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Atmospheric Measurement Techniques Discussions

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

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The referee had some major and some minor comments and raised several interesting questions and gave useful recommendations which we discuss in the following. The review was profound and constructive and helped us to strengthen the paper.

1 Discussion

Referee: *Manuscript would benefit from proof-reading by a native speaker*
Authors: One of the co-authors is a native speaker. Additionally, a graduate translator helped us with the manuscript. However, we had another proof read of the manuscript and eliminated some typos and grammatical errors.

2 Main comments

Referee: *1) a) 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).*
b) I would recommend treating both sets of test separately since they have very different meanings.
c) The manuscript would also benefit from proof-reading by a native speaker
Authors: a) Within the text of Sec. 4 (Error analysis) we describe for each scenario in detail which scenario served as basis. However, we agree that a simple overview would be a benefit. Therefore, we slightly modified Tab. 2 and its caption: "Some scenarios are intended to quantify the retrievals capability of reproducing modifications of state vector elements (109). The other scenarios are intended to additionally quantify the retrievals sensitivity to parameter vector elements (113) (i.e. to a non-perfect forward model)." We added the following text to Sec. 4.1 (The "dry run" scenario):

“The “dry run” scenario serves as basis for several other scenarios which are mainly intended to quantify the retrievals capability of reproducing modifications to a specific state vector element or to quantify the retrievals sensitivity to a specific parameter vector element.” Additionally, we added the following text to Sec. 4.2 (The “met. 1σ” scenario): “The “met. 1σ” scenario serves as basis for several other scenarios which are mainly intended to quantify the retrievals performance under more realistic conditions including also unknown parameter vector elements, i.e. an imperfect forward model.”
b) We agree, that both test classes have different meanings and that it is important to discriminate between them. However, it is also very important to discriminate between the individual physical parameter classes (cloud properties, aerosol properties, surface properties...). This means when grouping the scenarios into classes, one has to decide for a primary criterion. As clouds were identified as possible major source of error in the WFM-DOAS retrieval scheme, we decided to group the scenarios by physical properties. For this reason we would like to stand by decision. However, as shown by the following text passages, we always pointed out which scenario is attributed to modifications to state vector or parameter vector elements. Sec. 4.6 (Macro physical cloud parameter): “Up to this point, we only tested the retrieval’s ability to reproduce modifications to state vector elements. However, and as mentioned before, especially in respect to scattering, three state vector elements are by far not enough to entirely define the radiative transfer. For this reason, we analyze the retrieval’s sensitivity to different parameter vector elements...” Sec. 4.7 (Micro physical cloud parameter): “Scattering properties are defined within the state vector solely by these three parameters. The whole micro physical cloud and aerosol properties like phase function, extinction, and absorption coefficients are only defined in the parameter vector. Unfortunately, these micro physical properties are not known and also not constant in reality and the values that we define in the parameter vector are obviously only a rough estimate.” Sec. 4.8 (Aerosols): “Analog to the cloud scenarios, we estimated the influence of aerosol properties which are not part of the state vector.”
c) See discussion above.

Referee: *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) i) 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. ii) 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. iii) 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.
Authors: a) We modified two paragraphs. i) **old**: “Theoretical studies have shown that satellite measurements of CO2 have the potential to significantly reduce the surface flux uncertainties (Rayner and O’Brien, 2001; Houweling et al., 2004). This requires an accuracy and precision of the retrieved column averaged dry air mole fraction (XCO2) of 1% or better (Rayner and O’Brien, 2001; Houweling et al., 2004; Miller et al., 2007; Chevallier et al., 2007).” **new**: “Theoretical studies have shown that satellite

