

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

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First of all, we thank the referee for his useful comments which have helped us improving the description of our methods and the results. In particular, the revised version of this manuscript will follow the suggestions of the referee to clarify the description of the posteriori filtering. Information on the precision of the data has been improved in the revision, in particular in the discussion related to the daily variability. Below are our point-by-point responses to his comments and suggestions. Before each response, the reviewer comments have been quoted between [...]. Corresponding information and corrections have been added to the revised version of the manuscript. Technical comments are also included in the revised version.

Response to the Anonymous Referee #2:

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Specific comments:

[It should be stated in the abstract that the product is a total (stratospheric) column. Any information on precision or accuracy of the data product is missing, and related numbers should be added.]

We thank the referee for pointing out these missing aspects in the abstract. In the revised manuscript the product is now referred to as a total column, largely dominated by the stratosphere. Precision of data product have been added in the revised abstract.

[115: The term “local trend” should not be used if seasonal variation is meant. A trend is a long-term (over many years) variation.]

This has been changed in the revised version.

[Introduction: p8038, l1-2: Orsolini et al. 2008 and von Clarmann et al., 2009 are further references to be considered when citing global satellite observations of HNO₃.]

According the suggestion of the referee, these references have been inserted in the revised version.

[Section 2: p8040, l25: a step width of 10 for the relative humidity seems to be rather coarse given the dryness of the stratosphere.]

Unlike for nitric acid, the contribution of stratospheric water vapor to the IASI radiance spectra is totally negligible, and this is why the step width has been chosen so coarse. Refining the step width would not impact on the retrievals.

[Section 2.4, p8043, l6-11: From the text I conclude that retrievals have been discarded if the rms of the residual was above a certain threshold (however, the threshold is not given). This is described as removal of noisy spectra. However, the residual might be large due to other reasons as well, for example because a spectral fit has not been achieved (the convergence criteria as described on page 8042, lines 3-5, does not guarantee that the minimum of the penalty function has been reached). Removal of

noisy spectra should be based on the estimate of the noise of the spectra. In this sense, the authors should improve their description of what they have really done.]

We agree that the procedure was not sufficiently explained. Indeed, the RMS reflects both the error from radiometric noise and from the forward model parameters (HNO₃, water vapor, temperatures profiles, . . .). The filter which was applied refers to this root-meansquare (RMS) difference between observed and fitted spectra (or residuals). Retrievals with RMS larger than $3\sqrt{10^{-8}}$ W/(cm².sr.cm⁻¹) have been discarded. This has been clarified in the revised version (page 8043).

[Further (in lines 8-10) some emissivity features are mentioned. The authors should state clearly if the spectra/retrievals have been removed based on the identification of these emissivity features, or based on unreasonable HNO₃ values which are thought to be related to the emissivity features. The latter would be no satisfactory criterion, since a filter criterion should not depend on the values of the quantity to be filtered itself.]

Retrievals contaminated by emissivity features occur principally above deserts. We confirm that these scenes have been discarded based on the identification of the emissivity structures directly in the IASI spectra, not based on unrealistic values of HNO₃. The identification was achieved by selecting specific channels related to these emissivity features and by calculating simple brightness temperature differences. Moreover, the filter based on the rms of the retrievals also helps to discard these contaminated spectra. This has been clarified in the revised version (page 8043).

[Section 3: Section 3.1: It is not clear why the authors refer to retrieval tests with the Atmosphit code: the FORLI-HNO₃ code seems to provide all the necessary retrieval diagnostics as well. As already stated by referee#1, some minimum requirements to convince the reader of the validity of these tests for the FORLI-HNO₃ retrievals as well are: - the consistency of the Atmosphit and FORLI-HNO₃ code in terms of spectral simulation and retrieval results has to be demonstrated, or a reference to a respective

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paper has to be made;]

