

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.

Anonymous Referee #2

Received and published: 16 July 2008

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

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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.

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.

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.

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.

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 cross-sections 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 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

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should consider showing only the multi-annual overall mean of the vertical vs. seasonal variation.

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.

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).

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.

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