Atmos. Chem. Phys. Discuss.,
https://doi.org/10.5194/acp-2017-960-RC3, 2017

Atmospheric Chemistry and Physics Discussions

Interactive comment on "A Global Synthesis Inversion Analysis of Recent Variability in CO_2 Fluxes Using GOSAT and In Situ Observations" *by* James S. Wang et al.

Anonymous Referee #3

Received and published: 22 December 2017

The authors analyze the first year and a quarter of GOSAT column CO2 data (June 2009 - Sept 2010) with a Bayesian synthesis inversion approach, comparing it against a similar inversion of surface in situ CO2 measurements as well as to independent data from the JR-STATION network over Siberia, from the HIPPO transects, and from partial-column CO2 profiles over the Amazon. In the Bayesian synthesis approach, fluxes are estimated across 8-day spans for 108 pre-defined flux regions (obtained by sub-dividing the 22 TransCom3 regions) across late March 2009 through the end of September 2010; the flux patterns assumed inside of each region/span are taken to be the absolute value of the prior fluxes; transport is given by the PCTM off-line model run at 2.0x2.5 deg resolution (lat/lon) with 56 vertical layers. A key advantage of the

Bayesian inversion is that a full-rank covariance matrix is obtained for this discretization, providing accurate estimation errors and correlations for analysis purposes. A disadvantage is that flux patterns inside each 8-day span and region cannot be optimized, leading to possible representation errors.

The paper, though long, provides a clear and careful analysis of this initial period of the GOSAT data, attempting to tease apart the influence of errors in the ACOS v3.4 retrievals used from the true flux signals of interest. I believe it is a useful addition to the existing GOSAT literature and should be published here after a few points of clarification (listed below) are addressed.

The main weakness of the work here, in my view, is that the measurement span addressed is quite short and the influence of errors in the initial conditions are likely to be significant further into the flux analysis span than the 40 days at the beginning of the span that have been discarded here. In particular, the June-August 2009 period used to analyze the impact of the 2010 climate drivers for the northern land regions may be feeling the effects of these spin-up errors, since the inversion span begins on March 22, 2009. It is true that the authors attempt to correct for errors in the initial condition by solving for two scalars (a multiple of the initial pattern and an offset) and this may work well, but I would have liked to have seen some sort of sensitivity study addressing the impact of the initial conditions. The comparison to the JR-STATION data suggests that this impact could be substantial. Also, the 2 PgC/year difference in the global flux total estimated by the GOSAT-only inversion in comparison with the in situ inversion clearly points to the impact of the short inversion span: it might have been better to add an additional constraint on the total (land+ocean) flux solved for in the inversion to prevent this difference, since the data themselves do not contain enough trend information to constrain the total.

Overall, though, a lot of good analysis is presented here. I have made some suggestions below for clarifying certain points in the text.

Detailed comments:

line 36: Add "Northern Hemisphere" before "high-latitude ocean"? Or do you mean that this applies in the south, as well?

72: Add "land" before "vegetation"?

96: There is no "Chevallier et al. (2014)" in the reference list. Should the data on this reference be 2013?

112: After "exact solution" add "of the linear equations relating the targeted flux variables to the measurements"? Because the fluxes have been discretized at a fairly coarse spatial and temporal resolution, the approach here does not give an exact solution for the fluxes at fine scales, but it does do so at the coarser resolution targeted here, given the assumed shape of the flux patterns corresponding to each basis function used in the inversion.

120-122: A downside of this short span is that much of it may be corrupted by spin-up errors, which may last many months after the start of the inversion span.

184-185: For clarity, replace "...from fossil and biospheric gases" with "from the oxidation of non-CO2 gases from fossil fuel and biospheric burning"?

198-200: For the NOAA in situ data, you should give the specific ObsPack file name (which includes the version number) if it came from an ObsPack file, or something equivalent if from some other source.

226: "and apply a minimum value of 0.01 ppm": it is not clear what this phrase indicates. If there is already an error of 0.3 ppm for the first of up to two possible samples, why is there a need for an additiona 0.01 ppm?

232: Please indicate before this which measurements come at this 30-second frequency – the Japanese continuous sites?

251: add a comma before "other"?

252: Add "all" before "GOSAT"?

309-310: Please give the exact equations that implement what you have described here in words. This is needed, because there are different ways to implement what you describe, and these differences can matter to the inversion.

374: Add "assumed" before "well-mixed"? For the time after 13 months, was the pattern obtained at the end of the 13 months used in the Jacobian, or was the completely-mixed value used?

377-380: It would be useful to describe this SVD procedure in more detail, since discarding singular vectors can completely remove corrections to certain regions at certain times. It might be useful to plot the projection of the singular vectors retained in the fit onto the regions so that the reader can see where the corrections to the prior fluxes are possible and where they are not. What fraction of the original singular value spectrum is truncated and what is retained? Also, usually if one can take the SVD of a matrix, not much more work is required to obtain the full solution: it is not clear how using an SVD approach helps you deal with the large matrix. Please explain this more. What aspect of your SVD approach allows you to handle the otherwise too-large Jacobian successfully?

389-390: "gives mean differences not as close to 0 as in the comparison with the assimilated data": They are actually closer to zero for the in situ inversions, but, yes, quite a bit farther from zero for the GOSAT inversions.

