Geosci. Model Dev. Discuss., 3, C411–C421, 2010
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Geoscientific Model Development Discussions

Interactive comment on "Modeling global atmospheric CO_2 with improved emission inventories and CO_2 production from the oxidation of other carbon species" *by* R. Nassar et al.

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Section 2: I recommend that the authors include a table that summarizes the emission inventories used in this study (emission category, global-annual total flux, citation of the referenced paper, etc.). This information will be useful for the readers.

We thank the reviewer for this suggestion. We have now included a table, designated as Table 1 (which shifts the numbering of all other tables).

P.895, line 1-: The impact of the monthly-varying fossil fuel data on the performance of forward simulation is not clearly represented. It would be interesting to note the impact of monthly-varying emissions on the simulations of the observed seasonal CO_2

variations by comparing the simulation results (for both monthly-varying and annually-varying emissions) with observations. I recommend that the authors insert a figure pertaining to this information and discuss the comparison in Section 3.

It is well known that real fossil fuel emissions are not constant throughout the year. To represent this variation annual fossil fuel emissions can be parsed into monthly values. We use the CDIAC time series since it is the only time series we know of that does this on a global basis. Although we can demonstrate differences between simulations with the monthly-varying and annually-varying emissions, it would be very difficult to use observations to directly confirm that the monthly-varying emissions were superior, since the signals from the seasonality of the fossil fuels would be combined with those from the terrestrial biosphere and the oceans, which are not perfectly represented in the model. Furthermore, the terrestrial biosphere makes the largest contribution to the seasonal cycle in the northern hemisphere and over most of the globe, so the impact of the monthly fossil fuels would not easily be seen. Evaluating the impact of the monthly-varying fossil fuels could perhaps be done with inverse modeling experiments, but that is beyond the scope of this paper. Andres et al. (2010, Tellus, in review) provides a more detailed description of monthly fossil fuel emission inventory and its development. The focus of the present paper is not this specific inventory but implementing a suite of improved emission inventories and the CO_2 chemical source, with the motivation of creating improved simulations for inverse modeling with satellite observations.

P.895, line 19: How did the authors obtain the annually-varying emissions here? Although the information for the monthly-varying fossil fuel emission was cited from the study conducted by Anders et al. (2010), no information regarding the annually-varying emission was provided. Was the annual data obtained by averaging the monthly data for a year? Or did you use the global annual emission data used in the original version of the GEOS-Chem?

As we state in the Figure 1 caption, the annual fossil fuel total is the sum of the monthly fossil fuel totals. The global annual emission data from the original version of the

GMDD

3, C411–C421, 2010

GEOS-Chem CO_2 simulation mode were only available for 1995.

P.896, line 16: As with my previous comments, it is more important to investigate how the monthly-varying emissions led to better simulation results (by comparing with observations). Hence, I recommend that a comparison and discussion be included in Section 3.

Please see our response above.

P.898, line 4: "Growth patterns..." It is not clear to me how the author obtained the biofuel burning emission data for the years after 1995 for use in the forward simulation. Please clarify this.

Biofuel emission numbers for 1995 were used for all simulation years since we do not have an inventory with year-specific biofuel emissions. As stated in the text, it is not clear how biofuel emissions from the developing world will have increased since 1995 because although population growth has occurred, much of this growth has occurred in urban areas, where other sources of fuel for heating and cooking dominate. China and India were the largest source of biofuel emissions in the 1990s, but there has been a large shift to fossil fuels in those countries. Therefore, as we stated in the paper, "the error in assuming constant emissions from 1995 should also be small."

P.898, line 26: It would be interesting to see the impact of the inclusion of a diurnal cycle (with 3 h intervals) to the terrestrial biospheric exchange flux on the CO_2 simulation results. Can you provide some information on this phenomenon? If the impact is significant (e.g., a better representation of vertical CO_2 profiles near PBL), then consider focusing more on the relationship between this impact and the simulation results in the manuscript, because most atmospheric CO_2 models still use the monthly-mean flux.