measurements of CO2 have the potential to significantly reduce the surface flux uncertainties. This requires a precision of about 1% for regional averages and monthly means (Rayner and O'Brien, 2001; Houweling et al., 2004). However, undetected biases of a few tenths of a part per million on regional scales can already hamper inverse surface flux modeling (Miller et al., 2007; Chevallier et al., 2007)." ii) **old**: "The results presented here indicate that it is theoretically possible to retrieve XCO2 from SCIAMACHY nadir measurements meeting the 1% accuracy and precision requirement in many cases even in the presence of thin ice clouds." **new**: "The results presented here indicate that it is theoretically possible to retrieve XCO2 from SCIAMACHY nadir measurements with an accuracy and precision of about 1% in many cases even in the presence of thin ice clouds."
b) Eq. (3.16) of Rodgers (2000) says:

$$\hat{\vec{x}} - \vec{x} = (\mathbf{A} - \mathbf{I})(\vec{x} - \vec{x}_a) + \mathbf{G}_y \mathbf{K}_b(\vec{b} - \hat{\vec{b}}) + \mathbf{G}_y \Delta f(\vec{x}, \vec{b}, \hat{\vec{b}}) + \mathbf{G}_y \epsilon \tag{1}$$

Here, the first term is the smoothing error. If the parameter vector $\vec{b}$ is perfectly known ($\vec{b} = \hat{\vec{b}}$), the second term becomes zero. If a perfect forward model is assumed, the third term becomes zero. If no measurement noise is assumed ($\epsilon = 0$), also the last term becomes zero. Under these assumptions, the smoothing error equals the difference of the true state and the retrieved state ($\hat{\vec{x}} - \vec{x} = (\mathbf{A} - \mathbf{I})(\vec{x} - \vec{x}_a)$). We describe this by: "According to Eq. (3.16) of Rodgers (2000), the systematic errors given in Table 2 correspond to the smoothing error $(\mathbf{A}-\mathbf{I})(\vec{x}_t-\vec{x}_a)$ of the state vector elements. This applies to all scenarios in which only state vector elements but no parameter vector elements are modified. In these cases, errors due to noise, unknown parameter vector elements, and due to the forward model do not exist." However, we explicitly calculated the smoothing error of the reference scenarios and found good agreement with our values. The small differences between the calculated smoothing errors and the biases given in our paper can be explained by the non linearity of the forward model. Roughly every second scenario fulfills the assumptions mentioned above. Therefore, we think

that the reader gets a good impression on the magnitude of the smoothing error. Specifying the smoothing error for each scenario separately would require three more columns in table 2. We find, this would overload the table by giving only little extra information. Additionally, calculating the smoothing error requires knowledge of the true state, therefore it will be not easily possible to calculate the smoothing error when applying the retrieval to real data. In this context, Rodgers (2000) states: "Because the true state is not normally known, we cannot estimate the actual smoothing error. What is really required is a description of the statistics of the error..." This requires the smoothing error covariance. "To estimate the smoothing error covariance, the covariance matrix of a real ensemble of states must be known... It is not enough to simply use some ad hoc matrix that has been constructed as a reasonable a priori constraint in the retrieval. If the real covariance is not available, it may be better to abandon the estimation of the smoothing error..." For this reason, we would like to keep our error analysis as it is.
c) i) Not only the micro physical scenarios, CFC, CGT, multilayer but also the aerosol scenarios, and the calibration scenarios deal with an imperfect forward model. However, for the reasons mentioned above, we would like to keep our error analysis as it is. ii)Done. We added "However, it shall be noted that the calculated measurement errors are not utilized for adding noise to the simulated spectra." to section 4 (Error analysis). iii) Done. We added "The stochastic errors represent the a posteriori errors based on the assumed measurement noise and the assumed a priori error covariance matrix." to section 4 (Error analysis).
Referee: *3) a) 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.*
b) 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.
Authors: a) We believe that most of the 35 scenarios are of relevance for the application to real data except for the “dry run” scenario. The state vector includes very important elements e.g. surface pressure or CO2 mixing ratios. Therefore, it is essential and of practical relevance to test for the correct retrieval of these state vector elements. However, we agree that especially in the context of scattering properties the retrievals sensitivity to unknown parameter vector elements (i.e. the model parameter error) will be of special importance. Therefore, 14 of 15 scenarios with unknown parameter vector elements (i.e. with an imperfect forward model) are related to cloud and aerosol properties. Within the introduction, we note that “In this regard, special emphasis is put on cloud parameters which are not retrieved.” The reason for putting the focus on scattering properties can be found in the publication of Schneising et al. (2008). They showed that scattering at optically very thin cloud layers (Tau=0.03) can already result in XCO2 errors of several percent. In contrast to this, they estimated the sensitivity to atmospheric parameters like the temperature profile or water vapor profile to be less than 1% when using an US-standard atmosphere as linearization point. One can expect that the errors will be even less when using ECMWF profiles as linearization points. However, we agree that it is not possible to draw statistically correct conclusions of the errors that may be expected when applying our retrieval to world wide SCIAMACHY data. This would only be possible if realistic cloud statistics along the SCIAMACHY swath would be used as input for our calculations. In contrast to this approach, we are aiming to point out the retrievals weaknesses and strength at the example of a limited set of specific conditions which are typical for many conditions occurring in reality. Therefore, we tried to draw not to general conclusions on the accuracy and precision. For example, within the abstract, we already note, that we draw our conclusions from a set of test scenarios “Test scenarios of simulated SCIAMACHY sun-normalized radiance measurements are analyzed in order to specify the quality of the proposed method.” For this reason we tried to draw not to general conclusions e.g. within the abstract “This shows that the

