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Atmospheric Measurement Techniques Discussions

AMTD

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Interactive Comment

Interactive comment on "A method for improved SCIAMACHY CO_2 retrieval in the presence of optically thin clouds" *by* M. Reuter et al.

Anonymous Referee #2

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This manuscript of Reuter et al. presents an improved retrieval scheme for CO2 from SCIAMACHY spectra. This retrieval scheme is based on the optimal estimation method and is designed to retrieve CO2 in the presence of thin clouds and aerosols. The manuscript presents a description of the retrieval method and a series of theoretical tests to study biases introduced by different geo-physical and instrumental parameters. The paper concludes that this improved retrieval scheme should allow CO2 retrievals from SCIAMACHY with an accuracy and precision of a 1% even for scenes with thin clouds.

SCIAMACHY is the only satellite instrument that can provide total column CO2 observations for the 2003-2009 timeframe and thus more accurate CO2 retrievals from SCIAMACHY will be of great scientific value. The present retrieval method has a large potential to improve the currently available SCIAMACHY CO2 datasets and it can represent an important step forward towards the usefulness of SCIAMACHY CO2 data for carbon cycle studies. The manuscript will be clearly of interest to the readership of AMT and I recommend publishing the manuscript after appropriately addressing my comments below.

Manuscript would benefit from proof-reading by a native speaker

Main comments:

1) The manuscript is somewhat hard to read and it is not always clear what has been really done. The reader has to be very careful not to confuse the different tests (each given a different acronym), in particular since they have very different meanings. The authors should state more clearly which and why some tests are based on the 'dry run' scenario and others on the 'met1sigma' scenario. It should also be made very clear which of tests only represent a verification of the retrieval scheme (namely those that assume a perfect forward model) and which one are a more realistic assessment of the retrieval biases (namely those with an imperfect forward model). I would recommend treating both sets of test separately since they have very different meanings. The manuscript would also benefit from proof-reading by a native speaker

2) The main objective of this manuscript is to characterize the precision and biases of CO2 retrievals from a new retrieval scheme. However, I am not happy about the treatment of errors in the manuscript.

a) The authors claim several times that XCO2 retrieval need to be precise and accurate to 1% or better. According to Rayner and O'Brien and others, there is a requirement on precision of 1% or better (for regional averages and monthly means). The requirement on accuracy is much higher and is in the order of a fraction of a ppm (e.g. Miller et al., 2007) b) I am surprised to see that the authors do not calculate explicit smoothing and interference errors. Instead, they claim that the difference between true and retrieved XCO2 for the tests with a perfect forward model will correspond to the smoothing error.

This might very likely be the case, but there can also be other factors that can contribute to these differences. c) For cases with an imperfect forward model (the microphysical cases and the CFC, CGT and multilayer case), the forward model bias is inferred by subtracting a 'smoothing error' term that is taken from a 'reference' case instead of computing an explicit smoothing error for this case.

It is necessary to clearly separate forward model errors and smoothing/interference errors. It would certainly be a good idea to explicitly calculate smoothing and interference errors. It should also be made clear that these retrieval tests are carried out without adding noise to the simulated spectra and thus represent very ideal conditions. Furthermore, it should be pointed out that the stochastic error given for the tests represents the a posterior error which is based on assumptions about measurement noise.

3) The characterization of the retrieval algorithm is carried out for 32 different scenarios for 3 different solar zenith angles. However, only 14 of the 32 scenarios truly probe the accuracy, all of which have the same atmosphere and surface albedo. I don't think that the authors can draw general conclusion on the accuracy and precision of their CO2 retrieval based on this small set of tests. Most importantly, the authors have inferred their estimated of biases only for one surface albedo. This parameter will have a large impact on the results and the inferred biases will change substantially when a different surface albedo is used.

Minor comments:

p.2484: measurements of XCO2 -> XCo2 is retrieved not measured

P2484: the column averaged mixing ratio of atmospheric CO2 -> the dry-air column averaged mixing ratio of atmospheric CO2

p.2484: presented enabling accurate retrievals -> specify how accurate the retrieval is.

p.2484: In contrast to existing algorithms, the systematic errors -> In contrast to existing algorithms for SCIAMACHY retrievals, the systematic errors

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p.2484: In contrast to existing algorithms, the systematic errors due to cirrus clouds with optical thicknesses up to 1.0 15 are reduced to values typically below 4 ppm. -> 'typically' only for the studied set of scenarions and not 'typically' for true conditions

P 2484 This shows that the proposed method has the potential to reduce uncertainties of SCIAMACHY retrieved XCO2 -> Reduce by how much

p.2484 making this data product useful for surface flux inverse modelling -> making this data product potentially useful for surface flux inverse modelling.

p.2484: uncertainties of its natural global sources and sinks -> source and sink strength or distribution ?

p.2484: Ground-based CO2 measurements of networks -> networks of in-situ instruments ?

p. 2485: the averaging kernels of instruments -> define averaging kernel

p.2485: are much more constant -> what do you mean ?

p.2486: orbiting carbon observatory -> Orbiting Carbon Observatory

p.2491: Analog to Fig. 1, Fig. 2 shows for identical atmospheric conditions the weighting functions of the same scattering parameters but for the O2 fit window -> You should include surface albedo here. The surface albedo jacobian will very likely introduce significant correlations with the scattering parameters.

p.2493 + p.2497: Furthermore, we use only static a priori knowledge of XCO2 -> please explain what you mean by static

p.2494.2495: Note: using merged fit windows instead of performing the retrieval in two separate fit windows has two main advantages. . . -> I would argue that a combined retrieval has very little advantages over a sequential O2 + Co2 retrievals as the CO2 channel adds little/no information to the scattering parameters and surface pressure.

