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Atmospheric Chemistry and Physics Discussions

Interactive comment on "CO_2 flux estimation errors associated with moist atmospheric processes" *by* N. C. Parazoo et al.

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Anonymous Referee #2

The paper is about quantifying CO2 flux estimation errors from interpreting satellite observations of CO2 with atmospheric transport models. The focus of this paper is quantifying errors due to the atmospheric transport processes, in particular moist atmospheric processes (e.g., frontal systems). Interesting results with implications for the way we interpret satellite observations of CO2. I do wonder about the realism in the OSSEs (point 7), but on the whole the paper is worth publishing in ACP.

Specifics:

1) Page 9988. An end-to-end OSSE with satellite data would involve atmospheric

radiative transfer modelling. The study is a subset of the end-to-end calculations.

We thank the reviewer for this clarifying point. We have replaced "end-to-end OSSE's" with "a subset of end-to-end OSSE calculations"

2) Page 9989. Why is "better resolved" in speech marks? Are the authors trying to make the point that higher spatial and temporal resolution does not equate to improve accuracy/precision? Cryptic text is unwelcome.

This indeed was the intention, and we agree it was lazy and confusing writing. The authors feel that such discussion of model precision is beyond the scope of this study, which is a sensitivity analysis. We have therefore replaced "GEOS-5 is better resolved at" with "GEOS-5 has a native resolution of"

3) Over what lag window does the inversion scheme update fluxes from a particular month? Other studies recognize that measurements a few months after month X can still be useful in constraining flux estimates from that month. There is some text at the bottom of page 9992 and the top of 9993 but it is not completely clear why they have chosen two weeks.

We agree our discussion is unsatisfactory. We therefore include a separate paragraph at the top of P9993 discussing the data assimilation window and our choice of two weeks:

"Biases are assumed to be constant over the length of the data assimilation window. Lokupitiya et al. (2008) solved for 8-week biases by assimilating synthetic surface data over the same period. This assimilation window was found to reasonably recover fluxes given a sparse measurement network (Peters et al., 2005). Although this window is short relative to the length of time needed to effectively capture signals from source regions (Bruhwiler et al., 2005), this extra information is mostly diluted by atmospheric mixing and comes at a greater computational cost. Satellite observing systems such as GOSAT greatly improve spatial coverage, and thus longer windows may not be required. Here, we consider a 2-week window. Given the short assimilation window and grid scale inversion, strong covariance smoothing is applied at the first cycle of MLEF, using e-folding length of 800 km over land points and 1600 km over ocean points. Further details regarding the assimilation scheme are discussed by Lokupitiya et al. (2008)."

We assumed that higher sampling density from satellites would permit shorter assimilation windows. This may be true in the absence of temporal sampling biases. However, because sampling biases lead to flux biases, a longer data assimilation window may be necessary even with satellite data. We include the following paragraph in the conclusions discussing the implications of our findings for flux inversion of satellite data:

"While signal detection experiments suggest that two weeks of data assimilation may provide a sufficient constraint for flux inversion of satellite data, our results suggest that it may also be a cause of flux biases. If CO2 is mixed rapidly through the domain then we should expect that flux estimation is not sensitive to the timescale for data assimilation. If, however, the timescale for transport is long compared to the timescale at which CO2 is sampled, then biases may be introduced. If the time-scale for meridional transport is greater than two weeks, then a longer window may be preferable. Our experiments assumed that greater sampling coverage of satellites would permit shorter assimilation windows; we did not consider, however, that temporal sampling errors associated with moist frontal transport would bias the flux inversion. Future studies should more thoroughly examine sensitivity of satellite inversions to data assimilation window."

4) The authors note on page 9993 that they do not take into account aerosol effects. Presumably rapid vertical transport of surface emissions over urban areas (that include significant aerosol burden) would also affect the ability of the satellite to observe the frontal system but anything else?

We thank the reviewer for this comment. We have included a paragraph in the conclu-

sions discussing overall lack of realism of OSSEs, including neglecting aerosol effects, and speculate on impacts to the inversion.

"While these results are important for the interpretation of flux errors associated with satellite observations of CO2, we caution these errors in no way bracket the possible range of flux errors to be expected in an inversion of real data. We have eliminated factors such as transport model, assimilation system, and differences in specific humidity fields between GEOS-4 and GEOS-5, but have not addressed errors due to other differences in experimental setup, in particular the calculation of XCO2. For example, we have not considered aerosol effects, land surface type, or surface pressure. In the absence of condensation, aerosol effects may be important for frontal transport in urban areas in Europe and eastern N. America or in regions of biomass burning. High aerosol burden could have a similar impact on flux inversions as clouds, and should therefore be a focus of future studies."