proposed method has the potential to reduce uncertainties of SCIAMACHY...” or within the conclusions “...has the potential to drastically reduce systematic XCO2 errors compared to a WFM-DOAS like retrieval scheme...”
b) The seven albedo scenarios already deal with albedos that differ (partly strongly) from 0.2. However, we agree that the surface albedo can have a large impact on the results. Therefore, we set up two new sets of scenarios corresponding to table 2 but with an albedo of 0.1 and 0.3, respectively. The results are rather similar to those given in table 2. We added two new tables as appendix and refer to the new tables within section 4 (Error analysis): “Except for the “spectral albedo” scenarios, all calculations are performed with an spectrally constant Lambertian albedo of 0.2. Table A1 and Table A2 include corresponding results but for calculations with an albedo of 0.1 and 0.3, respectively.” and within the conclusions: “...all calculations were repeated with an albedo of 0.1 and 0.3, respectively. As all scenarios had a semi transparent atmosphere, the albedo strongly influenced the signal to noise ratios. As a result, the stochastic errors were generally higher for an albedo of 0.1 and lower for an albedo of 0.3. Additionally, a dependency of the biases on the surface albedo could be observed. The differences were largest (up to 12ppm) for the “micro physical cloud properties” scenarios. Otherwise, only minor differences (for most scenarios below 1ppm) were observed. For the majority of scenarios, the absolute values of the biases were reduced with increasing albedo.”.

3 Minor comments

Referee: *p.2484: measurements of XCO2 -> XCo2 is retrieved not measured*
Authors: Done.
Referee: *P2484: the column averaged mixing ratio of atmospheric CO2 -> the dry-air column averaged mixing ratio of atmospheric CO2*

