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Atmospheric Chemistry and Physics Discussions

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Interactive Comment

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

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Referee comments are *italicized*; authors' responses are in regular (upright) text.

Anonymous Referee #1

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 C629 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.

We thank the referee for their helpful comments. Our responses are interspersed below.

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

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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.

The distinction here is that the zonal mean view bears very little relation to the confinement of the vortex or its strength as measured by the permeability of its edge (e.g., by PV gradients along the edge). Indeed, in the zonal mean view, the vortex was weakest during January when the two offspring were individually strong and well-isolated – and widely separated, with anticyclones in between going around a latitude circle, such that they "cancelled out" in the zonal mean. In addition, the timing of the recovery depended strongly on altitude, with both westerly zonal mean winds and significant PV gradients along a well-defined vortex edge re-emerging more quickly at higher altitudes. We have added text in the discussion of Figure 6 (thus in the first paragraph of page 4989) saying: "Although the major SSW commenced and the vortex split in early January 2013 (with concurrent reversal of the high latitude zonal mean winds)..." and "Zonal mean winds (not shown) began to increase in February when the reforming vortex was relatively symmetric and pole-centered. The vortex recovered strongly in the middle and upper stratosphere by mid-February, but very weak PV gradients along the edge of the reformed vortex in the lower stratosphere indicate that it was an insignificant transport barrier there at that time." On page 4991 in the discussion of K_{eff} in Figure 8a, we have changed the wording to "...complete dissipation of the vortex as a significant transport barrier by late February...". On page 5001, we have changed the wording to "by mid-February, the vortex no longer represented a significant transport barrier." We hope that these changes clarify the sense in which we are talking about the disappearance of the vortex.

(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.

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(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.

Indeed, the uncertainties in the relatively long trajectories are a cause for concern. Manney et al. (2003) did sensitivity tests using varying durations and reinitialization for RT calculations very similar to those presented here, and concluded that calculations using trajectories 20–40 days long were reasonably accurate, depending on the meteorological situation. However, we also did a test reinitializing every 10–12 days for the period presented here, and did not find substantially different results (but did find some difficulties with advecting air from outside the initialization domain into the region of interest, which were exacerbated by frequent reinitialization). We have added a note to this effect to the paragraph ending on line 22 of page 4984. This is an interesting result, as it suggests that in this case the errors in the RT calculations are more closely related to issues with the 3-D motion fields' accuracy in highly disturbed conditions (and possibly limitations in the relatively coarsely resolved MLS data's ability to capture fully the more complex structure under those conditions) rather than directly to errors accumulated over the length of the trajectory calculations. We have added a note to this effect in the discussion of the RT results, in the paragraph ending on line 20 of page 4998.

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.

We have changed the wording in these instances to more explicitly state the key point (for this paper) of the effectiveness of the vortices as transport barriers, using phrasing such as "effective {or strong} transport barrier" or "within which air remained isolated" at various points.

(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.

The abstract has been condensed substantially and reorganized to address comments from both referees; it is now one paragraph and just over 300 words (down from about 470). The revised abstract is as follows:

"A sudden stratospheric warming (SSW) in early January 2013 caused the Arctic polar vortex to split and temperatures to rapidly rise above the threshold for chlorine activation. However, ozone in the lower stratospheric polar vortex from late December 2012 through early February 2013 reached the lowest values on record for that time of year. Analysis of Aura Microwave Limb Sounder (MLS) trace gas measurements and Cloud-Aerosol Lidar and Infrared Pathfinder Satellite Observations (CALIPSO) polar stratospheric cloud (PSC) data shows that exceptional chemical ozone loss early in the 2012/13 Arctic winter resulted from a unique combination of meteorological conditions associated with the early January 2013 SSW: Unusually low temperatures in

December 2012, offspring vortices within which air remained well isolated for nearly a month after the vortex split, and greater than usual vortex sunlight exposure throughout December 2012 and January 2013. Conditions in the two offspring vortices differed substantially, with the one overlying Canada having lower temperatures, lower nitric acid (HNO_3), lower hydrogen chloride, more sunlight exposure/higher ClO in late January, and a later onset of chlorine deactivation than the one overlying Siberia. MLS HNO_3 and CALIPSO data indicate that PSC activity in December 2012 was more extensive and persistent than at that time in any other Arctic winter in the past decade. Chlorine monoxide (ClO, measured by MLS) rose earlier than previously observed and was the largest on record through mid-January 2013. Enhanced vortex ClO persisted until mid-February despite the cessation of PSC activity when the SSW started. Vortex HNO_3 remained depressed after PSCs had disappeared; passive transport calculations indicate vortex-averaged denitrification of about 4 ppbv. The estimated vortex-averaged chemical ozone loss, $\sim$0.7–0.8 ppmv near 500 K ($\sim$21 km), was the largest December/January loss in the MLS record from 2004/05–2014/15."

(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.

This has been done, thanks for the 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?

There was, indeed, a brief period in January 2010 with record high PSC activity and

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documented synoptic-scale ice PSCs. This is discussed by Pitts et al. (2011) and Dörnbrack et al. (2012), and we have added a sentence to the discussion of Figure 4 describing this.

(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?

The altitude of the lowest values decreases from about 530 to 480 K between about 20 December and mid-January. We have revised the text to give this more specific information, with which we believe the reader can more easily pick out the decrease from the plot (we will also request that Figure 9 be made larger in the final published version, which should also make it easier to discern subtle features).

(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?

This is an extremely interesting question, but saying anything beyond speculation about it is well beyond the scope of this paper. Coy and Pawson (2015) show that the 100hPa vertical component of EP flux from early to past mid-December was small (negative on some days/in some wave numbers) and the website provided by this referee shows that values of 45-75N averaged 100hPa heat flux during this period were exceptionally low. We mention in the revised paper that the exceptional cold in much of December 2012 may be related to these unusually low heat fluxes, which suggest unusually little wave propagation into the stratosphere. It is not clear why this question is referenced to Page 4996, line 8, where the evolution of N_2O in January is being discussed. We have added the comment about the low heat fluxes in December 2012 to the discussion of Figure 2, the paragraph ending on line 25 of Page 4986.

(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?

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We checked the references to WMO reports, and found that we could, indeed, cover these by citing only the 2014 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?

The Figure 8 panels do not show any significant structure in the saturated regions. Figure 3a does show some structure in the saturated region, with the plot with an extended color range possibly emphasizing the similarities with Figure 3b more; the revised paper includes this extended color range version of Figure 3.

(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?

The zero point on the tracks is the point closest to the pole, at the turnaround of the orbit. The tracks are in very nearly the same positions on the two days. This information has been added to the Figure 10 caption, along with a brief description of the geographic location of those tracks, which cross over Iceland.

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.

Done. (HNO_3 was already defined in the introduction.)

(13) Page 4980, Footnote: Change "theta surfaces" to "potential temperature surfaces" or "isentropic surfaces".

Done.

(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.

The reports are indeed for 2006, 2010, and 2014, but the 2006 and 2010 reports were published in 2007 and 2011, respectively, and were cited by the year of publication. However, we now cite only the 2014 report (which was published in 2014) as suggested in an earlier comment.

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

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