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Atmospheric Measurement Techniques Discussions

Interactive comment on "Reviewing the development of a ground-based FTIR water vapour profile analysis" *by* M. Schneider and F. Hase

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Dear Editor, dear Referees,

we appreciate very much your comments on the manuscript. Your efforts allow a further improvement of our paper. In the following we address in detail your comments/questions:

Editor Comment, 26 May, 2009:

Concerning the title, it's true, that we review mainly our own work. This is due to the fact that we are the first working group to develop a ground-based FTIR water vapour profile analysis. What we do is novel. Consequently, we are not happy with the title you suggested: "Improving the accuracy of ground-based FTIR water vapour profile

estimation". The word improving may be misleading, since it would imply that there already exist retrieval setups allowing for a determination of water vapour profiles with useful precision from ground-based FTIR measurements. However, this is not the case. Our work is pioneering in the sense that it allows, for the first time, a useful estimation of water vapour profiles from ground-based FTIR. The paper documents in detail how the different innovations improve the quality of the water vapour profiles. We still think that the two central aspects of the paper, i.e. development and documentation how these innovations improve the quality of H2O profiles, is best described by the title "Reviewing the development...", and we would like to ask you to reconsider your decision. If you are convinced that the word "reviewing" should be omitted, what about simply: "Ground-based FTIR water vapour profile analyses"?

Anonymous Referee #1, 24 May, 2009:

We will consider all the technical comments for the final version of the paper. Concerning the comment "State what is important about high and low quantum numbers – presumably high and low temperature sensitivity.", we will add in Section 3.4: "The spectroscopic line parameters, used for describing the absorption signatures, are obtained from laboratory measurements interpreted in terms of quantum theoretical calculations. The measurements as well as the calculations are less reliable for transitions involving high quantum number states. These lines are very weak which makes accurate laboratory measurements difficult, and quantum theoretical calculations become delicate and less constraint by experimental data."

Anonymous Referee #2, 25 May, 2009:

1.the title is still discussed with the Editor.

2.We will add a graph with the typical averaging kernels calculated for a second very distinct site: the subarctic, low altitude site of Kiruna.

3.We agree. Here we deal with a non-linear problem and the avks depend on the state of the atmosphere. This affects the smoothing error. It is generally the larger the higher the PWV. Figure 1 depicts typical avks (for typical PVW). At lower PWVs the FWHM of the kernels is smaller and at high PWVs it is broader. This non-linearity also affects the sensitivity with respect to other error sources. The error sensitivity is the larger the smaller the PWVs. We will discuss this issue in the text, where we discuss the avks. In this context we will refer to Schneider et al., 2009, where we compared the FTIR profiles, measured during more than 4 years, with coincident radiosonde (Vaisala RS92) profiles. This comparison is representative for all atmospheric situations, and shows that the quality of the FTIR profiles is very good for all atmospheric situations.

4.The Izana station offers unique possibility for comparison with daily meteorological radiosondes. An empirical validation as shown in Table 1 and Figs. 4 is therefore only possible for the Izana observatory, not for the Kiruna site. As aforementioned we will add a plot with avks for Kiruna demonstrating the quality of the measurements at a low altitude site.

5.Yes, indeed, there is large effort in eliminating the inconsistencies between different radiosonde types. However, the errors of the sonde sensors not only depend on the type of the sensor but also on the date of manufacturing, time of storage, etc. (see Fig. 7 of Turner et al., 2003) It is difficult, if not impossible, to account for all these uncertainties. Furthermore, in particular at colder temperature (upper troposphere), radiosondes remain uncertain. The situation for gb FTIR measurements is different. At many stations the same instruments are applied over many years. Uncertainties in the spectroscopic line parameters cause mainly systematic errors. They do only very weakly affect the precision of the water vapour profiles. Furthermore, the raw data (measured solar absorption spectra) are stored and can be reprocessed whenever there is some progress in the characterisation of the spectroscopic line properties. We agree that a careful monitoring of the FTIR by performing regular gas-cell measurements is required to avoid spurious trends in the deduced H2O profiles, but all FTIR sites with such a record have the potential to generate H2O profile timeseries which

can serve as a more reliable source for H2O trend studies than sonde data, especially in the upper troposphere.

6.We agree. The measurement frequency depends on man power and weather conditions. We will delete “continuous” on page 1222, line 24, and discuss this issue at the end of the Conclusions Section. In particular, we will mention that the FTIR observations are limited to clear sky conditions which causes a dry bias. In this context we will refer to Schneider et al., 2009.

7.Yes, one could think about tropospheric CO, which is also variable in time and has a strong vertical gradient. Concerning O3, it was already shown in Schneider and Hase (2008) that the temperature retrieval is important. Notification: It is ln(vmr). We will correct it, where wrong.

New References:

Schneider et al., 2009: Atmos. Meas. Tech. Discuss., 2, 1625-1662, 2009. Turner et al., 2003: J. Atmos. Oceanic Technol., 20, 117-132, 2003.

Anonymous Referee #3, 5 June, 2009:

Major comments:

-“The language is often unprecise and unscientific”: We agree with most of the specific comments of the Referee. They are answered below. However, concerning the use of “we” and “our”, we think that it is a question of style and gustoes. For example, I personally prefer a “first person style” instead of a “passive style” (has been done, has been shown, etc.). Nevertheless, we will go through the manuscript and critically revise the use of “we” and “our”.