5) Are the humidity fields between GEOS-4 and GEOS-5 products different? I assume they are but how different? How important is this effect in calculation XCO2? How big is this effect for the different spatial resolutions?

We thank the reviewer for this comment since we had not considered the impact of humidity fields. The impact does not appear to be large. We have included a map (see Fig 1 at end of document) showing the difference between dry- and wet- air mole fraction in GEOS-4 (shaded) and GEOS-5 (contour), plotted in units of ppm. The map contours follow very closely, except for some minor differences in the tropics and subtropics that don't exceed 0.1 ppm.

We leave it to the reviewers' discretion as to whether to include this plot (or some equivalent) in the revised manuscript, but we have included text to address reviewer comments.

Results, P9998:

"The difference between flux errors in perfect transport (Experiment 2 and 4) and biased transport (Experiment 5 and 6) experiments primarily reflect differences in transport. Differences in the calculation of the dry air mole fraction of CO2 due to differences in humidity fields between GEOS-4 and GEOS-5 are small (< 0.1 ppm at grid scale in the annual mean) and therefore unlikely to contribute significantly to flux errors. With Experiment 6 as the reference and focusing on northern temperate and boreal regions, we therefore estimate that transport errors create a European source of 0.43 +/- 0.35 PgC year-1, Eurasian Temperate source of 0.15 +/- 0.32 PgC year-1, N. American Temperate sink of 0.04 +/- 0.45 PgC year-1, and N. American Boreal sink of 0.15 +/- 0.20 PgC year-1. The amplified European source and N. American Boreal sink are most significant relative to uncertainty between inversions."

Conclusions, P9999

"While these results are important for the interpretation of flux errors associated with satellite observations of CO2, we caution these errors in no way bracket the possible range of flux errors to be expected in an inversion of real data. We have eliminated factors such as transport model, assimilation system, and differences in specific humidity fields between GEOS-4 and GEOS-5, but have not addressed errors due to other differences in experimental setup, in particular the calculation of XCO2. For example, we have not considered aerosol effects, land surface type, or surface pressure. In the absence of condensation, aerosol effects may be important for frontal transport in urban areas in Europe and eastern N. America or in regions of biomass burning. High aerosol burden could have a similar impact on flux inversions as clouds, and should therefore be a focus of future studies."

6) The eddy and mean flow calculations is described very poorly. Are they simply using Reynolds averaging of the underlying wind fields?

We thought is sufficient in the original manuscript to cite Parazoo et al. (2011) for calculation of eddy and mean transport, but based on the reviewer comment, we now

feel it is more convenient for the reader to include a short discussion of basic principles of eddy decomposition and calculation of frontal CO2 transport. We therefore inserted an extra section in Methods (2.2) following discussion of Forward Simulations (2.1):

"2.2 Calculations of Frontal CO2 Transport Eddy decomposition of CO2 transport is described in more detail by Parazoo et al. (2011); here we provide a brief description. Frontal CO2 transport is diagnosed by parsing total column integrated meridional CO2 transport as described by a global tracer transport model into eddy and mean components of the large-scale atmospheric circulation. The eddy component arises from correlated variations of mass flux and CO2 mixing ratio. Frontal passage events are transient and migrating, tend to deviate strongly from the mean overturning circulation (e.g., Hadley Cell), and are therefore associated with strong variations of mass flux. When CO2 gradients align with frontal air parcel trajectories along the meridional plane, the eddy component amplifies. These conditions are often satisfied in northern mid-latitudes (Keppel-Aleks et al., 2011). The vertical coordinate that describes frontal transport is also crucial because variations of atmospheric fields are sensitive to trajectory. In order to capture moist ascent of air parcels along stormtracks (e.g., Pauluis et al., 2008), the analysis therefore relies on the calculation of eddy and mean transport on moist isentropic surfaces, which conserves energy as rising air condenses and releases latent heat."

7) How detailed is the calculation of XCO2 along the GOSAT orbit? Did they include changes in AOD, land surface type, surface pressure, etc? This information is not mentioned in the paper. Do they assume access to sun-glint and nadir observations? Have they used GOSAT observations with a high solar zenith angle?

We acknowledge that this discussion was lacking. We have expanded on some previous discussion of XCO2 calculations on P9993.

