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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.

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This paper presents the components of the system designed to produce a level-4 GOSAT model product, surface CO2 fluxes from an atmospheric CO2 inversion constrained by in situ atmospheric observations and GOSAT-retrieved XCO2. It uses a fixed-lag Kalman smoother with a three-month window to retrieve optimized fluxes over 64 land and ocean regions during parts of 2009 and 2010. In contrast to other recent inversion systems, it operates on a strictly monthly time step, using monthly-average observations to retrieval monthly-average flux adjustments. Atmospheric transport is simulated using 6-hourly JCDAS winds, augmented by 3-hourly ERA-interim PBL depths.

Much of the paper is concerned with introducing the terrestrial, fossil fuel, and oceanic emissions prior flux models, all of which were developed for use in the GOSAT effort. Little critical evaluation of the final estimated fluxes is available, suggesting that the purpose of the paper is more to document the structure of the system than to highlight flux results. Of course, part of this is due to the short time period of the estimated fluxes (14 months).

MAJOR COMMENTS

1. Empirical bias corrections of GOSAT retrievals are a potential source of error in inversion systems, and the globally-uniform 1.2 ppm bias correction used here requires careful attention. At the very least, we need to know how the bias correction differently impacts the categories of GOSAT observations: glint-mode over oceans, high-, and medium-gain observations over land. There is also a question of whether there are major geographical variations in the factors causing such bias: land vs. ocean, temperate vs. tropical latitudes, differing aerosol and cloud regimes. Should there prove to be systematic geographic or geophysical variability in the bias, then there are two implications. The first is that the statistical model for retrieval errors cannot be considered independent observations, since the errors driving such non-random errors are presumably dependent upon one another. Even the inflation of GOSAT errors to about one order of magnitude larger than the GV errors, as is done here, is insufficient to handle this problem since the current model insists that all the errors are due to indepedent stochastic processes. The second implication is that regional flux estimates will be directly impacted by regional retrieval biases. This is a fundamental concern for which I have yet to see good answers, but the current manuscript does not even acknowledge the problem. I'd recommend appropriate sensitivity tests at the very least, perhaps by estimating independent bias corrections for glint, high-, and medium-gain retrievals, or for different geographic and seasonal subsets of the retrievals.

2. When describing the terrestrial and oceanic CO2 prior flux models, characterizing the mean flux, its spatial distribution, and the seasonal and interannual variability rep-

resented by the models is vital. We have only the long-term mean from the Valsala ocean flux model (with its map), not its ENSO signal, long-term trends, or seasonal cycle. Does the Takahashi climatological influence dampen (and bias due to its exclusion of El Nino year data) any ENSO variability?

The VISIT information is limited to that presented in Figure 1, which includes simulated atmospheric observations using a transport model and biomass comparison maps, but not fluxes themselves. This information is required to understand how much flux change from the priors that the inversion system is required to estimate. As an example, it would be very helpful to evaluate the prior models bu comparing with other published flux results, such as those from TRENDY, RECCAP, Transcom, or CMIP5 efforts.

3. Given the use of statistical estimation techniques in the creation of the land and ocean prior models, there must be some uncertainty information for those priors that could be used to set the values of the prior error covariance matrix, C_m. Real propagation of errors from those prior uncertainties to the atmospheric inversion would be far superior to the current use of temporal standard deviations. Should the authors insist on their current system for assigning C_m values, they should present some justification of why modeled temporal standard deviations would be a reasonable estimate for the statistics of the anticipated flux adjustments. For instance, if the prior model tends to have too little interannual variabilty and too small an annual cycle in a given region (as many terrestrial models have), how does its resulting temporal standard deviation in any way represent the statistics of the m values the inversion is expected to generate? Those m values may need to increase the mean sink, increase the magnitude of the annual cycle magnitude, and increase the interannual variability. As an aside, it is not clear whether the current scheme attempts to retrieve off-diagonal elements of the C_m matrix...which it could. There is little justification for the assumption that flux adjustments would be independent from grid cell to grid cell.

