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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.

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General comments

The paper by Barkley et al. describes the improvements made to the WFM-DOAS method by Buchwitz et al. I think I don't have to stress the importance of precise and accurate CO_2 retrievals from space. The authors make use of additional a priori information in the retrieval scheme in order to minimize systematic biases that are so far still the biggest problems in remotely sensed CO_2 abundances. Although one of the most important changes in the algorithm, namely the inclusion of temperature and pressure ECMWF profiles as a priori, is already implemented in the IMLMv6.3

retrieval version of SRON (see, for instance de Laat et al, GRL, 2006 focussing on CO), I recommend publication as it analyses the benefits and systematic biases in detail and focusses nicely on CO_2 retrievals. The document is well written and strucured. However, some of the following comments should be taken into account in the revision:

Specific comments

page 2768, lines 7-12: Please give citations for the statements about the CO_2 budget.

page 2771, line 11 Especially temperature deviations (from the climatology) in the lower troposphere and the stratosphere are often anti-correlated (see, for instance, your ECMWF profiles in the tropics with a cold stratosphere), a simple derivative for a uniform temperature shift might not be sufficient. Did you try to use derivatives based on different climatologies (as in Frankenberg et al, ACP, 2005)?

page 2772, eq 2 You should be consistent with the definitions and use the representations of V as defined on page 2771, line 3-4

page 2772, line 25 Please provide some more details about the CO_2 climatologies (based on which dataset, etc.)

page 2773, 2nd paragraph I am still wondering about the usefullness of including a more realistic priori CO_2 profile. The reason for this is that the authors do not discuss at all the averaging kernels of the retrieval. When they compare retrieved and true columns, the true columns seem to be simply the column averaged mixing ratios not taking the instrument sensitivity into account (see, for instance, Frankenberg et al, JGR, 2006). So my question is: If you apply the AK correction to the true column and then compare with the retrieved values using only a standard a priori profile, will there still be the bias as shown in Fig. 2? If yes, then the retrieval really improves a lot. If not, an AK correction (afterwards) for a retrieval-model comparison (and thus also for inverse

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modeling purposes) is sufficient and probably less time consuming.

page 2773-2774, section 3.3 It would indeed be good to separate temperature and water vapour effects as it is not clear in this study which of these factors influences the retrieval the most. Furthermore: Wouldn't more iterations successfully account for high water vapour variations?

page 2775, section 3.5 Given the averaging kernels of the WFM-DOAS method (peaking at values > 1 at the surface), it is clear that any surface elevation will lead to a bias as this change will always be overestimated (AK>1).

page 2775, section 3.6 The analysis on surface albedo is somwhat too simple since this sensitivity strongly depends on the aerosols. As shown by Houweling et al. (ACP), a decoupled discussion of aerosols and surface albedo is hardly possible. This should at least be mentioned in the section.

section 3.7 See comment above! It is not true that aerosols mainly shorten the light path. Given high surface albedos, the opposite is often true (again, see Houweling et al.). Please clarify this.

page 2776, line 18 Why do the errros increase if the T-derivative is included?

section 3.8 In Frankenberg et al 2005, an increased sensitivity was not stated. On the contrary, the averaging kernels at the surface are lower as the IMAP-DOAS approach *forces* the AKs to be one in the 0-3km layer. This largely avoids the bias introduced by surface elevation (see section 3.5). Please clarify these issues.

page 2778, line 16 Undersampling the CO_2 bands or the ILS? I guess you mean the ILS.

page 2782, line 25 It is indeed not so easy to use O2 as proxy as both radiative transfer and averaging kernels largely differ from those of the CO_2 retrieval. This should be stated as the effects of aerosols and clouds do not really cancel out.

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EGU

page 2791, table 2 Is there any correlation of the error with, e.g., surface temperature?

page 2791, table 3 It would be good to have these errors calculated for different surface albedos as they largely determine the error induced by aerosols! (include low and very high albedos)

page 2800, figure 7 As said above, this behavior now strongly depends on the aerosol and lines for different aerosol scenarios should be included.

technical corrections

page 2774,line 18 knowledge ... CO_2 : add "of"

page 2775,line 3 ... profiles AND will ...: ??

page 2778,line 2 SCIAMACHY!

page 2781,line 6 ... then the that magnitude ...

page 2781,line 13 quite, stable: ?? Do you mean "quite stable"?

page 2782,line 11 maybe -> may be

page 2782,line 14 ; -> ,

page 2782,line 22 maybe -> may be

figure 6 function..

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