weak wave activity tend to have stronger / more widespread heterogeneous depletion in June–September and, consequently less ozone in the lower stratosphere in October/November. In contrast, middle and upper stratospheric ozone difference, while of the same negative sign, contributes very little to the overall difference. The efficacy of MPA as a diagnostic here results from the fact that MPA is highly correlated with wave activity, not from a direct contribution of upper stratospheric transport to the size of the ozone hole. Is that a correct interpretation? At the risk of repeating myself, I think this really needs more discussion because as the paper is written one gets the impression that mesospheric transport controls the TCO (as opposed to be simply correlated with it) in October, which I don’t think is correct.

LL350-354. Strictly speaking a metric can’t “contribute” to a physical process. I interpret this statement as saying that variability of mesospheric descent (which MPA is a measure of) is a major contribution to the size of the ozone holes. But that’s not the case. All this calculation shows is that MPA is a good diagnostic of the variability of October TCO, which is a fine result. But it does not demonstrate that mesospheric descent is responsible for this variability. Conversely, one could also say that TCO variability is a good metric of upper-atmosphere transport. It’s a correlation vs. causality type of thing. I really suggest that this be clearly stated.

LL367-368. Actually, since CO is averaged zonally, not within the vortex, this doesn’t tell us how much time the parcel has spent in the vortex. I would appreciate a comment on this.

Thank you,
Kris Wargan

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