-Yes, we agree. In the lower stratosphere there is almost no sensitivity. We will state this in the text and furthermore, cut Figs. 4 and 6 at 15 km. We will change the x-axis scale of the three bottom right panels of Fig. 4 to -50% to +100%.

Specific comments:

1.“We review the most important features of analysis and radiative transfer modeling required for monitoring tropospheric water vapour profiles by ground-based FTIR experiments” will be changed to: “We review the most important features of the retrieval and of the radiative transfer modeling required for a reasonable monitoring of tropospheric water vapour profiles by ground-based FTIR experiments”.

2.please see points 5 and 6 of answers to Refere #2.

3.“inversion of the atmospheric state ...” is a commonly used expression, and we agree, it is not correct! What is inversed is the spectrum. We will adopt the suggestion of the Referee and change these expressions with something like “the retrieval of the atmospheric state”.

4.We agree. We will change the sentence to: “The large vertical gradient and variability is the main reason why a standard retrieval setup is not suited.”

5.The instrumental line shape is the spectral instrumental response function. It is a well known term within the FTIR community.

6.In Section 3.2, we mention that the formalism requires Gaussian statistic. We will also mention it already here and change “Therefore, we have to minimise” to “Assuming pure Gaussian statistics we have to minimise”.

7.Equation (6) is general in the sense that the matrix A is for the same iteration step as the Jacobian matrix K used for its calculation. Typically one is interested in A at the solution state (last iteration step).

8.Yes, the kernel is meant?. We will change to respective sentence to: “The averaging kernel matrix A documents how the remote sensing system detects real atmospheric variabilities. It documents its sensitivity and how it smoothes fine vertical structures”.

9.A_hat is A at the solution state. We will explain it in more detail.

10.Yes, we agree. Here we deal with a non-linear problem. And the accurate way is to perform the smoothing by forward calculation and subsequent retrieval (as performed in Schneider et al., 2006a). Not considering this non-linearity introduces additional scatter in the RS92/FTIR intercomaprison. However, an accurate smoothing is more work intensive than performing the smoothing according to Eq. (8) and it is not necessary for our objectives: a documentation of the improvements achieved by the new retrieval set ups. We are not so much interested in the absolute values of the scatter between RS92 and FTIR, but on the improvements. Since we always use the same approximation, the documentation of how the new retrieval setup improves (oder: how each step of refinement improves the quality of the retrieved ...) the quality of the water vapour profiles is not affected by this simplification.

11.Yes, we agree. We will change the sentence to: “The result is an RS92 profile (xĖĘRS92) with the same vertical resolution and sensitivity as the FTIR profile”

12.“satisfactory” will be changed to “similar”

13.We will change the order of Figs. 4 and 5.

14.This is difficult to judge from Fig. 4 alone, because changing from linear to logarithmic scale retrieval also changes the avks. Applying the logarithmic retrieval we observe the tendency to higher sensitivity in the upper troposphere (see Schneider et al., 2006a), which may explain the larger scatter between RS92 and FTIR if compared to the linear retrieval which keeps more fixed to the a priori profile. A higher sensitivity means also higher sensitivity with respect to systematic errors like spectroscopic parameters. This may explain the increased systematic FTIR/RS92 difference above 8km.

15.FTIR PWVs are calculated from the profiles by integration over altitude (an explaining sentence will be added).

16.We compare when all three experiments: FTIR, Cimel, and RS92 coincide. However, these coincidences are not perfect, neither in time nor in space. For example we compare the measurements that are closest in time. For FTIR/Cimel this is quite straight forward, since both measurements are performed within several minutes. But for the RS92 the situation is more complex, since it need more than one hour for a measurement. This has to be kept in mind. We find that the root square sum of the scatter FTIR/Cimel and FTIR/RS92 is smaller than the scatter Cimel/RS92 and conclude that the FTIR cannot contribute much scatter, i.e. must be very precise. We will rewrite this paragraph in order to make these points clearer.

17.–19. yes, will be done.

21.we will also briefly discuss the reduction of the biases

22.we will change useless to unphysical

23.The use of signatures seems not incorrect to us. You think that the use of signatures is misleading here? Hmm, we will think about it.

24.we would prefer to keep the order, since it orders from the most important to the less important features.

25.According to Fig. 9 of Vömel et al.,2007 the residual uncertainty (the 1σ scatter they found between RS92 and CFH) is between 5 and 10% below 5 km, but it can reach 20% above 5km. But this values requires that the radiation correction is performed accurately (see Fig. 6 of Vömel et al., 2007), which is difficult since it depends on the intensity of the solar radiation (solar elevation, atmospheric aerosol loading, atmospheric water vapour, ...). On the other hand, there is a new study of Miloshevich et al., 2009, which reports radiation corrections for different solar elevations and a lower residual error (only 4%???). We will add a brief discussion about these radiosonde uncertainties.

Figure 6: The shaded area is the expected natural deltaD variability (estimate from in situ measurements). An explanation will be added to the Figure caption. Technical

comments: will be considered.

New Reference:

Miloshevich et al., J. Geophys. Res., 114, D11305, doi:10.1029/2008JD011565, 2009.

Interactive comment on Atmos. Meas. Tech. Discuss., 2, 1221, 2009.