Authors: Done.
Referee: *p.2484: presented enabling accurate retrievals -> specify how accurate the retrieval is.*
Authors: We specify this later on in the abstract: "...the systematic errors due to cirrus clouds with optical thicknesses up to 1.0 are reduced to values typically below 4ppm."
Referee: *p.2484: In contrast to existing algorithms, the systematic errors -> In contrast to existing algorithms for SCIAMACHY retrievals, the systematic errors*
Authors: Done.
Referee: *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*
Authors: Done. We now say: "In contrast to existing algorithms for SCIAMACHY retrievals, the systematic errors due to cirrus clouds with optical thicknesses up to 1.0 are reduced to values below 4ppm for most of the analyzed scenarios."
Referee: *P 2484 This shows that the proposed method has the potential to reduce uncertainties of SCIAMACHY retrieved XCO2 -> Reduce by how much*
Authors: This strongly depends on the compared retrieval algorithm and on the scenario. However, we give a number for the attained uncertainties ("4ppm for most of the analyzed scenarios").
Referee: *p.2484 making this data product useful for surface flux inverse modelling -> making this data product potentially useful for surface flux inverse modelling.*
Authors: Done.
Referee: *p.2484: uncertainties of its natural global sources and sinks -> source and sink strength or distribution ?*
Authors: Stephens et al. (2007) draw conclusions on the northern hemispheric and tropical land carbon uptake. Within their publication they say: "The full range of results in a recent inverse model comparison study (5), and in independent studies (3, 20, 21), spans budgets with northern terrestrial uptake of 0.5 to 4 Pg C year-1, and tropical

terrestrial emissions of –1 to +4 Pg C year-1."
Referee: *p.2484: Ground-based CO2 measurements of networks -> networks of in-situ instruments ?*
Authors: Done.
Referee: *p. 2485: the averaging kernels of instruments -> define averaging kernel*
Authors: We define "averaging kernel" in section 4.9, however, we replaced the sentence with: "In contrast to this, the sensitivity of instruments measuring reflected solar radiation in the near-infrared (NIR)/short-wave infrared (SWIR) spectral region is much more constant (with height) and shows maximum values near the surface, typically"
Referee: *p.2485: are much more constant -> what do you mean ?*
Authors: Done. We mean constant with height and added as explanation "...more constant (with height)...".
Referee: *p.2486: orbiting carbon observatory -> Orbiting Carbon Observatory*
Authors: Done.
Referee: *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.*
Authors: We agree that the surface albedo weighting function will introduce correlations. However, we put the focus of our publication on scattering at optically thin clouds. For this reason, Fig. 1 and Fig. 2 shall primarily explaining the problematic that scattering effects can most likely not be corrected from measurements in the CO2 band only. Nevertheless, the albedo weighting functions are shown in Fig. 4 together with all other weighting functions. In order to underline the fact, that not only the scattering weighting functions may introduce cross correlations, we added the following text to section 3.2 (State vector): " This figure shows that not only the scattering parameter weighting functions may have cross correlations with other weighting functions. In this context, e.g. the albedo weighting functions show strong similarities to the scattering related weighting functions."

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

Authors: Done. On p. 2493, we no explain what we mean with static: "Furthermore, we use only static (i.e. spatially and temporally invariant) a priori knowledge of XCO2."

Referee: *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.*

Authors: We agree, that the CO2 fit window adds only little information on scattering parameters. We highlighted this in section 2 (Physical basis). This means, we think that a merged fit window approach may have little advantages over a sequential O2+CO2 approach as long as the scattering information retrieved from the O2 fit window is made available within the CO2 fit window. Even though, this may be different for sensors with higher spectral resolution within the CO2. However, what we meant is, that a merged fit window approach has many advantages over separately fitting the CO2 and O2 fit window. In the latter case, the CO2 fit could not benefit from the scattering parameters retrieved within the O2. We try to make this more clear now: "Using merged fit windows instead of performing a CO2 and a O2 retrieval independently within two separate fit windows has two main advantages when..."

Referee: *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*

Authors: In section 3.2.3 (CO2 mixing ratio profile), we describe in more detail the definition of the ten state vector layers: "The CO2 mixing ratio is fitted within 10 atmospheric layers, splitting the atmosphere in equally spaced pressure intervals normalized by the surface pressure ps(0.0, 0.1, 0.2, ... , 1.0)." The mapping between both representations is of course mass conserving.

Referee: *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?

