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Atmospheric Measurement Techniques Discussions

Interactive comment on "Tropospheric water vapor profiles obtained with FTIR: comparison with balloon-borne frost point hygrometers and influence on trace gas retrievals" *by* Ivan Ortega et al.

Anonymous Referee #2

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1 General Comments

The paper deals with two main topics:

a) first the authors assess the limitations in retrieving the real Water Vapor (WV) vertical variability from the boundary layer to the upper troposphere - lower stratosphere, with a standard inversion of FT solar absorption measurements in the middle-infrared. The study includes the validation of WV profiles retrieved from

ground based FTS measurements operated from Boulder (Colorado) and Mauna-Loa (Hawaii), via intercomparison with WV profiles measured by state-of-the-art Frost Point Hygrometers (FPH) operated from balloons.

b) Secondly, a sensitivity study is presented, showing the error on retrieved HCN, CO, and C_2H_6 VMRs due to assuming a less than perfect WV vertical profile.

The subject of the paper is clearly within the scope of AMT. The methods used are scientifically sound, the presentation is sufficiently concise, however it could be improved by rephrasing a few sentences as outlined in the specific comments reported below. The paper does not introduce novel concepts or ideas, however the results of the study will be useful for other scientists using the data presented or data deriving from similar measurements. For this reason I recommend this paper for publication in AMT, after some revisions as outlined below.

My main comment or criticism is about the strategy the authors adopt to deal with the Averaging Kernels (AKs). I agree that the AKs may not be a sufficiently accurate tool to evaluate the smoothing error of the retrieved WV profiles. This is due both to the fact that AKs are only a "linear" approximation of the vertical response function of the measuring system (instrument plus retrieval algorithm), and to the fact that it is generally hard to setup a covariance matrix which represents properly the variability of WV from ground to the Upper-Troposphere / Lower Stratosphere (UTLS). To show the limitations (in your test case) of the smoothing error as derived from AKs and the Rodgers (2000) approach, rather than moving the AKs analysis to the supplemental material, I would have compared, in the main paper, the actual smoothing error (obtained via intercomparisons with FPH) with the estimate of the same error obtained from AKs.

The second general comment I have is about the sensitivity analysis presented in Sect. 5. To my opinion it would be worth to better explain why, after the analysis presented in the first part of the paper, then you start to study a quite different subject, such as the mapping of WV errors on subsequent VMR retrieval of other gases. More-

over, since your measurements cover the middle-infrared, I also expect a sensitivity of the retrieved VMRs to the temperature error. This is already shown in Fig. 3 for WV. What about the error on HCN, CO, and C_2H_6 VMRs due to the temperature error ? Do you suggest to retrieve also the temperature profile from the same measurements or you are satisfied with the temperature profiles taken from NCEP at NDACC ?

2 Specific Comments

P4 L27,28: Constraining is important to select the solution which, among the possible solutions of the ill-posed inversion, is the most likely on the basis of our prior knowledge.

P4 Eq.1: Here it is not clear if your retrieval performs only a single or several Gauss-Newton iterations, because you don't have an iteration index in the Equation. Please, also define clearly the meaning of $\mathbf{K}$. Do you compute it at each iteration ? I guess $\mathbf{K}$ is the Jacobian of the forward model with respect to the retrieval parameters, therefore it should be re-computed at each iteration and should show an iteration index.

P5 L2: It would be interesting here to know which is the spectral range covered by the spectrometers used, and which is the rationale behind the selection of the listed micro-windows (e.g. minimum retrieval error ?).

P5 L8,9: I got an idea of what the authors would like to say, however I suggest to re-phrase more clearly this sentence.

P5 L10: Which are the "relaxed covariance matrices" that induce oscillations ? Please clarify.

P5 L14: If apodization is not used, how broad is the ILS used in the forward model to emulate the instrument effect ?

P5 L25,26: Here it is not clear how the 0.5% rms error "on the fit" maps onto the retrieved WV. Does this rms error refer to the "residuals of the fit" or directly to the retrieved WV ?

P6 Fig.1: It would be better to show CH_4 and N_2O absorption contributions with different line colours.

