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Atmospheric Measurement Techniques Discussions

Interactive comment on "Global distributions of C_2H_6, C_2H_2, HCN, and PAN retrieved from MIPAS reduced spectral resolution measurements" *by* A. Wiegele et al.

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We would like to thank the reviewer for his careful and detailed comments. In the following, the original comment is inserted in *italic face* and our reply is typeset in normal face.

Wiegele et al. present follow-up work on Envisat MIPAS retrievals for C_2H_6, C_2H_2, HCN, and PAN for the reduced spectral resolution mode of the instrument (in operation since Jan 2005). They discuss modifications of the existing Karlsruhe retrieval schemes with respect to the high spectral resolution mode of the instrument (in oper-

ation from Jun 2002 to Mar 2004) and the corresponding impact on spatial resolution and retrieval errors. They present in detail the retrieval results for a single orbit as well as monthly means for Oct 2007. The paper is in the scope of AMT. It should be published after the following comments are properly addressed by the authors.

General Comments

1) This paper does not present any new measurement technique or retrieval concept. It presents some new data obtained by minor modifications of the existing and published retrieval schemes in Karlsruhe. However, a spectral degradation from 0.025/cm to 0.0625/cm and some change of the spatial sampling pattern do not seem to be such a big deal? It should be clearly pointed out in the abstract and the introduction that this is follow-up work. Or provide better motivation. What was the challenge?

For detection of minor species reduced spectral resolution sometimes is a challenge, mainly because the problem of interference of the target lines by lines of other species is worse. This actually has led to a situation that some species are no longer detectable with MIPAS. Furthermore, the data characterization (retrieval errors, spatial resolution) is different for measurements of reduced spectral resolution. In the second sentence of the abstract there is an implicit statement that this paper contains follow-up work to related MIPAS high resolution spectra analysis ('The retrieval strategy followed that of the analysis of MIPAS high resolution measurements'). In the introduction a related statement indeed was missing and has now been added.

2) The introduction should include at least one paragraph on the scientific motivation of the paper. From my point of view this is required even in a journal like AMT, since scientific questions are of concern when a new retrieval dataset is produced. For example, the choice of the regularization or smoothing constraint made by the retrieval expert has impact on the noise-resolution trade-off of the result. Why was the smoothing constraint set to a particular value? Do the results fit the needs of the scientific users of the dataset?

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We will include information on this in the introduction.

3) The introduction as well as the rest of the paper should provide more references to external work, e.g. by providing information on other retrievals of the analyzed species based on measurements made by other sensors. The presentation is too strictly focussed on work done in Karlsruhe.

We agree that we missed this important point in the original version. We will include references related more to the scientific application in the introduction, and references related to retrievals in the introductive paragraphs of the retrieval section (because the latter references are gas-specific and would thus interfere with the current structure of the introduction).

4) In the results section of the paper it should be pointed out which fraction of the MIPAS data was processed with the new scheme. Just the sample orbits and Oct 2007 or are there more data available? A scientific user of the data would certainly like to know that.

That's a good point: We will add the information that meanwhile data analysis has been completed for the whole V4O period from January 2005 to January 2010.

Specific Comments

p. 5390, l. 1: The abstract should include one sentence that an example orbit as well as monthly means for October 2007 are discussed in the paper.

Ok, will be done.

p. 5390, l. 13: Place reference (Fisher et al.) behind 'MIPAS instrument', add reference to Envisat satellite.

The Fischer et al. reference will be moved as suggested. A suitable printed Envisat reference is hard to find, so we refer to a web address instead.

p. 5390, l. 15: 'a high number of species' is unspecific. Please clarify.

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Ok, will be done.

p. 5390, l. 25: 'which is one of different MIPAS-Envisat processors' What do you mean by that? Missing word?

We intended to say that there exist further MIPAS processors besides the IMK processor. For clarity, was will delete this subordinate clause here and will instead make reference to the other processors in the sections where this information is relevant.

p. 5391, l. 2: Provide information on inclination and local times of orbit.

Ok, will be done.

p. 5391, l. 13: In this paragraph information and references to cloud filtering are missing. However, this is an important issue for UT/LS retrievals.

Ok, information and reference will be included.

p. 5392, l. 12: How often do you encounter convergence failures? Is this an important issue? Do you provide quality flags to allow the scientific user of the data to identify possibly obscure results?

Although our convergence criteria are very rigorous, convergence failure does not occur very often (typically less than 1% of the retrievals). Results of non-converged retrievals do not find their way in our results database, so a related flag is obsolete. These data thus are not distributed to users nor are they internally used for any scientific analysis.

p. 5392, l. 20: Why do you extend the retrievals up to 52km altitude? For C_2H_6, C_2H_2, and PAN the AVKs shown in the paper drop to nearly zero at 30km. Isn't that inefficient in terms of CPU-time?

