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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Retrieval of global upper tropospheric and stratospheric formaldehyde(H_2CO) distributions from high-resolution MIPAS-Envisat spectra" *by* T. Steck et al.

T. Steck et al.

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Final author reply by T. von Clarmann on behalf of all co-authors

We thank referee 2 for his/her helpful and comprehensive comments. With respect to his/her suggestions we will perform the following changes (for convenience, the review is inserted in *italics*:

Reply to the general comments:

I have trouble reconciling the incredibly low spectral signal strength of formaldehyde as compared to the other species in Figure 2 with the relatively low systematic errorin

Figure 4. Could the authors show the relative spectral influence of H2CO and other species by plotting the diagonal of sqrt(KSKt) for water, H2CO, or other gases, as well as compared to the spectral noise; where K is the Jacobian and S is the error matrix (or a priori matrix in the case of H2CO)? This will give the expected spectral variability caused by H2CO variations, as compared to the spectral influence of other species and the measurement error.

There seems to be a misunderstanding on what the noise and systematic error components in Figure 4 are reflecting. In our terminology, the systematic error contains only errors due to uncertainties in spectroscopic data and instrumental line shape (ILS), as indicated in Figure 4 and Section 4. Since interfering species (as e.g. water vapor and ozone) are jointly retrieved with formaldehyde, their uncertainties are described by the retrieval error covariance matrix, and their error propagation to H_2CO, as characterized by the relevant off-diagonal elements of this covariance matrices, are already included in the variances determined for H2CO. We will reword the relevant parts of the paper to avoid this misunderstanding.

Smoothing error (Rodgers, 2000) is usually the dominant error source for constrained retrievals, where smoothing error is (I-A)S_a(I-A)^t (Rodgers, 2000). Why isn8217;t this term discussed or included? The smoothing error term should be shown in Figure 4 and included in calculations of the estimated error for H2CO. An estimation of Sa should be available from model climatologies or other satellite datasets.

According to Rodgers (2000) (page 48/49) there are two possibilites to deal with the smoothing error. It can either be evaluated using his Eq. 3.17, or its evaluation can be abandoned and “the retrieval can be considered as an estimate of a smoothed version of the state, rather than an estimate of the complete state". We have decided for the latter option (and we report the altitude resolution), because the evaluation of the smoothing error requires the covariance matrix of a real ensemble of states. The latter is not available and cannot be constructed as the reviewer suggests, because neither global model climatologies nor other satellite datasets include the small scale variability of the true state but are smoothed versions of the complete state themselves. Further, other satellite datasets include measurement errors which cannot easily be distinguished from the natural variability.

The validation shown in Tables 2 and 3 do not seem to adequately support the validation of MIPAS H2CO.

By intention, we have not used the term “validation" in the paper at all. The section on “comparison" shall serve the purpose to put our work into the context of existing published literature, and to give an initial idea if our retrieved values are of similar size compared to those of other satellite measurements. Validation on the basis of unpublished data from other instruments, involving also the instrument scientists of the validation experiments to obtain correct instrument characterization etc, is beyond the scope of this paper. Nevertheless we will try to improve the comparison as outlined in the following.

In Table 2, it is hard to evaluate whether MIPAS and ACE agree without error bars, with just a single comparison case...,

We have not found any more detailed information on ACE H_2CO in the paper by Coheur et al.. The only H_2CO information in their paper is a profile measured in the biomass burning plume without error bars. Nevertheless, we feel obleged to put our work into the context of prior publications. What we can do is to include the MIPAS standard error.

... and without knowing the prior.

To our knowledge, ACE retrievals are based an a maximum likelihood retrieval where no formal a priori information is involved and where the altitude resolution is purely determined by the tangent altitude spacing. The MIPAS a priori information is 8211; as stated in the text 8211; a flat all zero profile, which, along with the first order differences operator, just smooths the retrieved profile without pushing it towards a specific a priori

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value.

I think that the following could be done to make this table useful (1) show comparisons between MIPAS and ACE for BOTH a plume region AND a nominal region to show that the MIPAS results are from sensitivity rather than biases or fluctuations.

To our knowledge, there are no such ACE profiles available in the scientific literature.

(2) Show predicted errors for the MIPAS results so that differences can be evaluated compared to reported errors.

Since in Table 2 we do not report a single MIPAS profile but an average of all 10° N profiles analyzed from 8 Sep to 1 Dec 2003, the appropriate measure of variabilty is the standard error, because this also includes the uncertainty due to natural variability, not only the predicted errors. We will add this information.

(3) Show the MIPAS prior values, to evaluate the starting point for the retrieval.

As stated in the paper and said above, the MIPAS a priori profiles are flat all zero profiles, and they act, along with the first order differences regularization operator, only as a smoothing constraint. Since we iterate until convergence is achieved, the initial guess profiles chosen as starting point (which in our retrieval are distinct from the a priori profiles) have no influence on the result. Please note, that, encouraged by the other reviewer, this part of the paper has been considerably changed: Given the coarse altitude resolution of the MIPAS measurement, it is not meaningful to compare profiles. We now compare partial zenith column amounts instead.