This would be interesting to examine, but the objective of the present paper is not an analysis of the impact of the diurnal cycle on atmospheric CO_2 abundances.. The CASA diurnal fluxes were described in Olsen and Randerson et al. (2004), where

GMDD

3, C411–C421, 2010

Interactive Comment

they examined the impact of the diurnal cycle on the surface CO_2 concentration and on the CO_2 column abundance. We are only applying them in this work and they will simply be used as an a priori for our ongoing flux inversion work (Nassar et al., 2010 in preparation) that will use these simulations. In the future, we plan to improve the terrestrial biospheric CO_2 component of GEOS-Chem and will conduct comparisons similar to those proposed by the reviewer at that time.

P.900, line 24-26: Add the phrase “obtained from Takahashi (2009)” after the term “the new climatology” in this statement. Further, replace the term “the 1997 work” with “Takahashi (1997).”

These changes have been made.

P.904, line 1-3: “this altitude is...” Why is this part important? (Probably for inversion or for the assimilation study?) Clarify this reason for the readers.

Added the text “and is therefore relevant for modeling work involving comparisons with or used in conjunction with those observations”.

P.907, line 29-: It is not clear that the chemical pump will have a “significant impact” on inverse modeling from this study. Hence, remove the sentence “which will have a significant...”

Figure 8 indicates localized surface differences as high as 0.8 ppm. Deviations of CO_2 of this size will have an impact on inverse modeling. Earlier work (Suntharalingam et al., 2005, Table 2) quantified the impacts to be as high as 0.27 PgC/yr for northern land regions. Rather than removing the entire sentence, we have removed the word “significant”.

P.910, line 2: Some more details regarding the CO data assimilation should be provided. I recommend that you add a few sentences to describe the data assimilation settings and consider the addition of a figure (or, at least, a relevant discussion) to evaluate the assimilated CO fields by comparing them with the free model simulation *result and any observations. This indicates the performance of data assimilation and demonstrates (although indirectly) the reality of the estimated CO_2 chemical production rate.*

TES assimilation is described in Parrington et al. (2008). Although an in-depth discussion of this is beyond the scope of this paper, we have added a brief description and an additional figure demonstrating the improved CO fields as a result of TES CO assimilation.

P.911, line 20: "Overall, the ..." This sentence refers to a very general discussion and is not necessary here. I recommend that you delete it.

We are specifically referring to the difference in our original Figure 10 (now Figure 11) and have now clarified this in the text.

P.912 line 6-: As investigated by the authors, it is important to consider representation errors when comparing the simulation results with in situ observations. Similar discussions have been conducted in several previous studies. I recommend that you refer to the relevant papers (e.g., Pillai et al. (2010, ACP, 10, 83-94)).

We thank the reviewer for reminding us of this paper which includes a thorough investigation of representation errors for satellite CO_2 observations. We have now added the sentence: "This issue has been investigated by Gerbig et al. (2003 a,b) for in situ observations of CO_2 and by Pillai et al. (2010) for satellite observations." along with these three new citations.

P.912, line 6-: It would be useful if the authors discuss the time representation (sampling) error (e.g., day/night time difference) along with the spatial representation error. How did the authors sample the model output (e.g., time interval)? Does the sampling time correspond well with the observation time? If not, then please discuss the problem related to the time representation error in the manuscript (e.g., related to diurnal PBL and synoptic transport variations).

GMDD

3, C411–C421, 2010

Interactive Comment

For some model applications, temporal representation errors can also be important. For comparisons with CONTRAIL (now shown in Figure 16), we attempt to minimize representation errors by sampling the model output close to the times of the CONTRAIL measurements (within ± 3 hour), which should suffice for the upper troposphere. We have added text in Section 3.2 explaining this. Our time series comparisons in our original Figure 12 (now Figure 13) employ daily averages based on four points in the diurnal cycle spaced by 6 hours, which we now describe in Section 3.1. All of our other figures deal with monthly averages or annual averages for which temporal representation errors should have little impact.

P.914, line 1: As mentioned in the manuscript, a spin-up obviously requires several months. I recommend that the authors show the simulation result for after the spin-up period (e.g., from 1 January 2005) and present the related discussions in a more concise form.

