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Atmospheric Measurement Techniques Discussions

Interactive comment on "

A new optimal estimation retrieval scheme for carbon monoxide using IASI spectral radiances – Part 1: Sensitivity analysis, error budget and simulations" *by* S. M. Illingworth et al.

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Thank you to all of those involved, and to the referees for their helpful and insightful comments, which have been taken onboard. We have carefully considered the questions and suggestions of the referees, and detailed responses to these questions and

suggestions are indicated. Reviewer comments are quoted using [...].Some changes in the manuscript have resulted from the referee's suggestions, resulting in us submitting a revised paper for publication in AMT. Response to the "Anonymous Referee 1": [The title is a bit misleading as the scheme is not really brand new built from scratch. It is, as explained in the text, adapted from the Oxford RFM and GENLN2. I would suggest to simply entitle it something like: "ULIRS, an Optimal...".] [My second concern is the fact that the paper is divided in two parts. The second one being announced in the conclusion as "an inter-comparison with MOPPIT". I'm not sure the second part would be suitable for AMT. As such I believe that this paper is self-consistent and does not need a "Part I" label.]

The title has now been changed to 'An Optimal Estimation retrieval Scheme for CO using the IASI Instrument', and is to be considered as a stand-alone paper for publication in AMT. [Figure 1 as well as figure 9 are probably useless to the good comprehension of this paper and are more relevant to a IASI description. I suggest discarding them.]

Done.

[Section 3.1.1: authors claim that they are using a high resolution solar spectrum in order to account for the reflected solar flux in the TOA radiance. It would be instructive to provide a characterization of the impact of using such a high resolution spectrum instead of a simple black-body radiance.]

A retrieval using both a high resolution solar spectrum and a simple black body radiance has been performed and is now reported in Section 4.3. A figure (Figure 14) has been added to demonstrate the difference that arises in the retrieval, and which is found to be non-negligible, especially near the surface.

[Section 3.1.3: Is the problem so non-linear that is deserves a Levenberg-Marquardt treatment?]

Yes. A study into the non-linearity of the retrieval, using the following test (from

Rodgers) was carried out: cˆ2=δyˆT S_yˆ(-1) δy, where: δy=F(x)-F(x_a)-K(x-x_a), and x is calculated as being one standard deviation away from xa. The resulting values were sufficiently far away from unity so that the problem could be considered to be non-linear. Also, using a Lev-Mar method will not give the wrong answer; as if it is more linear than expected then it will simply approach the Gauss-Newton approximation.

[A word should be added about what continua were included to the forward model.] In the spectral region of interest, the forward model contains continua contributions from N2, CO2 and H2O. This has now been included in the text in Section 3.3.1.

[What is the impact of the temperature profile retrievals on the final product, and how does the retrieved temperature profile compare with the ECMWF ones?]

Temperature profiles were retrieved, as it was found that not doing so resulted in non-negligible forward model parameter errors. On average the retrieved profiles are very similar to those of the ECMWF.

[It is also not clear if layers average values or levels local values are actually fitted.]

The RFM produces Jacobians for a triangular perturbation centred on the pressure level that ends at the adjacent pressure levels, but that the retrieved profile itself is on a grid which corresponds to the pressure levels. This has now been made clear in the text (Sec 3.2.2).

[Section 3.3.2: Is a time interpolation applied on the ECMWF data? Please clarify.]

A time interpolation is not applied to the ECMWF data, rather the ECMWF data which is closest in time to the IASI measurement is chosen. This has now been made clear in the text.

[Section 3.5.1: As it is, to my knowledge, the first paper presenting this for IASI, I suggest that authors should give a detailed characterization of the impact of using a non-diagonal S_y matrix. And compare it to a pure diagonal one.]

A discussion of the difference between using a non diagonal Sy and a purely diagonal one is now given in Section 3.5.1, along with a figure to demonstrate the difference that arises, and which is found to be negligible.

[The "RFM resolution" is very confusing. Does this mean the sampling used to compute the "unapodized" spectra or what? Applying the IASI ILS to an already finite resolved spectrum is not the proper way.]

'RFM resolution' has been replaced with 'spectral fine grid', so as to remove any confusion. When the ILS convolution in the RFM is performed it reverts to a very fine mesh calculation 0.0005 cm-1 (some finite level is required, as one cannot model an infinitely resolved spectra), this has also been made clearer in the text (Sec 4.1)

[Last paragraph of section 4.1, the error at the edges of the spectrum are well known "spoiling" artifacts described in all good textbooks on FFT (see e.g. Numerical Recipes). It is maybe useless to insist on that point here.]

Section removed from text to avoid confusion.

[Section 4.2.2: Cautions should be taken in the error comparison between algorithms. The actual error reduction, as well as DOFS, depend on the prior covariance used. Strictly speaking only retrieval made with the same a priori are directly comparable. However the variability you use seems comparable to what is presented in George et al. (Atmos. Chem. Phys., 9, 8317–8330, 2009).]

