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Interactive Comment

Interactive comment on "Precision validation of MIPAS-Envisat products" *by* C. Piccolo and A. Dudhia

Anonymous Referee #1

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General:

The paper validates MIPAS estimated errors of temperature and various trace gases due to spectroscopic noise by comparison with the standard deviation calculated on basis of matching profiles under certain coincidence criteria. In the framework of the MIPAS validation this is a very valuable analysis since, first, in standard validation work often the bias between two instruments is the major target compared to the validation of estimated errors. Second, when comparing two instruments on a statistical basis one can only validate the combined estimated error terms and it is difficult to decide which of the instruments (or both) under/overestimate their errors. In addition, the mismatch error due to the variability of the atmosphere is a further complication. In the actual paper the uncertainty due to another instrument is avoided by comparing MIPAS to

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itself. However, the contribution of atmospheric variability remains uncertain. Despite some technical clarifications/corrections it, thus, would be helpful if the authors could elaborate somewhat more on the possible causes of disagreement (for details see below).

Specific:

p. 914, l. 5: 'Assuming that the radiance is proportional to the product of the Planck function and the VMR, then the precision (in ppmv) should be proportional to the noise and inversely proportional to the Planck function (therefore atmospheric temperature), but should be independent of the VMR of the gas.'

-> This should be explained a bit more in detail: (1) mention that you can assume the proportionality only in the optically thin case; (2) the radiance is proportional to the Planck function B(T) and the absolute concentration of the trace gas, which is the air density times the vmr. Thus, the air density which is proportional to Pressure/Temperature is forgotten and you have a dependence on B(T)/T. For the non-mid-IR community you should perhaps explain why this is still dominated by B(T).

p. 914, l. 23: 'Figure 3 shows the time series of MIPAS-retrieved temperature.'

-> It seems that Fig. 3 is wrong. It does not show retrieved temperatures, but random errors.

p. 915, l. 5: 'but is also slightly dependent on the atmospheric signal (larger radiance also implies larger NESR).'

-> To further substantiate this statement could you also plot the nesr at the highest tangent altitude? Due to less atmospheric influence there, one should also see much less seasonal variability.

p. 925, Fig 4:

-> Is there any explanation for the NESR maxima in bands AB,B,C, and D, for 80N-90N in December 2002 and December 2003?

p. 915, l. 14: 'However, since the seasonal noise variations are only 15 of the order of $\pm$20% while the signal variation is nearer $\pm$100%'

-> How have these values been derived? Don't they depend on the band used for data analysis?

p. 916, l. 17: 'The use of the averaged gradient, rather than a simple pressure interpolation on a profile-by-profile basis, is to avoid introducing any extra smoothing to profile 2.'

-> Would a linear interpolation in log(p) really smooth the profile? As far as I know, for radiative transfer in the retrieval process, a linear interpolation of the profiles in altitude is assumed. Thus it would be reasonable to interpolate in the same way. What is the difference in the following analysis when a direct interpolation is used?

p. 918, l. 1: 'Instead, here the originally-defined E matrices have been used ...'

-> Could you explain a bit more detailed how these E matrices have been calculated?

p. 918, l. 18: 'and in general for HNO3, the pT induced error has a large contribution to the precision.'

-> Any idea why this is the case for HNO3? At http://www.atm.ox.ac.uk/group/mipas/err/ the temperature error also for H2O above 10 hPa is always larger than the random part.

p. 919/920: In the discussion of possible explanations for over/underestimation of the estimated precision values, some points are not mentioned and some should be more elaborated:

-> 1. The overestimate of MIPAS precision values for HNO3 and O3 in the troposphere: As far as I know, the MIPAS level 2 dataset does not report negative values, but small positive numbers instead. Thus, the standard deviation derived from this dataset would

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underestimate the variability caused by measurement spectral noise if such cases had not been filtered out. Has this been taken into consideration?

-> 2. The influence of non-detected clouds might also contribute to instabilities in the retrieval and add to the variability not accounted for by the precision estimates.

-> 3. Especially at higher altitudes (at least for O3) photochemically induced day-night variability might increase the derived variability because the selected matches are from ascending and descending i.e. night- and daytime parts of the orbit.

-> 4. Could you estimate the contribution of the atmospheric variability to the derived variability by continuously increasing the allowed distance between matches and look whether this 'curve' (variability versus distance) is still decreasing at your selected match criteria or if it is already flattening out. The latter would indicating that the atmospheric variability is no more a significant term.

Technical:

p. 914, l. 14: '0.0625'

-> mind that the spectral resolution is better than that (about 0.061 cm^{-1} for 8.2 cm maximum optical path difference of the interferometer)

p. 918, l. 21: Is there intentionally only one subchapter (4.1) in chapter 4?

p. 925, Fig 4:

-> In the caption it should be mentioned that the NESR values are averaged over each band and that there is only one point per month. Thus, I would prefer a figure with only symbols and no connecting lines. Otherwise one could think that e.g. the decrease of the noise in band A begins before the decontamination has taken place.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 911, 2007.

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