Atmos. Chem. Phys. Discuss., 13, C9741–C9745, 2013
www.atmos-chem-phys-discuss.net/13/C9741/2013/

Atmospheric Chemistry and Physics Discussions

ACPD

13, C9741–C9745, 2013

Interactive Comment

Interactive comment on "Inferring regional sources and sinks of atmospheric CO_2 from GOSAT XCO_2 data" *by* F. Deng et al.

Anonymous Referee #2

Received and published: 9 December 2013

This study investigates the use of GOSAT retrieved total column CO2 for estimating global CO2 fluxes using inverse modeling. Such studies are much needed to support the discussion of what can be expected from space borne C-monitoring and what is needed in the context of future missions. The results confirm the sensitivity of inversion-estimated fluxes to systematic uncertainties in the measurements, and the spatio-temporal variability of measurement coverage. Inconsistencies remaining connecting surface data to total column data, which are corrected without investigating the underlying causes. To avoid confusion on this topic some further effort is needed as will be explained below. Overall, the study is carried out well and results are documented in a transparent manner. In my opinion a few important issues remain in connecting results to conclusions. If these are addressed in a satisfactory manner I see no reason

Full Screen / Esc

Printer-friendly Version

Interactive Discussion

Discussion Paper

to uphold publication in ACP.

GENERAL COMMENTS

Bias correction and comparisons to TCCON

Given the importance systematic measurement errors, it is important to accurately define and evaluate the correction methods. Some further attention is needed here. From the description on page 26334 it is not clear whether there was no bias correction applied to the b2.10 data or that they were filter and corrected as in XCO2_B. The next obvious thing to do is evaluate the agreement of the GOSAT optimized model against TCCON (Table 3) to find out whether the corrections did what they were supposed to do. Comparing RUN_A and RUN_B in table 3, it is not obvious at all that the 4 parameter bias correction improved the fit to TCCON. It is true that this comparison is indirect, since the correction is on the GOSAT data – which are not compared to TCCON here. The question remains whether the bias in correction of the data that went into RUN_B did improve the comparison to TCCON (which I suppose should be the case), and if so why no improvement is seen in the comparison between RUN_B and TCCON. The next question is how the differences in table 3 and 2 relate to the adjustments to the initial conditions that were applied. I assume that Table 2 is for the uncorrected initial conditions (this info is missing in the caption). It is surprising to see that RUN_C leads to a satisfactory small offset to TCCON as well as the surface data, without any correction to the initial field. In the case of RUN_A and RUN_B there is this $\sim$1 ppm adjustment needed to match surface to total column. Some discussion is needed as to why this is. The 1 ppm correction suggests some problem connecting in situ data to total column data, which may be due to the model or TCCON (provided that the GOSAT data are bias corrected to be close to TCCON). The results for RUN_C, however, rather suggest that there may be no inconsistency problem at all.

NH Extratropical sink and the seasonal cycle

It is concluded that GOSAT data increase the C-sink in the NH extratropics. This is

ACPD

13, C9741–C9745, 2013

Interactive Comment

explained by the larger seasonal cycle amplitude in the GOSAT data compared with the prior, as confirmed in the comparisons to TCCON. In my opinion there is no logical connection between the seasonal cycle amplitude and the annual mean flux. There is some loose argumentation (which could be made explicit) that the GOSAT data provide a stronger constraint in the growing season, and so the seasonal cycle amplitude is corrected more strongly in the summer than in the winter, which would impact the annual mean flux. However, all this depends on what happens in winter. Is it true to that fluxes are adjusted less in winter, because of a weaker observational constraint? Does it affect the comparison to TCCON during winter? In my opinion this conclusion is made too quickly and needs further support.

Measurement footprint analysis

The difference between the inferred fluxes over the European and American continents are explained by differences in data coverage and footprints. Although some results are presented supporting the ideas of how these factors influence the inversion derived fluxes, it is not clear how important they really are. E.g there is no prove of the impact of the Eurasian data on the flux uncertainty reduction over America. It might be that the measurements over the American continent are far more important. Without stronger evidence of these relationships the wording in the conclusion section regarding the important of long-range transport and the needed for measurement coverage over the ocean should be more careful. One might think of additional experiments to provide such prove, such as truncating response functions (shortening the optimization time window) or data thinning. This may be out of scope of the paper, but right now the conclusions are not sufficiently supported by the evidence that is provided.

SPECIFIC COMMENTS

Page 26335, line 5: What is the logic of quantifying the representation error by comparing the fit of the a priori model to the data. This difference is accounted for in part by the a priori flux uncertainty, which seems to be counted twice in this approach.

Page 26337: at what altitude are aircraft emissions introduced in the model?

Page 26340: The uncertainties are scaled to satisfy the chi2=1 criterion. It is not clear how this is done for the observation and regularization terms separately. There is only one chi2, which seems to be used to infer 2 scaling factors. Or does 'reduced chi2' mean that the observation and regularization terms should contribute equally to the overall chi2. I don't see a reason why this should be the case.

Page 26340: Some scaling factors of a priori uncertainties are postulated without motivation. What evidence is supporting these corrections.

Page 26343: It is suggested that 2 inversions do not satisfy the global budget constraint as derived from the global growth rate. Does it mean that these inversions don't reproduce the observed CO2 increase? Or that something is going wrong internally?

Page 26346: The effect of neglecting spatial correlations is not so easy to assess, since it will depend on the scale at which fluxes are evaluated. Local fluxes may become better constrained by adding positive correlation to regions that are better constrained by the measurements.

Page 26347, line 24: "CO2 concentrations" io "CO2 fluxes"

Page 26348, line 19: Is sounds kind of strange to choose poor priors intentionally. Then you end up intentionally degrading the quality of your solution also, which doesn't make sense.

Table 2: The caption should mention how the initial condition is treated to arrive at this comparison.

Figure 3: It would be useful to be able to compare the differences with a posteriori uncertainty ranges.

Figure 12: Do I understand correctly that the black boxes are also the areas from which CO2 is emitted to obtain the results of Figure 13 and 14 (if so this should be

mentioned explicitly). In that case I would think it would be fairer to choose regions of approximately the same size. Otherwise the 1 PgC pulse doesn't lead to the same emission per m2. Figure 13 and 14 are difficult to integrate spatially by eye. As a result, a more intense pulse may show up more prominently than a more diffuse one.

Figure 13 and 14: The color scales should be the same to allow proper inter-comparison.

Figure 14: The middle panel looks strange to me. It suggests that the pulse takes a month to travel from Europe to Siberia. I suppose this has to do with the transition across the month. Emissions in the final day of April, will contribute to the response on the first of May. If you want to assess the impact on a monthly time scale it would be better to release instantaneous pulses and evaluate after a month. What you get now represents a range of time scales from 1 day to a month.

Figure 15: If you do instantaneous pulses, then you need to average many of them. Otherwise the results may well be influenced by specific meteorological conditions, whereas the results are interpreted as a more general finding. Else, please improve the quality of this figure (thin lines, poor resolution).

Interactive comment on Atmos. Chem. Phys. Discuss., 13, 26327, 2013.