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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Global CO_2fluxes estimated from GOSAT retrievals of total column CO_2" *by* S. Basu et al.

Anonymous Referee #2

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1 Overview

The manuscript by Basu et al. uses a combination of GOSAT and surface (flask) measurements to constrain grid-scale CO_2 fluxes. They use a highly advanced inversion system and do a thorough job of accounting for and assessing potential sources of error in their inversion framework. For example, they repeat the inversion using different model resolution and meteorological fields to estimate impacts of model transport error. Compared to TCCON and aircraft measurements, the inversion results appear to be a significant improvement over the prior in the northern hemisphere. In the tropics and southern hemisphere, the results are less clear, and issues related to potential measurement bias over the ocean are identified. Inversion results are also compared

to other top-down constraints. The technical capabilities are impressive, and the authors make several minor improvements over previous works in terms of specification of error statistics. The manuscript is well written, although I do have some suggestions for organization. Overall, I found the paper to be of very high quality and suitable for publication following minor revisions described in detail below.

2 Specific comments

- p4537: The comparison of GOSAT, OCO2, etc., to satellites not primarily designed to measure CO_2 seems a bit unfair; I'm not sure the point.
- The introduction mentions in many places that the remote sensing observations based on instruments whose sensitivities peak at various altitudes in the troposphere have correspondingly variable sensitivity to surface emissions. While intuitively this makes sense, the extent to which this really matters hasn't been shown. Given their adjoint of TM5, the authors could easily quantify this through sensitivity simulations.
- p4541: Can the authors explain why assimilating the full set of hourly observations leads to biased inversions? This reason doesn't seem obvious. Rather, it seems there is a sampling bias by selecting observations only at particular times. Further does, the selection criteria introduce correlations in the observations by considering e.g., only nighttime measurements from high altitude sites?
- p4542: hint on → hint at
- p4543: Noting that there is 4.5 times as many remote sensing observations as there are surface observations is a simple straw man argument and could be dropped. Just present that actual sensible approach, rather than suggesting a transparently flawed view first (i.e., "going by the numbers").

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- 4543: What are the values of R and T? I don't see that included here. If they are mentioned elsewhere, it would be useful to repeat that here where the variables are introduced?
- Eq 3: what is the meaning of the "hor" subscript on i?
- p4546: what is Q_{10}?
- p4547: Given the off diagonal elements and size of B, were there any numerical issues involved with calculating B^{-1}, particularly for the high resolution inversion tests?
- p4547, 19: Did the authors find a tuning that was unique, or are multiple possible values possible?
- p4549: I applaud the authors attempt to quantify the model representativeness error. It's clear that their understanding of what this term is suppose to represent is correct. Too often (i.e., one of the other reviewers for this manuscript) this term is interpreted as "model error", which it is not. The approach taken here to estimate the representativeness error, however, doesn't seem the best. The goal is to quantify the degree to which sub-grid scale variability will make accurate matching of the observations impossible, even if x is perfect. I doubt however that the gradients across the coarse $6^\circ \times 4^\circ$ grid cells give a good sense of the subgrid variability. Instead, the authors should make use of their $3^\circ \times 2^\circ$ model run to evaluate where their are strong gradients near observations.
- p4550,8: I don't understand what the authors mean by σ_{mod} of the satellite observations, since this is the model representativeness error. Were they using gradients in GOSAT observations as an estimate of sub-grid scale variability to calculate the model representativeness error? This would make more sense than gradients in the $6^\circ \times 4^\circ$ grid cells . . . Sorry if I'm confused here, but perhaps it could just be explained a bit more clearly.

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- p4550,11-15: I understand what the authors mean, but the way it is currently written sounds as if less averaging leads to less noise, where the opposite is true, so perhaps they could address this more carefully as well.
- p4550: I love this book as much as any, but Tarantola 2005 is an odd reference to provide for gradient-based approaches to minimizing the cost function.
- p4550,19: Meirinik et al. (2008)
- p4551: One of the few areas where the methodology is a bit weak is the convergence criteria, which seems a bit arbitrary. Have the authors considered other standard evaluations of convergence such as χ^2 test, or comparing the magnitude of J to the number of observations, etc.?
- p4552: I think that ideally an inversion is judged by how well it constrains the sources, which we care more about than the actual distribution of CO_2.
- p4554: "spot on" is a bit casual for a journal article. I suggest the authors be more quantitative here. Specifically, statistics such as error, bias, correlation, etc., should be provided for the different inversion results. The can be included in the white space on the plots, or consolidated into a table.
- p4555 / Fig 3: The selection of these 4 stations still seems arbitrary. A table presenting overall performance statistics to the observations should be presented.
- Fig 3: Is is really necessary to show the results at such high frequency? There is no discussion of the high frequency aspects of the data, so I suggest smoothing the data would make it much easier to distinguish the different results, particularly for Park Falls.
- p4556: It seems like this excessive drawdown is almost as prevalent in the norther data on the left of Fig 5 as well, so it's not clear why it is only mentioned with respect to the tropics.

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- Please include in all figure captions an explanation of the legends to clarify, for example, the difference between GOSAT assimilation results and GOSAT observations.
- Fig 6: I would think that the GOSAT assimilation would lie between the prior and the GOSAT observations, but in many places (e.g., $\sim$Sep 2009) this isn't the case. Please explain.
- One of the main novelties of this work, as expounded upon in the introduction, is the use of GOSAT and surface flask measurements individually and in tandem for the assimilation. However, while section 4 includes a detailed description of the GOSAT assimilation, it doesn't directly explain the flask-only assimilation or the joint assimilation. Results from these are apparent in the figures and appear scattered throughout the text, but there isn't any section dedicated to them. I suggest these two other assimilation results be specifically discussed in Section 4.
- p4558: It is interesting that the higher resolution source regions contain less correlations. Can the authors comment on this with regards to inversion techniques using grid-scale vs aggregated source regions?
- Did the authors consider that timing of the GOSAT observations (i.e., only at 13:00 local time) could potentially bias the inversion? Is the mean CO_2 at that time an unbiased estimate of mean CO_2, and would that make a difference?
- p4543/4544: It seems the discussion of σ_b here is out of place. Wouldn't it fit better in section 3.1.4, which describes R?
- p4566,11: enough to detect
- p4569,13: Chevallier et al. (2011)

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