Point-by-point responses to the comments from the Editor and from the referees for acp-20130782

Editor' comments

Estimating Asian terrestrial carbon fluxes from CONTRAIL aircraft and surface CO2 observations for the period 2006 to 2010

Revised Submission

Editor Decision: Reconsider after minor revisions (Editor review) (26 Mar 2014) by Ralph Keeling

Comments to the Author:

Please revise according to the new comments of the reviewers, which I agree with.

In re-reading the draft, I also found these additional issues (all somewhat minor):

I agree with Referee #2 that some additional discussion is needed of role played by CONTRAIL data in yielding changed estimates of the Asian sink. What feature in the CONTRAIL data is responsible for this change?

Response:

Thank you for your suggestion. We agree that the impact of CONTRAIL on CTDAS simulation is very important. So, the impact of CONTRAIL on Asian CO_2 flux was discussed in Section 4.1 "Impact of CONTRAIL". Also, the influence of CONTRAIL on CO_2 concentration simulation has been added in our revised paper (*see lines 15-18, page 12*). In addition, we evaluated the performance of CTDAS for simulation of the vertical layers by comparing with the CONTRAIL data in the revision (*see lines 9-14, page 13*).

Page 13, line 12. "ppm" is missing on the first number.

Response: Done (*see page 12 line 13*).

Figure 2 caption: "These observation data download from the NOAA-ESRL and WDCGG network in Figure 2a ". The meaning of "these observation data" is unclear, and note the recursive reference to Figure 2a.

Response: Done (*see Figure 1 in page 37*).

Figure 3: The meaning of the solid blue points in the airborne plots is unclear. They need to be identified in a legend.

Response:

Thank you for your suggestion. We have updated Figure 2 in the revision (*see Figure 2 in page 39*).

Figure 5 and related text: Please explain how it is possible to use atmospheric data, which provides only large-scale information, to resolve land carbon fluxes by ecosystem type. Referee # 1 made a similar point.

Response:

Thank you for your comment. As described in lines 26-30, page 7 that **"...** the global domain that is divided into 11 land and 30 ocean regions according to climate zone and continent. Nineteen ecosystem types (Olson et al., 1985) have been considered in each of the 11 global land areas (Gurney et al., 2002), dividing the Globe into 239 regions (239 = 11 land × 19 ecosystem types + 30 ocean regions)**..."**, the land carbon fluxes by ecosystem type are retrieved from the inferred 1× 1^{o} terrestrial CO_2 fluxes which are considered nineteen ecosystem types in each of 11 global land areas based on Olson et al., (1985). We also realize that possible large uncertainty in land carbon fluxes categorized by ecosystem types, so we discussed the uncertainties of these inferred forest/cropland/grass CO_2 flux in *line 1 of page 21* to *line 6 of page 22* (see also response to Comment #1 of Referee #1).

I have heard that CarbonTracker cannot be used in its normal configuration to assimilate airborne data because the several week window used for the optimization is too short compared to vertical transport time scales. If I'm wrong about this, you can ignore, but otherwise please explain how this obstacle was overcome in this study, thereby allowing the use of the CONTRAIL data.

Response:

It is indeed true that in the original CarbonTracker, airborne data was not assimilated partly because of concerns that transport within the five-week window would be too inaccurate, and partly because we feared that the flux signals would be at the edge of our five week window. This was based on a 2-3 week mixing time for air masses around the northern hemisphere, and a similar mixing time to the upper regions of the troposphere. Airborne data were thus kept as independent checks for several consecutive years. The low mismatch to these data, as well as several tests with airborne data in different configurations of lag, gave us more confidence that we could actually assimilate it. In the 2013 version of CarbonTracker Europe (published in Peylin et al., 2013), several aircraft sites are now actually assimilated, while in CT North America this is under consideration. For CONTRAIL, we have similarly first investigated the performance from the non-assimilation run to ensure we have acceptably small observations-model differences (0.18 +1.83 ppm), also compared to for instance the aircraft profiles at Ulaan Baatar, Mongolia (ULB), and are now more confident in our ability to assimilate them.

The document needs editing by a facile English speaker (several coauthors are suitable) to fix some obvious language issues.

Response:

Thank you for your suggestion. This manuscript was carefully polished by our co-authors following this suggestion.

Figure resolution was poor on the submitted drafts. Higher resolution images are needed for final publication.

Response:

Thank you for your suggestion. We will provide high resolution images and figures for publication.

Responses to Referee #1

The article has improved significantly from the ACPD version. However, some corrections are needed before acceptance for publication in ACP.

Comment #1:

Table 4. Not sure whether the fluxes should be presented in so much detail. For example, how well is your inversion constraining the fluxes from Conifer forest. There is no site in Siberia. I do not think the transport model is as accurate as that is needed for separating fluxes for each ecosystem types from CONTRAIL data at 10 km.

