

Interactive comment on “Global distributions of nitric acid from IASI/MetOP measurements” by C. Wespes et al.

Anonymous Referee #1

Received and published: 14 May 2009

General Comments

This paper reports the first global data set of nitric acid total columns from the IASI/METOP instrument. The data discussed in the paper cover a 10 month time period starting March 1, 2008. The paper discusses seasonal and latitudinal variations, with a focus on the polar vortex regions, in particular the denitrification that occurs when temperatures are sufficiently low for PSC formation. The observed global features indicate that the total column is dominated by the stratospheric contribution to it. The paper shows some qualitative comparisons with MLS data in the low stratosphere (at 46.5 hPa).

The paper is very well written and clearly structured. The paper discusses a data set that may be very valuable. At present however, it is very difficult to evaluate really the

C829

precision of these data and hence, the extent to which the data set will be useful to investigate particular chemical processes, and to estimate seasonal and interannual variabilities quantitatively. The authors pay very little attention to the uncertainties associated with the data. From Figs. 6 and 8, one sees that the 3sigma variability within a grid box (1°lat, 2° lon) is of order 100%. It is not clear what is real spatial and daily variability and what comes from the measurement uncertainty. The comparison with MLS (Fig. 6) is rather good at first sight but again, at some stations and for some time periods, the differences between both data sets are large and they are not discussed at all. The authors claim that it is impossible to make a quantitative validation 'due to the absence of archived data from ground-based measurements'. Even if data from 2008 are not fully archived yet, it should be possible to get valid ground-based total column data for HNO₃ from many NDACC PIs individually. Figures 3 to 8 (especially 3, 4, and 7) are really too small. It is impossible to distinguish the PV and T contours, and to get a good view on the variabilities and gradients.

Specific comments.

- Abstract, line 15: I do not like the word 'trends' to indicate seasonal variations. Trends rather refers to long-term evolutions.

- Introduction, pg. 8039, line 5: in order to really discern the full seasonal variation, one would need at least a 12 months data set. I wonder why the paper shows no data for January and February?

- Section 2.1, pg. 8040, line 4: IASI provides global coverage twice daily because of its high temporal sampling 'and large swath width'. The swath width is very important to achieve the coverage.

- Section 2.2: pg. 8040, line 17: please explicitly say that this is the HITRAN2004 database. Have the database updates beyond 2004 (before the release of HITRAN2008) been included ?

- FORLI-HNO3: the present paper does not give any reference to a previous paper that provides more details about the performances of this software. How do the results from FORLI-HNO3 compare to those from the line-by-line code atmoshit ? Have detailed comparisons been made ? Has the reliability of FORLI-HNO3 been demonstrated ?
- In particular, Section 2.2 pg. 8040, lines 20 to 25: the steps in T and relative humidity seem rather large taking into account that the dependence of the absorption cross-sections on these parameters is not linear, and that a linear interpolation in the tables (LUT) is performed. This joins the previous questions about the verification of the performances of FORLI-HNO3.
- Section 2.2, pg. 8041, line 7: does the contribution from the downward flux include both the thermal and solar radiation ? Please specify.
- Section 2.2, pg. 8041, line 15: pixels with a cloud coverage of less than 25% are kept. If there are clouds (within the 25% limit), these will mostly mask the tropospheric column. Can you give an estimate of the possible uncertainty this induces on the total column?
- Section 2.3, pg. 8041, line 19: 'The retrieval scheme used here is the Optimal Estimation Method... (the word 'is' is missing).
- Section 2.3, pg. 8042, line 10: '...by limited strong overlapping other trace gases absorption features...': do you mean: '...by a limited number of strong overlapping absorption lines from interfering trace gases ...' ? The statement should be written more comprehensively.
- Table 1 and Section 3.1: It looks like you have adopted another Sa matrix in the atmoshit evaluation than in the FORLI approach. In particular, Sa seems to be diagonal in the FORLI approach with constant values (100%) at all altitudes, while these diagonal elements vary in the Sa adopted for the atmoshit evaluation, and the latter Sa has non-zero off-diagonal elements. Please clarify. In that case, is the atmoshit

C831

evaluation then representative of the FORLI results ?

- Section 3.1, pg. 8044, line 9: do the spectral fit (Fig. 2) show results from the atmoshit fit or from a FORLI fit ? If Fig. 2 represents atmoshit results, then you should comment on the differences between an atmoshit and a FORLI result.
- Section 2.3, pg. 8043, line 4: by accounting for the smoothing error, the measurement noise error and the errors from the fitted model parameters, you do not have the complete error budget. For example, you miss the systematic errors from the spectroscopic uncertainties. You also miss the errors from the not-fitted model parameters like the T profiles, the adopted emissivity, etc. Therefore, the error budget is underestimated. Please provide a complete error budget and discuss the results taking the error budget into account.
- Section 2.3: no information is given about the emissivity adopted in the model: please provide enough information and indicate the impact of these emissivities on the retrieval results and associated uncertainties.
- Section 4, pg. 8050, line 23: the authors claim that the daily variations of the total columns in the grid boxes are relatively important. As already expressed in my general comments, it is difficult to believe that these variations are real variations of HNO3. I suspect that part of the variability comes from measurement/ retrieval uncertainties. Similarly, I am not convinced from the actually presented data set that 'in-depth analysis of chemical processes' (Section 4, pg. 8051, line 11) is already possible. But I agree that these 'preliminary' data indicate a high potential of IASI.
- Fig. 2 caption: 'dark grey line' should be 'the blue line' ?
- Fig. 8, high latitude plots: why is the ratio HNO3/O3 columns almost 'symmetric' around the denitrification period? I would expect higher values just after that period (early October) because of the ozone depletion at that time ?

Interactive comment on Atmos. Chem. Phys. Discuss., 9, 8035, 2009.

C832