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Interactive comment on "Interpreting the variability of CO$_2$ columns over North America using a chemistry transport model: application to SCIAMACHY data" *by* P. I. Palmer et al.

Anonymous Referee #3

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Review of "Interpreting the variability of CO2 columns over North America using a chemistry transport model: application to SCIAMACHY data" by Paul I. Palmer , Michael P. Barkley , and Paul S. Monks

This paper describes GEOS-CHEM simulated column CO2 values and those from the SCIAMACHY product for the year 2003. The two products are used to interpret column CO2 variability over North America, and to delineate the influence of different CO2 sources, and different CO2 source regions. The paper is generally well organized and clearly written, and presents two products (model and SCIAMACHY retrieval) that will be of interest to the wider scientific community, especially with the upcoming launch

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of dedicated CO2 monitoring instruments. Although the analyses presented contain some promising first looks at column CO2 variations in GEOS-CHEM, the paper does not go much beyond previous studies of column CO2 (Olsen and Randerson, 2004; Miller et al., 2007, Barkley et al., 2007, Yang et al., 2007) including that of the co-authors in this same journal in 2006 with the TM3 model. This stems partly from some methodological shortcomings in the model setup, and partly from the lack of rigorous assessment of both model and satellite product against available in-situ observations. As a result, the paper spends much time benchmarking a model against a satellite product that itself is still under close scrutiny because of unexplainably large variability (Schneising et al., 2008, currently in open discussion of this journal). My recommendation is to consider this paper for ACP after major revisions are implemented, guided by the more detailed comments below.

(A) In the model setup, the authors have used annually balanced, daily mean, biosphere fluxes for 2001 because the appropriate 2003 fluxes were not available. This goes against the recent trends in the CO2 modeling community to use year-specific, day-specific, and even hour-specific fluxes from the biosphere in order to get the correct covariance between transport and fluxes at diurnal to seasonal time scales. The effects of this on CO2 are generally large, and using appropriate fluxes was shown to substantially increase the correlation with in-situ observations. The longer term averaging done in this study might at first glance put less emphasis on shorter time scales, but the selective sampling of the CO2 field to coincide with SCIAMACHY overpasses might actually make the results quite dependent on higher frequency variability (Corbin et al., 2008). This variability is now implemented incorrectly as the synoptic variations of 2001 cannot be expected to mimic 2003. This is all the more surprising as the second and third authors of this paper *did* use 2003 specific biosphere fluxes for their earlier model study of 2003 CO2 columns over North America. The lack of annual mean uptake in CASA will furthermore bias the model by several ppm during the growing season, when the amplitude of the uptake might be up to 25% larger than is now included (see for instance Yang et al., 2007). With several non-balanced, and higher

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resolution biosphere model fluxes available from the modeling community, I want the authors to improve the treatment of biospheric fluxes and demonstrate that synoptic variations are well captured in their framework.

(B) The comparison to observations leaves the reader guessing as to which product is more credible: the GEOS-CHEM model that is not optimally set up to reproduce 2003 CO_2, or the SCIAMACHY satellite product which seems to give a seasonally dependent bias of 6-10 ppm wrt to the model, and gradients over North America that are 2x to 4x larger than simulated. Very little guidance is given in the paper to this apparent contrast, but overall the authors seem to favor the satellite as the benchmark ("On a continental scale, the model has relatively little skill in reproducing SCIAMACHY CVMRs, capturing only a few percent of the observed variability"). However, *observed* column mixing ratio variability over North America available from nearly 1300 flights over the continent suggest the gradients to be on the order of a few ppm only! Typical monthly mean gradients over North America within any growing season month measured with high-precision instruments is 2-4 ppm, and even the largest gradients observed between any two individual profiles in a month is below 20 ppm (this is in the 5-sigma range of the distribution!). And this analysis is likely to overestimate the gradients seen by a satellite as fewer profiles make up the monthly mean, many profiles go only up to 6 km thus oversampling the large surface gradients in the column mean, and the profiles are constructed from air samples rather than a much larger satellite footprint. Recent investigations by Schneising et al. (ACPD, 2008, should be discussed in the paper) discuss the possible biases in the SCIAMACHY product in some detail, and indeed suggest that gradients are larger than would be expected based on observations and based on a model simulation that is more firmly rooted in atmospheric observations. I want the authors to include available observational data from North America column CO_2 in the analysis (FTS data from the TCON network and Egbert, aircraft data from NOAA ESRL, or the COBRA mission, or the recently completed upper air CO_2 archive from LMDZ) to help the reader out of the confusion created by the model-satellite product comparison.

