Interactive comment on “Regional CO_2 flux estimates for 2009–2010 based on GOSAT and ground-based CO_2 observations” by S. Maksyutov et al.

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Reply to Comments by R. Nassar

Authors are grateful to Ray Nassar for his review comments and suggestions. As a result we made several changes and corrections to the manuscript intended to clarify our presentation.

Replies to the review comments follow:

1. *Perhaps the most important point that needs clarification is the approach used for calculating the model values in the 5_x5_ cells to determine the observation-model differences (section 3). GOSAT observations were averaged in monthly 5_x5_ cells, but when the equivalent averages were calculated for the model, did the authors use the model values at only the observation times in the monthly average or the entire month? Clearly using the model values at the observation times is preferred. If this was their approach, at what temporal resolution was the model output archived? (Half of this value would indicate the largest time mismatch.) Transport of CO2 plumes, atmospheric variability over a month (especially during months that transition between seasons), and the diurnal cycle are all factors that could bias the results if the model was not sampled at the observation times. Much like averaging the non-uniform spatial distribution of the GOSAT data or GLOBALVIEW product would give a biased a global mean, averaging the temporally non-uniform GOSAT observations for comparison with a true model monthly mean for a given location will also give a bias, although this approximation may be reasonable with GLOBALVEW since it is provided on a regular time interval (1/48th year).*

Reply:

The horizontal and vertical resolutions of our atmospheric transport model output (NIES-TM) are 2.5 degree, and 32 levels, respectively. Every model time step (10-15 min) we performed linear interpolation (in space and time) to obtain model-predicted concentrations for each of GOSAT retrievals and GLOVALVIEW (GV) values falling in current time step. After that, we calculated the monthly-means of GV and 5°×5° grid-box GOSAT retrievals, which were then used as input to the inversion. The GOSAT retrievals and GV values themselves (the "observed" concentrations (we acknowledge that GVs are not actual "observations" but curve-fitted values)) were also aggregated into monthly-average values prior to the inversion. As described in Chapter 3 of the manuscript, the forward concentration simulation with NIES-TM was based on daily NEE (VISIT terrestrial biospheric model), monthly fire emissions (GFED), monthly anthropogenic emission (ODIAC/CDIAC), and monthly ocean-surface exchange (OTTM).

P 29238, line 10. Crisp et al. (2012, AMT) should be added to the retrieval citations.

Reply:
Thanks for suggestion. Paper by *Crisp et al. (2012, AMT)* is a second part of a two-paper series, where we chose to cite *O'Dell et al. (2012, AMT)* (part I) as it provides an algorithm description.

P 29238, line 15. The discussion of filling gaps in the network, suggests that the surface in situ observations and the satellite data simply mesh together to increase horizontal coverage, but this is not really the case since the vertically-averaged XCO2 measurements really contain a different type of information about surface fluxes. Perhaps the wording here can be changed.

Reply:
The suggestion to change wording makes good point, as we can not fill the gaps in ground based network in conditional sense, but do fill the gaps in the observational coverage with a different type of the observations. The wording is changed in the paper accordingly

P29238, line 28. The authors refer to GLOBALVIEW-CO2 "ground-based observations", which is not an accurate description for two important reasons. The GLOBALVIEW-CO2 product is derived from mostly ground-based measurements, but also some ship-based and aircraft profile measurements, which I assume were also used. More importantly, one should not call the GLOBALVEIW-CO2 product "observations" since the actual measurements have been filtered, smoothed and interpolated (referred to as "data extension and data integration" by the data providers, see Masarie and Tans, 1995), much like a Level 3 satellite data product should not be called an observation. My recommendation would be that the authors improve their description of the GLOBALVIEW product to be consistent with the description from the GLOBLVIEW website, that they cite Masarie and Tans (1995) and that in future versions of the GOSAT L4 product, they strongly consider using real flask and continuous in situ CO2 observations in place of GLOBALVIEW-CO2.

Reply:

Following the reviewers suggestions we replaced the term “GV observations” with “GV data”, added explanation of term “ground-based” observations and added reference to the smoothing procedure. In the future versions the transition to suggested treatment of the observations as separate events is natural following the provision of the actual observations in Obspack (http://www.esrl.noaa.gov/gmd/ccgg/obspack/). We also added few words on rationale for using Globalview, as most geographically and vertically extensive data set, allowing for more realistic estimate of the relative value of the GOSAT observations in terms of the information content.

