

Interactive comment on “The high Arctic in extreme winters: vortex, temperature, and MLS and ACE-FTS trace gas evolution” by G. L. Manney et al.

G. L. Manney et al.

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We thank the reviewer for their positive and helpful comments. Our responses to the questions and suggested changes are as follows:

The figures in general are significant, although I think that figures 1 to 3 should be a bit larger to give more details. Furthermore I would like to suggest to use different colours for the lines in the figures 7 to 9. It is not so easy to distinguish between orange and light red, and using all the same symbols (dots in this case) does not make things easier.

We will make Figures 1 to 3 substantially larger for the final publication.

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Discussion Paper

We will also change the symbols and/or colors in Figures 7 to 9 (and 13 for consistency).

One first comment about the citations: A lot of papers cited here are still submitted, to be submitted or in press. This is not too surprising (and not too much of a problem), since the paper under review is to appear in a special issue. But some of the most interesting features in the winter 2006 are described in the paper Manney et al. (2007b) submitted to GRL, which to my knowledge had not been published yet. I could not download it from the website mentioned, where this paper should be available. Since Manney et al. (2007b) seems to be very interesting concerning the issues raised in the paper under review, I would like to suggest to give more information about the 2006 major warming in this paper, if Manney et al. (2007b) will not be published soon.

We are quite frustrated about this paper (Manney et al., 2007b)! It has been submitted to JGR for over two months (after being rejected by GRL for reasons unrelated to the scientific content), and the reviews are now overdue – when I inquired two weeks ago, they were waiting for “the last” review, which was due on 6 September. So we are hopeful that it will be published prior to this one. The material in the JGR paper does, indeed, shed light on some aspects of the results in this ACPD paper, and if it is not published by the time we revise the ACP paper, we will add additional summary material on its results where appropriate.

My apologies for the problem with the link to the MLS website for this one – this has been corrected, and the JGR manuscript is now available on that site (we also appreciate any comments from anyone interested on that paper).

The authors use several different meteorological data sets. From the discussion of Figure 5 (and later Figure 13) it seems, that the GEOS-4 data set may be not the best choice, especially for strongly perturbed winters. Do other meteorological data sets (e.g. ECMWF, which is used in Figure 9) show better results compared to MLS and SABER? Looking at Figure 9, I would suggest to have a look at ECMWF data for the

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winters 2004 and 2005 as well, even if the high vertical resolution with 91 level came in operation as late as February 2006. Even the 60 level version may show better results for the altitude range above 1 hPa than GEOS-4.

If the GEOS-4 data have problems with accurately capturing the stratopause behaviour after the SSWs, and other data sets have not, would using a different data set then result in substantial changes concerning the synoptic overview?

The above two questions are closely related, and to a large degree are addressed in Manney et al. (2007b). It is shown therein that even the L91 ECMWF data do not, in fact, do any better in reproducing high altitude (upper stratosphere/stratopause/lower mesosphere) behavior than does GEOS-4 (nor does GEOS-5 do substantially better, beyond a slight improvement in stratopause temperature) – it does very poorly in different ways (e.g., stratopause too cold rather than too warm). On anonymous ftp on mls.jpl.nasa.gov in pub/outgoing/manney/sparcda/slidesDAWG2007p13p15.pdf are two slides (from talk at SPARC Data Assimilation/IPY Workshops, 4-7 Sep 2007, full slides soon to be posted on website for those Workshops) showing versions of Figures 5 and 6 from the ACP paper with a number of other analyses shown (ECMWF for 2005-2006), which also indicate the same results. In addition, they show that all analyses capture the tropopause behavior quite well, with GEOS-4 doing approximately as well as GEOS-5 and ECMWF. We will add text to this effect in the final ACP paper, but, with the citation to Manney et al. (2007b) (with a summary such as this included if that paper is not published yet), we do not think it necessary to add to the already complex figures shown here.

A similar question arises regarding the discussion of the temperature cross-sections (Figures 10 and 11). If there are features (like stratopause double peaks or strong temperature gradients) which are not observed by SABER or MLS, but are modelled by GEOS-4 with a resolution of 1 x 1.25 degrees horizontal resolution, can the same features be found in the much higher resolved meteorological data like ECMWF? Concerning Figure 6: I miss an explanation for the dotted lines in the upper panel. And

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maybe the Figure caption (as well as that of Figure 5) should include the fact, that these timeseries are for Eureka.

Again, Manney et al. (2007b) show that on selected days during the 2005-2006 winter, the ECMWF data do not, in fact, show additional structure that can be related to measured features at higher altitudes. Because of the Rayleigh friction that is imposed above 5 hPa, we would not necessarily expect this despite the higher resolution. Because the L60 data (prior to 2005-2006) have an even lower top, and poorer vertical resolution, they would be even less likely to capture additional features (and, in fact, do not in general show additional features for the cases we have examined at levels up through 1 hPa, the highest level for which we have the ECMWF L60 data). Nevertheless, we agree with the reviewer that this should be explored further, and plan to examine cross-sections like Figures 10 and 11 for the dates of lidar measurements in 2006 when the L91 ECMWF data are operational, as soon as we can get access to the ECMWF data for those periods extracted on a high-resolution grid. It will probably take about 4-5 weeks to get the data and complete the analysis, and we will add discussion of the results to the final version of the paper, in addition to noting, as stated above, reasons why we would not necessarily expect “better” performance in this respect from the ECMWF analyses.

While GEOS-5 or ECMWF-L91 might (arguably) be slightly preferable to GEOS-4, none of the analyses does well in capturing behavior near or above the stratopause; we thus selected GEOS-4 as being the only one of those products that is available for each of the three years that we focus on. For periods during which ECMWF-L91 and/or GEOS-5 data are available, none of our conclusions or significant results would be changed by using these either of these analyses in place of GEOS-4.

The dotted lines in Figure 6 show the WMO tropopause on the vertical scale used for the coldpoint, for comparison – this information will be added to the figure caption. We will also specify in the Figure 5 and 6 captions that the timeseries are at Eureka.

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4 Typographical Corrections

Page 13, 1st para: Something went wrong with the first sentence. I guess there are to sentences, which have been merged, but something is missing in between...

Page 15, 2nd para: Change 'day-to-day' into 'Day-to-day'...

I believe the reviewer is looking at the version we originally submitted – I have verified that both of these were corrected in the version published on the ACPD site.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 10235, 2007.

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