Agreed. The statement made in Sec 4.2.2 relating the reduction of the error (compared to the a priori) in comparison to other groups has been removed, and a more considered approach is now discussed in Sec 4.2.1.

[Section 4.3: I appreciate the sensitivity tests made on emissivity and surface elevation errors.]

Noted.

[As IASI is throwing 1.3x10ËĘ6 spectra a day, it would be interesting to provide a speed performance information.]

Our method is not designed to be fast but accurate. Also a speed performance would obviously depend on processing power and CPU specification, and also the average number of iterations that are required for convergence. Therefore we do not think this suggestion is useful and we would prefer not to give such a number.

[Figure 6: I recommend presenting the covariance as a vertical profile of the square root of the diagonal and as a contour plot of the correlations. This usually enlightens better the correlation between altitudes. Also, as it is a matrix, try to have the picture presented as a square instead of a rectangle. On the caption split "TheCO" in "The CO"]

The caption has been split, and the Covariance matrix has been plotted as a square, however it is our belief that Figure 6 and Figure 5 (in the form of the horizontal error bars) give the required information for the CO a priori covariance matrix, and that no further plots are needed.

[Figures 7 and 8, could you add the dashed lines that show the limits of the fitting interval.]

The limits of the fitting interval are now indicated on Figs. 7, 8, and 11.

[Figure 19: Why not using a 0-10 ppbv horizontal axis?]

The x axis has now been changed so that it is on a 0-10 ppbv horizontal axis.

[Reference Ceccherini et al. in ACPD 9 is now published as ACP,10,3131-3139,2010.]

This reference has now been updated.

Response to the "Anonymous Referee 2":

[Introduction Authors should add references for SCIAMACHY and MOPITT NIR CO

products.]

These references have been included.

[Sect. 2. In second paragraph, authors report radiometric accuracy of IASI, but not noise characteristics. Both quantities relate to instrument performance and should be discussed in this section.]

Both of these quantities are now discussed in greater detail in this section.

[Third paragraph should note that clouds greatly reduce the actual daily retrieval coverage from the ideal.]

The paragraph has been changed to reflect this comment.

[The description of the forward model requires greater detail, since even small deficiencies in the forward model can result in large retrieval biases. Specifically, the following questions should be addressed. Does the longwave radiation term account for atmospheric emission reflected by the earth's surface? How is the reflection modeled (e.g., specular vs. Lambertian)? The representation of the solar term (Eq. 2) seems overly simplistic, since it neglects the dependence of the optical depth on wavelength, CO concentration, water vapor concentration, etc. What evidence is there that this equation is appropriate? Has this parameterization been validated? What is the radiative uncertainty associated with this parameterization? How does this radiative uncertainty quantitatively affect the retrieval errors? What is the uncertainty in the forward model due to uncertainties in the spectral database (HITRAN)?]

The RFM does include a term for the atmospheric emission reflected by the earth's surface, modelling this reflection as specular. The text has been changed to encompass this information. The solar term is calculated by using the forward model to simulate an optical depth, so it does not neglect the dependence of the optical depth on wavelength etc, but actually includes this. This wavelength dependence should have been made more explicit in the equation. The equation itself is a standard equation that is given by

many textbooks to represent the solar reflected term in the absence of scattering.

The uncertainty in this parameterisation is included in the final error budget, and is represented by the Ïţsolar term.

[Sect. 3.1.3. The Levenberg-Marquardt method is often used for highly nonlinear problems which are not suited to simpler iterative methods (e.g., Gauss-Newton iteration). Why were simpler methods not considered?]

A study into the non-linearity of the retrieval, using the following test (from Rodgers) was carried out:

cˆ2=δyˆT S_yˆ(-1) δy,

where: δy=F(x)-F(x_a)-K(x-x_a),

and x is calculated as being one standard deviation away from xa. The resulting values were sufficiently far away from unity so that the problem could be considered to be non-linear. Also, using a Lev-Mar method will not give the wrong answer; as if it is more linear than expected then it will simply approach the Gauss-Newton approximation. [Sect. 3.2.1. Authors should clearly state how each state vector parameter is quantified (e.g. VMR vs. logVMR for CO, specific humidity vs relative humidity for water vapor, etc.).]

The quantification of the state vector parameters has been clarified in the text.

[Sect. 3.2.2. Arguments in first paragraph concerning grid selection are relevant to visualization and interpretation of K and A, but not to their validity. Using an irregular grid by itself is not a source of retrieval error. This point should be made.]

This point has been included in the text.

[Sect. 3.3. The meaning of the last half of the sentence 'Being an OEM retrieval scheme, the ULIRS makes use of a priori knowledge relating to the quantities that are to be retrieved, with the a posterior retrieval weighted by the choice of the inputted

data.' is unclear.]

Agreed; this sentence has been removed for clarity.