While Atmosphit is a versatile line-by-line RTM (accommodates different geometries, instruments specifications etc. . .), it is not suitable for treating the huge dataflow from IASI. FORLI-HNO₃ has been constructed specifically to retrieve operationally total column for HNO₃ from the IASI spectra. From the radiative transfer perspective it should be considered has an upgraded version of Atmosphit. It is faster because it relies on pre-calculated tables of absorbances and makes use of analytical derivatives as much as possible. Because it only provides a total column, posteriori characterization in term of vertical sensitivity can not be performed. In the paper, we use the Atmosphit code only to provide insights into the inherent sensitivity of the IASI measurements to the vertical structure of HNO₃, and proceeding like this to justify the choice of one total column retrieval in FORLI. We do not believe that a thorough comparison between FORLI and atmosphit would add to the paper, in particular because the Atmosphit HNO₃ retrievals have never been validated neither. However we agree with the referee that the switch from one software to the other was not well explained in the paper. This has been improved in the revised manuscript (page 8042 and 8045). In order to be more quantitative a simple comparison between both procedures has been made only for two days and several locations, spanning a range of concentration values (Fig.1 attached). It shows a fairly good agreement between the two procedures. However, we have not included the figure in the revised manuscript in order to avoid further lengthy technical descriptions. Furthermore, in Figure 2 of the manuscript, two lines showing the average RMS at a global scale from FORLI retrievals have been added in order that the reader can judge on the fit quality of this software in comparison to Atmosphit.

[a good reason has to be given why the Sa matrix used within the FORLI-HNO₃ approach is different to the one used within the Atmosphit retrievals and a quantitative assessment on the impact of different Sa matrices on the retrieval has to be provided;]

As only a total column is retrieved, Sa is not a matrix but a scalar value. The latter was chosen to be 100% to allow sufficiently variability. It is worth pointing out that

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it encompasses the variability associated to each altitude level of the full Sa matrix, which was built from the real ensemble of profiles used to generate the a priori profile, as explained in the text (page 8044, line 4).

[does the Atmosphit approach retrieve in the log space as well? If not, the differences in the retrievals due to log(column) space versus column space have to be quantified.]

The latest Atmosphit version also uses the log space. As shown above, there are no major differences between Atmosphit and FORLI column retrievals. Although the purpose of this paper is certainly not on the software comparison, it is true that this comparison is comforting.

[page 8044, line 5: does "full covariance matrix" mean that non-diagonal elements are also considered? How does this affect the retrieval compared to the diagonal covariance matrix used within the FORLI-HNO3 retrieval?]

Again, the distinction has to be made between a profile retrieval, which requires obviously the use of a full diagonal matrix with off-diagonal elements to account for correlation between the levels, and the FORLI-HNO3 total column retrieval that only scales the profile by a given amount. Profile retrievals are only performed to provide an information content analysis and in no way to discuss the HNO3 profile retrievals, as the vertical information is just not contained in the measurement.

[page 8044, lines 9-14: I understand that the spectral fit shown in Fig 2a belongs to the Atmosphit 9-layer retrieval. How do the residuals look like for the FORLI-HNO3 log(column) retrieval?]

See above: we added two lines in the residual to show the average RMS value obtained with FORLI on the global scale. A sentence in the text has been added to highlight this point (page 8045)

[Section 3.2: p8046, l22-24: I don't right understand the conclusion here: It was stated in the introduction that the tropospheric amount of HNO3 is about 5% of the total

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amount (in terms of partial column, I guess); further an assessment of the contribution of the tropospheric part to the total column can be made from the a priori profile. Is this sentence meant as a confirmation that the tropospheric column indeed is as low as assumed in the a priori profile? A study on the impact of the shape of the a priori profile on the retrieved column would help to assess this point. I suggest either to provide this study or just to remove this sentence.]

P8046, l22-24: "These large-scale spatial and seasonal variations in the HNO3 total columns are fully consistent with the photochemical processes occurring in the stratosphere. This result, which was anticipated from the vertical sensitivity analysis, points to a weak impact of the tropospheric HNO3 component on the total column" We meant here that the variations in the total columns, although possibly including some tropospheric contribution, are fully understandable in terms of the stratospheric chemistry. The isolation of the tropospheric component from the total column will accordingly be a challenge. We agree, however with the referee that the last sentence could be confusing. It has been removed.

