Author Reply (*in italics*) to AMT Report #1 of January 29

Suggestions for revision or reasons for rejection (will be published if the paper is accepted for final publication)

I am glad to hear that my comments have been useful to the authors and that they have considered them all. I am satisfied with the new version and hence I recommend its publication in AMT.
There are a couple of minor points that the authors may still want to consider.

1) Fig.1. The authors clearly explain (and revise the text, L151-153) the low values found at high altitudes at latitudes polewards of 50S.
However, Fig. 1 also shows a maximum at 30S-50S of similar magnitude and at very similar pressure levels than the tertiary maximum discussed in the NH near ~67ºN. I think this region is not affected by the night/day terminator. Since this enhancement is rather unusual in comparison with measurements of other instruments I think they authors should comment on it.

2) Lines 370-372. About the new Fig. 12 and the evolution of the tertiary maximum in this winter. I think the high value of ~6 ppmv of LIMS is realistic and its difference (larger) compared to those reported in Lopez-Puertas et al. (2018) can be reasonably well explained. First, it is not clear to which plot of that reference the authors compared to. One should have in mind most of the data plotted in that work are monthly means and in some cases averaged over several years. The other reason is given by the authors in their reply:
"The enhanced values in February follow the minor SSW of late January and the final SSW event of mid to late February." MIPAS data for 11 Feb 2009, when the stratopause was rather elevated after the large strat-warm of that winter, show O3 peak values of 5.7 ppmv. Note also that SABER data (Smith et al. 2009, Fig. 2, bottom panel) show enhanced O3 tertiary values at 70-83N in 2009 after the strat-warm. Hence, I think LIMS data in Fig. 12 are very reasonable and in good general agreement with other measurements. The strat-warms induce significant enhancements.

1—Figure 1 and lines 151 ff—Thank you for raising this concern. We agree that there is an ozone anomaly in the upper mesosphere near 45S. We now include a sentence about that feature here, in more detail in the last paragraph of Section 6, and with the aid of Figure S5 (see below) of the Supplemental Materials. We believe that the enhanced ozone is an artifact of not accounting for path gradients in the retrieval of V6 temperature and from the use of the incorrect temperatures for the retrieval of ozone.

2—lines 370-372 and Fig. 12 (which is now Fig. 14)—We considered Lopez-Puertas et al. (2018, their Fig. 15), but we have not looked at daily MIPAS or SABER ozone data, as you may have. Accordingly, we made minor changes to the last paragraph of Section 5 to be clear about that.

Additional changes—We removed statements of uncertainty in Section 4 about the chemical changes for air parcels that end up in the region of the LOP. After we submitted our revised manuscript, we conducted chemical calculations along an air parcel trajectory ending in the region of the LOP on January 28. We now show those results in new Figures 11 and 12 (see below) and discuss them in the final paragraph of revised Section 4. Original figures 11-13 are now figures 13-15.

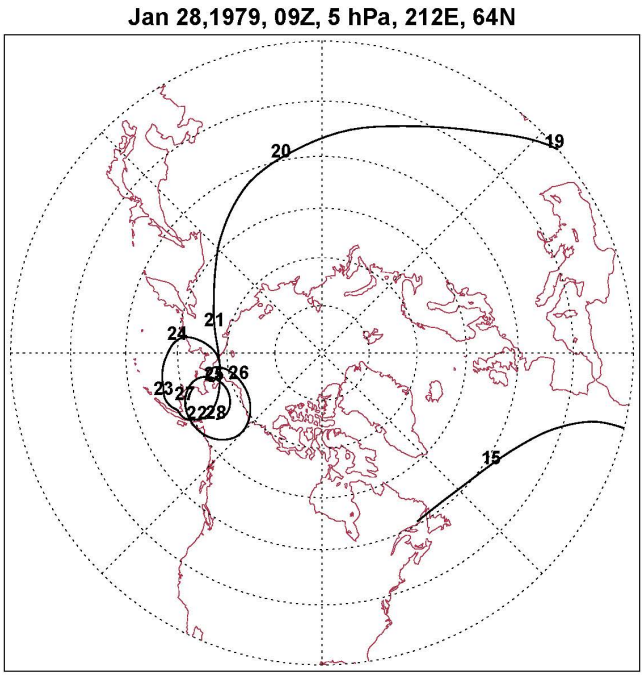

Figure 11—Trajectory of air parcel that ends on January 28 at the location of the LOP. Numbers on the trajectory denote the date, beginning with January 15; the parcel is equatorward of 30°N on January 16-18.