P29240, line 4. Was the VISIT output actually used at a “daily time step” as stated or was effort made to account for the diurnal cycle? Olsen and Randerson (2004, JGR) simulate the amplitude of the diurnal cycle due to terrestrial vegetation indicating that it is about 1.0 ppm for XCO2. Keppel-Aleks et al. (2012, Biogeosciences) using TCCON measurements at Park Falls show that the XCO2 diurnal cycle amplitude is about 2.0 ppm, thus the diurnal cycle is not negligible and the GOSAT XCO2 observations used will be within minutes of the equator crossing time (12:49) for its sun-synchronous orbit. Although the fact that the early afternoon is close to the diurnal mean value (Olsen and Randerson, 2004), would make neglecting the diurnal cycle for the VISIT simulation or the sampling the transport model output less significant than if dealing with an early morning or late afternoon observation.

P29240, line 28. Similar to above, the authors describe their meteorological data as “daily mean” implying that they have neglected the diurnal cycle of terrestrial vegetation flux.

Reply: To both comments above

Use of daily time step in VISIT is done for computation convenience only, otherwise this version of VISIT (*Ito, 2010*) was developed for analysis of the flux tower data and internally uses hourly time step for photosynthesis rate simulation. As justification for this simplification we use results of studies by *Olsen and Randerson, 2004* who found the early afternoon simulated values with daily fluxes are close to those produced with sub-daily fluxes. Another study by *Patra et al 2008* found little improvement with diurnally varying fluxes over monthly and daily fluxes for simulation of the synoptic scale variability at most continuous observations sites. In future versions diurnal cycle is planned to be resolved in order to reduce the mentioned biases introduced by diurnal dynamics.

P29241. It would be helpful to clarify if VISIT was run in a balanced mode or if there was net uptake and if so provide the global net uptake in PgC for the period or annual values for 2009 and 2010.

Reply:

Although effort was made to balance the CO_2 uptake and release with a long spin up, the mean flux of -0.7 Gt/year is estimated for the period of study.

P2942. It is not clear why the authors use Takahashi et al. (2007) pCO2 data rather than the Takahashi et al. (2009) data. I am not sure if the pCO2 data in these datasets are the actually the same (with 2009 data set also deriving fluxes), but the 2009 data set has had at least 3 corrections applied (2009-06, 2009-10 and 2010-12). Although one of the corrections is only for the ocean CO2 flux, two relate to pCO2 interpolation, thus if not included in the 2007 dataset, would add a (very minor) source of error.

Reply:

Both references are related to the same dataset. Fluxes and pCO_2 maps constructed by *Takahashi et al (2009)* are based on pCO_2 observational dataset known as LDEO dataset, now cited as (*Takahashi et al. 2011*) as we use LDEO/NDP-088 v. 2010 in the flux update.

P2942. It would be helpful for the authors to state the resolution of the ocean tracer transport model work. Figure 2 looks like the model is run at a very high spatial resolution, but it is not clear if the figure just has some interpolation applied.

Reply:

The OTTM tracer model was run at 1x1 degree resolution with 40 vertical levels, with first 26 levels in upper 300 meters of the ocean. In the given figure the fluxes are shown at 1x1 degree resolution, and therefore no interpolation has been applied. The text has been updated.

P29244, lines 4 and 6. "Proscribe" should probably be "prescribe". Since proscribe means to forbid, prohibit, denounce or banish.

Reply:

The mistype was corrected

P29245-2945. The fossil fuel emission dataset described here is one of the most sophisticated for use in global CO2 inversions in the scientific literature. However, there are a few points that should be clarified. The version of EDGAR should be stated. Older versions of EDGAR have inadequate ship emission distributions, but versions 4.x are much better. A proper distribution of aviation emissions would include both horizontal and vertical distribution. Are the data provided by Aero2K horizontally and vertically distributed? Did the authors also distribute vertically or just horizontally? Perhaps this is explained in Oda et al. (in preparation), but should be clarified here. If a vertical distribution was used, how many levels did it involve? Lastly, it would be helpful to state the global fossil fuel (and cement) emissions for the period in PgC (including both the national and bunker fuels) or alternatively, the annual values for 2009 and 2010.

Reply:

Although *Aero2* data are prepared at separate levels in the ODIAC, they are introduced at surface layer due to lack of the proper options in the transport model.

P29245-29246. The level of detail provided in the GFED description here is not required since the data product was essentially used "off the shelf" and is not the work of the authors, hence the details are given in the GFED papers, although van der werf et al. (2010) is missing from reference list. One point that should be stated clearly is if the GFED product used had the standard temporal resolution of 1-month or something else, since variations of GFED exist at higher temporal resolutions (8-day, daily, 3-hourly, etc.). This is just for clarification and is not meant to imply that higher temporal resolution is needed.

