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Atmospheric Chemistry and Physics Discussions

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.

Dr. Nassar (Referee)

ray.nassar@ec.gc.ca

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This manuscript presents the global CO_2 flux inversion method and one year of results using GOSAT XCO_2 and CO_2 derived from in situ observations. These new flux inversion results will be publicly disseminated as the GOSAT Level 4 CO_2 data product. The manuscript builds upon Takagi et al. (2011), a short paper that used an earlier version of the XCO_2 retrievals to derive preliminary Level 4 results. The current manuscript is a major improvement since it now presents more detail on their method and uses the much-improved version 02.00 XCO_2 retrieval product, which enables a more meaningful discussion of the results. It is well-known that many other groups

are working toward a similar goal of producing CO_2 flux estimates from GOSAT data (with or without additional in situ based data). Due to the fact that inversion results with GOSAT CO_2 data from different groups will undoubtedly differ and such intercomparison projects are now underway to understand these differences, the authors have done a very good job of interpreting the results with an emphasis on the information content of the observations, rather than the fluxes themselves, and make some modest but useful conclusions about the benefits of using the GOSAT data for CO_2 flux inversions. Overall, I think this paper makes a positive contribution to the field and represents a step forward on a topic relevant to ACP readers. In general, I would consider the model framework employed to be among the more sophisticated ones in use; however, I have a few suggestions on how the description of the method can be clarified in the current manuscript and how the method can be improved for future versions of the Level 4 data product.

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 CO_2 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).

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Specific points

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

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 XCO_2 measurements really contain a different type of information about surface fluxes. Perhaps the wording here can be changed.

P29238, line 28. The authors refer to GLOBALVIEW-CO_2 "ground-based observations", which is not an accurate description for two important reasons. The GLOBALVIEW-CO_2 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-CO_2 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 CO_2 observations in place of GLOBALVIEW-CO_2.

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 XCO_2. Keppel-Aleks et al. (2012, Biogeosciences) using TCCON measurements at Park Falls show that the XCO_2 diurnal cycle amplitude is about 2.0 ppm, thus the diurnal cycle is not negligible and the GOSAT XCO_2 observations used will be within minutes of the equator crossing time (12:49) for its sun-synchronous orbit.

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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.

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.

P2942. It is not clear why the authors use Takahashi et al. (2007) pCO_2 data rather than the Takahashi et al. (2009) data. I am not sure if the pCO_2 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 CO_2 flux, two relate to pCO_2 interpolation, thus if not included in the 2007 dataset, would add a (very minor) source of error.

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.

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

P29245-2945. The fossil fuel emission dataset described here is one of the most sophisticated for use in global CO_2 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

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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.

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.

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.

P29249. In Belikov et al. (2012a), problems are seen with the TCCON CH_4 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?

P29249, line 10-13. CO_2 is most often treated as chemically inert, but this is a simplification or approximation, since in reality, some quantity of CO_2 is produced in the atmosphere from the oxidation of CO and hydrocarbons including CH_4. I would recommend replacing the current sentence with "For the case of estimating surface fluxes of a gas species such as CO_2 which is approximated to be chemically inert, the relationship between the measured data values and their theoretical predictions based on

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physical process modeling is linear."

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.

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.

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

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

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.

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).

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

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

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include regions where GOSAT provides the most information to constrain fluxes?

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

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