4. The C_d values used in this study deserve some scrutiny. The GV residual stan-

dard deviation is a plausible quantity to use for in situ data since it is a summary of differences between observations and a model of the observations. That statistical model significantly includes low-pass filtered differences between data and the trend-plus-seasonal cycle function fit; that LPF goes a long way towards identifying features that high-resolution transport models might represent. This logic is not present in the use of averaging statistics for GOSAT retrievals. It could therefore be argued that the in situ data error model attempts to represent problems associated with coarse-resolution simulated transport, whereas the GOSAT retrieval error model does not. Perhaps this is mitigated by the strong difference in minimum values for the two types of data, but I'd very much like to hear from the authors how they perceive this issue.

5. There is a set of standard diagnostics that can be extracted to evaluate any inversion, deriving from the statistical assumptions made when configuring the estimation problem. Notably, flux adjustments "m - m_prior" and observation residuals "v" are assumed to be Gaussian and independently distributed with specified variances. It is relatively straightforward to evaluate these assumptions by inspecting the flux adjustments and posterior measurement residuals for non-zero means, serial correlations, and other established goodness-of-fit measures. Given the extra step of imposing complex, and probably incorrect, atmospheric transport between the unknown fluxes and the resulting atmospheric CO2 mixing ratios, atmospheric inversions tend to find a careful compromise among all the uncertainties, including those like transport which are most difficult to quantify. While we have significant experience about how these compromises work for systems constrained by in situ data alone, the impact of remotely-sensed column-averaged observations on these inversions is not yet established. Including such diagnostics is vital for revealing how robust the up front assumptions are, and would allow us to understand how well the system performs compared to its design.

6. A significant part of the results involves comparing GV and GV+GOSAT inversions, in part to quantify the impact of satellite information on estimated fluxes. The authors

claim that fluxes from the GV+GOSAT inversion are closer to the priors than the GV-alone inversion. This is not surprising, since geographic sampling biases in the GV dataset mean that some regions are much more strongly constrained by in situ data than others. Typically this results in large and potentially unrealistic flux adjustments in underconstrained tropical regions to balance overall atmospheric growth rates. The authors present analyses of uncertainty reduction (UR) and differences in flux adjustments (delta-m-squared) to defend their conclusions.

In general, uncertainty reduction percentages are difficult to interpret, since their magnitudes depend so strongly on assumed prior errors. Making this measure relative between the GV and GV+GOSAT inversions further ambiguates the measure and obscures the contribution of in situ data to the final answer. I think it is crucial to show a map of the UR for each of the two inversions relative to the assumed prior, with a straight difference map giving the required information on added constraints from the GOSAT retrievals. The current figure 7 hints at some greater tropical influence for the remotely-sensed data, but seasonal variability in the geographic distribution of GOSAT data may be obscuring the importance of the tropical influence. Can a long-term UR map be shown instead of these four sample months?

The histogram of delta-m-squared in figure 13 is not particularly informative. The delta-m-squared quantity is not perfectly suited for defending the stated conclusion in part because sign differences in the flux adjustments are destroyed by the squaring. It seems that a direct comparison of the m - m_prior values between the two inversions–perhaps as a scatterplot–would be more informative. Further restricting the histogram to region-months where the uncertainty reduction exceeds a threshold further removes the reader from understanding where and when this effect is most pronounced (those significant region-months could be highlighted as a second color in a scatterplot).

7. The GV+GOSAT inversion should have poorer agreement with in situ data than the GV-alone case. Is that true? Can the authors provide an explanation of this effect?

MINOR COMMENTS

Abstract: The term "integrated" is used in to describe the inversion system both in the abstract and in the summary/conclusions. I did not find any explanation of why this term is used. Please explain what is integrated, or remove this overly generic term entirely.