Authors: We fit the FWHM for two reasons: 1) The true FWHM may differ from the nominal FWHM. Within the development of the WFM-DOAS algorithm, Schneising and Buchwitz found that on average best agreement between measurement and simulation is achieved when assuming a FWHM of 1.40 and 0.45 for the CO2 and the O2 fit window, respectively. This differs from the nominal values of 1.48 and 0.48 as listed in the SCIAMACHY book (http://atmos.caf.dlr.de/projects/scops/sciamachy_book/sciamachy_book.html). 2) The true slit function can potentially depend on the homogeneity of the scene. The SCIAMACHY instrument may in principle be affected by the smile effect. This means a part of the scene that illuminates the slit at one edge may have a shifted slit function compared to a part of the scene which illuminates the slit in the middle. Therefore, if the slit is homogeneously illuminated the effective slit function will be broader compared to a inhomogeneously illuminated slit.

Referee: *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?*

Authors: Within section 3.2.2 (Albedo) we included the explaining text: "We assume a Lambertian surface with an albedo α with smooth spectral progression which can be expressed by a 2^{nd} order polynomial separately within both fit windows.

$$\alpha_\lambda = P_0 + P_1 * \frac{\lambda - \lambda_c}{\lambda_{max} - \lambda_{min}} + P_2 * (\frac{\lambda - \lambda_c}{\lambda_{max} - \lambda_{min}})^2 \quad (2)$$

Here, P_0, P_1, and P_2 are the polynomial coefficients, λ the wavelength, λ_c the center wavelength, λ_{min} the minimum, and λ_{max} the maximum wavelength within the fit window. ... We use an a priori uncertainty of 0.05 for the 0^{th} polynomial coefficients. The first guess and the a priori values of the 1^{st} and 2^{nd} polynomial coefficients are zero. Their estimated a priori uncertainties are 0.01 and 0.001, respectively. The magnitude of these values is typical for 2^{nd} order polynomial coefficients fitted to the natural surfaces albedos shown in Fig. 5."

Referee: *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 ?*

Authors: The factor of 4 was chosen a bit arbitrary. We think that the original statistics calculated from CarbonTracker data may be a lower boundary. Additionally, we wanted to ensure that the retrieval is dominated by the measurement but not by the a priori.

Referee: *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?*

Authors: Yes, the RT is carried out plane parallel. We have chosen this geometry due to processing speed considerations. Within our publication, we apply the retrieval only to simulated measurements. The simulated measurements and the forward model of the retrieval rely on the same assumptions. Therefore, we do not expect large differences. Additionally, only calculations with SZA less equal $60°$ have been used. Using plan parallel RT calculations when applying the retrieval to real data could certainly introduce errors at large VZA or SZA values. For this reason we are thinking of using a spherical geometry when applying the retrieval to real data.

Referee: *p. 2499 In addition 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.*

Authors: There are three minor comments which are related to the definition of the scattering aerosol and cloud particles. These comments have in common that they ask for the optical thickness of clouds and aerosols. We believe, that we describe very well the scattering properties of the given clouds. For all mentioned clouds, we give the volume scattering function (Fig. 3) which defines the extinction coefficient

