

## ***Interactive comment on “Is our dynamical understanding of the circulation changes associated with the Antarctic ozone hole sensitive to the choice of reanalysis dataset?” by Andrew Orr et al.***

### **Anonymous Referee #1**

Received and published: 23 January 2021

Review of “Is our dynamical understanding of the circulation changes associated with the Antarctic ozone hole sensitive to the choice of reanalysis dataset?”

In this study the authors compare the response of the Antarctic polar vortex to ozone depletion over the years 1980-2001 among four current reanalysis products. The study is well motivated, very well written and, for the most part, most of the main conclusions are supported by the text and figures. To this end, I recommend publication pending minor revisions. At the same time, however, the analysis does omit certain points (i.e. exclusion of parameterized waves in wind budgets) that should be addressed more

C1

explicitly both in the text and through incorporation of a new supplementary figure (see comments below).

Major Comment: The budget analysis does not include the effect of parameterized gravity wave drag. Nor is the tendency due to the potential imbalance caused by the incremental analysis during data assimilation included. While neglecting these terms may not matter much in the lower stratosphere, I have am not convinced that neglecting these terms is likewise trivial in the upper/middle polar stratosphere. At present, though, because the residuals are not shown the reader has no way of evaluating how well the zonal wind budgets are closed in the upper/middle stratosphere. In particular, is it possible that one of the main drivers of the differences between CSFR and the other reanalysis datasets is the contribution from parameterized GWD? Have the authors done the analysis? Is there really no way of getting access to these missing tendencies? If the answer to the latter is no, then at the very least the authors should provide a new supplementary figure that shows how well they can balance the budget for each reanalysis product.

Minor Comments:

1. Line 109: It is a bit odd that there is no mention of vertical resolution in the text nor in Table 1, especially since one would expect this to have an important impact on the representation of the (wave-driven) residual mean circulation. How does vertical resolution differ among the reanalyses? At the very least this information should be added as a new column in the table.
2. Missing primes in labels in Figures 4-8.
3. Line 133-134: The authors write that they do not examine the individual EP flux components as they “requir[e] the vertical derivative of temperature. . .resulting in noisy wave driving”. And yet, later on they examine the EP flux divergence (which will partly reflect some of this undesirable noisiness, albeit somewhat smoothed). The analysis of the EP flux divergence is certainly fine but I would suggest that the authors rephrase

C2

their earlier caveat because it seems inconsistent with the EP flux divergence analysis presented later on in the manuscript.

4. The dynamical inconsistency exhibited by CFSR (i.e. weaker reduction in upward wave activity despite stronger positive wind trend) is an important result as it exemplifies why caution is needed when analyzing momentum budgets in systems utilizing data assimilation. However, assimilation issues aside, how much of this apparent inconsistency just reflects a lack of correctly accounting for the tendency contributed by (unresolved) gravity waves? I suppose the authors address this in Line 417 but the description is extremely brief and speculative. Is there really no way to access these terms (see Major Comment)?

5. Line 420: In reference to the sentence beginning with “They showed that the sum of the wave driving...”. Is this true throughout the vertical? Is the contribution of parameterized waves really not important in the middle and upper stratosphere?

6. The authors never explicitly show what the differences in the trends in the polar vortex and upper troposphere/lower stratosphere imply for the surface trends. Is the SH surface jet response also anomalous for CFSR? This seems like an important point that should be discussed.

---

Interactive comment on Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2020-1288>, 2020.