Reply:

We feel some short description of fire dataset is better to have as it is contributiong a lot to variability of the CO_2 emissions. Reference to van der Werf, 2010 appears in first paragraph of the section. Notice of temporal and spatial resolution is added to the transport model description.

P29248. Although many papers using TCCON data neglect to specify the data version, it would be beneficial to provide the version here, especially for the comparisons in Figure 10 and pages

29256-29257.

Reply:

The analyses done here are based on the 2009 release of the TCCON data (the latest 2012 version was released in October 2012, after the submission of our manuscript). We added a note in the manuscript (29257, Line 7) to indicate the data version. (The quality of this GV-based global CO_2 field was examined with the TCCON references (*data version: GGG 2009 release*).)

P29249. In Belikov et al. (2012a), problems are seen with the TCCON CH4 comparison at Sodankyla (67.37_N), hence this point is excluded from the values provided. As a result, the highest latitude point is Bialystok (53.22_N). Does this suggest that we should have less confidence in the high latitude results in the current manuscript?

Reply:

High latitude seasonal cycles are constrained with a number of the ground-based and aircraft observations, so fairly good match can be expected, however the plots of Sodankyla we not added as the number of the observations at the site was limited for the analysis period (June 2009 – May 2010). Fluxes used by Belikov et al 2012a were not inversion optimized, so some underestimation of the seasonal cycle is visible in that publication. Recently the analysis was extended to high latitudes by Belikov et al (2013) and it shows good match in seasonality.

Belikov, D.A., Bril, A., Maksyutov, S., Oshchepkov, S., Saeki, T., Takagi, H., Yoshida, Y., Aoki, S., and Yokota, T.: GOSAT retrievals and NIES transport model simulation of column-averaged CO_2 concentrations in the subarctic, Polar Research, submitted, 2013.

P29249, line 10-13. CO2 is most often treated as chemically inert, but this is a simplification or approximation, since in reality, some quantity of CO2 is produced in the atmosphere from the oxidation of CO and hydrocarbons including CH4. I would recommend replacing the current sentence with "For the case of estimating surface fluxes of a gas species such as CO2 which is approximated to be chemically inert, the relationship between the measured data values and their theoretical predictions based on physical process modeling is linear."

Reply:

Agree with suggestion. The sentence was corrected.

We agree that chemical source is substantial part of the total emissions. Current approach to modeling the CO2 fluxes is in fact close to trying to emit total carbon both antropogenic and natural (biosphere and fires), and approximate it with CO2 alone as a proxy to total carbon, which leads to small biases in CO2 gradients (order of 0.2 on N-S gradient if we sum up CH4 and CO gradients). This of course should be improved in the future, the only obstacle presently is significant delay with production of the Globalview datasets for CH_4 and CO.

P29250, line 25. 3 ppm for monthly 5_x5_ averages is a conservative estimate, as stated by the authors. It would be helpful if they provided some justification for this choice.

Reply:

A very similar comment was made by the other reviewer (A. Jacobson). The following is our reply to that comment and shows the basis of our choice:

The validation of the ver.02.00 GOSAT X_{CO2} retrievals have revealed that the mean and the standard deviation (SD) of differences from TCCON references (data collected at 11 TCCON sites worldwide) are -1.44 ppm and 2.10 ppm, respectively. This global-mean SD of the TCCON-GOSAT differences (1.97 ppm) is on the same order as the global, annual average of the 5°×5° grid-box SDs (1.59 ppm). The monthly distributions of the 5°×5° grid-box SDs, shown in Fig. A, indicate the maximum level of stochastic variability in the current GOSAT retrievals, which are approximately 5 ppm (seen over northern parts of North America and Eurasia during summer months and over eastern part of China during winter). Our assumption here was that it would be conservative to regard that the precision of the current version of the GOSAT retrievals would not be any better than the level of the TCCON validation SD. Assigning this SD to each GOSAT retrievals as data error was an option, but at the same time it was also necessary to account for those large stochastic variabilities of which coarse-resolution forward concentration simulation would be difficult. Considering these aspects, we decided to use the 5°×5° grid-box SD distributions as a GOSAT retrieval error model, with the minimum data error set to 3.0 ppm that resulted from inflating the TCCON validation mean SD (1.97 ppm) to take into account the estimated forward modeling error of approximately 1 ppm (conceptually similar to the approach employed in TransCom 3 studies, as demonstrated by Gurney et al. (2003), where they use 0.25 ppm as minimum uncertainty to account for modeling errors in Southern hemisphere.).