[p8048, l16: The authors suggest that the large scatter in the data is due to "daily variability" which I understand is meant as natural variability. I doubt, similar to referee1, that the natural variability indeed is that large. The error threshold of 32% (1sigma, I guess) applied in the a posteriori filtering is compatible with a 3sigma "variability" of maximum 100% as seen in Fig. 6. This points towards the precision of the data as source for the high scatter, and not the natural variability.]

The grey areas (3sigma around the daily mean values) have indeed been drawn to represent the daily variability of the total columns observed for the different selected boxes. The errors bars associated to the retrievals are largely lower than the observed daily variability. For instance, retrieval errors (errors bars which represent 1sigma error) associated to the total columns are shown for one of the six selected regions in the Fig. 2 attached. For that particular region, the 3sigma retrieval error is on average around 26% whereas the daily variability is on average 52%, mainly associated to the rapid

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zonal transport of air masses in polar region, and it can be as high as 170%. These numbers have been added in the text to clarify the discussion. In the equatorial belt on other hand, the 3s errors are indeed much closer to the variability and the variations should be looked at with more caution. We agree with the referee that the errors should have been better referred to in this part of the discussion. This has also been better explained in the text (page 8048).

[Conclusion: p8051, l 4 and 6: Again, the term "trend" should not be used for a seasonal variation.]

This has been corrected in the revised version.

Technical comments:

[p8038, l2 and l9: "Fisher" should be "Fischer" p8046, l11: typo in "contrast"]

This has been corrected in the revised manuscript.

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

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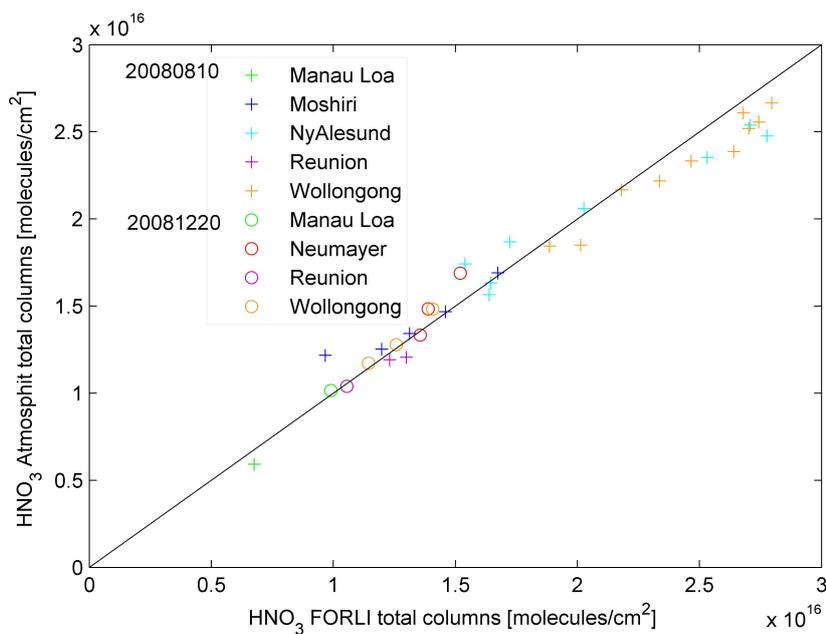


Fig. 1.

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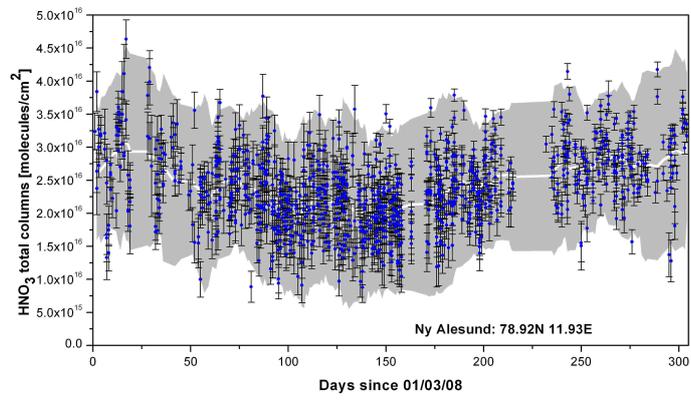


Fig. 2.

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