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Atmospheric Chemistry and Physics Discussions

Interactive comment on "First direct observation of the atmospheric CO_2 year-to-year increase from space" *by* M. Buchwitz et al.

C. Frankenberg (Referee)

cfranken@iup.uni-heidelberg.de

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The paper by Michael Buchwitz et al. describes a new retrieval version of CO2 from SCIAMACHY for a three year period from 2003 through 2005. Focus is on a comparison with the carbon tracker model and an analysis of the seasonal cycle and trend of CO2. The article is, with the exception of the error analysis, concise and very nicely written. Retrieving CO2 from space is a demanding work and this paper shows that even a "not dedicated" instrument can already retrieve some information on the global CO2 distribution. These results look very promising, esp. as an outlook in the future having dedicated CO2 satellites. The scientific significance (ie, the added value) of these results is still somewhat unclear, as so far only a known trend and a known seasonal cycle are reproduced from space. However, as this is a new and very demanding

task, the paper should be published in ACP after consideration of the following comments (esp. wrt error analysis and estimated numbers for precision and accuracy).

Specific comments: Abstract, line 18: The authors should state more clearly what they mean by “seasonal cycle can be retrieved ... a precision of about 2ppm”. What can be retrieved? The amplitude, peak-to-peak amplitude? Although this is clarified in the main text, it should be stated in the abstract as some people will only read this. Further: In my point of view it should read accuracy and not precision. For the comparisons of the seasonal cycle you take averaged SCIAMACHY measurements that have a certain precision. As you have plenty of data, the standard error in the mean, esp. given the large latitude bands that you analyze, due to a precision error will surely be very very low. The 2ppm thus refers not to statistical error but a bias in the retrievals. Concerning this topic it would also be very helpful to give numbers for the estimated precision of a single measurement.

Page 6721, line 23: Systematic errors are definitely more problematic (not probably). Given the requirements by Rayner et al (8*10deg, monthly average), it should be easy to get at least 1000 measurements a month from SCIAMACHY in such a grid cell. If the measurements had no bias, a single measurement precision of 20% would then be sufficient to achieve a standard error in the mean of 0.7%!

Page 6722, lines 1-5: What do you mean by optimised for CO2? It would be good to outline in which sense they will be better than SCIAMACHY (which will not harm SCIAMACHY)

Page 6724, line 21: 1558-1594nm. You actually fit not only in SCIA channel 6 but also in channel 6+ which has far more noise and dead/bad pixels. Did you check whether it makes any difference to constrain the fit to channel 6 only? It might make a difference.

Page 6724, line 28: The description of the different SCIA formats is somewhat confusing for people who are not working every day with SCIA data. Please briefly explain (for a general reader) what it means.

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Error analysis: Most of the paper is really nicely written but the error section, esp. the analysis of the bias wrt AMF, seems to be written in a hurry and remains relatively unclear. First, the authors re-explain the analysis of Barkley et al. in great detail. I think a shorter summary would be sufficient. Second, the determination of the bias wrt the AMF is unclear. Given the numbers, I would get a peak-peak bias of 7.2ppm in the southern hemisphere. However, the authors derive a smaller bias in the northern hemisphere and claim that this number (ie roughly 4ppm peak-peak) is the one to use. How can you be sure? Given that the authors derive a correction that actually depends on the geographic location is somewhat worrying as spatial biases are the most crucial problem if this dataset is to be used in inversion schemes. Without any figures (AMF vs difference Carbon-Tracker-SCIA for different locations), it is hard to follow the explanations. Further, the latitude bands chosen are rather large. Do the results change if they are constrained?

Another remark concerning the error analysis: I agree with the first reviewer that a proper analysis should also investigate the effect of partially cloudy pixels and their impact on the O2 and CO2 retrievals. Seasonal changes in either surface albedo (different for the O2 Band and the CO2 band) or occurrences of clouds could also lead to systematic biases depending on season and geographic region.

In the last paragraph of the error section (page 6729, line 18-23), the trend analysis is shown. In principle, the error of 1ppm is determined from only two measurements of the year to year variation. Is this comparison also based on co-located CarbonTracker results? Otherwise, decontamination periods or data unavailability might lead to a bias as different months get a different weight. Also in this context, I would say rather accuracy than precision for the estimated error in the trend. The definition of accuracy and precision is anyway a bit complicated for a highly complex target variable such as a seasonal cycle or a trend. Anyway, the authors should clarify whether they expect these errors to be random or systematic (I would expect systematic). For the determination of the trend in CO2, a linear regression analysis using all SCIAMACHY data would be

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better (including an uncertainty estimate for the slope). The determination of the trend is the title of the paper but hardly fills a small paragraph of the main text.

Technical comments: All the technical comments that I found are already raised by the first reviewer.

Interactive comment on Atmos. Chem. Phys. Discuss., 7, 6719, 2007.