Atmos. Chem. Phys. Discuss., 7, S5855–S5859, 2007
www.atmos-chem-phys-discuss.net/7/S5855/2007/

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.

Anonymous Referee #2

Received and published: 10 October 2007

This paper introduces the retrieval of a new species, H2CO, from MIPAS data. It discusses the characteristics and validation of this species using ACE and Odin data, and the Reprobus model. This paper concludes that the H2CO from MIPAS has 2-4.5 degrees of freedom and is useful for zonal mean values which have reported errors of about 3%. The methodology is clearly defined with overall good figures and data comparisons except as noted below.

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 error

Full Screen / Esc
Printer-friendly Version
Interactive Discussion
Discussion Paper

in Figure 4. Could the authors show the relative spectral influence of H2CO and other species by plotting the diagonal of sqrt(KSKˆt) 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.

Smoothing error (Rodgers, 2000) is usually the dominant error source for constrained retrievals, where smoothing error is (I-A)Sa(I-A)ˆt (Rodgers, 2000). Why isn'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.

The validation shown in Tables 2 and 3 do not seem to adequately support the validation of MIPAS H2CO. In Table 2, it is hard to evaluate whether MIPAS and ACE agree without error bars, with just a single comparison case, and without knowing the prior. 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. (2) Show predicted errors for the MIPAS results so that differences can be evaluated compared to reported errors. (3) Show the MIPAS prior values, to evaluate the starting point for the retrieval.

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 table's current form Also, Reprobus does not seem to agree with MIPAS or Odin but I don't see discussion of this.

ACPD

7, S5855–S5859, 2007

EGU

Figures 7-9 are very compelling and interesting. The patterns are discussed in broad terms; but the how do these results compared 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!

Specific comments

Section 3, "A regularization strength alpha of 104 was found optimum."; It isn'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?

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?

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"

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.

ACPD

7, S5855–S5859, 2007

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

Section 4. I don'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?

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

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."

Specific wording suggested changes

Abstract I might simplify this wording, which is currently: "Formaldehyde single profile retrieval is formally possible, however with a large noise error (more than 60%), which is the dominant error source" Calculation of zonal mean values for 30 days of data during 8 September 2003 and 1 December 2003 reduces the noise induced error by a factor of 20 or more.

To this wording: "Single profile retrievals of formaldehyde are dominated by a 60% noise error, however zonal mean values for 30 days of data during 8 September 2003 and 1 December 2003 reduces this error by a factor of 20 or more."

Section 1.1, line 6, "First stratospheric measurements of formaldehyde have been made" I would change to "The first stratospheric measurements of formaldehyde were made..."

Section 1.2, line 24, "MIPAS operated in its nominal mode from July 2002 to March 2004 in high spectral resolution of nominal 0.025 cm-1." I find this sentence confusing. Do you mean, "MIPAS operated in its nominal mode from July 2002 to March 2004 with

ACPD

7, S5855–S5859, 2007

high spectral resolution of 0.025 cm-1."?

Section 2, line 20, "to the instrumental noise which is in the order of" I would change to "on the order of".

Section 3, "where x is the estimated and xt is the true H2CO profile, simplifies for xa = 0 to x = Axt." I would switch this wording to "...simplifies to x = Axt for xa = 0"

Section 3, "This is again to ensure that all structures visible in the retrieved distributions originate from the measurements and are not artifacts due to any constraint." I would suggest changing the wording (the use of the word "constraint" might be confusing as it is used here to mean more than the a priori constraint matrix) to "...are not artifacts resulting from the retrieval setup."

Figure 2- "inclued" misspelled.

Conclusion "To reduce the large noise error component, profiles have been averaged leading to a reduction by a factor of 20 or more.". Can the authors state the final error, with something like, "To reduce the large noise error component, profiles have been averaged leading to a reduction of the noise error to 3% or less."

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