392: "...and have independent random errors": how does the fact that the fit to data not used in the inversions is worse allow you to say that the errors are independent?

430: "fractional": it is not clear here what you mean by this – clarify?

432-433: "accounting for error correlations": since you are just aggregating means instead of uncertainties, it is not clear why you need to worry about error correlations – why do you mention it?

434-439: Since you have the exact covariances for each region, you could aggregate these (accounting for correlations) and get a posteriori uncertainties for these larger regions. Then you could compare the observed variability to these to see whether random estimation uncertainties do indeed account for this variability or not. Doing this would be better than just speculating, as you do now.

443-447: It is important that you mention here that the observed variability could also be due to systematic errors in either the measurements (especially for the GOSAT case) or in the transport model (especially for the in situ case). By computing the expected random error from your a posteriori covariance matrix, you could potentially rule out random error as the cause, allowing you to attribute the new variability to either a real flux signal or to systematic errors. This is a key reason why you should use your covariance matrix calculation in this analysis.

446: The dipole behavior mentioned here, in particular, would be reflected in the covariance matrix, if that is in fact the cause of much of the variability.

474: It might be helpful to mention here in the text that you are comparing your in situ results (not GOSAT results) to CarbonTracker, which also uses only in situ measurements.

489: The sentence starting with "Results" could perhaps be deleted to save space, as it repeats the first sentence of the paragraph.

499-502: This is another place where the text could be compacted somewhat – it seems repetitive.

Fig.8 Caption and elsewhere: To avoid having to use the "NEP (x-1)" phraseology everywhere, why not just say you are solving for NEE (which is approximately equal to -NEP)?

515-516: "Such a large difference ... is plausible": what evidence can you give to back up your assertion? You have pointed to some plausible causes for the difference, but

even so, the difference seems larger than expected. Why did Houweling et al get a difference that was an order of magnitude lower for inversions across a similar span?

line 538 and Figure 8: You have used the same term, "Southern Ocean", to refer to the true Southern Ocean (as defined, for example, by TransCom3 as everything south of about 45 deg S) as well as the extra-tropical southern oceans (everything south of 23 deg S). I suggest changing what you call the latter area for clarity.

568, 571, and Fig 10: The text refers to sub-panels of Figure 10 (a-e), but these labels are absent from the actual figure – please add these labels on the figure.

568-570: "Evaluation of the inversions against latitudinal profiles constructed from HIPPO aircraft measurements, which provide additional sampling over the Pacific, indicates an overestimate by the GOSAT inversion relative to HIPPO in parts of the tropics at lower altitudes": my reading of the figure shows only one, maybe two, points from the GOSAT case that are outside of 1 standard deviation of the observations – this certainly does not seem to be a strong feature of the plots, according to my reading of them. It is not until about 40 deg N that the GOSAT results move positive in panel a).

632: Replace "elaborate on the subject of" with "discuss"? Less wordy...

695-696: "accounting for the riverine flux, the 1σ range for the in situ inversion overlaps with that of GCP": I believe you are incorrectly applying the riverine flux correction here. The GCP number of -2.5 should be decreased to -2.0 PgC/yr when turning it from an anthropogenic uptake into a total net (anthropogenic+natural) uptake, since the natural cycle (driven by the riverine fluxes into the ocean at the river mouths) has a net 0.5 PgC/yr outgassing – that outgassing counteracts a corresponding amount of anthropogenic uptake, reducing the total uptake to -2.0 PgC/yr. I.e., -2.5 + (+0.5) = -2.0. Given that, both your GOSAT-only and in situ-only ocean uptakes are still outside the 1 sigma ranges for the GCP number.

Figure 14: I would suggest using some color other than cyan to depict the tight-prior

GOSAT results here. As things stand now, it is much too easy to confuse that case with the in situ-only results on Figure 9. One has to read the caption and legend carefully to see that you have changed what is shown in cyan at the moment.

724: "substantially larger global total budget": it would be clearer to say that the total flux is more positive in the GOSAT case, since "larger" depends on whether the fossil fuel has been added onto the total or not.

734-735: "and an increased source in the tropics of $\sim$2 Pg C y-1 in the GOSAT inversion relative to the in situ inversion.": I think that it is important to note that this change from the in situ-only results in the tropics is accompanied by a change in the global total of the same magnitude and sign; in other words, the change is directly related to the fact that the global total is not well-constrained in this short-span inversion. This might be expected to change in an inversion over a longer span, for which the global total is better constrained.

767-799: This whole discussion of the Eurasian source in 2010 and the examination of JR-STATION sites suggests to me that the growing season results in 2009 could well be affected by spin-up issues in the inversion. That could explain why the GOSAT inversion results agree with the data at VGN, AZV, and KRS in 2010, but are too negative in 2009. If that is the explanation, the agreement with the Guerlet (2013) result would be more due to that modeling issue, rather than any real climate-related driver.

899-901: "Thus, it may not be accurate to assume that year-to-year posterior flux differences are insensitive to satellite retrieval biases, as was done in the other study." This would be a good place to note that spin-up errors in this study (as well as the Houweling study) could also be adversely affecting the 2009 flux results, as well as the 2010-2009 shift.

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