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p.2495: The radiative transfer calculations are performed on 60 model levels, even though our state vector includes only a ten-layered CO2 mixing ratio profile -> How is the mapping between 60 and 10 layers carried out

p.2496: The state vector accounts for fitting a wavelength shift and the full width half maximum (FWHM) -> What is the reason for fitting the FWHM as well?

p.2496: We assume a Lambertian surface with an albedo with smooth spectral progression which can be expressed by a 2nd order polynomial separately within both fit windows -> What exactly do the values for the 1. And 2. order of the polynomial represent and how strongly constraining is the chosen a priori covariance value?

p.2497: As a result, the a priori uncertainty of XCO2 increases from 3.9 to 15.6 ppm. -> Why has a factor 4 been chosen ?

p.2499: Within the parameter vector we define that scattering at particles takes place in a plane parallel geometry -> Is the radiative transfer carried out plane parallel? If so, why has this been chosen instead of a more accurate pseudo-spherical approximation?

p. 2499 In addition 5 scattering happens at a standard LOWTRAN summer aerosol profile with moderate rural aerosol load and Henyey-Greenstein phase function.

-> The important values are the optical depth, its vertical distribution, single scattering albedo and Angstrom coefficient . Please provide these values. This should be provided for all aerosol and cloud scenarios.

p.2500: Calculation of XCO2. Is this for the dry-air? Is the water column included in the surface pressure?

p.2502: The “met. 1_” scenario -> Can you clarify if the a prioir statevector elements are perturbed at the same time -> Are the perturbations done in a random way ?

p.2503 Calibration:It would be interesting to have runs where FWHM and/or dispersion is perturbed as well

p. 25403: For this purpose, the simulated intensity of the “dry run” was scaled by a factor by 10%. -> Both bands at the same time? What happens if you only perturb 1 band?

p. 2505: Can the second order polynomial interfere with O2 or Co2 absorption? By how much do the a priori for the first and second order terms differ from truth?

p.2510: The clouds we use for the scenarios of this section, consist of fractal ice particles with 100 and 300 μm effective radius (“ice frac. 100” and “ice frac. 300” scenario), hexagonal ice particles with 25 and 50 μm effective radius (“ice hex. 25” and “ice hex. 50”2510 scenario), and water droplets with a gamma particle size distribution and an effective radius of 6, 12, and 18 μm, respectively (“water 6”, “water 12”, and “water 18” scenario). -> As for the aerosol, please give their optical depth

p.2512: Aerosols: what are the aerosol optical depth profiles for these cases. What is their single scattering albedo

p.2512: What is the aerosol test based on? The ‘dry run’ or ‘met’ run?

p.2515: Eq. 14 I guess this should include the true statevector and not the first guess.

p.2515: What do these error estimates in the table 2 tell me? Linearity is only assumed for the step size of the retrieval and for the error analysis.

p.2517: The precision of the retrieved XCO2 was between 3 and 4 ppm for most of the analyzed scenarios which is smaller but similar to the 1–2% precision range experimentally determined for the WFM-DOAS 1.0 retrieval scheme -> The theoretical estimate of precision should be large with this approach since more statevector elements are retrieved. So this tells you that a precision estimates on the measurement noise only is usually an underestimation.

p.2517: At solar zenith angles of 40, the presence of ice clouds with optical thicknesses in the range of 0.01 to 1.00 contributed with less than 0.5 ppm to the systematic absolute XCO2 error. -> please add ‘if a perfect forward model is assumed’

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p. 2518: The systematic XCO2 errors of the "micro physical cloud properties" scenarios with ice clouds were most times below ±4 ppm. -> only true for the set of investigated scenes for a surface albedo of 0.2

p. 2519: from SCIAMACHY nadir measurements meeting the 1% accuracy and precision requirement -> Where is this requirement coming from? It is not from Rayner and O'Brien

p.2527: Table 1: Caption does not discuss the aerosol case.

p.2528: Table 3: Please give values to with the same number of significant digits. An error given as 0.00 is not very meaningful

p. 2536: Figure 7: What is the meaning of the red shaded area?

p. 2538: Figure 9: Figure is quite confusion and it is hard to identify the individual kernels. You might consider drawing lines.

Interactive comment on Atmos. Meas. Tech. Discuss., 2, 2483, 2009.