[Sect. 3.3.3. The development of the CO a priori profile is unusual and will clearly lead to retrieval biases. The authors state that '... to avoid the a priori being heavily biased by background concentrations of CO, only proïnËĞAËŻles where the surface concentration of CO was greater than 100 ppbv were considered.' This seems backwards. By excluding all the profiles that represent background concentrations of CO, the a priori profile will be valid only for polluted conditions. When such an a priori profile is used for retrievals in relatively clean areas (near background concentrations), retrievals at levels with low sensitivity will be positively biased. Preferably, the a priori profile should be recalculated without discarding any profiles, since this will lead to a much better estimate of the true climatological CO profile. At the very least, the authors should clearly discuss the expected positive biases resulting from the method used to calculate the a priori profile.]

We partially agree. All a priori profile formulations lead to bias against truth, particularly for levels with low sensitivity. The a priori in this paper was chosen to be most suitable for retrievals over land in this part of Africa and this should have been clearly stated; hence we expect the largest retrieval biases will indeed occur over ocean scenes with background concentrations in the lower troposphere; results for these retrievals will be most susceptible to information projection [Deeter at al, 2010]. Of course, the retrieval methodology itself is not limited by the a priori, since for other geophysical situations new a priori profiles and covariances can be utilised. We have updated the paper in sections 3.33, 5, and 6 to clarify the specification of the a priori, to note the likely information projection in the ocean case, and to note that the ULIRS is flexible so different a priori profiles and covariances can be used.

[Sect. 3.5.1. Should the text 'where m is the number of measurement vectors' really be 'where m is the number of measurements'? Isn't there just one measurement vector?]

Agreed; the text has been changed to: 'where m (the measurement vector) is the number of measurements', clarifying this point.

[Sect. 3.5.2. The argument in the first paragraph (concerning radiative effects of scattering) considers the effects of absorption, but not emission. These arguments are valid for nearinfrared wavelengths, but not thermal infrared (which is more complex). I would suggest deleting this paragraph, since it's already clear that the radiative transfer model is only valid in clear-sky scenes.]

This paragraph has been removed.

[Sect. 4.2.1. Maps of DFS over the study region (daytime and nighttime) would be interesting and would clearly demonstrate the geographical dependence of the retrieval performance.]

We appreciate this point but we do not see this as essential particularly since the retrieval is not global. A range of DFS values have been demonstrated in the retrievals already included in the paper.

[Sect. 4.2.2. The first paragraph requires more detail concerning the calculation of forward model parameter error (epsilon_param). For example, what assumptions were made regarding the variability of the non-retrieved trace gases?]

The forward model parameter error for each of the trace gases was calculated using the equation from Rodgers (2000):

Parameter error = G * K_b * S_b * (K_b)ˆT * GˆT

Where the diagonal elements of the a priori covariance matrices for the non-retrieved trace gases have been calculated using the one sigma reference atmosphere files developed by Remedios et al. (2007), and the off-diagonal elements have been calculated using the Gauss-Markov equation. The text has been changed to clarify these points.

[Sect. 4.3. This section clearly demonstrates that the retrievals are sensitive to the presence or absence of the solar term, but does not demonstrate that the solar term is properly represented (see comments for Sect. 3.1.1. above).]

We agree that the proper representation of the solar term has not been proven, although clearly good residuals are obtained in our retrievals. We have added a sentence in the conclusions to indicate that further studies exploring the accuracy of solar representation would be desirable.

[Sect. 5. The four profiles selected for simulations are somewhat limited. In particular, the inclusion of a CO profile exhibiting a clean lower troposphere and polluted mid-troposphere would be an interesting case. Inspection of Figs. 20 and 21 shows that the retrieval uncertainty is reduced (relative to the a priori uncertainty) at all levels, including the surface. This is somewhat surprising for the over-ocean retrieval where the Jacobians are likely to be very weak at the surface. This likely indicates the 'information projection' effect discussed in Deeter et al. (2010) and should be discussed.]

We agree that there are many cases that would be interesting. We feel that the examples provided already are sufficient to demonstrate the main characteristics of the retrieval. Text has been added in Section 5 to discuss the possibility that the reduction in a priori error may be because of projection effect, brought about by the use of a single a priori profile and covariance matrix in the retrieval process.

[Conclusions. In the second paragraph, simulation results for retrieved total column are reported (e.g., total errors ranging from 18 to 34%). These results do not seem to appear in the main body of the paper. These results should be provided with more detail somewhere in the main body of the paper. The authors should discuss the practical limitations of the new retrieval method. Specifically, is the computation time a factor in processing large amounts of data (e.g. months or years) over large areas (e.g., globally)? What steps would be required to allow its use to produce operational retrieval products?]

The last paragraph of Sec. 4.3.1 now includes these error terms that are mentioned in the conclusions. Currently the computation time is a factor in processing large amounts of data over large areas, but given that the RFM does not have appropriate look up tables for this retrieval it is unlikely to be improved in the future, without moving to a faster forward model. These limitations have been included in the conclusions section.

[Technical Corrections. Right parenthesis missing at end of line 22 on p. 3752.]

This has been corrected.

Interactive comment on Atmos. Meas. Tech. Discuss., 3, 3747, 2010.