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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Acetylene C₂H₂ retrievals from MIPAS data and regions of enhanced upper tropospheric concentrations in August 2003" *by* R. J. Parker et al.

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Response to Anonymous Referee 1

(Responses in bold)

1 General comments

1. The discussions reported in the paper could be more concise. I feel that the abstract, the conclusions and especially Sect's 4.3 and 4.4 could be shortened significantly. For example, there should be no need of a Section summary also at the end of each

Section...

We accept that some sections are particularly verbose and will address this in the final manuscript.

2. The study of the dynamics presented in the paper would be much stronger if the authors could show that similar results are obtained when data relating to other years (e.g. 2002 and/or 2005) are considered.

Unfortunately MIPAS high-resolution data is only available for the 2003 monsoon period. After 2003, MIPAS was forced to operate in an "optimised resolution" mode with higher vertical resolution but lower spectral resolution; hence the spectral signature of C_2H_2 is not as distinct. Work is ongoing to continue the retrieval of C_2H_2 at the optimised resolution mode but that is beyond the scope of this paper.

In terms of the dynamics, we believe that model-data studies such as those of Park et al, 2008 (see paper) for the monsoon anticyclone, and Sauvage et al, 2005, for the outflows from Africa demonstrate that similar dynamical behaviour occurs in other years. We recognise that we need to make more use of the literature in confirming the robustness of our results and will do so in the revised manuscript.

Sauvage, V. Thuret, J-P. Cammas, F. Gheusi, G. Athier and P. Nedelec Tropospheric ozone over Equatorial Africa: regional aspects from the MOZAIC data, Atmos, Chem. Phys,, 5, 311-335, 2005; SRef-ID: 1680-7324/acp/2005-5-311

3. The paper shows the enormous potentiality of space and time coverage of MIPAS data. For this reason, the choice (of the European Space Agency (ESA), I guess) of not retrieving the C_2H_2 VMR routinely from all MIPAS spectra seems a shame and also a waste of the money invested in the whole mission. Therefore the authors should comment on the feasibility of the routine retrieval of this species and, if feasible, they

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should issue a strong recommendation (for the ESA) to do so for the whole MIPAS mission dataset.

Thank you for this comment. We completely agree on the enormous potential of MIPAS data as we demonstrate in the paper. As highlighted above, the reduction in MIPAS spectral resolution post-2003 complicates the issue and this is ongoing work. We agree that a routine retrieval, where possible, of these trace species would be highly desirable and this is being communicated via our involvement in the MIPAS Quality Working Group. We will add this to our conclusions.

2 Specific and minor comments

1. P.29736, L.2: The acronym MIPAS is used in the abstract without being specified.

Will rectify

2. P.29736, L.6-7: In our C_2H_2 retrievals ... This sentence has no meaning in the abstract, what is the meaning of caution here ? I suggest to remove this sentence.

Will rectify

3. P.29739, L.11: 0.025 unapodized spectral resolution. The units are missing here ($cm-1$). In addition, how do you define the spectral resolution ? Is it the Full Width Half Maximum of the spectral instrument line shape or just 1/(2 MPD), with MPD = Maximum Path Difference of the interferometer ? Please specify.

Will clarify definition of resolution in text

4. P.29739, L.16: I heard that ESA or some other scientific institution now started to retrieve routinely also F11, F12, $ClONO_2$ and N_2O_5 from MIPAS spectra. Is there a reference ?

As far as we are aware there is no peer-reviewed literature at this time for the new ESA operational products for these species. Since these data sets were not employed in this paper, we do not think this is relevant to our paper.

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5. P.29739, L.20: L1B. Please define the acronym.

Will rectify

6. P.29739, L.22: Replace 0.06 cm−1 with 0.0625 cm−1.

Will rectify

7. P.29740, L.7: Please specify the meaning of contribution. Is it the limb-radiance as if in the atmosphere there was only a specific gas ? In this case the plot of Fig.1 is not very illustrative because this spectral region could be (globally) very opaque and the line of C_2H_2 could also be not detectable. I guess what you plotted is instead the "total" limb radiance and the differences between "total" radiance and radiance obtained from an atmosphere where a specific gas has been removed.

