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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Measuring atmospheric CO_2 from space using Full Spectral Initiation (FSI) WFM-DOAS" *by* M. P. Barkley et al.

Anonymous Referee #2

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General comments:

The topic of the paper is the retrieval of carbon dioxide columns from satellite spectral measurements. Global satellite measurements of atmospheric CO_2 (in combination with inverse modeling) have the potential to advance our understanding of regional CO_2 surface sources and sinks globally by reducing CO_2 surface flux uncertainties. This however requires high precision (1% or better) and very low biases. Therefore, the paper covers an important topic, namely the accurate retrieval of CO_2 columns from SCIAMACHY near-infrared (NIR) nadir spectra. The authors use the WFM-DOAS retrieval algorithm first described in Buchwitz et al. (2000) and aim at further developing the current implementation of WFM-DOAS for CO_2 retrieval (Buchwitz et al., 2005) which is based on a fast look-up table approach to avoid time consuming on-line

radiative transfer simulations. The authors partially repeat and extend the error analysis that has been performed for the look-up table version of WFM-DOAS (Buchwitz and Burrows, 2004; Buchwitz et al., 2005) to show that errors can be reduced by using a more flexible retrieval scheme based on (very time consuming) on-line radiative transfer simulations using improved a-priori information (e.g., temperature profiles from meteorological analysis).

The paper covers an important topic, provides new results (from simulations and from analysis of real data) and is well written. I therefore recommend its publication after the detailed comments listed below have been considered by the authors.

Specific comments:

Abstract, lines 14-19:

The authors write that they show that it is necessary to include suitable a priori information to minimize errors. From this one has to conclude that the retrieved CO_2 depends on the a priori information used for the retrieval. At the end of the abstract they write that they show that the results are not biased from the input a priori data. This appears to be a contradiction and needs clarification.

Abstract, line 15:

The authors write that a "new CO_2 retrieval algorithm" has been developed. They use however exactly the same WFM-DOAS (retrieval) equation as published in Buchwitz et al., (2000, 2005). The "only" difference is that they use different linearisation points for the computation of the radiance and its derivatives. It is therefore not appropriate to classify the FSI-WFM-DOAS algorithm as a new algorithm. I recommend to replace "new CO_2 retrieval algorithm" by "a more flexible implementation of the WFM-DOAS CO_2 retrieval algorithm".

Abstract, line 17:

The authors write that they compute the reference spectra using "the known properties

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of the atmosphere and the surface". I recommend to replace "known" by a less strict term (e.g., "estimated") because none of the parameters is exactly known at the location and the time of the measurement and for the resolution of the measurement (this holds even for the meteorological data and the surface reflectivity but especially for the aerosol information used).

Abstract, line 21:

What does the statement mean that "they also contain significant spatial features". Just that there are spatial features (which might be entirely due measurement error) or that spatial features are observed that are due to atmospheric CO_2 variability. If the latter is meant how can the authors be sure that this is the case? Please clarify.

Page 2768, line 21 and following:

O'Brien and Rayner (2002) and Kuang (2002) have not used DOAS, i.e., differential absorption spectroscopy. Buchwitz and Burrows (2004) and Buchwitz et al. (2005) have used DOAS but these references are missing and need to be added. The paragraph needs to be adjusted taking into account these comments.

Page 2769, lines 1-3:

The first peer-reviewed paper on SCIAMACHY CO_2 retrieval is Buchwitz et al. (2005) and this reference needs to be added. The first peer-reviewed paper on SCIAMACHY CO retrieval is Buchwitz et al. (2004) and this reference needs to be added. The first peer-reviewed paper on SCIAMACHY CH_4 retrieval is Buchwitz et al. (2005) and this reference needs to be added.

Page 2771, 1st paragraph:

Is the temperature profile scaled or shifted? The paragraph contains contradicting information.

Page 2773, line 3 and following:

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The authors have found out that it is necessary to know the shape of the CO_2 profile as otherwise errors of up to 0.5% are introduced. This raises the question to what extent the profile is known? The authors should comment on this. Has this error analysis been performed using a single (US Standard) temperature profile (or is the error shown in Fig.2 also due to varying temperature?)?

Page 2773, subsection 3.3:

The authors say that a similar error analysis for temperature and water vapour profiles has also been performed by Buchwitz and Burrows (2004). Are the results of both studies consistent? The authors should add a discussion on how their results compare with the findings of Buchwitz and Burrows (2004).

The error analysis has been performed using ECMWF temperature and water vapour profiles and the overall errors are given. It is however not clear what the separate errors due to temperature and water vapour are. This is important information that needs to be added. This would allow the reader to distinguish between temperature errors (which are reduced by including the temperature weighting function) and errors due to water vapour (which is highly variable).

Title of subsection 3.4:

The title needs improvement (what is an atmospheric profile?).

Page 2775, subsection 3.5:

A similar error analysis for surface elevation/pressure has been performed in Buchwitz et al. (2005) who found an error of -2.4% per 400 m (see their Figure 1). This is consistent with the -0.3% per 50 m reported here. This should be mentioned by the authors.

Page 2777, line 11 and following:

The authors report about the differences of using a total column derivative versus using

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a lower atmosphere sub-column derivate. The authors say that their findings are in contradiction to the findings of Frankenberg et et (2005c). This is an issue that needs clarification and the authors should provide more details on this (potentially) important aspect of the CO_2 retrieval.

