**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.

Anonymous Referee #2

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Review of the manuscript "Modeling global atmospheric CO_2 with improved emission inventories and CO2 production from the oxidation of other carbon species" by R. Nassar et al.

General comments:

In this paper, the authors introduced and evaluated a new global model for the forward transport simulations of atmospheric CO_2. The motivation of this study is to improve CO_2 forward simulations for use in inverse modeling or data assimilation studies by providing a better representation of emission inventories. The improvements are primarily based on the consideration of the monthly-varying fossil fuel emission, the shipping

and aviation emissions, and the chemical production of CO_2, which provide significant information on atmospheric CO_2 modeling. The new emission inventories are documented reasonably, and the simulation results are evaluated by comparing them with the observations. The manuscript also addresses several important topics pertaining to atmospheric CO_2 modeling. However, there are a few issues in the paper, regarding the methods of comparison between the simulations and the observations. This paper can be submitted for publication in GMD after rectifying these issues by considering the comments given below.

Specific comments:

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.

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.

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?

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.

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.

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.

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)."

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.

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..."

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.

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.

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)).

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).

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.

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

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).

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.

Figure 13: Can you comment on why the simulated CO_2 concentration is largely over-estimated 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?

P.916, l15-: 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.

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.

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.

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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.

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).

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.

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).

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.