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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Polar processing in a split vortex: early winter Arctic ozone loss in 2012/13" *by* G. L. Manney et al.

Anonymous Referee #1

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General Comments

The paper presents the dynamical and chemical evolution of the 2012/2013 Northern Hemisphere winter stratosphere using satellite observations of trace gases and polar stratospheric clouds (PSC) along with data assimilation produced meteorological fields. In addition to a wide range of polar vortex averaged diagnostics, the study uses trajectory techniques to isolate the chemical evolution of trace gases from transport during the early winter. Satellite observations from Aura MLS (Microwave Limb Sounder) and CALIPSO (Cloud-Aerosol Lidar and Infrared Pathfinder Satellite Observations) provide a detailed view of PSC processing and associated ozone loss. The two vortices produced by the major SSW (Sudden Stratospheric Warming) of January 2013 are tracked separately in terms of both dynamics and trace gas evolution. These

results are placed in the context of past Aura and CALIPSO observations including direct comparisons with a non-SSW winter (2010/2011) and a SSW winter (2009/2010).

Overall this is an excellent discussion paper. The writing is clear and concise and the figures provide meticulously detailed documentation of the unique, early winter ozone loss of 2012/2013. New results include not only the record early winter ozone loss but also shows the usefulness of tracking the individual parts of split vortex evolution as each part of the split vortex encounters different conditions of sunlight and dynamics. Especially innovative is the use of trajectories grouped by each part of the split vortex to document the differences in chemical ozone loss in each part. The documentation of the 2013/2013 winter ozone loss and polar processing combined with innovative analysis and diagnostics should interest many ACPD readers.

Specific Comments

There are two main points that the authors should address more completely:

1) In several places the authors state that the polar vortex dissipated in mid-February:

Page 4974, Line 22: ". . .vortex dissipated in mid-February."

Page 4991, Line 12: ". . .complete dissipation of the vortex by late February."

Page 5001, Line 18: "...; by mid-February, the vortex became ill-defined. . ."

Yet, the zonal mean winds at 60N recovered during February and remained strong through March and April. Presumably, the post mid-February vortex edge had weaker EPV and trace gas gradients, however, the vortex still had a relatively strong circulation associated with it (see plots for 2012-2013 available on http://acd-ext.gsfc.nasa.gov/Data_services/met/ann_data.html). The final warming appears to be in April. In mid-February the vortex appears to be reforming, not dissipating.

2) Page 4983, paragraph starting on Line 25, concerning the trajectory calculations: Using nearly month long trajectories seems problematic as the longer time trajectories

should have larger errors than the shorter trajectories. Why not use more frequent initial states and keep the trajectories more equal in length? Results in Morris et al. (1995) show trajectory errors getting larger with time, with large errors after about 15 days. The Morris et al. (1995) time dependence of the error growth (due to input wind uncertainties, for example, their Fig. 4a) seems similar to difference between the trajectories and the MLS observations seen in January in Fig. 13a and b for nitrous oxide. Has the trajectory error for long trajectories been evaluated for 2012/2013? Can some of the difference shown in Fig. 13b be explained by the longer trajectories used for times near the end of January? Would the N2O descent rates improve with shorter trajectories?

Reference: Morris, G. A. and co-authors, 1995, Trajectory mapping and applications to data from the Upper Atmosphere Research Satellite, J. Geophys. Res., 100, 16491-16505.

Minor Points:

3) In several places the polar vortex is described as being "well-confined" (Page 4974, Line4; Page 4975, Lines 9 and 14; Page 4991, Line 11, Page 5001, Line 12). From context, this appears to be shorthand for describing a vortex with strong, well-defined, sPV and tracer gradients at the vortex edge. That is, the trace gases are confined within the vortex, not that the vortex itself is confined. However, the phrase "well-confined vortex" could also be interpreted as a small vortex or a vortex that remains confined over a particular region. One solution would be to define a "well-confined vortex" when first used, or alternatively, write out a more complete description of what specifically is meant each time.

4) The abstract gives a good summary of the work, however, it is longer than needed for an abstract and should be edited down to a single paragraph.

5) Page 4979, Line 1-2: The non-standard reference:

http://gmao.gsfc.nasa.gov/products/documents/GEOS-520_to_5110.pdf

can be replaced with:

Molod, A., L. Takacs, M. Suarez and J. Bacmeister, 2014: Development of the GEOS-5 Atmospheric General Circulation Model: Evolution from MERRA to MERRA2. Geosci. Model Dev. Disc., 7, 7575-7617, doi:10.5194/gmdd-7-7575-2014.

The above reference covers the relevant material found in the non-standard reference.

6) Page 4988, Line 7: Is there an explanation for the very large, off-scale peak of vortex-integrated CALIPSO backscatter in January 2010?

7) Page 4992, Line 2: ". . .the altitude of the lowest values decreasing gradually through January." The low HNO3 values (440-580K) in January are visible, however the altitude decreasing with time is difficult to see in Fig. 9b. Is it very small?

8) Page 4996, Line 8: Any ideas or speculation of why there was extensive cold air and PSC activity in early December 2012 that played a key role in the early ozone loss?

9) Page 5011, Line 30: Unless specific figures or text from earlier versions are being referenced, why not reference only the most recent WMO ozone report?

10) Figures 3a, 8c, 8d, 8f: The filled contour colors appear to run off scale at the highest values. If they do, is anything important missing at the high end? In particular would 3a peak more sharply, showing more detailed agreement with Fig. 3b?

11) Figure 10: Are the orbits plotted relative to the vortex edge? Are the orbits nearly the same for 16 and 28 December cases shown?

Technical Corrections

12) Page 4979, Line 12: If this is the first mention of a chemical formula in the body of the text, then the chemical formulas should be spelled out as well.

13) Page 4980, Footnote: Change "theta surfaces" to "potential temperature surfaces"

or "isentropic surfaces".

14) Page 4983, Line 27: "(e.g., WMO, 2007)": The WMO reports cited in the reference section are for 2006, 2010, and 2014. Please correct the text.

Interactive comment on Atmos. Chem. Phys. Discuss., 15, 4973, 2015.