Interactive comment on "The major stratospheric final warming in 2016: Dispersal of vortex air and termination of Arctic chemical ozone loss" by Gloria L. Manney and Zachary D. Lawrence

Anonymous Referee #3

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This paper is certainly comprehensive, appropriate for ACP and most probably, correct. It was, however, also difficult to read and review. It comes across, at least to this reviewer, as a so-called "core dump" of information. As a consequence, I readily admit that this review is probably incomplete and that I probably missed pieces of information that to the authors, at least, they would deem critical. My comments are therefore (with one exception) more editorial than scientific.

We thank the referee for their valuable comments regarding our manuscript. As we describe below and in our responses to the other reviewers, we have made numerous modifications that we hope clarify our paper and elucidate how the information we have included is focused on a comprehensive analysis of the major final warming in 2016.

General 1. One general science question that I think could use a greater exposition is the question of the MFW. The authors imply that this hybrid event in 2016 is unusual. Some context as to its occurrence frequency would be helpful. Is this the first to occur in the AURA record?

This event was not the first in the Aura record; the 2004/2005 Arctic winter was similar in that its unusually cold conditions were cut short by an early final warming around 10-12 March 2005. However, 2015/2016 was much colder, and the potential for polar processing more severe. In addition, final warming events as early as these are uncommon. According to the table provided in Hu et al., 2014, only 13 winters between 1958 and 2012 had early final warmings before 1 April, of which only 5 occurred before March 15. We have included additional text in our conclusions section that discusses these points, including differences and similarities in the 2005 and 2016 events.

2. As far as presentation, Section 3 is a case in point as exemplifying my concerns. Figures 1-3 are introduced in rather random order, with lots of information, which, while not technically wrong, may well be irrelevant. The authors present a whole bunch of figures (14 panels in all for Figures 1-3) and then jump back and forth in a scatter shot discussion. This is very taxing to read. The first three paragraphs do not even discuss the 2015-16 season, but rather present a literature review of 3 three previous winters.

Line 27 on page 7 is a good example. The statement is simple- temperatures in a particular year (a year which was not the subject of the present paper) were cold enough to activate chlorine for a prolonged period of time. So why do we need to refer to four separate figure panels (Figure 1a and b Figure 2d and e?) to make this simple point (which again is irrelevant to the subject of this paper that is nominally about 2016)? In fact, I don't understand why Figure 2 is referred to here. Is it because ClO was going up? That is not explained.

We have made several revisions to section 3, including adding text to clarify that we are discussing Figures 1 and 2 together so that we can draw the direct connection between the meteorological diagnostics and the implications of their evolution for changes in the trace gases (e.g., minimum temperatures are directly related to the evolution of HNO3 and H2O via PSC formation and denitrification/dehydration; these are in turn linked to chlorine activation and deactivation; sunlight exposure shown in Figure 1 is directly linked to elevated ClO and ozone loss shown in Figure 2; etc). By discussing these figures in a unified way, we elucidate the dependences of the composition on the meteorological conditions. We have also simplified Figure 1 by removing the V_{NAT}/V_{Vort} panel, which the referee points out below provided little additional information, and reducing the number of lines on the sunlit vortex area panel (now Figure 1c).

3. Adding up all the panels in 15 figures, the paper contains 128 separate graphs. I confess that I found it difficult to subject each and every one to the scrutiny they probably deserve; I do nonetheless strongly suspect that they are not all necessary. As an example, I did examine one specific panel- that of Figure 1b. All references to Figure 1b occur with a simultaneous reference to Figure 1a. I therefore conclude that Figure 1b can't be necessary since it never is referred to independently of Figure 1a. So it should be deleted. Especially since they never describe it (what is V_nat/V_vort?-they briefly mention it on page 9, but not in the context of Figure 1).