Recommend removing this Table altogether as the results are also shown in Fig. 5

Response:

Thank you very much for your suggestion. We agree that there is a possible large uncertainty in the separated fluxes for individual ecosystem types for the reasons mentioned by the reviewer, and in an earlier version we therefore aggregated only to larger areas and gave numbers for Boreal/Temperate/Tropical Asia. However, we got a response from an earlier reviewer that our results as presented were incomplete, and made it hard to assess the actual estimates. Hence the addition of this Table, in which we decided to not make any aggregation beforehand, or leave out systems which we deem less well constrained. It might otherwise make the impression that we are purposely not giving the complete picture (as the other reviewer even seemed to imply). Where possible, we compared these inferred fluxes of forest/cropland/grass with other "bottom-up" results (*see line 1 of page 21 to line 6 of page 22*). Hopefully these inverted terrestrial CO_2 fluxes for individual ecosystem types are useful to some readers, despite the noted lack of constraints on some of them.

Comment #2:

Table 6 is revised, but two problem remains:

1. Why the Niwa et al. numbers are gone?
2. Results from two CTE2013 and CT2011_oi are not independent. They should choose only one, perhaps CT2013.

Response: Thanks for these suggestions and which were followed. To make Table 6 more clearly, we removed some data. The Niwa et al. numbers were added in and the results of CT2011_oi were removed (see Table 6 in page 35). The reason we removed Niwa et al.

numbers in the previous version because these data were obtained by personal communication. Now we have added these numbers to Table 6 in the revision (see *Table 6 in page 35*). We agree that the results from CTE2013 and CT2011_oi are not independent, but there are interesting differences between the two that make quoting both useful. These are the differences in zoomed transport, differences in state vector configuration, and difference in prior biosphere models used. To make this remark by the reviewer more obvious to less informed readers, we have added a footnote to the table stating that the CTE2013 and CT2011_oi estimates oars not independent, and share the TM5 transport model and ObsPack observations sets.

Comment # 3:

Table 7: Rearrange the columns (rows for CTE2013 and CT2011 will be gone):

Year Boreal | Temperate | Tropical

This work CTE13 | This work CTE13 | This work CTE13

Or something like that. You do not need CT2011_oi as that is not independent of CTE2013. The IAV for whole Asia does not make any sense as the causes for IAV in each of the three regions are so different. There are seminal papers published on the regional land flux IAVs and their drivers (you can find references in Gurney et al., 2008).

Response:

Thanks for this comment. We have revised Table 7 in the revision. CT2011_oi was removed from this Table following your suggestion (*see Table 7 in page 36 and associated text in lines 13-22 of page 20*). We agree also with the comment that the difference in drivers makes an assessment of whole-Asia flux IAV somewhat pointless, and have removed the first column from the table.

Comment # 4:

Figure 1: Can easily be combined with Fig. 2 or just mention the Lat,Lon for 4 corners of the high resolution domain in Fig. 2 caption (already in text?).

Response: Thanks, This suggestion was followed(*see Figure 1b in page 37*).

Comment #5:

Supporting Information should be moved to Supplementary file. Remove from the main article file.

Response: Thanks. This suggestion was followed.

Responses to Referee #2

General comments

The authors could successfully revise the manuscript according to reviewer's comments. The paper is significantly improved after their revisions and supporting information. I consider this paper is suitable for ACP publication after revising some minor comments describing below.

Specific comments

Comment #1:

P7, line 22:

The authors should clarify a type of meteorological data (Re-analysis (ERA Interim?) or operational analysis?).

Response:

Thanks. This suggestion was followed. We clarified the meteorological data we used in the revision (*see lines 10-11, page 7*).

Comment #2:

P.11, line 27:

The authors should show brief difference between both fossil fuel emissions (CDIAC+EDGAR and Wang). This information could help us for interpreting the results.

Response:

Thanks for this comment. We agree with the reviewer that a brief description of the differences between the two fossil fuel emission datasets (CDIAC+EDGAR and Wang) can help readers to understand the results. We added in one paragraph in the revised version (*see lines 31 page 10 to line 4 page 11*).

