Atmos. Chem. Phys. Discuss., 8, S3457–S3463, 2008
www.atmos-chem-phys-discuss.net/8/S3457/2008/

Atmospheric Chemistry and Physics Discussions

ACPD

8, S3457–S3463, 2008

Interactive Comment

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.

P. I. Palmer et al.

Received and published: 6 June 2008

We thank the reviewer for a very thorough examination of our paper. Below are our responses to specific comments (shown in italics).

The paper does not go much beyond previous studies of column CO28230;including that of the coauthors in the same journal in 2006 with the TM3 model.

The principal subject of our paper is explaining spatial and temporal distributions of column CO2 to individual local, regional and long-range transported (LRT) sources and sinks. This has been a major shortcoming of all previous studies associated with column CO2. Olsen and Randerson used a 3-D model of CO2 to compare differences between surface and column CO2. They did not account for the instrument averaging

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Interactive Discussion

Discussion Paper

kernel as done here, a key issue that will change the interpretation of the column data. The Barkley et al paper did use the averaging kernel but did not show a quantitative assessment of SCIAMACHY data, instead focusing on the distributions of observed and model column CO2. The Yang et al, paper, while a valuable contribution to the field, focused on total columns from two measurement sites over North America. This work emphasized the large uncertainty in current biospheric models.

As an approach to putting our model results into context we also presented a quantitative assessment of the model and observed CO2 columns and show they do not agree. We only included a limited evaluation of GEOS-Chem CO2 columns during 2003, reflecting the small amount of in situ data; however, it is clear from this and other work that SCIAMACHY columns do suffer from an absolute bias but the observed variability is consistent with in situ data (e.g., Barkley et al, 2007). Whether this bias varies on spatial and temporal scales that would compromise inverse model calculations is outside the scope of this work. Given the parallel submission of Schneising et al, ACPD, 5477-5536, 2008 we will reduce the emphasis of model evaluation (as suggested by this reviewer in a later comment) by putting an extended version of the current model evaluation in an appendix. As part of the extended model evaluation we will include 2003 Egbert FTS CO2 column measurements, and present a model timeseries of CO2 column over Kitt Peak and Park Falls to test quantitative consistency with measurements in later years. We will further evaluate the model using data from the LSCE airborne data archive available from the GEOMON website. From the summary files, it appears that the only data available for 2003 is COBRA-2003, an aircraft campaign that spans much of the troposphere that we will acknowledge.

Comment A (paraphrased): The model set-up uses biospheric fluxes for 2001 rather than 2003, going against the recent trends in the CO2 modelling community

As outlined above, the principal focus of this paper was to assess the importance of local, regional and LRT signals in CO2 columns observed from space. We chose 2003 because we have a complete seasonal cycle of SCIAMACHY CO2 data over North

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America. The biggest practical issue here was daily biosphere fluxes were available to us for 2001; we are currently not running such a global biosphere model at Edinburgh. Analysis of monthly fluxes from CASA over North America between 1997 and 2004 did not show a great deal of year-to-year variability (<10

Corbin et al, [2008] highlighted the importance of using correct model synoptic transport because transport error could be misdiagnosed as errors in sources and sinks. This emphasizes the requirement for not only high-resolution transport models to interpret column CO2, as stated by the authors, but also rigorous (and robust) estimates for model error to include in the inverse model algorithms. Corbin et al, [2008] state that in their North American simulation, the main driver of total column CO2 temporal variability was synoptic-scale weather systems and we have no reason to believe this is not the case for our simulation over the same region. A Fourier analysis of their model revealed a peak at 3.5 days that the authors interpreted as model fronts. GEOS-Chem is forced using NWP analyses from the GEOS model based at NASA Goddard (which we degrade the horizontal resolution to 2x2.5 degrees), and previous studies that use these analyses have successfully reproduced observed distributions of CO and CO2 (and other chemical tracers) from ground-based and aircraft observations. SCIAMACHY has an approximate repeat time of 3 days (exact repeat within 30 days) so the instrument would effectively be subsampling the continuous record that the authors present – whether this subsampling favours high-frequency variability is not clear to us. Provided that the model is sampled in a manner consistent with the observations (which it is) subsampling a continuous record should not pose a problem. To address the comment about model ability to capture synoptic variability, we will present 1) model surface CO2 over the GLOBALVIEW sites shown in the submitted manuscript but will not subsample to account for the SCIAMACHY spatial coverage (data associated with overpasses with emphasized), and 2) a comparison between model and observed CO2 columns over Egbert, although as other authors have noted this site is noisy [Barkley et al, ACP, 6, 4483-4498, 2006].

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Given that the focus of this paper is to demonstrate the complex source/sink relationships with the CO2 columns and that there now exists a more comprehensive assessment of SCIAMACHY CO2 (Schneising et al, ACPD, 2008) we suggest that a thorough treatment of biosphere fluxes be left for further work.

Comment B (paraphrased): Is the more model or the measurements more credible?

