Atmospheric Chemistry and Physics Discussions

Interactive comment on "Simultaneous atmospheric measurements using two Fourier transforminfrared spectrometers at the Polar Environment Atmospheric ResearchLaboratory during spring 2006, and comparisons with the AtmosphericChemistry Experiment-Fourier Transform Spectrometer" *by* et al.

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General comments

This paper deals with partial column measurements of ozone relevant stratospheric species. Three related issues are covered, namely intercomparison of two different

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ground-based FTS instruments, validation of a new atmospheric chemistry satellite mission (ACE), as well as a geophysical analysis of the stratospheric chemistry and dynamics evolution over Eureka in spring 2006.

The paper is generally put on a good scientific basis and the covered issues fit well into the journal scope. The following general and specific remarks are intended to help along the process towards final publication in ACP.

Side-by-side intercomparisons of ground-based remote sounding instruments are of very high interest within the NDACC (Network for the Detection of Atmospheric Composition Change) community. There have been earlier studies (e.g., Murphy et al., 2001; Wunch et al., 2007) and there are planned future intercomparisons (e.g., Sung et al., to be submitted to ACP, 2008). It would be desirable to make it more transparent to the reader what the complementary information of this paper is relative to these others. Also the relation of this paper compared to other ACE validation papers could certainly be made more obvious.

The message of the paper could be presented more efficiently. The subsequent specific remarks are intended to provide some hints where to shorten or restructure the wording, and correct details. In addition, since co-authors are involved who have shown many times before their ability to write excellent papers, it would be highly desirable if they would actively contribute to this process.

Specific comments

5305/1

Title is lengthy. Probably it could be shortened.

5307 <Abstract>

The abstract as is tells in a qualitative way what has been done: FTS-FTS comparisons, FTS-ACE comparisons, time evolution of ozone relevant trace species. This could be said in one or two sentences at the beginning. But what then is missing is

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the outcome of what has been done, which would be the main content of the abstract. Give a very short summary of all important results of your paper. It is certainly more than the statement that your intercomparisons are consistent with earlier studies. Give numbers or at least quantitative wording for all important new findings.

5308 <Introduction>

Some reorganization/grouping would help to improve readability. Usually, in the introduction the state of the art would be described, then some deficiency or lack within this, and then how the present paper intends to improve upon this deficiency, i.e., the focus or goal would naturally be developed towards the end of the introduction. Currently, something about the focus/goal is said already in the first paragraph (5308/16) and then again and again at several places throughout the whole introduction.

5309/5-17

This section, describing instrumentation which is not exploited within this paper, could be cancelled or at least shortened.

5311/5 <2 Ground-based instrumentation and observations>

This section could be shortened since both PARIS-IR and the DA8 at Pearl have been described in earlier papers. Restrict to changes/amendments relative to what has been said in earlier papers.

5314/13 <. . . the DA8 FTS used a Hamming function>

Explain: why not boxcar?

5314/22 <. . . the ILS parameters obtained from the cell measurements were used as a priori inputs and values were retrieved from the atmospheric spectra . . . >

Specify: which parameters exactly did you retrieve?

What would be the difference if you retrieve ILS parameters using ideal ILS parameters

as a priori?

Explain: why do you additionally retrieve the ILS parameters from the atmospheric spectra and not just input the ILS parameters you retrieved form the cell measurements as fixed parameters into the forward model?

5315/13 <Smoothing error, also known as null space error in the Rodger's OEM formalism, arises from the limited altitude resolution of the observing system. . . >

Rodger's -> Rodgers

I know this is a frequently made statement, which is not correct, however. Limited altitude resolution is not the only contribution to smoothing error. There are other possible inherent physical contributions, e.g., the loss of sensitivity above certain altitudes (this is an effect in addition to the limited altitude registration/resolution, it is, e.g., due to line saturation), or non-physical secondary contributions like the smoothing effect from the (mathematical) regularization imposed upon the retrieval to reduce (trade off against) retrieval noise error. These things might be difficult to become both detailed and correct here. I suggest to skip any hand waving explanation, and just give a specific literature reference instead.

5315/25 <. . . (DOFS), which are the number of independent quantities obtained from the observations>

you probably mean: obtained from the retrievals; or: retrieved from the observations.

5315/26 <. . . (the vertical partial columns in a specified altitude range, in this case)>

This is a frequently made miss-interpretation: DOFS gives the number of independent pieces of information; this is not equal to a number of partial columns that can be retrieved independently: you can easily check this out by looking at your n=DOFS different partial column averaging kernels: they are never rectangular functions, which your statement implies, however.

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5316/5 $<$According to the Rodger's OEM theory, the averaging kernel is the derivative of a derived parameter with respect to its a priori state value . . . $>$

Rodger's -$>$ Rodgers

First of all: don't talk about a derived parameter, talk about a retrieved state vector instead (averaging kernel for a retrieved scalar parameter is a very special issue of rare practical relevance).

Secondly, the averaging kernel matrix is the derivative of the retrieved state vector with respect to the true state (not with respect to the prior state).

Thirdly, be more correct in wording, please distinguish: i) averaging kernel matrix, ii) averaging kernels (which are the rows of the matrix), and iii) total column column averaging kernel.