"GOSAT uses a sun-synchronous orbit with early afternoon sun-lit equator crossing time ($\sim$1:30 pm local time) and orbital inclination near 98°. Synthetic GOSAT retrievals are generated in forward simulations of the nature run (Step 1) using pressure-weighted column averaging of vertical CO2 profiles. Since GOSAT measures CO2 absorption using reflected solar radiation, PCTM is sampled only during daytime of the descending mode of orbit. Subsequent orbits are separated by $\sim$25° in longitude and $\sim$99 minutes apart. GOSAT points near-nadir as well as at the sun glint spot, which greatly increases the signal over the ocean. We assume a 5-point cross-scan track, which was used on GOSAT between 4 April 2009 and 31 July 2010, with footprints separated by $\sim$ 158 km cross-track and $\sim$ 152 km along track (Crisp et al., 2012). Soundings are sampled at the native resolution of the meteorological analysis in the nature run (see below) at 1:30 pm local time, and are assumed to represent the grid scale average. No temporal averaging of synthetic retrievals is applied. A maximum of 281 points are sampled by GOSAT in one hour, corresponding to 94,416 points over the 2-week assimilation period. All possible glint retrievals are retained, including those beyond +/- 20° of latitude from solar declination. In practice, however, glint mode is only used by GOSAT at latitudes within 20° of latitude from solar declination. We therefore run an additional OSSE in the signal detection experiment (Experiment 3, described below), to test for the impact of high latitude glint data on flux recovery. We prescribe a uniform uncertainty of 3 ppm to GOSAT retrievals; this is chosen as an upper bound from values computed by Chevallier et al. (2009) due to measurement noise, smoothing error, interference error component, and overall random contribution of aerosols to retrieval noise."

8) I found the concluding remarks a little weak. Joint inversion of column CO2 and surface CO2 would not help fix any of these transport errors or indeed identify them unless with careful analysis. Under extreme conditions the model might not be able to reconcile the column and surface CO2 measurements, with the resulting posterior flux being even more grossly in error. Similarly, running an ensemble of meteorological states if the underlying model parameterization is in error will also not help. Only better characterized model parameterizations, developed based on extensive in situ meteorological measurements, will help address transport models. This is an unescapable

truth.

The authors agree completely with the reviewer here, but we thought it important to highlight some techniques currently in use to address transport model error. After reflecting on our original conclusions, we regret the use of the statement "techniques to alleviate the effect of transport errors exist." We have revised the final paragraph to point out things scientists have done, but with the caveat of uncertainty. We also borrowed a portion of the reviewer comment (second to last statement above) as it leaves the reader with little doubt about what needs to be done.

"There is little doubt in the CO2 inversion community that priority should be given to improving sub-grid vertical transport. Despite efforts to treat transport error, including joint inversion of column/surface CO2 and meteorological data (Chevallier et al., 2011; Kang et al., 2011) and implicit accounting for transport errors through Monte Carlo and ensemble approaches (Chavallier et al., 2007; Teixeira et al., 2008; Liu et al., 2011), it is not clear to what extent these techniques fix or exacerbate transport errors. It seems that only better characterized model parameterizations, developed based on extensive in situ meteorological measurements, will help address transport models."

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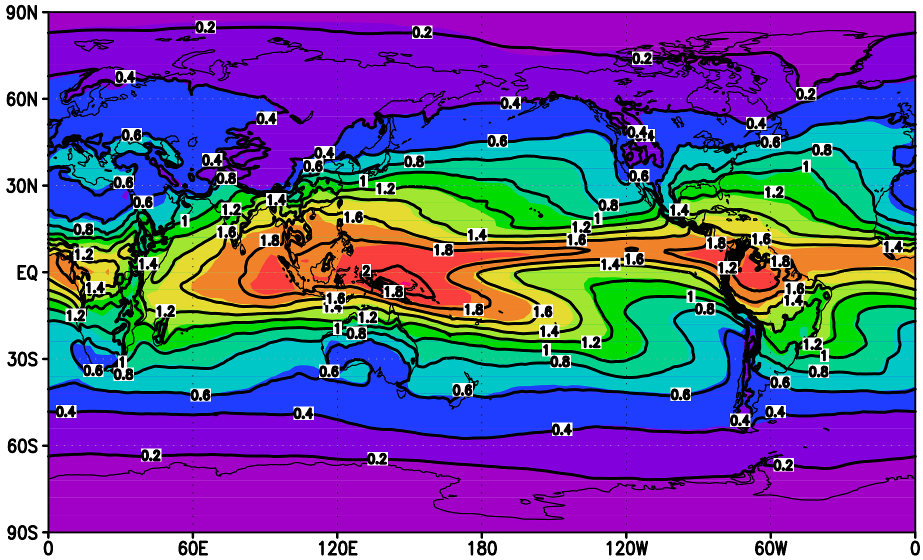

Fig. 1. Difference between dry and wet air mole fraction in GEOS-4 (shaded) and GEOS-5 (contour), in ppm.