We have reduced figures and figure panels where possible, including removing the panel in Figure 1 that the reviewer mentioned as a candidate for deletion. In addition, we have removed two panels of Figure 3, all of Figure 12 (four panels), and four panels in each of Figures 13 and 14 (now Figures 12 and 13). We do stress, however, that one of the main points of our paper is that there is good agreement between the dynamics (represented by diagnostics derived from MERRA-2) and chemistry/transport as seen by measurements from MLS, which all paint a consistent picture of what happened throughout the 2015/16 Arctic winter season, and in order to make this point, we need to show these fields and their evolution. Every figure panel that is included in the paper is used to support some point; a majority of them are referred to not only when initially

discussed, but also referred back to to support points made about or show consistency with succeeding figures.

4. I also think Figure 3 is unnecessary. Not that it's technically incorrect, but it adds no new information that is not conveyed in Figures 1 and 2. Indeed, their concluding sentence on lines 19-20 of page 9 can easily be gleamed from Figures 1-2.

As per our reduction of figure panels mentioned above, we have removed the panels showing the vertically summed number of days below the PSC thresholds. We now only show the winter mean V_{NAT}/V_{Vort} and V_{ice}/V_{Vort}. Although there is some overlap with the information conveyed in Figures 1 and 2, Figure 3 is still necessary to show that the winter mean polar processing and ozone loss potential in 2015/16 was unusually large, comparable to that in 2011; this point cannot be seen by looking at Figure 1 alone.

Minor

1. Figures 5-14 (with the exception of Figure 8) are essentially 3 sets of three figures for 850, 490, and 550 potential temperature surfaces. It would be introductory few sentences at the beginning of Section 4 explaining why they chose these three levels. Even if it was empirically determined that they were good representative levels, they should at least say that. As it reads, it just says (for example) Figures 5-7 without telling the reader where you are going with this. You have to read almost 2 pages of the draft before you find out that these 3 figures are for three separate altitudes.

At the end of section 3, we have a transition paragraph that points out the three levels we are going to focus on in section 4 and their significance. In addition, we have added text at the beginning of section 4 noting that parallel figures are going to be shown at three levels to contrast/compare their behavior.

2. Figure 6 vs Figure 7. If I understand correctly, the text on page 11 suggests (line 25, compared with line 7) that one difference is that N2O and O3 do not show mixing out of the vortex at 550K but they do at 490 K. Looking at Figures 6c and 7c, I see no difference. Am I supposed to?

We apologize for a lack of clarity in these statements. The main point in the paragraph on 550K was the consistency of persistent strong trace gas gradients along the vortex edge with the stronger persistent transport barrier seen in PV gradients and K_{eff} at this level. This does indeed imply less mixing out of the vortex at 550K than at 490K. We have revised the text to clarify both points.

3. Figure 1c: what is Max PVG? Those three letters do not appear anywhere else in the text or figure captions.

We apologize for this oversight; max PVG stands for “maximum PV gradients.” We have changed the text in the Figure 1 caption to read: “maximum gradients of scaled potential vorticity as a function of EqL (Max PVG) ...” to make this clear.

4. Figure 15: What do the colors mean? There is a label that says “first”, “second” etc, but doesn’t explain what those terms mean other than “bulk”. Are they related to the colors of various fragments in Figure 14. If so, it should say so.

We have clarified our references to the colors and the regions in the text. Rather than referring to the offspring regions as first, second, etc, we now refer to them as “parent”, “offspring-p”, and “offspring-s” to describe the amount of time these regions existed (the “p” and “s” in offspring-p and offspring-s stand for “persistent” and “short-lived”, respectively). Vortices that persisted for about a day or less are labeled “transient”. We have also added text to the Figure 14 caption to explain these names/references, and they are explained in the text where they are first introduced (in conjunction with Figures 10 and 11).

5. Abstract: Line 20. Where do they show chlorine in the offspring vortices? Figure 15 does not show chlorine. There are cryptic references to chlorine activation and deactivation scattered throughout the text, but I could not find where it pointed to a figure saying “this shows the deactivation of chlorine etc. etc.”

We note explicitly in the text related to Figure 14 regarding shorter lived species that the average values of ClO, HCl, and HNO3 are very similar across the parent and offspring vortices, and hence showing the evolution in individual vortices (as in Figure 14) would only add panels without adding information. We have added text explicitly noting that how ClO evolves in each vortex can be deduced from Figure 2e. Also, the time evolution of ClO is shown clearly in Figures 4e and 6e.