While one might save CPU-time by avoiding calculation of the Jacobians for the uppermost layers, retrieval of the entire profile is a nice self-consisteny test.

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p. 5392, l. 20: You should add a sentence that the lowermost tangent altitude varies due to cloud filtering. Since these are UT/LS retrievals it would be interesting to know what fraction of tropospheric data is lost due to cloud filtering. Which measures were taken to reduce this number as far as possible?

A statement on this will be added as suggested. We have found that even a small cloud signal can cause major artefacts in the results, so we are reluctant to take measures to reduce the number of lost spectra, in order to avoid the risk of cloud-related artefacts. The percentage of lost spectra depends largely on altitude, latitude and season, so that it is not meaningful to report average numbers.

p. 5392, l. 26: What is the reason for the negative side wiggle of the 5km AVK at 10km? Optically thick conditions? Did you check the kernel functions (Jacobi matrix)? Which influence has this undesired feature on the retrieval results? Is there a typical decrease in the profiles at the lowermost altitudes?

The atmosphere is colder in 10 km than in 5 km. If there is less C_2H_6 at 10 km, more radiance from 5 km will reach the instrument. In an ideal retrieval this effect is accurately modeled by the radiative transfer model but in a regularized retrieval radiative transfer calculations are based on smoothed vertical profiles. Regularization will spread retrieved C_2H_6 from the cold point towards adjacent warmer layers, resulting in overestimated modeled signal which the retrieval will reduce by reducing the C_2H_6 amount at 5 km to minimize the residual. Indeed some retrieved profiles do not decrease monotonically with altitude, presumably sometimes due to this effect. This is why averaging kernels are essential to correctly interpret the results.

p. 5392, l. 28: Here, as well as in the subsections for the other species you included the information on vertical resolution in the text, only. It would be much more convenient if this information is also available in Table 4, it already provides information on horizontal resolution.

Ok, will be done.

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p. 5393, l. 7: Is the estimate of the horizontal resolution of the retrievals based on analyzing just a single, horizontal line of the 2-D averaging kernel matrix (i.e. the one referring to the altitude of the 2-D grid point) or is the full 2-D field taken into account. The 2-D AVKs may have a complex shape and analyzing just a single row of the 2-D field may underestimate the horizontal resolution. It would also be interesting to know if the 1-D estimate of vertical resolution fits to the results of the 2-D analysis.

2D averaging kernels are based on a full multi-tangent altitude 2D Jacobian, as discussed in the paper referenced in this context. So the risk of underestimation does not apply to our method. Vertical resolutions estimated by this method and the usual 1D averaging kernels are consistent.

p. 5393, l. 13: Got a bit confused about the statement 'is attributed to parameter uncertainties discussed below'. I guess it refers to the ozone uncertainties mentioned in the next paragraph?

No, ozone is the predominating parameter error at 15 km and above but not at 11 km, the altitude of the residual spectra shown. At 11 km line of sight elevation errors are the leading error source (c.f. Tab. 3)

p. 5393, l. 16: It is mentioned that propagation of ozone uncertainties causes a significant retrieval error at altitudes above 15km. I was wondering if a joint-fit rather than a pre-fit may improve the results in this case? I noticed that the joint-fit approach was selected for the other species to avoid just this problem.

Joint-fitting does not always improve the retrievals, since each additional fit parameter destabilizes the retrieval. Our retrieval setup has been defined on the basis of numerous tests involving various options including joint-fitting interfering species.

p. 5393, l. 24: Is the modification of the microwindows based on a complete re-run of the optimal selection procedure or was it an ad-hoc choice?

The optimal selection procedure is optimal only with respect to errors which can be

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estimated ex ante, i.e. on the basis of calculated radiance differences. As soon as suspicious residuals can be observed in real spectra, we consider interactice microwindow selection superior. In our case, various setups have been tested (with respect to error estimates, residuals and results) and the most robust setup has been chosen.

p. 5394, l. 3: The stronger side-wiggles of the AVKs may indicate that the vertical smoothing constraint is too weak. How was the constraint chosen? Based on a parameter study?

Both under- and overregularization can cause side-wiggles (the latter via progagation of the smoothing error in altitude). The regularization was chosen by test retrievals for multiple setups. Evaluation and final decision was based on the χ^2, oscillations in the profiles and obvious systematic artefacts in the profiles.

p. 5394, l. 9: 'by a few times' is unspecific. Please clarify.

Ok, will be specified.

p. 5394, l. 13: 'at high mixing ratios' is unspecific. Please clarify.