We initially showed the spin-up to illustrate that the spatial distribution in the model arises as a result of model processes and not simply the initial conditions; however, we accept this recommendation and have modified the time series panels to begin on 2005-01-01.

P.914, line 8: Define the "free-running model." This is the first occurrence of this term in the manuscript.

We are referring to the model not being constrained by assimilated data, which is explained in the last sentence of that paragraph. To avoid confusion, we simply remove the term "free-running".

P.914, line 15: "the drift is not a problem..." I do not agree with this sentence. Data assimilation is a technique for correcting the forecast in order to obtain the analysis using observations within a data assimilation window. The drift has a very weak signal and is too slow for capturing from the OmF statistics within a data assimilation window; thus, I believe it is difficult to correct such a signal through data assimilation. To reiterate, *the motivation of this study is to better represent emission inventories. I recommend that (more positive) discussions be presented on how to better represent emission inventories in order to provide high-performance forward simulation (c.f., without drift or bias).*

We respectfully disagree with the reviewer on this point. The drift in this case represents our inability to simulate accurately the net terrestrial and oceanic uptake of CO_2, which is exactly what atmospheric CO_2 inversion analyses (using data assimilation) are trying to quantify. Numerous papers describe the approach of assimilating CO_2 observations as a means of removing model drift (i.e. Jiang et al. 2008, Simulation of upper tropospheric CO_2 from chemistry and transport models, GB4025; or many TransCom publications). In some circumstances, model a priori surface fluxes are even set to zero, while in other cases non-zero a priori fluxes are used as we have done. We do agree with the reviewer that a "high-performance forward simulation" is desirable, but as we state multiple times in this manuscript, our objective is to produce a CO_2 transport model that can be used in inverse modeling work to constrain terrestrial biospheric and ocean sources and sinks of CO_2 using satellite observations. Improved emission inventories are a large part of that effort. We have modified the text and instead of stating that the drift is not a problem, we mention the need for precise observations to better constrain these sinks of CO_2.

P.915, line 1- Figure 13: Why did the authors not average the model concentration over observation longitudes? The longitudinal variations in CO_2 concentration near the surface are very large. The present comparison (between the zonal mean model concentration and the in-situ measurements) does not seem to provide any information regarding model validation.

In our original Figure 13 (now Figure 14), we include both the zonal means and the model sampled at the GLOBALVIEW observation locations. The model – GLOBALVIEW differences pertain to the model sampled at the GLOBALVIEW locations, not the zonal mean. We include the zonal means for the purpose of comparing the chem-

GMDD

3, C411–C421, 2010

ical and no-chemical-source simulations as well as the same simulation at the two altitudes (surface and 5 km).

Figure 13: Can you comment on why the simulated CO_2 concentration is largely overestimated in the tropics and subtropics (approximately between 20S-20N)? Is this related to the sampling error problem or the model transport (e.g., uplifting by tropical convergence zone) problem?

In our original Figure 13 (now Figure 14), for 2005 and 2006, agreement in the tropics and sub-tropics is very good when the model (indicated by the colored symbols) is sampled at the observation locations. In 2007, agreement in the sub-tropics slightly degrades with a high model bias. This may result from transport errors, large natural deviations from the biospheric climatology, or errors in the emission inventories, such as the fossil fuel scaling which used preliminary energy numbers for 2007, as discussed at the end of page 914. The model zonal averages (the colored lines) show much higher CO_2 abundances in the tropics, but this is not necessarily an overestimate since nearly all stations at these latitudes are deliberately measuring background CO_2 and are far from strong emission sources that increase the zonal average.

P.916, line 15-: Spatiotemporal variation of CO_2 concentration is substantially smaller in the SH than in the NH. Accordingly, the difference between the model and observation is obviously smaller in the SH, as investigated by the authors. Even though the difference is smaller in the SH than in the NH (it is not important to evaluate and improve the forward simulation), the error (the difference between the model and observation) in the SH in January is very concerning. I recommend that the authors remove the concerned sentence and add a discussion regarding the possible reasons for the significant difference in the SH in January.