Page 2778, line 3 and following:

The authors state that no iterations are performed because of computer time. Water vapour is highly variable and therefore an iteration might be necessary. The authors should add how large the error is if no iterations are being performed.

Page 2779, line 15 and following:

The authors use three (constant) aerosol scenarios to deal with aerosol variability. Taking into account the high variability of aerosols it is not clear why this should be a significant improvement over the approach used by Buchwitz et al. (2005) who use a single aerosol scenario. The approach to use a constant urban aerosol scenario if a big city is in the field of view of SCIAMACHY can introduce errors (CO_2 jumps) that are larger than using a constant aerosol scenario (and if there is a jump one does not know if this is due to CO_2 or due to the scheme to deal with aerosols). The authors should add that their scheme to deal with aerosols is currently very simple and needs significant additional study and further improvements.

Page 2780, line 6:

The authors refer to mixing ratios but have so far only discussed columns. Please clarify.

Section 4 general:

The authors have performed an error analysis for FSI-WFM-DOAS in comparison to the look-up table implementation of Buchwitz et al., 2005 (let us call this LUT-WFM-DOAS). It would be very good for the reader if the authors provide a table giving an overview about the various errors by listing all investigated and not investigated error sources

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and to provide - if they exist - values of the errors for LUT-WFM-DOAS (which uses a single US Standard temperature profile, a constant albedo of 0.1, a single aerosol scenario etc.) and FSI-WFM-DOAS. The table should also include errors resulting from error sources not further reduced in FSI-WFM-DOAS because of computer time limitations such as non-linearities (e.g., no iteration is performed despite high variability of water vapour) and using only an airmass factor correction for the line-of-sight scan. I also consider errors due to aerosols as errors that are essentially not further reduced in FSI-WFM-DOAS for the reasons given above. The authors might also consider to estimate the overall error (e.g., by assuming that the individual errors are uncorrelated). This table would provide the user with important information on the overall improvement of FSI-WFM-DOAS compared to LUT-WFM-DOAS.

Section 5:

See detailed comments given below for Figures 11-14.

Page 2790, Table 1:

Most of the numbers are given with format X.YY but some numbers are given with format XX.Y (e.g., -04.2 row three, column 1.03/TWF). Probably these are typos? Please correct for this and carefully check the entire table.

Page 2795, Fig.2

Please add legend.

Page 2798, Fig.5:

Please add a legend.

Page 2800, Fig.7:

Please add for which solar zenith angle these results are valid for.

Page 2801, Fig.8:

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The formula given under item (i) is probably wrong. It guess that what is shown is the CO_2 total column weighting function times the retrieved total column and not the total column weighting function times the difference between the retrieved column and the a priori column? If this is right please correct the figure caption.

Page 2804, Fig.11:

There appears to be a pre-defined lower value of the retrieved column which depends on the a priori column, i.e., only retrieved columns are shown which are higher than a certain threshold value. This is most probably due to the fact that only mixing ratios in the range 340-400 ppmv are accepted (as written on page 2780, line 21). This however results in a higher correlation coefficient between the retrieved column and the a-priori column than it otherwise would be. The correlation shown is not a correlation between independently determined quantities as it should be but the correlation has been forced to be high. The authors should add this information and they should also add how the correlation coefficient would be if all values are used and not only the values in the accepted range.

Page 2805, Fig.12

Please describe how the surface albedo has been determined. This is important information that is missing in the paper. Please also add how the retrieval error has been calculated.

Page 2806, Fig.13:

Here similar remarks apply as are given above for Fig.11 concerning the cut-off of the low values of the retrieved CO_2 column and the correlation coefficient. The authors should add this information as requested for Fig.11 and they should also add how the correlation coefficient would be if all values are used and not only the values in the accepted range.

Page 2807, Fig.14:

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The authors have shown using simulated (!) measurements that their more flexible but significantly more time consuming WFM-DOAS implementation results in smaller errors compared to the look-up table implementation of Buchwitz et al., (2005). The important question now is by how much this helps to improve the quality of the CO_2 retrieved from real SCIAMACHY measurements? The authors have not attempted to answer this important question. One should exclude that due to noise or systematic errors of the spectra the improvement are not significant. The authors should show also for real data that FSI-WFM-DOAS gives improved results or at least significantly different results compared to the look-up table implementation LUT-WFM-DOAS. A first important step to answer this question would be to provide a similar figure as Fig.14 but derived using LUT-WFM-DOAS (i.e., using only a single temperature profile but including the temperature weighting function in the fit, a constant albedo and only one aerosol scenario, etc.). It would be very interesting to know if a figure generated using this approach would look different than Fig.14? I recommend to add such a figure to the paper because the authors have not shown that the retrieval of real SCIAMACHY data improves using their (computationally much more expensive) method.

Technical corrections:

Page 2770, Eq.(1):

The first "ln" is (wrongly) italic.

Page 2778, line 2:

Typo SCIAMACHY.

Page 2781, line 6:

Typo: "then the that".

Page 2786, line 27:

Several typos reference Hoogen: Institut, University of Bremen.

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Page 2794, Fig.1:

Typo "November".

Page 2799, Fig.6:

Typo ".." end of first sentence.

Interactive comment on Atmos. Chem. Phys. Discuss., 6, 2765, 2006.