The reviewer makes a good point here and we will address it using the available in situ aircraft data for 2003. As mentioned above, we will also sample the model over Park Falls and Kitt Peak to show that the magnitude and seasonal cycle of CO2 columns is consistent with observations from later years. GEOS-Chem is more consistent with in situ data that SCIAMACHY, even after considering SCIAMACHY bias, and we will convey that message more strongly in the revised manuscript.

Comment C (paraphrased): cut down on figures and focus the paper more on the specific contributions to the column, especially in light of recent results that show that many transport models fail to properly capture free tropospheric variations in the NH and therefore miscalculate NH and tropical sink strengths. Downplay the SCIAMACHY comparison, light of a more rigorous assessment of these data in a recent ACPD paper.

Agreed. The purpose of this paper remains the flux contributions to the total CO2 but without some evaluation of the model the study is a wasted exercise. In any case, we will give less prominence to the SCIAMACHY comparison, as suggested, and relegate the model evaluation to an appendix.

Abstract:

the 10-15 ppm bias is largely driven by the dramatic and unrealistic drawdown inferred from the SCIAMACHY CO2 columns. GEOS-Chem timeseries of CO2 columns are Kitt Peak and Park Falls are consistent with ground-based measurements from later years. We conclude that the model is more consistent with the truth over those regions than the observations. The wording will be changed to reflect this.

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Introduction: credit collectors of data.

As discussed later, not citing GLOBALVIEW was an egregious oversight and will be corrected.

Introduction: point B.

See response to point B.

Introduction: point A.

See response to point A.

Introduction: Typo about section 5.

Noted and corrected.

Section 2, condense Figure 1; why are removing columns that don't fall between 340 and 400ppm?

We think it is useful to see where the differences between model and observed CO2 column occur, as well as showing the scatterplot. The 340-400 ppm is adopted in the retrievals of CO2 to remove anomalous results resulting from undetected clouds or from aerosol scattering [Barkley et al, 2006], ie to adequately constrain the light path.

Section 2:

Agreed, the authors misinterpreted graphs from Olsen and Randerson. Text will change accordingly.

Section 3.3. Focus on model assessment with GLOBALVIEW data:

We wanted to show that even with the sampling provided by SCIAMACHY, the instrument could still provide information on seasonal cycles. We agree with the reviewer that we also need to thoroughly test the model. We will sample the model throughout the year for the GLOBALVIEW sites but highlight the data that would be available from SCIAMACHY.

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Section 4. Suggest show Gaussian plots of model-data residuals rather than contour plots of CVMR.

With respect we disagree with the reviewer on this point. But we do agree that including a summary of the Gaussian statistics within the main text.

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

The FSI algorithm attempts to minimize SZA biases by computing each reference spectra at the corresponding measurement SZA. This is in contrast to using interpolated reference spectra,stored in a look up table, which are pre-calculated at fixed SZAs (eg, Buchwitz et al, ACP, 5, 3313-3329, 2005). However, whilst past validation studies (eg. Barkley et al, 2006, 2007) do not indicate a systematic SZA bias. we acknowledge it is possible that a small residual bias may exists when considering, for example, the SZA influence on saturation effects of strongly absorbing non-resolved lines [Schneising et al, ACPD, 2008] and/or calibration issues. Errors due to polarization effects on the CO2 retrievals have not been quantified.

Section 5.1 Source/sink contributions to CVMR would carry more weight if the model were known to be accurate relative to high-precision CO2 over North America. Even for 2003, and certainly for later years, much is available.

More evaluation will be included in the revised manuscript, as discussed above.

Figure 8 would be better served by adding WLEF data, even if the kernels and retrieval details are not exactly the same. The continuous record of CO2 at WLEF is a better benchmark than the GLOBALVIEW when assessing your model.

This is a good suggestion. We will add the max/min lines for the WLEF data for a later year. We also recognize that WLEF is only one site and an evaluation of a global model cannot rely on data from one location no matter how accurate and precise they are.

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Section 5.2: Check the model sampling of the Wendover Utah site, in relation to Salt Lake City and its fossil fuel emissions.

The reviewer is correct that SLC (40.75N -111.9W) is in the same 2x2.5 degree grid box as the Wendover site (39.90 N, -113.72 W). We will clarify this in the revised text.

Section 6: Expand on your discussion on the analysis of the Jacobian matrix, particular with reference to the signals observed by satellites.

Agreed.

Section 6: Include a discussion of results from Miller et al, (2007) who discuss the potential signals of flux at different scales, as well as the impact of biases (much smaller than SCIAMACHY).

Agreed.

Section 7: WLEF peak to peak signal is 20 ppm rather than 29 ppm. Typo corrected.

Section 7: Please clarify the following statetment "Estimating systematic bias with a model is of little value because our current quantitative understanding of the carbon cycle is incomplete."

This statement should read "Attempting to directly estimate systematic bias of satellite data with a model is of limited value because our current quantitative understanding of the carbon cycle is incomplete."

Acknowledge GLOBALVIEW.

This is an egregious oversight for which we apologize and correct in the revised manuscript.

Interactive comment on Atmos. Chem. Phys. Discuss., 8, 7339, 2008.

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