We added some text hypothesizing that the differences could be due to deviations from the CASA biospheric fluxes or biomass burning emissions, and furthermore, we have slightly reworded the paragraph.

P.917, line 19-: I recommend that the authors sample the model output during the CONTRAIL measurement periods and compare it with the CONTRAIL data (especially if the number of CONTRAIL data used is very small). How many CONTRAIL data (flights) were included in the comparison for each month? This information should be helpful for determining the temporal sampling error.

We agree with the reviewer that sampling the model at the correct positions and times of the CONTRAIL observations would make a better comparison than our approximation using the mean contrail longitude and altitude, so we have changed the model transects in the figure (red/blue lines). There were 1 or 2 CONTRAIL flights/transects per month in 2006 (except for January) and each is shown as a separate line in the figure.

P.918, line 2: The impact of the monthly-varying fossil fuel emission data on the simulation of the observed seasonal CO_2 variation is not discussed in this manuscript. The authors should ideally add a figure or present discussions to show the impact of using the monthly-varying emission data instead of the annually-varying emission data by comparing the simulation results with observations.

Please see our earlier responses involving the monthly-varying fossil fuels.

Figure 1: Too many figures are provided in Fig. 1 but are not discussed in the text. I recommend that the authors remove the forward simulation results and show only the differences between two simulations.

We have removed the forward simulation panels so that only those showing the differences remain.

Figure 5: Add labels (a, b, c, d, e, f) in the figures. It is not useful to use a logarithm scale only for (d).

The log scale in panel d has been changed to a linear scale and the labels (a, b, c, d, e, f) have been added.

GMDD

3, C411–C421, 2010

Figure 6: It would be better to show the chemical production + surface correction instead of only the chemical production, because this sum is added to the emission inventories.

Since the surface correction only changes the emissions at the lowest model level (nominally 0.00-0.13 km), adding it to this figure would be of little benefit since it would be very difficult to see.

Figure 8: Change the caption to explicitly describe that this "chemical production" includes surface correction (c.f., there are negative anomalies at the surface level because of the surface correction).

This change has been made as requested.

Figure 16: Several figures are plotted for different months, but the seasonal difference is not discussed in the manuscript. Remove most of these figures (if the comparison for just one month is sufficient for the discussion) or add the related discussions.

Our intention with this figure is to illustrate the changing locations of convection differences between two sets of meteorological fields to demonstrate the complicated problem of dealing with model transport biases, therefore we prefer to keep the figure in its present form.

Interactive comment on Geosci. Model Dev. Discuss., 3, 889, 2010.

GMDD

3, C411–C421, 2010

Interactive Comment

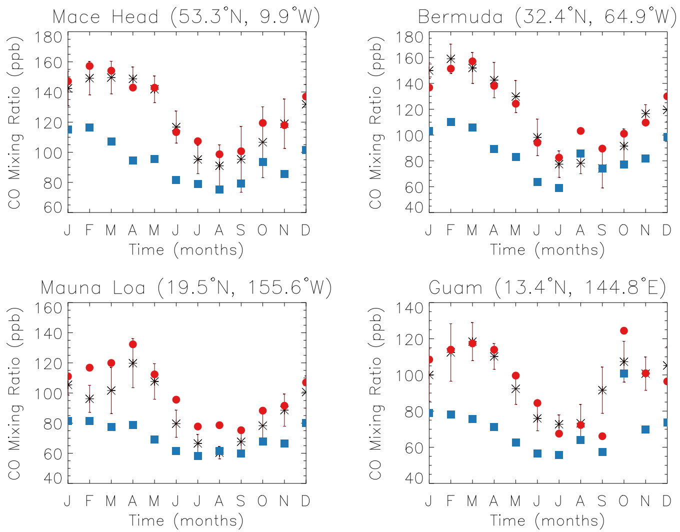

Figure: Monthly-averaged model CO (blue squares) and the model with assimilated TES CO (red circles) compared with CO surface measurements (black asterisks) in 2006. Error bars on the measurements denote the one standard deviation variability in CO at that station for the given month during the period of 1996-2006.

Fig. 1.

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