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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global peroxyacetyl nitrate (PAN) retrieval in the upper troposphere from limb emission spectra of the Michelson Interferometer for Passive Atmospheric Sounding (MIPAS)" *by* N. Glatthor et al.

Anonymous Referee #3

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Global peroxyacetyl nitrate (PAN) retrieval in the upper troposphere from limb emission spectra of the Michelson Interferometer for Passive Atmospheric Sounding (MIPAS)

Glatthor et al.

General Comments

This is a very good paper which builds on the detection of PAN in spectra recorded by the MIPAS-B2 instrument (and published in ACP) and the detection of PAN in spectra recorded by the MIPAS instrument on ENVISAT. It presents the first global retrievals

of PAN from MIPAS and this is a valuable contribution to the scientific literature. The retrievals are well-characterised and averaging kernels show the vertical sensitivity of the PAN retrievals. Results seems to show the influence of biomass burning (but see following comments). Hence I am very happy to recommend publication after due care has been paid to the following comments.

Given the authors good knowledge of MIPAS data, it is very surprising that there is no reference to the fact that PAN spectral detection has been demonstrated for the MIPAS instrument on ENVISAT as well as MIPAS balloon. This must be corrected since it is important to acknowledge work which has already been achieved with reporting of MIPAS on ENVISAT PAN detection by derivation of spectroscopic shape in two bands. This could be noted at the end of Section 1. Suitable references would be Remedios, J.J., G. Allen and A.M. Waterfall, Infra-red remote sensing of organic compounds in the upper troposphere, in Procedings of the "First Conference on Atmospheric Science" (ed. H. Lacoste), ESA SP-628, ESA Publications Division, European Space Agency, Noordwijk, The Netherlands, 2006 AND/OR Allen, G., The infra-red remote sensing of Peroxyacetyl Nitrate in the upper troposphere, Ph.D. thesis, University of Leicester, http://www.leos.le.ac.uk/publications/pdfs/theses/GA_thesis_final.pdf. 2005.

The error analysis is mostly very good but I have one comment and one query. My comment is that the spectroscopic uncertainties in absolute terms are probably underestimated at some altitudes if 3% is assumed, given the 4% uncertainty in the peak absorptivities at 250 K reported by Allen et al., and more importantly the range of temperatures below 250 K. I suggest an updated mean PAN uncertainity is used based on the authors estimate of possible biases resulting at lower temperatures. My query concerns the water vapour error which does not seem to be included. Does water vapour not matter (in a small way) in the tropics?

I have some concerns over the biomass burning sections although I agree with the authors main thrust. First of all, the authors have not really noted how many profiles give rise to the large values (of greater than 500 pptv) at 8 km and to a lesser extent

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at 12 km. The authors should comment on whether the values here are distorted by sampling as would be indicated by the gridding. Average values seem closer to 300 pptv in the biomass burning plume. Secondly, the authors use C_2H_2 as an indication of burning which is probably correct but they should justify why C_2H_2 is suitable by reference to the published literature as this is really the critical point given the absence of fire maps and trajectories. Are there other ways in which C_2H_2 can be enhanced?

The evidence presented that PAN is elevated in the Northern hemisphere due to industrial activity is suggested but no justification is presented for this assertion. This may well be correct but justification should be presented e.g. by quoting consistency with literature which is consistent with this statement.

Specific minor comments

1) p. 1394 Line 1: The statemnet of PAN transport over more than 10000 km needs a reference.

2) p. 1394. Line 12: Is PAN really an indicator of hydrocarbon-NOx photochemistry. Surely PAN is a reservoir species removing some organic fraction and NOx from active chemistry?

3) p. 1396 Line 20: Surely an all zero a priori profile has some influence on the shape of PAN as it should increasingly bias the PAN towards low values as the signal-to-noise become worse?

4) p. 1396 Line 20. What is the source of the climatological profile for PAN and is there any first guess dependence to the retrieval?

5) Section 3. No cloud flagging is mentioned.

6) p.1398 Line 23. and is repeated twice.

7) Section 4 needs to include references to already reported PAN spectral detection for the band considered here for MIPAS on ENVISAT as discussed above. What is

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also convincing is Figure 4 where the spectral shape apparent in the residuals (no PAN fits) reduces and flattens when PAN is included in the fit. This suggests that there is a residual spectral feature that is fitted well by PAN as would be expected. Strictly speaking the RMS alone cannot do this as the RMS simply tells you that introduction of a broadband contaminant improves the fit but it could be that another broadband contaminant would do a similar job (it is the flatness of the residual which is crucial). In fact since we already know that PAN has been detected by spectral shape in MIPAS-B2 and MIPAS on ENVISAT data, this is a pedantic point but certainly the authors approach shows rigour in diagnosing their retrieval.

8) p. 1406 Line 4. This sentence does not seem to make sense. What do the authors really mean here.

Technical comments

1) There are a number of minor English errors which need to be corrected but no doubt the authors planned to do this anyway.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 1391, 2007.