and phase function, we give the phase (water/ice), height, and geometrical thickness. Within the manuscript we state: "Note, in this context, specifying only the optical thickness is not appropriate to describe the scattering behavior of a cloud. Knowledge about phase function, extinction, and absorption coefficients is required in order to make the optical thickness a meaningful quantity." In respect to the description of the aerosol scenarios, we wanted to produce results which are comparable to those of Schneising et al. (2008) for this reason we used the same scenarios which are more comprehensively described in the cited publication of Schneising et at. (2008). However, we agree that optical thickness is a quantity of interest and now provide values for all cloud and aerosol scenarios: "In addition, scattering happens at a standard LOWTRAN summer aerosol profile with moderate rural aerosol load and Henyey-Greenstein phase function and a total aerosol optical thickness of about 0.136 at $750nm$ and 0.038 at $1550nm$."; "This corresponds to a cloud optical thicknesses of the a priori cloud of 0.16"; "The corresponding cloud optical thicknesses (at $500nm$) are 0.25 ("ice frac. 100"), 0.08 ("ice frac. 300"), 0.52 ("ice hex. 25"), 0.29 ("ice hex. 50"), 0.80 ("water 6"), 0.39 ("water 12"), and 0.26 ("water 18")."; "The "dry run" scenario includes a thin cirrus cloud with a CTH of $10km$, a CWP of $10g/m^2$, and a COT at $500nm$ of 0.33."; "The "met. 1σ" scenario includes a thin cirrus cloud with a CTH of $15km$, a CWP of $15g/m^2$, and a COT at $500nm$ of 0.49."; "The "OPAC background" scenario consists of continental relatively clean aerosol in the boundary layer and the free troposphere. Its total aerosol optical thickness is 0.099 at $750nm$ and 0.026 at $1550nm$. The "OPAC urban" scenario has continental polluted aerosol in the boundary layer and continental average aerosol in the free troposphere. Its total aerosol optical thickness is 0.196 at $750nm$ and 0.066 at $1550nm$. The "OPAC desert" scenario consists of desert aerosol in the boundary layer and the continental clean aerosol type in the free troposphere. Its total aerosol optical thickness is 0.264 at $750nm$ and 0.188 at $1550nm$. The "extreme in BL" scenario has strongly enhanced urban aerosol in the boundary layer with a visibility of only $2km$ and relative humidity of 99%. Its total aerosol optical thickness is 2.528 at $750nm$ and 1.056 at $1550nm$."

Referee: *p.2500: Calculation of XCO2. Is this for the dry-air? Is the water column included in the surface pressure?*
Authors: XCO2 corresponds to the dry-air mixing ratio. The surface pressure includes water vapor and the surface pressure weighting function includes (among others) the modification of CO2, O2, and H2O. In addition, the H2O weighting function accounts for a scaling of the H2O profile, only. However, the scaling of the H2O profile would result in a modified surface pressure in reality which we do not account for. This means, in cases with very bad first guess estimates for the H2O profile, this may result in additional surface pressure uncertainties of about $1hPa$. However, the XCO2 calculation should not be affected hereof.
Referee: *p.2502: The "met. 1σ" scenario -> Can you clarify if the a prioir statevector elements are perturbed at the same time -> Are the perturbations done in a random way ?*
Authors: No, the a priori state vector elements are not perturbed at the same time. For this scenario, we give detailed information of all a priori, first guess, true, and a posteriori state vector element within table 3.
Referee: *p.2503 Calibration:It would be interesting to have runs where FWHM and/or dispersion is perturbed as well*
Authors: We made such runs and found very stable results especially for the O2 fit window. As visible in Fig. 4, the FWHM and $\Delta\lambda$ weighting functions have large differences. The values for the degree of freedom as given in Table 3 are 1.00 and 1.00. for the O2 and 0.99 and 0.90 for the CO2 fit window. The error reduction is also close to one.
Referee: *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?*
Authors: We have not explicitly tried this. However, in general this should not hamper the retrieval because the albedo P0 weighting functions (which mainly compensate calibration errors) are separated in O2 and CO2 fit window. Some of the albedo