(C) The authors have used a large number of figures/panels to illustrate their results, but some of them are not focused towards the more interesting results of this study. These are (in my opinion) the analysis of source specific contributions to the column, as well as the contribution of different continents to the CO_2 column over North America. This is relevant in light of recent results that shows that many transport models fail to properly capture free tropospheric variations in the NH (Yang et al., 2007; Stephens et al., 2007), and therefore miscalculate NH and tropical sink strengths. The GEOS-CHEM analysis could help focus the effort to improve CO_2 models. Also, the suggestion that large-scale gradients might be used more readily when estimating source/sink distributions than the absolute values is interesting and could be developed further, for instance by analyzing the expected signal strengths and length scales for different components of CO_2, as well as their already shown decay time. I would like the authors to give these novel results a more prominent position in the paper. As a much more rigorous assessment of SCIAMACHY columns is (unfortunately) published at the same time as this paper, the comparisons involving SCIA should then be downplayed in this work.

Detailed comments and questions:

Abstract: The stated large positive model bias (10-15 ppm in midsummer) is hard to reconcile with the convergence of model and data in summer, and the general ability of GEOS CHEM to reproduce the magnitude and seasonal cycle of CO_2 over North America stated in the next sentence. What is going on in summer, and how sure are you that the model reproduces observed CO_2 magnitudes and seasonal cycles?

Introduction: The references Bousquet et al., Palmer et al., Chen et al., and Stephens et al., do not credit the collectors of this data, nor reference inversion studies that the sentence refers to. Please change.

Introduction: "We focus on North America because of the extensive multi-platform measurement programme which can be used to help evaluate SCIAMACHY via the CTM."

This is an excellent argument, and I would like to see the authors follow up on it (point B)

Introduction: "Nonetheless, recent work has highlighted the requirement of using accurate, synoptic-scale atmospheric transport to interpret CO2 column data in order to minimize errors associated with spatial sampling, particularly over geographical regions with active weather systems (Corbin et al., 2008)." Again, a very good argument and nice reference. It goes right back to point (A).

Introduction: Final sentence has an incorrect reference to section 5.

Section 2: Figure 1. Is there some way to condense this information into a simpler figure? As the authors state, we're in fact looking at surface orography repeated 12 times. The real information is in the small x-y plots on the right hand side. Why are values outside the range 340-400 ppm excluded from the scene? Are these not caught by the other filters implemented? I do not agree with a selection of data without a good physical, understood reason to do so. Please state this reason.

Section 2: The peak-to-peak signal of 15-20 ppm over the growing season is in fact not consistent with Olsen and Randerson, (2004). The maximum amplitude of any seasonal cycle found in that study was 10 ppm, whereas the FTS data from Paul Wennberg at WLEF (with a strong uptake signal) suggests 11 ppm peak-to-peak (Washenfelder et al., 2006). The investigations with the MATCH model presented by Miller et al., (2007) do also not sustain such large gradients (typically 1 ppm in XCO2 over 460km in July). The Barkley et al.,(2006) paper also suggests an overestimate of the SCIA amplitude at Egbert. These results should be incorporated in the discussion, and the claim of agreement removed.

Section 3.3: It is not clear to me exactly how the sampling of the model was done to compare to GLOBALVIEW. What is the advantage of sampling the model according to SCIAMACHY when the CO2 product you compare it to was created with grab samples from different days/hours? I would think that a comparison of the model to GLOB-

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ALVIEW should focus on the model skill when assessed properly. This means that in order to compare to GLOBALVIEW, the model should be sampled on 4-5 individual days of the month for continental sites, to construct a monthly mean to be compared to the data product. For background sites, the co-sampling might be less important. Perhaps I misunderstood the author's intentions in this section, so please clarify them.

Section 3.4: It is nice to see the care taken in the sampling procedure, including the kernel. This has been shown to be very important for evaluation and is not trivial.