All in all, I suggest to avoid any self-made explanations of elsewhere described issues, just talk about total column averaging kernels here.

5316/6 $<$. . . when this normalized derivative is small (nearly 0) all of the information comes from the a priori and when it is large (near 1) then the information in the retrieval comes mainly from the measured spectra (Rodgers, 1976, 1990, 2000).$>$

The derivative for calculating averaging kernel matrices is per definition unitless - in this sense it is trivial that it is normalized and this must not be mentioned. Confusion arises from the fact that the state vector quantity can be normalized (or not): you can calculate an averaging kernel matrix for some state vector quantity (e.g., profile given in VMR) or for a normalized state vector (e.g., scaling factors for each layer of a profile, which is the case with SFIT2). In both cases the averaging kernel matrix is unitless as said above. The rows of the matrix of the second case are sometimes regrettably called normalized kernels, although it is meant that the underlying state vector is normalized. Anyway, it is important to be aware of this difference since the resulting averaging kernel matrix will look different in general.

Again, I suggest to just talk about total column averaging kernels (since this is what is shown in Fig. 1, cancel <normalized> in the Figure caption), and avoid all unnecessary explanations. Your wording <normalized derivative> causes also some additional confusion, since total column averaging kernels can per definitionem not be directly derived from a retrieval using a normalized state vector like SFIT2.

Finally, since the wording is not absolutely clear, I like to ask this question on the underlying procedures: how do you exactly calculate total column averaging kernels shown in Fig. 1, knowing the fact that SFIT 2 is internally using a normalized state vector? Detail the individual steps of your procedure, please.

5316/9 <. . . (Rodgers 1976, 1990, 2000)>

Averaging kernels are not treated in Rodgers (1976).

5316/9 <. . . averaging kernel profiles . . . >

unusual and never defined term, use: total column averaging kernels

5316/13-14 refer to Table 1 here for the first definition of (MW 1120)

5321/12-16 < For all six of the species investigated, the comparisons using daily mean values show larger differences (by up to 13%) between two FTSs than those using the individual observations recorded simultaneously. Variation in the measured total column densities during each observation day, as shown in Figs. 3, 4, and 5, arises from the temporal and spatial differences in Arctic atmospheric composition.>

I agree with the overall tendency but I do not strictly see this from Table 3: e.g., the difference is much higher for ClONO2 (17.62/4.28) than for HCl (7.84/3.22). Why? Wouldn´t another/additional explanation be a possible zenith-angle dependency of the retrieval/raytracing - could you try to check/exclude this?

Optional remark: you have a highly valuable a data set at hand. Some further exploitation could allow a step forward in ClONO2 spectrometry. This would be of very

high interest to the ground-based community: Discuss the potential reasons for the discrepancies (line shape, zero line, interfering species, resolution?) and an estimate of their relative contribution to the discrepancies observed. Have you checked these instrumental things for the two different instruments? What do you find? Did you play with different implementations of the Reisinger dual micro-window approach (fitting interfering species as profiles or only via column scaling? What is the impact on the discrepancies between the two instruments?)

5323/11 I suggest you cancel eq. (3) because it is a trivial and well known relation.

5326/26 <The largest variation in the ratios is for ClONO2 for which the standard deviations of the mean difference are 28.3% and 14.2% for DA8 FTS and PARIS-IR, respectively. To some degree, this reflects the challenge in retrieving this molecule from the ground-based spectra.>

What do you mean by the second sentence? Please explain in some detail.

5326/29 <In general, the results from PARIS-IR show better agreement with ACE-FTS than the DA8 FTS results. This is most likely due to better temporal coincidences.>

I am lost here: Are you still talking about ClONO2 only at this point? I ask because the same holds true mainly for the Table 5 numbers for O3 and HCl, but it does not hold true for NO2.

5332/21 <. . . PARIS-IR and the DA8 FTS, these differences can be taken to be due to instrumental differences such as spectral resolution.>

I understand it is just an assumption that resolution effects dominate. You could easily try to verify this by artificially degrading the spectral resolution of the DA8 spectra and show whether the discrepancies decrease or remain.

5341 Table 1, footnote c: <Several spectral ranges from individual spectra were used in the retrievals simultaneously>

Which ones exactly were fitted simultaneously? What about ones that were not fitted simultaneously? Were the results averaged together (via some weighted mean?);

5341 Table 1, footnote c <simultaneously; also known as "multi-microwindow" fitting.>

can cancel this without loss of information

5342 Table 2, footnote c < Total Error= sqrt ((Smoothing Error)2 + (Retrieval Noise Error)2 + (Model Parameter Error)2).>

Just one optional remark: we have recently shown that an additional class of errors, namely interference errors can become significant, i.e., comparable to the smoothing error (Sussmann, R. and Borsdorff, T.: Technical note: Interference errors in infrared remote sounding of the atmosphere, Atmos. Chem. Phys., 7, 3537-3557, 2007). Implementation of this interference error quantification may be difficult at this time for all the different retrievals of your paper, but perhaps this and also the possible minimization of the interference errors might find your interest within future work.

5351 Figure caption: cancel <normalized> without loss of information.

Ralf Sussmann, 22 April 08

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 5305, 2008.