

Interactive comment on “Spatiotemporal variations of NO_y species in the northern latitudes stratosphere measured with the balloon-borne MIPAS instrument” by A. Wiegele et al.

A. Wiegele et al.

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We would like to thank A. Dudhia for the valuable comments which we answer in the following (A. Dudhia's comments are inserted *in italics*).

1) I didn't really understand the argument about varying the azimuth angle to keep the sun-los geometry the same. If you're studying the time evolution of NO_x wouldn't it be simpler to keep the same azimuth direction so that you're always looking at the same airmass?

Keeping the same azimuth direction does not guarantee to observe the same air-masses since the air is moving anyhow. Following exactly the same airmasses would need to adjust for the airmass displacement during the observation time. The observa-

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tions were done between 2:25 and 7:07 UT. Following the airmasses exactly is simply not possible since the wind vector is not homogeneous over altitude. Looking at the same azimuth direction over 5 hours would have meant that the airmasses would have moved through the observed area between 100 and 600 km (depending on altitude). However, much more important, the sun azimuth would have changed by about 70 degrees. This would have imposed significant inhomogeneities of the NO₂ distributions anterior and posterior of the tangent point. The latter was the main reason why we have chosen a mixed approach: We kept the azimuth for each about 30-40 min and thereafter adjusted it for the next bunch of measurements to adopt the new sun azimuth. Given the fact that the photochemistry of NO₂ is significant and fast air mass effects can be neglected. Most importantly, the approach has ensured to avoid problems with inhomogeneous illumination conditions along the line of sight which would have imposed great uncertainties in the retrieval (cf. the rapid change of NO₂ of about a factor of 3 at 31 km within less than one hour, Fig. 9).

2) I know that the IMK group retrieve all these species, using similar software, from the MIPAS satellite instrument, which was also operating on that day. Why no results from that? It would have given a useful overview of the global distribution as well as cross-validation.

Actually, a part of the MIPAS-B flight was dedicated to ENVISAT validation. The validation results for MIPAS on ENVISAT from this flight have been published elsewhere. In our opinion, the results of the MIPAS satellite instrument of that day are not really useful for the study we are presenting here, since we are focusing on small-scale variations in space and in time which cannot be resolved by the satellite instrument passing the scene with 7 km/s. The so-called “global distribution” is in fact a trace of two overpasses with a time mismatch of several hours. To be of added value for the study presented here we would have needed an assimilation of MIPAS ENVISAT data in a good 3-D model able to provide a synoptic distribution of the species over the time frame we have covered. Such a study would have been well beyond the scope of our dedicated measurements.

3) p4697 line 20 refers to “a priori” information, which might give the impression that this is an optimal estimation type of retrieval. Is that the case, or is it a regularised least squares fit? In any case, a sentence describing the type of retrieval that you use would be helpful.

A Tikhonov-Phillips regularisation approach was applied which was constrained with respect to the form of an a priori profile. We have inserted this sentence at p. 4697, line 16.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 4693, 2008.

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