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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Nitric acid in the stratosphere based on Odin observations from 2001 to 2007 – Part 1: A global climatology" *by* J. Urban et al.

J. Urban et al.

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Reply to anonymous referee 2

General comments:

The paper presents a valuable data set on stratospheric HNO3 from ODIN/SMR observations providing global coverage over 6 years. The data set is discussed in terms of seasonal, latitudinal and altitudinal variation and is compared to the climatological HNO3 data set from MLS/UARS which covers most of the 1990s. The ODIN HNO3 data will be, without any doubt, of great value for atmospheric studies, in particular for validation of atmospheric models. The paper is of considerable interest for a wider community, the presentation of the data set is done

thoroughly, and the paper is clearly written. My main comment refers to the fact that at several places throughout the paper, the discussion of inter-annual differences is announced in section titles etc., but nearly never really performed. The authors should decide either to provide this discussion (in terms of analyzing the physical/chemical reasons for significant (?) deviations in specific years from the multi-annual mean distribution) or to reword the related titles and sentences. Finally, I would like to encourage the authors to consider a trend analysis based on the MLS/UARS and ODIN data which, together, span more than a decade of data. This, however, is certainly left to the authors decision alone. The manuscript should be published in ACP after several, mostly minor revisions.

A section on the temporal variability in the tropical stratosphere has been added, including two new figures. A discussion on possible trends in HNO3 is done within this section, however a quantitative trend analysis is beyond the scope of this work.

Specific comments:

Section 2.1, one-but-last para: Is there any explanation for the altitude shift to be applied to HNO3 profiles in order to achieve better agreement with other measurements? Is such a kind of altitude shift necessary for and applied to other trace gases retrieved from Odin/SMR, too? Some comments on this issue should be included in the paper.

Since the pointing offset is retrieved independently for each band, only species retrieved in the same (544.6GHz) band might suffer the same problem. However, we only have evidence from different validation studies for HNO3 (e.g. comparisons to MIPAS, ACE, AURA) and no such evident systematic shift was yet reported for other species (H2O, O3, temperature) retrieved from this particular band. Since I don't want to include a lengthy discussion on other species retrieved from the same band and why the effect might not have been found (possibly hidden behind other instrumental

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effects), it is preferable to leave everything as is.

Section 2.2, 2nd para: No discussion on the interannual variation at 520 K is provided, and little on the other potential temperature levels. The authors either should provide such a discussion, or restrict the figure to a multi-annual overall mean of the seasonal vs. latitudinal variation.

The manuscript has been edited in order to include a qualitative discussion. However, inter-annual variability is mainly addressed in Section 3. Figures 1-3 serve mainly to describe the overall morphology of the spatio-temporal HNO3 distribution.

What are the white areas in Fig. 1 at high northern and southern latitudes during polar summer? Missing data due to incomplete global coverage? A note in the figure caption should be made.

Done.

Section 2.3: The section title is somewhat confusing, since high equivalent latitudes have already been discussed before. Maybe a better title would be Vertical crosssections for high latitude or something similar? The discussion in section 2.3 should be done along potential temperature (at first place, not pressure or altitude), since this is the representation in Figs. 2 and 3.

The section title has been modified. Potential temperature is discussed first, and approximate altitudes are indicated in the text. Altitude contours are now shown in the Figures. The discussion referring to pressure levels has been removed and pressure contours were removed from the Figures.

The same comment as to section 2.2, 2nd para applies here: Either the inter-annual variation as presented in Figs.2 and 3 should be discussed in the paper in terms of physical/chemical reasons for the observed deviations from the mean in some years, or, if the discussion remains restricted to features seen similarly every year, the authors should consider showing only the multi-annual overall

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mean of the vertical vs. seasonal variation.

Inter-annual variability was already mentioned in this section. Physical chemical reasons are however mainly addressed in Section 3. Nevertheless, Figure 1-3 serve to show the overall morphology of the data set and cannot be replaced by an average over all years.

Similar to reviewer 3 I am not happy with the term reversed tape-recorder effect since it is not clearly defined. The tape recorder refers to the imprint of a seasonal variation to the distribution of another parameter (usually the variation of tropopause temperature imprinted on the stratospheric water vapour distribution is meant), and the upward transport of this signature. I don t right see the seasonal variation of which parameter is imprinted on the HNO3 distribution, in particular regarding the low-HNO3 phase of the so-called reversed tape recorder; at least the authors should explain this point more clearly if they want to stay with this expression.

The explanation has been slightly expanded. Similar as with the tropical water tape-recorder analogy, a time dependent signal is imprinted on the air masses during polar winter and then advected. "Reversed", since transport is here downward, not upward.

Section 3:

Again the inter-annual variations are not in the focus of discussion, but the authors search for similarities among the years; instead, the seasonal and latitudinal variations and how these agree with MLS are extensively discussed. I would be happy with this section if the authors chose another title. If the title is kept, a more thorough discussion is required on the physical/chemical reasons for deviations from the mean in some years (e.g. July-August 2003 and 2005, December-January 2001/02 and 2003/04).

Such a discussion is now included.

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Regarding the comparison with MLS, I would like to suggest that an attempt could be made to derive a decadal stratospheric HNO3 trend from the MLS-Odin comparison. This, however, would require, besides a more careful statistical analysis of the data sets, a more thorough consideration of the differing vertical resolutions of MLS and Odin, which, at the current state, is a little hand-waving. For the discussion of the effect of differing vertical resolutions it is crucial if the comparison is made at the peak of a vertical distribution (the peak value is reduced in case of the poorer vertical resolution) or in the wings of the vertical profile (where the poorer vertical resolution produces higher values). A well-founded statement on the existence (or not) of a decadal HNO3 trend would be a valuable complement of the paper.

We have added a discussion of the combined time-series of UARS/MLS and Odin/SMR HNO3 in the tropical stratosphere, a region where the seasonal variability is smallest. Due to the gap between both data sets from 1998 to 2001 and the limited absolute accuracy of the HNO3 data (with possibility of biases in both data sets), it is however not possible to conclude that HNO3 is increasing, what the absolute values might suggest. The issue of the differing altitude resolution is here dealt with by showing various levels which show a similar behaviour. See Section 3.5.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 9569, 2008.