General: it is not entirely clear what time and space resolutions are used by the various components of the system. In particular, GOSAT retrievals seem to be averaged to 5x5-degree grid cells, atmospheric transport performed at approximately 1-degree resolution, and the flux priors are available at a different resolution. Perhaps a brief statement of how these models are all coupled for the inversion could be included in the methods section.

Page 29237, lines 5-10: Land use history and the long-term effects of land management practices are implicated in the terrestrial carbon sink, and should be included in this list (cf. Casperson, Pacala et al. Science 2000).

Page 29237, line 15: It is incorrect to imply that CarbonTracker is a regional CO2 analysis. While it has a regional focus, CT belongs in the category of global CO2 inversion systems. Better examples of regional CO2 analyses–those that must infer a CO2 boundary condition–are available.

Page 29240-1: Does VISIT know anything about land-use change? Does it have any coupling with the imposed GFED biomass burning fluxes? I.e., how does it react to a particular grid cell having been burned?

Page 29240: There is no diurnal cycle in the VISIT fluxes. Any diurnal covariations of PBL mixing and land surface fluxes is thus not represented in this system. This has potentially significant implications for vertical transport of flux signals to GV observing locations. Since atmospheric transport is already resolved at sub-daily time scales, it remains only to infer a diurnal cycle for the land priors, a suitable method for which is availble in Olsen and Randerson (2003).

Page 29241, line 5: "were simulated" is repeated.

Page 29241: I'd like some more information on how VISIT is optimized. This brief overview doesn't do much more than give a pointer to other papers. The present authors have the opportunity to summarize the strengths and weaknesses of the VISIT model learned during this optimization procedure. This is especially important for understanding the application of that model to the present inverse analysis. Does the optimization result in an unbiased prior model for terrestrial fluxes, or is there still work to be done in representing the terrestrial sink?

Page 29242: The NDP-088 data have undergone several revisions. Please specify which version of the data have been used.

Page 29245: Are aviation fossil fuel emissions emitted at altitude into the transport model?

Page 29247: As I understand the text, the model uses 6-hourly archived winds and 3-hourly archived PBL depths. What, however, is the time step of the integration itself (line 14)?

Page 29248, lines 11-15: Claims that the vertical coordinate choice is responsible for good agreement with observations are made without justification. Perhaps a reference could be supplied?

Page 29250, eqns 2-4: In all 3 equations, various matrix or vector quantities are sometimes presented without the bold font that is intended to indicate that they are not scalars. Example: m in Equation 2 is both bold and non-bold.

Page 29251, line 12: It is confusing to state that the GOSAT retrieval bias is an optimized parameter, unless it is also specified that this optimization is different from the one used to produce the final flux estimates.

Page 29252, lines 5-10: The description here of a pre-optimization step for determining the global 1.2 ppm GOSAT bias correction and corrections to the initial atmospheric CO2 distribution is too brief compared to its significance. I would like to have enough information to evaluate the impact of this step on the following inversions. For instance, it is clear that a single global retrieval bias value was being sought, but it is not clear how many degrees of freedom are represented by the initial condition correction. It is not clear whether GOSAT data were used as optimization constraints. It is not clear the extent to which the resulting posterior estimates in this step are independent from one another. I suspect that they have significant posterior error covariances, because otherwise there would be no particular reason for a pre-optimization step like this.

Page 29254: What is GECM?

Page 29259: The symbol of delta-m-squared could be interpreted as delta-of-m-squared or as the square of delta-m. The latter is intended, but to be ambiguous it should probably be written (delta-m)ˆ2.

Page 29271, line 19. The dataset is NDP-088, not NDP-08.

Figures 8 and 9: The color scales should be tightened up (use a smaller range) so that the colors are more saturated. Now they are simply too pale to perceive differences.

Figure 10: It is difficult to assess whether the GV or GV+GOSAT simulations is better at representing these TCCON retrievals. Can a statistical summary of the mismatches be presented to quantify the residuals?

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