scenarios deal with albedos that are very different in the O2 and CO2 fit window. Here we observed no problems due to the large differences.
Referee: *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?*
Authors: Q1: There are small interferences possible especially within the CO2 fit window. Nevertheless, the degree of freedom for P2 is still rather high (0.99 in the O2 and 0.93 in the CO2) and the interferences seem not to significantly influence the retrieval. Q2: This depends on the scenario. The a priori values for P1 and P2 are always zero. Therefore, the difference to the truth is also zero for all scenarios except for the "albedo scenarios". Within the albedo scenarios the true absolute values of P1 and P2 are most times below 0.01 and 0.001, respectively. For this reason we used these values as a priori uncertainty.
Referee: *p.2510: The clouds we use for the scenarios of this section, consist of fractal ice particles with 100 and 300μ effective radius ("ice frac. 100" and "ice frac. 300" scenario), hexagonal ice particles with 25 and 50μ 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 18μ, respectively ("water 6", "water 12", and "water 18" scenario). -> As for the aerosol, please give their optical depth*
Authors: Done. See discussion above (minor comment: "p. 2499 In addition...").
Referee: *p.2512: Aerosols: what are the aerosol optical depth profiles for these cases. What is their single scattering albedo*
Authors: Done. See discussion above (minor comment: "p. 2499 In addition...").
Referee: *p.2512: What is the aerosol test based on? The 'dry run' or 'met' run?*
Authors: Done. The aerosol scenarios are based on the "no cloud" run. Table 2 was updated to make this more clear (see discussion of main comment 1)
Referee: *p.2515: Eq. 14 I guess this should include the true statevector and not the first guess.*
Authors: We meant the first guess state vector. We use Eq. 14 to estimate the errors

that would occur when using a one-step retrieval (assuming linearity of the forward model) compared to the iterative retrieval. $\vec{F}(\hat{\vec{x}},\vec{b}) - \vec{F}(\vec{x}_0,\vec{b}) - \mathbf{K}(\hat{\vec{x}} - \vec{x}_0)$ describes the difference (within measurement space) between the (true) measurement vector at the retrieved state and the measurement vector at the retrieved state if the forward model was linear

Referee: *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.*

Authors: The error values given in table 2 represent the systematic error $\pm$ stochastic error. The systematic error (i.e. the bias) is calculated by the difference of retrieved state vector and true state vector. The systematic error corresponds to the smoothing error in cases with unperturbed parameter vector. The stochastic error corresponds to the a posteriori error resulting from Eq. 4 (see also our answers to main comment 2).

Referee: *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.*

Authors: Certainly, the number of retrieval parameters will influence the estimated error. However, the a posteriori error depends not only on the measurement error but also on the a priori knowledge. Therefore it is not compelling that the a posteriori errors of our retrieval must be larger than the stochastic errors of the WFM-DOAS retrieval (which uses a simple least square optimization). Nevertheless, we agree that errors may become larger when applying our retrieval to real data.

Referee: *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'*

Authors: Done.

Referee: *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*

Authors: Done. We added corresponding tables with the albedos 0.1 and 0.3 as appendix and discuss the results within the conclusions. See also discussion of main comment 3.

Referee: *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*

Authors: Done. We modified the text as follows: "The results presented here indicate that it is theoretically possible to retrieve XCO2 from SCIAMACHY nadir measurements with an accuracy and precision of about 1% in many cases even in the presence of thin ice clouds." See also the discussion of main comment 2.

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

Authors: Done. See discussion of main comment 1.

Referee: *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*

Authors: We agree, some of the values in table 3 have a improper number of digits. However, for reasons of readability, we wanted to avoid giving all numbers in exponential notation. We now have increased the number of digits within some lines of the table and give the following explanation within the header of table 3: "Note: $\vec{x}_t$, $\vec{x}_0$, $\vec{x}_a$, $\hat{\vec{x}}$, and the corresponding errors are rounded to the same number of digits within each line."

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

Authors: Done. We updated the legend of Fig. 7 (red = a priori, red shaded = a priori uncertainty).

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

Authors: We think, both kinds of displaying the averaging kernels have their advantages. However, we have chosen this level-wise design to be consistent with the other figures showing profiles (Fig. 5 and Fig. 7). This design shows the layering structure of the state vector. In this way misinterpretations due to interpolation between points can be avoided. Additionally, we think that the main features of Fig. 9 which we discuss in section 4.9 (Column averaging kernel) can be seen quite well. Therefore, we would like to keep Fig. 9 as is.

Please also note the supplement to this comment:
http://www.atmos-meas-tech-discuss.net/2/C1108/2010/amtd-2-C1108-2010-supplement.pdf

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