P29251, line 10. The uncertainty on the terrestrial biospheric flux was set at twice the standard deviation of the VISIT monthly NEE at 1_x1_ for the past 30 years. It would be helpful if the authors could give an estimate of the magnitude of the uncertainties from this approach.

Reply:

The other reviewer (A. Jacobson) commented on a similar matter. In Table A we show the a priori flux uncertainties contrasted to what were used in the TransCom 3 studies (22 source regions).

Page 29258, line 15. Some elaboration on the method of Canadell et al. (2011) would be helpful for the discussion.

Reply:

We added more details to the text to make it more understandable.

Figure 2. The line in the lower panel connecting the points seems redundant.

Reply:

We prefer to keep the line to emphasise the temporal variation.

Figures 7-9. Showing panels for one month from each season gives an adequate sampling of the results, but perhaps a new figure with the annual mean/total uncertainty reduction, flux and differences could be added (even to the supplementary data) since annual mean fluxes and uncertainties would give a nice summary of the results and are still the most policy-relevant temporal scale.

Reply:

The annual-mean values of UR, posterior flux, and flux difference are all presented in Figure D.

Figure 8. The figure caption should clarify if the fluxes shown here include fossil fuels (as stated in the text). Also, the abrupt changes in the terrestrial biospheric fluxes do not appear natural in many places due to the vertical or horizontal boundaries of the 42 land regions. In future work, region boundaries based on an 'eco-regions' approach might reduce the aggregation errors that result from the region selection (however, this point is debatable).

Reply:

The caption for Figure 8 modified as follows:

Monthly fluxes (gCm^{-2} day^{-1}) estimated for the 64 subcontinental regions using GV data and GOSAT X_{CO2} retrievals, for the months of August 2009 (summer in the Northern Hemisphere), November 2009 (fall), February 2010 (winter), and May 2010 (spring). The value presented here are is the sum of a priori fluxes (terrestrial biosphere exchange or ocean exchange + anthropogenic emissions + forest fire emissions) and the correction to the a priori flux determined via the optimization. Note the different color-coded scales for land and ocean regions.

Figures 8-9: The different land/ocean color scales in Figures 8 and 9 are a nice idea.

Reply:

We presented the terrestrial and oceanic fluxes separately using two difference color scales because otherwise the changes in oceanic fluxes are not visible (oceanic fluxes are nearly one-order-of-magnitude smaller than the terrestrial ones).

Figure 10. A legend would be preferable to stating all the information (such as colors) in the caption. More importantly, the differences in this figure when GOSAT data are used or not used are very minor. Is this simply because these 5 TCCON sites do not include regions where GOSAT provides the most information to constrain fluxes?

Reply:

We modified the figure caption as follows (there were some errors):

Fig. 10. Time series of data collected at five TCCON sites (green), and corresponding forward simulation results based on a posteriori fluxes estimated from GV alone (red) and GV and GOSAT retrievals (blue). The five TCCON sites are Ny Ålesund, Norway (78.55N, 11.55E), Bialystok, Poland (53.23N, 23.03E), Park Falls, USA (45.95N, 90.27W), Tsukuba, Japan (36.05N, 140.12E), and Wollongong, Australia (34.41S, 150.88E).

The small differences between the GV-alone and the GV+GOSAT cases, as shown in these panels, are reflective of either 1) GOSAT X_{CO2} retrievals were not available nearby for constraining fluxes (thus the estimation was based on GV data), or 2) GV and GOSAT X_{CO2} retrievals were both available for flux estimation but because the data uncertainties assigned to GV values are nearly one-order-of-magnitude smaller than those assigned to GOSAT retrievals, constraint by GV values was dominant (e.g. North American and some of European regions).

Figure 11. The differences in Figure 11 are surprisingly large (sometimes 5 ppm). Are the averaging kernel and prior used in these comparisons?

Reply:

We note here in Figure 11 that larger differences (dark-colored grids) are found mostly in regions where GV sampling is poor (lower South America, equatorial Africa, and central Asia, as mentioned in the text) and therefore the GV-only fluxes estimated for those regions are associated with relatively larger uncertainties than the others. (The X_{CO2} adjustments with averaging kernels and a priori concentration profiles used in the retrieval of TCCON values are not applied in this analysis.)