Reply (large font) to comments of Referee, John Austin (smaller font):

Thank you, John, for your careful and constructive review of our manuscript, which is essentially an update of your original and important paper in GRL (1986).

General remarks:

1. It is common and convenient to refer to pressure-altitude as an altitude in km based on pressure and a nominal temperature profile. However, much of the paper has pressure (correctly) as the vertical coordinate so regularly referring to it as "pressure-altitude" is confusing.

1—The terminology was changed in each instance.

2. The paper is a bit woolly in identifying the extent of the denitrification due to PSCs. I think this could be strengthened by using the information in the last few figures. For example, could the authors not set up a linear correlation between PV and HNO3 as a function of equivalent latitude? In the case of the later it should be possible to identify a 2 ppbv or so loss in HNO3 by the end of January encompassing the major PSC period. (Thereafter the seasonal change in sunlight may be interfering) The point is that the regression would give the loss of HNO3 with some uncertainty limits.

2—Simple linear correlations between nitric acid and PV are not so straightforward to interpret. For example, in original Figure 15 there are increases in PV at the end of January and also just after mid-February (note that the tic marks on original Figures 15 and 16 refer to the middle of a month). Those PV increases correspond to the effects of diabatic descent of higher PV within the vortex as a response to the stratospheric warming activity. Figure 16 shows decreases in HNO_3 at those same times that are a result of the associated descent of lower values of HNO_3 to the 550 K surface.

We have replaced original Figure 13 with new Figures 13 and 14 (see attached). Our best quantitative evidence for a local uptake of gas phase HNO_3 comes now from the data of 1-20 January (see also Figure 11). We separated the points of Figs. 11 and 13 into successive 5-day periods for Figure 14, after first removing those points that have corresponding values of $O_3 > 6$ ppmv. The centroids of each 5-day, cluster of points show declining HNO_3 values with time, indicating an uptake of the order of 1-3 ppbv. The MLM averages of HNO_3 in original Figure 16 also indicate an uptake of HNO_3 from 15-17 January and between equivalent latitudes of 80 and 90˚N, where PV was increasing in Figure 15 and where there is a temporary, average decrease of HNO_3 of about 1 ppbv (orange to yellow in Figure 16). That amount of uptake is considered a lower limit, however, because it is the MLM average around the PV contour. Nonetheless, that result is consistent with the visual findings of a local dip in HNO_3 to less than 10 ppbv for 14-15 January in Figure 11. These interpretations are now part of the discussion in Sections 7 and 8 of the revised text.

3. I suppose it's still journal policy, but I must say that the format of the paper for review was a challenge. In this day and age of limiting the amount of printing, I like to review

entirely on screen. With the paper presented in a traditional separated manner this was a particular nuisance as I had to keep scrolling from the text to the references to the figures to the table and back again. Personally, I would like to see figures and tables in the text where they are first referenced. I don't know the purpose of the separate list of figures which just added to the effort taken to review.

3—No copy editing occurs by the AMT journal now, prior to the review stage.

Other comments:

Abstract
The fact that the emissions from PSCs occur 1-2 km below the altitude of minimum temperature and the temperature are slightly lower than saturation for NAT (lines 65-69 and 505) is worthy of inclusion in the abstract. As it is, the abstract is a bit vague on lines 36-38

Abstract—An addition was made clarifying the relation between the observed locations of the minimum temperature with respect to tops of the PSCs.

l.133. The convention used to refer to the narrow band CO2 is a bit suspect bearing in mind the N might be interpreted as nitrogen.

Line 133—That convention is clear in the text now.

l.300-307. This is a bit confused. It starts referring to Knudsen 1996 conclusions but then "later" refers to conclusions from Crutzen and Arnold (1986).

Lines 300-307—Order of the published findings is chronological now in the revised text.

l.632. I think "imply" is too strong and would use "suggest" instead. The correlations provide a starting point for comparisons but are not in themselves conclusive. I see that on line 644, the word "likely" is used.

Line 441 and Line 632—Corrections were made in both instances.

l.688. There is a mix of coordinate systems here. The planetary vorticity is expressed in spherical polars, so the relative vorticity should be as well.

Line 688—You are correct; relative vorticity is given in polar coordinates now on a pressure surface.