The latter is correct. We will clarify the definition of "contribution" in the text to make this more explicit.

8. P.29740, L.8: The acronym for Reference Model should be RM (not RFM).

The Reference Forward Model (RFM) is the forward model used in this study, developed at the University of Oxford. Will amend to include "Forward" in the full name.

9. P.29740, L.18,19: From Fig.1 it is not clear to me which is the NO_2 contribution (orange line) in the region under the C_2H_2 line. What did-you assume for NO_2 ? Day or night distribution ? In order to be conservative I would recommend to use the night distribution.

The NO_2 has a very small contribution to this microwindow with a maximum contribution of less than 20 nW/cm^2 sr cm^{-1}. We assumed a daytime RAMSTAN reference climatology but found the effect of changing this to a nighttime climatology to be negligible.

10. P.29740, L.24: University of Oxford.

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Will rectify

11. P.29741, Eq.(1). The following quantities are not defined: iteration index i, K, D, xa. Please define them. Please clarify also what you have included in the state vector x. Is atmospheric background emission also a fitting parameter ?

The atmospheric continuum level is also a retrieved parameter. Will add these definitions to the text.

12. P.29741, Eq.(2): if the cost function is linear in x there no need of an iterative procedure to find the minimum. Please correct the expression of χ^2.

Will clarify.

13. P.29741, L.15,16: a priori covariance equal to 1000% means standard deviation approximately equal to 33%, which seems quite small compared to the final retrieval error...

Thank you. It is indeed the case that the a priori standard deviation is set to 1000%, not the covariance. We will correct this in the manuscript.

14. P.29741, L.16: what is the correlation length ? Please specify or provide a reference

Will define correlation length exactly in the paper but essentially it is the length used to control the off-diagonal decay of the a priori covariance with altitude.

15. P.29741, L.19,20. Why didn't you use the spectral noise estimates included in the MIPAS L1B files ? These are specific to the individual measured spectra and therefore should be much more accurate than the estimates of Fischer et al. 2008. Later (P.29742, L.9) you state you are using apodisation. Did you account for the correlations introduced by apodisation while building the covariance Sy ? If not, can you demonstrate that the effect is negligible ?

The variability in the noise was found to be minimal. The forward model used an

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apodised ILS and we assessed the effect of correlating the noise when apodising. We will clarify this in the text.

16. P.29742, L.1: please explain why you did not consider NO_2 interference during the night. See also previous comment regarding the NO_2 distribution assumed for the calculations reported in Fig.1.

The NO_2 contribution was negligible and calculations showed it was not important in this microwindow.

17. P.29742, L.13: χ^2 less than 2.

It is a normalised chisq. Will clarify in text.

18. P.29742, L.26: see above comments regarding the possible NO2 interference.

See above comments in the reply.

19. P.29774, L.10,11: the mentioned differences are hardly visible with the color scale of Fig.4a.

The ratio plot (Fig 7) is optimised for the colour scale to highlight these.

20. P.29774, L.18: I would put a "comma" after further.

Will rectify

21. P.29752, L.15-17: correlations are not the primary cause of the problem here. The problem arises from the low sensitivity of the measurements to the composition in this height range (stratosphere).

Whilst we agree that there is low sensitivity, the point we were making was that we did observe an enhancement of retrieved C_2H_2 values at this altitude. The issue however is that whether this is real or an artefact due to vertical correlations.

22. P.29752, L.9-17: see the above comment regarding the conciseness of the discus-

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Interactive Comment

sion. Is it really needed a Section summary at the end of each Section ?

Will address conciseness issue

23. Figures. Please make sure that units are correctly reported on the axes of all figures and with the same conventions. E.g. units are missing from the vertical axis of Fig.2, they are wrong in the vertical axis of Fig.1. Sometimes units are reported within round brackets, sometimes after a "/". Please use the journal standards.

Will rectify

Interactive comment on Atmos. Chem. Phys. Discuss., 10, 29735, 2010.