Section 4: The Gaussian nature of the model-satellite differences is interesting and worth showing in a figure. I suggest that in figure 2, the x-y scatter plots are shown together with a month-by-month distribution of the residuals instead of the 12 very similarly colored panels. The large range on the colorbars precludes any assessment of these maps besides the difference in scale. This is an important point though, so perhaps one of these maps from figure 2 can be shown alongside one from figure 1. From the distributions, the reader can assess both the bias, its seasonal nature (it seems to be largest during the peak growing season and smaller during the shoulder seasons), and the difference in scale. Interestingly, if the residuals are indeed Gaussian it would suggest that the satellite retrieval (or model) sees atmospheric signals attenuated (or dampened) relative to the other, which might hold some clues as to the nature of the difference.

Section 4: How does your description of a latitude dependent problem in the 50-70N region relate to the discussion of possible solar zenith angle biases due to errors in the neglect of polarization, or spectroscopic data in Schneising et al., 2008 (ACPD)?

Section 5.1: This discussion of absolute contributions from different processes/regions would carry a lot more weight if the simulated signals (especially from the biosphere) could be shown to be accurate relative to observed high precision CO_2 over North America. Even for 2003, and certainly for later years, much is available.

Section 5.2: Figure 8 paints an interesting picture in the sense that the model seems

to capture the amplitude of the surface CO2 signal (A), then predicts (as expected) a smaller seasonal cycle for the column (C), yet the SCIAMACHY data go through a much larger seasonal range (black line). Here, a simple addition of the WLEF FTS column data would be helpful, even if the kernels and retrieval details are not exactly the same. One should furthermore be cautious when using GLOBALVIEW as a substitute for real observations especially over the continents as flask samples do not characterize the variations there very well. The continuous CO2 record at WLEF is a better benchmark to use when assessing your model.

Section 5.2: The location of Wendover, Utah is quite difficult to capture in coarse transport models because of the proximity of Salt Lake City. On a 2.5x2.5 grid scale this city and its large fossil fuel emissions might be in the same gridbox as sampled for assessment of the model. Please check this.

Section 6: In principle, this is an interesting exercise that could give some new insights and I encourage the authors to expand it. It raises a few questions though. For instance, a response of 75e-5 ppm/Tg CO2 as is maximally seen for fossil fuels over North America (top left figure) suggests that the signal from typically 2 PgC/yr emissions (=611 TgCO2/month) gives a 0.46 ppm response in the column, consistent with figure 7 (top left). The outlook for the biosphere is better with a peak uptake of 12 PgC/yr over North America suggesting a 3 ppm peak influence. The signals after 3-4 months will be at least 3x times smaller. This is a tough target for a satellite given the typical precision of the satellite retrieval. It is of course likely that the signals in the first 4 weeks are much larger than can be assessed from figure 10, but the errors of the satellite instrument are also increasing rapidly at such scales (Miller et al., 2007; Corbin et al., 2006, 2008).

Section 6: The statement "However, as we discussed earlier and show in Figure 7 the distributions of many of the dominant flux signatures are sufficiently separated in space and time to permit independent estimation of individual fluxes; this needs to be confirmed with inversion calcula-

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tions." seems a little premature based on the analyses of figures 7 and 10. It also does not do justice to the elaborate investigations of Miller et al., (2007) who discuss the potential signals of fluxes at different scales, as well as the impact of biases (much smaller than shown for SCIAMACHY). This work should at least be discussed by the authors.

Section 7: the WLEF peak-to-peak signal was earlier stated as 20 ppm (which looks correct in the figure) and not as 29 ppm.

Section 7: "Estimating systematic bias with a model is of little value because our current quantitative underst anding of the carbon cycle is incomplete." I do not understand this argument. What lack of understanding are you referring to here that precludes the detection of systematic biases using models? I agree that real bias estimation can only be done through careful in-situ observations, but models that can demonstrably match these constraints based on our current knowledge of the carbon cycle can be a useful tool to assess the global CO_2 mixing ratio distribution. Please comment.

Section 7: The final sentence offers an interesting way to proceed with using satellite retrieved CO_2 signals in the presence of (known) biases. It does put extra importance on getting the spatiotemporal gradients correct of course, and gets back to the variograms constructed in the Miller et al., paper showing the large-scale coherence of the XCO_2 field.

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