In Table 3, it is again difficult to see whether MIPAS H2CO is doing well. It might be useful to calculate the rms and bias of MIPAS vs. Odin, MIPAS vs. Reprobus, and Reprobus vs. Odin. It would be useful to see if the rms and bias improves over the MIPAS a priori. This would support that the MIPAS retrieval adds value over the a priori. It is difficult to evaluate the value added by MIPAS in the table8217;s current form.

This comment seems to be caused by the same misunderstanding as discussed above.

We don8217;t do an optimal estimation retrieval. Any deviation of the MIPAS profiles from zero is measured information, degraded only by noise and smoothing. While optimal estimation based retrievals just reproduce the a priori information when the measurement does not contribute any information, this is not the case with our retrieval. The test suggested by the reviewer seems not applicable, i.e., not meaningful to our retrieval. What we could do instead is to include the standard errors of the mean profiles for MIPAS.

Also, Reprobus does not seem to agree with MIPAS or Odin but I don8217;t see discussion of this.

Given the fact that MIPAS does not use a priori information in the sense of optimal estimation but rather more or less verifies the expected amounts of H_2CO, we think that the agreement is not that bad. Nevertheless we will add some discussion of the deviations.

Figures 7-9 are very compelling and interesting. The patterns are discussed in broad terms; but how do these results compare to models or previous studies, particularly for the diurnal variations. Is this a new result? I look forward to reading the revised paper, and congratulations on the new MIPAS species!

Initial comparisons with the MESSy (Modular Earth Submodel System) model show similar patterns in both the model and our zonal mean values. The diurnal variation is also visible in the model. In section 5 we will add a link to the MESSy webpage.

Reply to specific comments:

Section 3, “A regularization strength alpha of 104 was found optimum."; It isn8217;t entirely clear what is being optimized. Could the authors specify what is being optimized? Is it the minimum of Trace(A) + Sqrt(Trace(Sn))? Also, did the authors consider optimizing towards the best regional average or focus on optimizing a single retrieval?

The term “found optimum" suggests a more formal optimization than actually has been

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Interactive Comment

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done. We will replace this statement by 8217;has been selected as a reasonable trade-off between the number of degrees of freedom and the noise error8217;.

Section 5 Line 19 "In the upper tropical tropopause region, mean values exceed 60 pptv. These larger values are attributed mainly to biomass burning emission."; Could the authors clarify this statement, either state a source for this attribution, or explain why they are attributed this to biomass burning emission, since there are other sources of formaldehyde?

Indeed this statement is too strong and we don8217;t know the actual sources of the H_2CO enhancement. We will reword the text as to avoid over-interpretation.

Section 3, "...and L1 the first order derivative matrix (as discussed by Steck, 2002), which smoothes the solution without biasing it towards the a priori profile."; I think that this statement should be clarified. The first derivative matrix biases the profile shape rather than value. In regions of reduced sensitivity, this matrix pegs the retrieval to the closest sensitive point and propagates the a priori shape throughout the insensitive region. From looking at the averaging kernel plots, this could be occurring below 15 km and above 50 km. This can result in a biased column which is weighted to the sensitive regions. I would change this statement to, "...which smoothes the solution without biasing it towards the a priori profile in regions which have at least some sensitivity"

We agree that smoothing actually can introduce some bias, and abundance information at altitude regions where the instrument is not sensitive should not be used to calculate column densities. We will change the wording towards one not as strong as our original one. However, we think the wording suggested by the reviewer is not fully exact, because the smoothing can also bias the retrieval at altitudes where the profile contains some sensitivity (e.g. maxima will be systematically low, minima will be systematically high).

Figure 2-Can this figure caption include, "the instrumental noise is on the order of 3 nW/(cm2)"? I would say include this in the plot, but neither scale is conducive to this.

Also, could the selected windows be over-plotted with a different color, like red, on this plot?

Agreed, will be done.

Section 4. I don8217;t see definitions for how line-of-sight uncertainty, total systematic error, spectroscopic data uncertainties, or instrumental line shape errors were calculated. Could equations or references for these errors be provided?

Agreed, will be done.

Figure 4- It is hard to distinguish colors; can the plotted lines be made thicker?

Both on our printed version and on the screen the lines appear quite thick. However we appreciate that the black and the violet line can hardly be distinguished. We will change this.

Conclusion "Comparison with other satellite instruments (ACE-FTS and Odin-SMR) show good agreement."; The statement "good"; needs to be quantified, such as something like, "...with comparisons between MIPAS and Odin improving over the MIPAS prior values, and enhancements seen for both ACE and MIPAS in biomass burning regions as 12 km."

Partly agreed; the wording will be changed in order to present a more specific conclusion. For the issue with the MIPAS a priori profiles, see our reply above.

Specific wording suggested changes

All wording issues raised by the reviewer will be clarified.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 13627, 2007.