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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Validation of MIPAS HNO_3 operational data" *by* D. Y. Wang et al.

D. Y. Wang et al.

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Many thanks for your comments and suggestions. We have revised the manuscript accordingly. Our responses are detailed below, following your comments.

General comments:

The paper by Wang et al. presents a comprehensive study on the validation of the operational MIPAS HNO3 product of the ESA processor. The authors compare MIPAS with a number of ground based, air, and space borne remote sensing instruments and different measurement techniques. The results suggest the high suitability of the MIPAS HNO3 data for use in geophysical studies and this is a very valuable information for the scientific community. The paper is well structured and written and it addresses sufficiently all major topics of a good publication. The paper should be published in ACP after only some minor revisions addressing the specific comments listed below.

Specific Comments

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Interactive Discussion

Discussion Paper

EGU

The presentation of extensive analyses in a publication is a very difficult task, especially for validation purposes. Like in many similar papers of the topic validation this paper is overburden by the information of different instruments, error characteristics, acronyms, and the detailed description of good agreements and discrepancies. It will be difficult to condense the manuscript, however to my mind this would improve the quality of the paper.

Introduction:

It looks to me that the authors tried to compile a complete list of satellite instruments have measured and currently measure global HNO3 data. If this is the case, I am missing the CRISTA instrument.

Our response: In the revised version the CRISTA instrument is mentioned by a reference to Riese et al (Geophys. Res. Lett., 27(15), 2221-2224, 2000).

There are many details given on formerly validation results on HNO3. I don't think this is necessary, because they are not related later to the results of the new comprehensive study.

Our response: Details given on formerly validation results, in particular for the comparison between ESA and IMK data products, are deleted.

Section 2: Please give a short comment why you treated p/T-error as a systematic error with random variability ...

Our response: Two sentences are added and read as:

"We decided to handle the error propagation from MIPAS-retrieved temperature profiles into HNO3 volume mixing ratios in this way since for statistical comparisons over a large number of profiles the temperature error is also mainly of a random nature. This procedure is in agreement with other MIPAS validation papers, e.g. Wetzel et al (Atmos. Chem. Phys., 7, 3261-3284, 2007)."

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Interactive Comment

Section 4: To my mind too many details on the station analyses.

Our response: We have shortened it.

Section 5:

p5187: For arguments about the AVK it would be helpful to know the SAFIRE-A vertical resolution as well.

Our response: Lines 6-7 on p5187 in the original manuscript already gave the vertical resolution for both SAFIRE-A and MIPAS-STR. “Their vertical resolution (approximately 1–2\,km) is slightly better, but still comparable with the one of MIPAS ESA operational v4.61/v4.62 data.”

p5189: Which in situ measurements are addressed here as a reference for strong vertical gradients?

Our response: SAFIRE measurements are addressed here as a reference for strong vertical gradients. We re-phrased the sentence to make it clear. It reads as “The occurrence of strong vertical gradients is highlighted in the comparison with SAFIRE measurements (see, for instance, the profile of MIPAS/ENVISAT orbit 5250/scan 21) and can account for the observed differences with the satellite data,”

p5190: In the discussion of horizontal gradients of HNO3, frequently accounted for the reason of discrepancies, it would quite helpful to know some number of typical gradients of the vortex edge by in-situ or remote sensing instruments from the literature.

Our response: The following sentences are added.

“From in-situ observations onboard the NASA ER-2 (Kawa et al, Geophys. Res. Lett., 17(4), 485-488, 1990), one can derive maximum values of the horizontal gradient for the Arctic of NOy (which is mainly in the form of HNO3 at these latitudes) of up to 0.02 ppbv/km, which makes around 8 ppbv variation along the line-of-sight within a 3 km thick tangent layer of MIPAS. In NASA DC-8 in-situ observations of the lowest stratosphere in the Arctic winter 1988/89 even gradients of up to 0.13 ppbv/km are observed over short distances (Hübler et al., Geophys. Res. Lett., 17(4), 453-456, 1990). Over longer paths (200 km) a mean gradient of NOy of about 0.05 ppbv/km is visible."

Section 6.2: Is the Glatthor et al. the real reference for the ESA cloud index? Is the ESA cloud clearing not a general source of problems for all comparisons at lower altitudes?

Our response: A general description of the cloud index approach in case of MIPAS is given in Spang et al. (Adv. Space Res. 33, 2004). This paper is now quoted in the revised manuscript. We cited the Glatthor et al., 2006 paper since it contains a sensitivity study of the effect of different cloud index thresholds on the retrieval of ozone.

We agree with the reviewer that the cloud-clearing might also be a problem for other ESA trace gas retrievals.

The original sentence is modified as

"These polar stratospheric clouds (PSCs) have not been detected with the ESA cloud index limit (Spang et al., 2004) but with the more stringent IMK index (Glatthor et al. 2006). Cloud-clearing is a general source of problems for all comparisons of ESA MIPAS trace gas retrievals at lower altitudes. In case of HNO3, which may be used to infer denitrification/uptake processes by polar stratospheric clouds in the polar stratosphere, it is especially important to exclude such erroneous retrievals by, e.g., checking the cloud index values."

Conclusions:

The conclusions are too much detailed. It might help just to summarise with focus on different altitudes ranges like in Table 8 and not to bring up many of the details listed in the sections above.

Our response: We re-wrote the Section. In the revised version the common results of the different comparisons are summarized, instead of discussing all cases individually as it was done previously. The problems and the possible reasons for deviations between the different instruments are listed now. See the revised manuscript for details.

Technical corrections:

p5179: Is ACP forcing to use the short cut 'Sect.' for section?

Our response: That is provided by the ACP copy editor. (Even we used “Section” in our previous manuscript.)

p5181: please introduce I

Our response: The unit matrix is defined now.

p5182: please introduce N as the number of pairs in the ensemble, correct?

Our response: Defined.

p5186: why are just these instruments related to acronyms of institutes?

Our response: Deleted.

p5197, L6-9: repetition on Oxford error analysis

Our response: Deleted.

Figures: Some of the labels in the figures are quite small Fig. 2,5, 6 and might need improvements

Our response: The same question was raised to the copy editor, and new plots were provided during our proofreading. However, the editor said that this problem in the online display could be avoided in the journal printout.

Table 5: not really helpful, one should add the number of coincidences per flight

Our response: The number of coincidences per flight is added in Table 5.

Table 7: Is the small mean miss-time for Odin and ILAS caused by the use of miss-time values with negative and positive values for the mean/std calculation? If yes, this needs correction to absolute values.

Our response: Yes, negative and positive values are used for the miss-time. The mean and standard deviation give a clear picture of the temporal distribution for the 700 - 900 coincidence events, perhaps better than using absolute values.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 5173, 2007.

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Interactive Comment