Ok, will be specified.

p. 5395, l. 8: 'outmatches ... significantly' is unspecific. Please clarify.

Ok, will be specified.

p. 5396, l. 12: On how many profiles are the monthly means for Oct 2007 based? Do they include data for every day or just for selected days in that month?

10 days, 10541 limb measurements. This info will be added to the text.

p. 5397, l. 1: I did not find the information where on Earth the example orbit is located. Maybe provide the reader with a map or the longitudes of the Equator crossing?

We will include the locations of the whole orbit shown in Figure 9 in the map of Figure 10. Additionally the mixing ratios at 8 km or 12 km (depending on latitude) will be plotted

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as symbols with the same color code as the monthly mean values in the background.

p. 5397, l. 8: It may be confusing if it is mentioned first that C_2H_6 is mainly produced by biomass burning (or anthropogenic activity) and than the maximum concentration in Oct 2007 is found over the southern Atlantic Ocean. Add a sentence if this is due to advection.

Ok, will be done.

p. 5397, l. 15: For the other species you present not just a comparison with MIPAS HR measurements, but also initial comparison with other datasets or climatologies. Would be nice if the same is possible for C_2H_6.

We agree, this will be done.

p. 5399, l. 16: The reason for the N/S difference of PAN is not explained.

We have found a large N/S difference also in the PAN precursors in measurements made slightly before the measurements presented in our paper. This will be discussed in the revised manuscript.

p. 5400, l. 8: 'Plumes of different compositions and different mixing ratio enhancements can be found.' This is a very generic sentence and not really useful, I think. Maybe expand a bit in terms of scientific motivation for the paper. Is the analysis of plumes a major topic for the new data set? Since the conclusion section seems rather short at the moment, maybe repeat the retrieval characteristics (noise, resolution) once more, if there is nothing else to add?

We prefer the first option offered by the reviewer and instead of repeating technical information, we try to close the circle by referreing again to the scientific motivation we have now included in the introduction.

p. 5404, Tab. 1: What is the rationale to select if information on interfering species is determined by 'pre-fit' or 'joint-fit'?

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The use of pre-fitted mixing ratios of interfering species is computationally cheaper and (more important!) leads to better constrained retrievals; thus this approach is used by default. If we run into problems with this approach, e.g. because of spectroscopic line data inconsistencies, we consider jointly fitting the interfering species. The selection is based upon sensitivity studies.

p. 5406, Tab. 3: Maybe explain a bit better that 'LOS' refers to vertical pointing errors. Does 'ILS' refer to the ILS width or other uncertainties as well? 'gain' should be explained better as well (radiometric calibration error). In the table you can use 'ngg' instead of » in LaTeX.

Ok, LOS, ILS, and gain will be better explained; ILS refers to the width uncertainty.

p. 5406, Tab. 3: Did you analyze the retrieval errors due to uncertainties of spectroscopic data? I would have guessed that these are quiet important errors, assuming that the spectroscopic parameters may not be well known?

That's a good point. We will report spectroscopic data uncertainties in the revised version. However, since some of these error estimates need some discussion, we prefer to provide this information in the text rather than in the table.

p. 5409, Fig. 1: This type of plot typically includes an additional curve showing the area of the averaging kernels as a function of altitude. It would be helpful to include it here to infer the altitude range where the retrievals are sensitive to measurement information. This aspect should also be discussed in the text of the paper.

We don't do optimal estimation but use a first order difference matrix to construct the regularization matrix. This means that the areas of the averaging kernels are always close to unity. In other words: Our retrieval is a smoothed version of the truth but does not include external prior information on the gas amounts. The amounts themselves are in the nullspace of the regularization matrix.

p. 5410, Fig. 2: I see zero radiance rather than 'gaps' in the plot?

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The figure caption will be corrected.

p. 5417, Fig. 9: What do '+' and 'x' in the plot indicate? Add additional x-axis labels showing the longitudes of the measurements or provide data on Equator crossing at least.

These symbols indicate daytime and nighttime measurements. Instead of an additional axis in Fig. 9 we will include the measurement geolocations of the shown orbit in the map in Figure 10.

p. 5418, Fig. 10: What kind of smoothing method or algorithm was used to produce these map plots? Was the data preprocessed with a box mean? Which box sizes? It looks a bit, as if outliers are present in the results, e.g. the red triangle at (50N, 90E)? You should add the information that missing data (white areas) is due to cloud filtering.

Monthly means are calculated for latitude-longitude boxes of 5 times 15 degrees. This information will be included in the text.

Technical Corrections

All technical corrections will be implemented. Thanks a lot for spotting them!

Interactive comment on Atmos. Meas. Tech. Discuss., 4, 5389, 2011.