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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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1. *Page 893, lines 7-14: this seems like a repetition of previous statements.*

After the description of previous work, we outline what we have done in the current paper. This text is the first description of our work in the introduction. It does resemble a similar but briefer description that appeared in the abstract, which we believe to be common practice.

2. *Page 894, line 9: are those biospheric fluxes of all carbon species or just CO_2?*

Our inverse modeling work estimates biospheric CO2 fluxes. We have added "CO_2" to

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the text to make this clear.

3. *Page 895: please redo Figure 1, it is way too small.*

This figure has been redone to show only the monthly minus annual differences based on a request from the 2nd reviewer. The new figure is not as wide so it can be shown larger.

4. *Page 897, lines 1-5: this seems very much like an ad hoc choice. Could you substantiate?*

The scaling factors applied for 2009 are of course only estimates, but some estimates are required in the absence of reported numbers in order to run the model for 2009, so we document our approach here. Our estimates are based on recent trends in national emission totals along with numbers from LeQuéré et al. (2009), where global emissions for 2009 are stated to have decreased to near 2007 values. LeQuéré et al. (2009) show that the sum of emissions from Annex B (developed) countries was mainly constant over 1990-2008 while emissions from non-Annex B (developing and least developed) countries were responsible for most of the recent growth. Since the US and Australia showed declines in preliminary 2008 values relative to 2007, whereas Europe as a whole did not, it was presumed that these trends would continue into 2009. China's emissions were then used to balance the global numbers. The 2009 estimates are admittedly somewhat ad hoc and may prove to be poor when fossil fuel values for 2009 are reported later, but they are of little consequence to the paper since the simulated CO_2 from 2009 is not used for any figures or calculations in the paper. We chose to document this in the paper as a reference for GEOS-Chem users who run CO_2 simulations for 2009 before a more accurate 2009 inventory is available.

5. *Page 900, lines 1-9: this section needs clarification as it is unclear if the results discussed here are in agreement or not with other studies.*

This climatology was derived from the other studies, therefore it is in agreement by

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design. As we noted in the manuscript, the TransCom climatology is from Baker et al. (2006) and the GFEDv2 climatology is from van def Werf et al. (2006). We have added references to Baker et al. and van der Werf et al. again on this page.

6. *Page 900, line 25: add minus sign for consistency on negative numbers as sink. In addition, it is not clear to me if the ocean sink should be represented as a sink or sink rate? Since the models are changing the CO_2 distribution, one would think that a rate would be more appropriate. But maybe the uncertainty is so large that this is not important.*

We had stated an air-to-sea flux (which is common in ocean research) making the number positive, whereas throughout most of the paper, we described fluxes to the atmosphere, which have an opposite sign. This was a simple way to report this single number rather than putting a negative sign on it with the direction of flux reversed. While our original approach was correct, we have made the requested change anyway and also changed the text to state "ocean-atmosphere flux", where required. The distinction that the reviewer makes between sink and sink rate is not clear. We state the flux as Pg C/yr which is an annually-averaged rate equivalent to the total sink in Pg C for a given year.

7. *Page 907, line 25: Could CH_4 be playing a role in this distribution?*

CH_4 does indeed play a role in CO_2 chemical production rates. The band of dispersed CO_2 production bounded by about 60S-60N is related to the CH_4 distribution as well as OH levels. However, the reason for the low CO_2 production over the Amazon where methane, isoprene and monoterpene levels are relatively high is unknown.

8. *Page 909, lines 1-8: without the imbalances, would the change in CO_2 column be exactly 0?*

If the CO_2 was balanced, then the globally-averaged change in CO_2 should be essentially zero, but a very small difference would probably result in the tropospheric num-

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bers since CO_2 produced in the troposphere would be transported to the stratosphere sooner than CO_2 emitted at the surface.

9. *Section 2.7.3: It is unclear why this is not actually taken into account with the emission correction. Changing the CO distribution only means that the surface emission correction is different?*

It is unclear as to what exactly the reviewer is inquiring about with regard to "why this is not actually taken into account with the emission correction." Interpreting the comment as "Why was CO_2 production not balanced with the surface correction?", our response is that certain inventories do not require a surface correction (for example GFED) since CO, CH_4 and other carbon species are not counted as CO_2, however these species still oxidize contributing to CO_2 production. If atmospheric CO levels are incorrect, then CO loss rates will also be incorrect, and these loss rates are used to determine CO production rates. Interpreting the comment as "Why was assimilated CO (the topic discussed in section 2.7.3) not used for the entire CO loss and CO_2 production scheme?", our response is that TES CO observations are not available for all years for which we have developed the CO_2 chemical source (2000-2009). TES CO spans late 2004 to present, but data up to December 2005 are of lower quality due to problems of weak signals (as noted in the manuscript). AIRS CO goes back about a year earlier and before that MOPITT CO data were available. Applying such data sets in data assimilation is not trivial and may be done in the future, but that is beyond the scope of the present work. We simply took advantage of the availability of the TES assimilation from the Parrington et al. (2008) study, rather than invest in setting up an assimilation system for the other data products.

10. *Section 3.1: a scatterplot (Figure 10) might help visualize the results better.*

We appreciate this suggestion, but the removal of the CO_2 distributions from Figure 1 (requested by reviewer 2), makes our original Figure 10 the only figure showing the horizontal distribution of CO_2 from our model, hence we think it is important to keep

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the figure as is. We agree that a scatter plot would be of some value but would require an additional figure beyond the original 16 that we have, and one additional figure requested by the second reviewer that we have now added.

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