P6 L14-16: This bias is with respect to the a-priori state vector which, in turn, will probably have some bias with respect to the real profile. Note that if the bias of the retrieval was known both in sign and amplitude, then it would be possible to correct for it...

P7 L1-5: Here I would state clearly which is your retrieval vector. Do you retrieve a WV profile using the discretization mentioned in the colour scale of Fig.2a ? Do you include further fitting parameters ? (Such as atmospheric continuum, for example).

P7 L7-9: Smoothing the high-vertical-resolution profiles via the averaging kernels of the coarse-vertical-resolution experiment is not mandatory, especially if you attribute a "smoothing error" to the profile differences or if you want to try characterizing the smoothing error itself. Therefore I would simply state your choice here, without trying to find a justification, which is also rather fuzzy to my view.

P7 L18: Off-diagonal elements of $\mathbf{S}_e$ may play a very important role if the spectrum is oversampled (wrt interferogram) and/or if apodization is used. Please state explicitly that, apparently, this is not your case.

P8 L8: Please define also the symbol $\mathbf{K}_b$.

P8 L17-ff: In Fig.3a the error due to interfering species is also shown. Which are the considered interfering species that are not simultaneously retrieved with WV ?

P9 L1: This sentence is not clear and may be questionable. Does this mean that your AKs are not a good estimate of the vertical response function of your system (instrument plus inversion scheme) ? Why ?

P9 L7: Please note that re-gridding via interpolation is, on its own, an arbitrary smoothing. So, I do not fully understand why you do not want to use AKs to smooth and resample high-resolution profiles prior to intercomparison (as it seems you already did in the plots presented in the supplemental material).

P9 L12-14: As shown in Fig. 2c, the FTS retrievals have less than 3 DOFs therefore, why using so many layers for the intercomparison ? The risk is to find biases of different sign in adjacent layers.

P10 Sect. 4.1: The underlying idea is good, however, please note that the variability evaluated here could underestimate the real WV variability, due to the constraint of the retrievals towards the a-priori state vactor. I would have estimated from measurements the variability of the spectrum vs time and would have derived the corresponding "time-mismatch" error covariance matrices relating to the individual WV profiles, using Eq. 3. I suggest to include a comment on this regard (or change approach...).

P11 Sect. 4.2: Here I did not understand if, from this analysis, you also derive an estimate of the error component to be attributed to the difference between FTS and sonde WV profiles, due to the spatial mismatch of the measurements. I agree that it is hard to derive such an error estimate however, lacking this estimate, I do not see very much the usefulness of this section. Please explain.

P13 L2,3: The second effect of a-priori WV on the solution is not clear. Did-you mean that the a-priori WV influences the solution also because it is used as initial guess for the Gauss-Newton iterations ? (This latter effect should be negligible if the retrieval converges properly).

P16, Sect. 5: the link between this Section and the work presented in the previous Sections of the paper is non very clear. Maybe you could state at the beginning of this Section how the work you are going to present is linked with the analysis presented earlier.

P16 L1,2: What is the meaning of "expecting WV" in this context ? Usually an optimized microwindow selection scheme tries to avoid spectral interferences from WV and related isotopologues. From the second sentence, however, I understand the opposite.

P17 Table 3: Which is the rationale for the adopted sorting of interfering species in the Table ? Perhaps their relative importance ? In this case one should assess the retrieval errors due to the interference of ozone. In this spectral region I also expect temperature knowledge to be of importance (see general comments above). Moreover, I understand that the micro-windows used were selected in already publishes papers, however you could at least mention here the rationale with which they were selected / optimized (e.g. with the aim of minimizing the total retrieval error of the gas to be retrieved).

3 Technical Corrections

P2 L26: Remove double comma between "Zugspitze" and "Germany".

P4 L3: Description... described elsewhere (rewording needed).

P7 L3: but as explained before, one of the goals...

P11 Caption of Fig. 5: Same as Fig. 4 but for MLO.

P12 L9: As mentioned above, the initial spatial difference...

P13 L17: I do not see the usefulness of putting some figures in the supplemental material when these are recalled and described in the text of the main paper. I would put all the figures in the main paper file (of course only if this operation does not cost too much!)

P16 Fig. 9: in the vertical axis labels "ppmv" has a small "v", is this an intentional choice ?

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