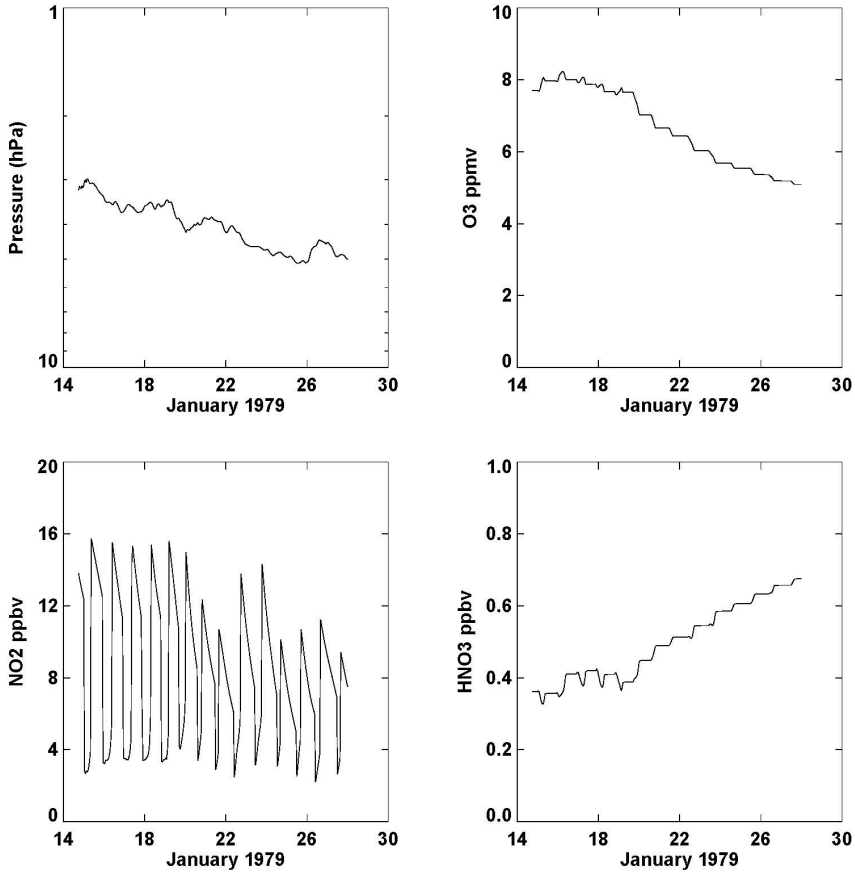

Figure 12—Air parcel history of the changes in its (top left) pressure, (top right) ozone, (bottom left) NO_2, and (bottom right) HNO_3.

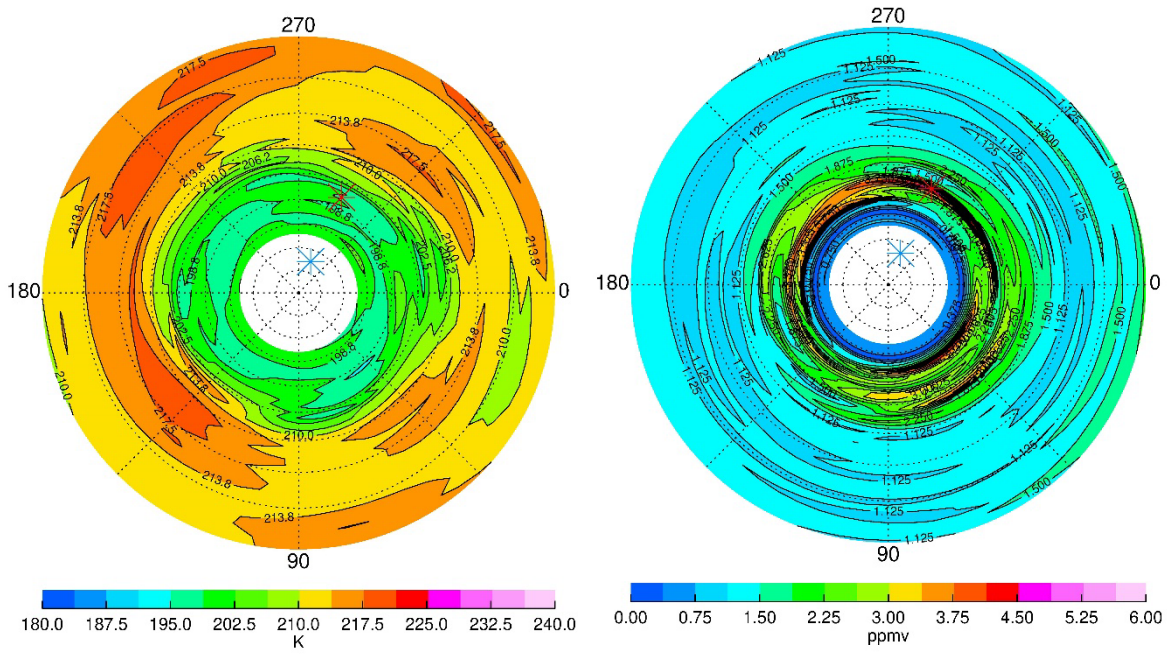

Figure S5—SH temperature (left) and ozone (right) at 0.032 hPa on January 15. Blue asterisk denotes satellite position (-75°S, 291°E); red asterisk denotes tangent point (-45°S, 294°E). LIMS view is from satellite to tangent point along temperature gradient maxima. Day/night terminator is near 60°S.