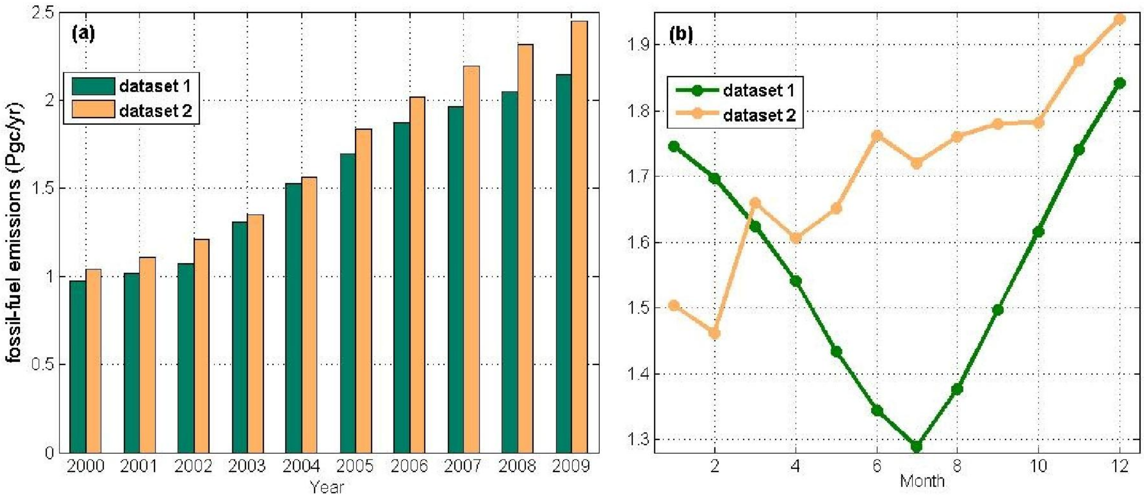

Figure R1. Comparison between Chinese fossil-fuel CO2 emission datasets from “CDIAC+EDGAR” (dataset 1) and Wang, et al.(2012) (dataset 2): (a) Inter-annual variations; (b) Seasonal variation. Both datasets show an increased trend over the time. The dataset 2 has stronger release than dataset 1, with the largest difference in 2009. The seasonal variations are very different between two datasets that the dataset 1 has the largest carbon emission in December and the smallest carbon source in July every year, while dataset 2 has the largest carbon emission in January and the smallest carbon source in February or March during 2000-2009.

In Figure R1, there are two sets of Chinese fossil-fuel CO_2 emissions presented. One set of fossil-fuel emission inventory called " CDIAC+EDGAR " (dataset 1) is obtained from independent global total fossil fuel emission of the Carbon Dioxide Information and Analysis Center (CDIAC) (Marland, et al., 2003) from global average total fossil fuel combustion (http://cdiac.ornl.gov/trends/emis/meth_reg.html) and spatially and temporally-resolved inventories based on the EDGAR (Emission Database for Global Atmospheric Research) database (Boden, et al., 2011, Thoning, et al., 1989, Commission, 2009, Olivier, et al., 2001). Another set of fossil-fuel data is taken from Wang, et al. (2012) (dataset 2), which calculate carbon emissions from energy consumption, transportation, household energy consumption, commercial energy consumption, industrial processes and waste. The seasonal variations between two datasets are very different. The fossil-fuel CO2 emissions of "Miller" (dataset 1) have increasing trend over the period of 2000-2009, with the largest carbon emission in January and the smallest carbon source in July every year. This is seasonal amplitude is consistent with the change of United states and Europe, which indicated fossil emissions have greater emissions during winter months and less during summer months(Gurney, et al., 2005). Similar to the "Miller" (dataset 1), the emissions of "Wang, et al." (dataset 2) increase with years, but the seasonal variations are different. Due to spring festival, many factories and companies consumed less energy in February or March of China (Gregg, et al., 2008), which induced the smallest fossil-fuel CO_2 emissions of "Wang, et al." (dataset 2). Here we use these different sources to test the sensitivity of annual, seasonal and inter-annual terrestrial CO_2 fluxes to variations of fossil-fuel CO_2 emissions. We want to quantify the impact of amplitude varying fossil-fuel emissions on terrestrial sink as well as temporal-spatial varying over time. The brief difference between two datasets of fossil fuel emissions have been added in our revision (*see lines 31 page 10 to line 4 page 11*).

Comment #3:

P13, line 11- :

In this manuscript, the authors compared simulated concentration with only one surface site (WLG) in free troposphere. Their key results (Asian CO2 flux are much decreased when they adopt CONTRAIL observation data in their data assimilation system) maybe come from mismatch between simulated concentration and CONTRAL observation data, so they should discuss about this matter in detail. Adding some discussions about bias and RMSE (root mean square error) for each vertical layer is helpful. The bias and RMSE looks larger in high altitude and summer/winter season in Fig. 3b -3d.

Response:

Thanks for this comment. We agree that the one site's comparison may not clearly describe the performance of the CTDAS, so we listed all Asian observations' model-observation mismatch in Tables 1 and 2 (see column of " Bias (modeled) ") , which comprehensively check the accuracy of the model simulation. Also, the impact of CONTRAIL on the CO_2 concentration simulations was included in the revision (*see lines 15-18, page 12*). In addition, we have added discussion about RMSE and correlations of model-observation mismatch for vertical layers (*see lines 9-14, page 13*).

Comment #4:

P18, line 16-:

In page 12, Case 6 is based upon Case 2. The difference of the three-year annual Asia CO2 flux between Case 2 and Case 6 is small (0.08PgC/yr). The authors should show the meaning of “largest sensitivity” in detail.

Response:

Thanks for this comment. That's a mistake. We have modified this paragraph in the revision (see lines 7-14, page 17).

Comment #5:

Page 48, Figure 7:

The authors should show target area (Asia) in the caption.

Response:

Thanks for this comment. We have updated the caption with target area (Asia) information (*see Figure 6 in page 43*).