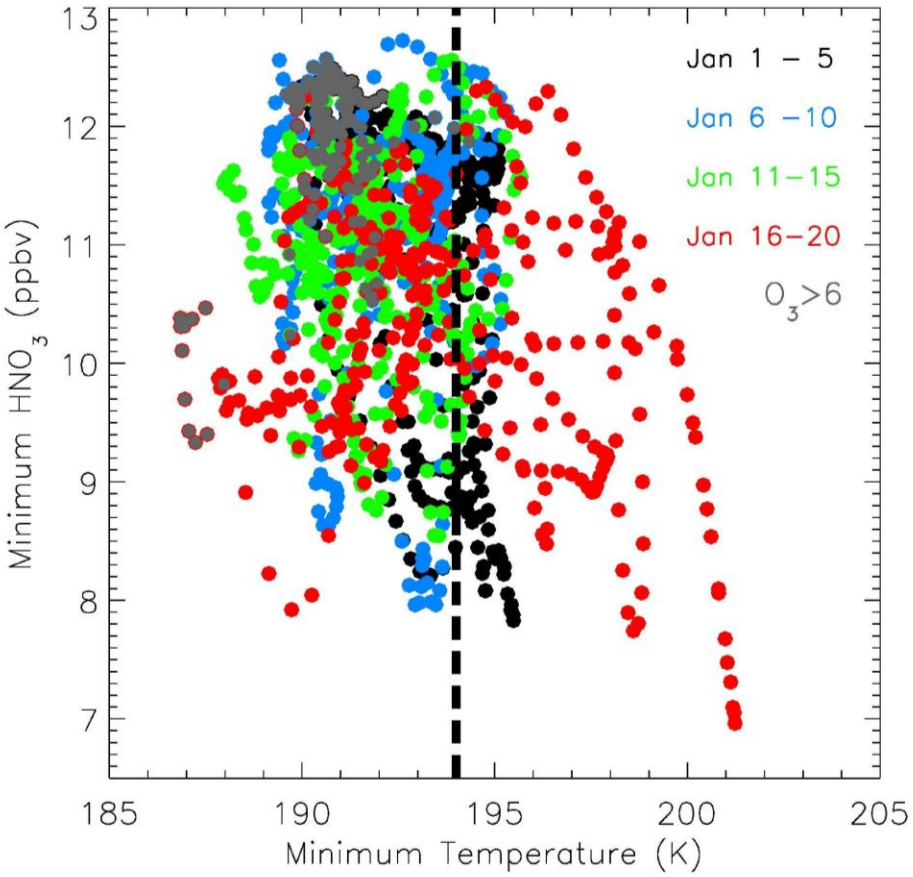

Figure 13—Scatterplot of minimum values of HNO_3 versus minimum values of temperature for the domain of 70˚N to the Pole at 550 K for the period of 1-20 January (from Figure 11); the vertical dashed line denotes 194 K. Colors correspond to data values in each of the separate 5-day periods. Points having corresponding values of ozone > 6 ppmv are shown in gray.

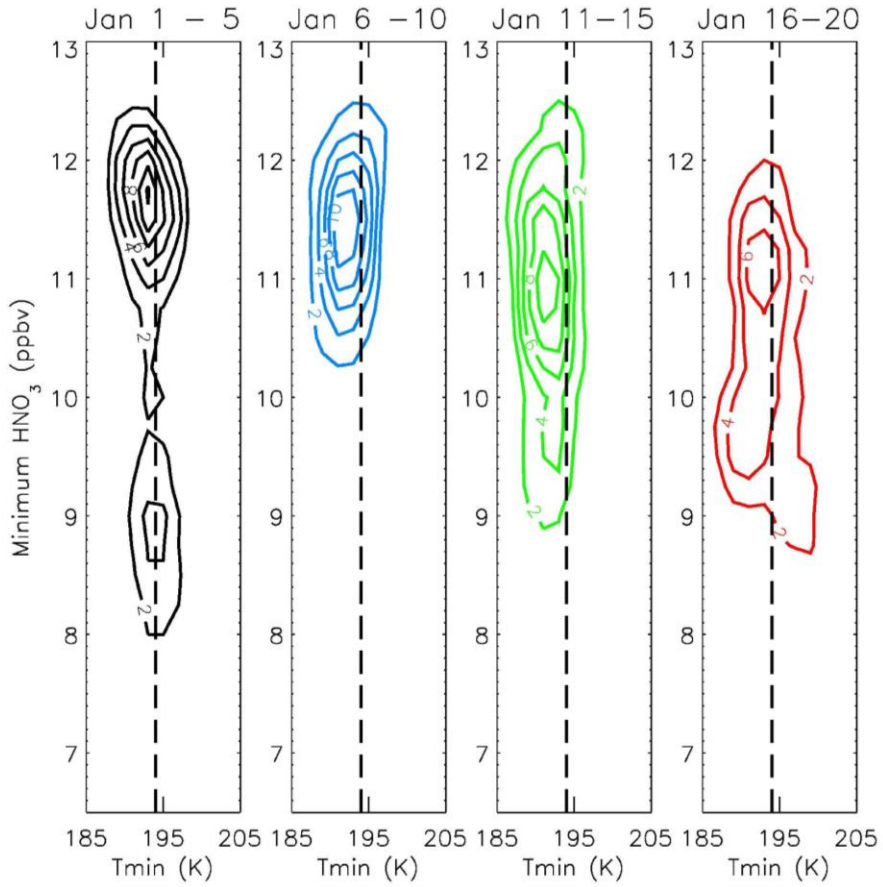

Figure 14—Contour plots of the frequency distributions of points from each of the 5-day periods in Fig. 13. Points from each distribution are binned according to intervals of 2 K and 0.25 ppbv HNO_3, and their cumulative numbers are contoured at a spacing of 2. Dashed vertical line is for 194 K.