

Interactive comment on “A comparison of atmospheric CO₂ flux signals obtained from GEOS-Chem flux inversions constrained by in situ or GOSAT observations” by Saroja M. Polavarapu et al.

Anonymous Referee #2

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Overall impression

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According to my understanding, this manuscript addresses two major topics. The first is how adjustments to surface fluxes (posterior minus prior) manifest themselves in the atmosphere. This is done by performing inversions for the first two years of GOSAT data using a variational GEOS-Chem system, and propagating the posterior and prior fluxes through a transport model. Along the way, the authors perform some evaluation

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of their inverse results, such as comparison to TCCON and HIPPO. The second is how that manifestation varies if a different higher resolution online atmospheric transport model is used. In my opinion, the authors spend too much time on the first topic and not enough on the second, which makes the work not significant enough for a journal like Atmospheric Chemistry and Physics. If this focus were reversed, or the first topic were explored further (explained below), it would make for a much more interesting and scientifically significant paper.

The authors perform inversions of GOSAT and in situ data for two years, and look at the fluxes and resultant atmospheric CO₂ fields in the first two years of GOSAT, primarily focusing on 2010. They use a variational inversion technique using the GEOS-Chem transport model. Their conclusions are very similar to previously published literature, such as Houweling et al (2015), Basu et al (2013), Chevallier et al (2014), which they cite. In fact, a very similar (if not identical) set of inversions was already submitted by some of the co-authors to an intercomparison of GOSAT inversions published by Houweling et al (2015). As far as I can tell, there is nothing new or unique about their inversion or analysis compared to the multitude of GOSAT inversions already published for 2010, and this part of the work does not add to the body of existing knowledge about GOSAT retrievals and derived fluxes in and around 2010. GOSAT has been up for eight years now, and retrievals of column CO₂ from GOSAT exist for the majority of that period. I do not understand why the authors have limited their study to the first couple of years of GOSAT data. If the authors want to publish a GOSAT inversion study that would be of value to the scientific community, I would recommend performing a longer term study, such as (say) the inter-annual variability of fluxes as seen by GOSAT, or the longer term trends in atmospheric CO₂ and CO₂ fluxes as seen by GOSAT. The current inversion study, focused on 2010 (with some padding on either side), is of limited interest.

The second thread in their work, however, is more interesting. They perform forward runs with two different models of atmospheric transport driven by the same fluxes and

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look at the difference in the “flux signal” in the atmosphere. The non-GEOS-Chem model is the higher resolution GEM-MACH-GHG, a fairly new addition to this community (Polavarapu et al, 2016). Not only did they transport CO₂ with GEM-MACH-GHG, they also perturbed the transport with analysis errors from the meteorological assimilation system, thereby simulating the impact of uncertainties in the met fields on CO₂ variations. They derive a “baseline” CO₂ variation from this error propagation, contending that variations smaller than this detected by an observing system cannot be reliably ascribed to fluxes. This, to my knowledge, is fairly unique in the tracer transport community, and provides a recipe for deriving transport errors in CO₂ space. Such errors can be used, e.g., if GEM-MACH-GHG or a derived offline model is used for trace gas inversions. This technique may also be valid for deriving “baseline” transport errors for an offline model if an ensemble is run for the parent model with greenhouse gases (e.g., GEOS5 for GEOS-Chem).

If the authors would like to revise their manuscript and make it scientifically significant enough for this journal, I can offer two different suggestions. Either they need to extend their GOSAT analysis to 5+ years and address questions such as long term trends and interannual variability of CO₂ fluxes. Or they need to more or less excise the GOSAT inversions and focus on the performance of GEM-MACH-GHG in simulating atmospheric CO₂ and its meteorological errors.

For the first choice, I would suggest questions such as:

1. Do GOSAT retrievals estimate a stronger European sink consistently over time, as first suggested by Reuter et al (2014) with SCIAMACHY and a single year of GOSAT data?
2. Do GOSAT retrievals require a stronger northern hemisphere uptake consistently, as noted by Houweling et al (2015) for one year?
3. According to GOSAT, which region contributes most to the interannual variability of atmospheric CO₂, the Tropics or semi-arid ecosystems? This has been an ongoing

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debate in the atmospheric carbon community, see e.g., Baker et al (2006), Poulter et al (2014) and Ahlström et al (2015).

4. Are there persistent differences between GOSAT and surface data inversions across multiple years?

These are just some suggestions, and I'm sure the authors can think of many such questions to address with a multi-year GOSAT inversion.

On the other hand, if the authors choose to focus on GEM-MACH-GHG, then that would make for a very interesting paper as well. The authors have already addressed some of the interesting questions that arise from using a high resolution online model for CO₂ transport. Some additional questions could be:

1. Are high frequency variations of CO₂ near the surface better represented by the higher resolution model? If yes, we could potentially move to assimilating more data from surface measurement sites in the future with online models such as GEM-MACH-GHG.
2. Can one construct a “look up table” for the baseline transport-driven errors using GEM-MACH-GHG, varying (say) by region and season? How do those errors differ between surface and total column measurements? I'm looking for something like Figure 17, but much finer grained than three zonal bands. At the very least, ocean sites, coastal sites and continental sites should be separated. Similarly, for total column measurements, ocean and land soundings should be separated.
3. If inversions were performed using errors from step 2, versus more traditional prescription of errors, how do the fluxes change?
4. Is it true that transport errors matter less in assimilating a total column than assimilating surface sites or a vertical profile? This was first suggested by Rayner & O'Brien (2001), but to my knowledge never explicitly demonstrated. The crucial thing to compare here would be the size of the transport error and the size of the flux signal, since

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that is small as well in the total column.

5. Is there any covariance between transport error and CO₂ variation, especially along weather fronts? This has also been a topic of much debate, especially whether assimilating high frequency CO₂ measurements can improve weather forecasts (Engelen et al, 2001), and whether CO₂ inversions need to assimilate met observations.

Again, these are just some suggestions, and I'm sure the authors could come up with an interesting set of questions relevant to the atmospheric CO₂ community that they could answer with GEM-MACH-GHG.

Other comments

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1. The prior fluxes in Figure 5 look strange. It is rare for me to see a terrestrial flux prior that is positive in the annual aggregate over North America, and Boreal and Temperate Eurasia. Where do those priors come from?

2. In Figure 7, I would prefer to see time averages, say over a week or month, instead of snapshots. Snapshots often display misleading variations that do not matter for what the authors are considering. This comment only holds, of course, if an equivalent of Figure 7 still exists in the revised manuscript.

3. I was surprised to see no data providers as co-authors in an inverse modeling paper. It is usual in this field to offer co-authorship to data providers, which they may or may not accept. In fact the ObsPack fair use policy explicitly states:

“Your use of this data product implies an agreement to contact each contributing laboratory to discuss the nature of the work and the appropriate level of acknowledgment. If this product is essential to the work, or if an important result or conclusion depends on this product, co-authorship may be appropriate. This should be discussed with each data provider at an early stage in the work. Contacting the data providers is not optional; if you use this data product, you must contact the data providers.”

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Were the data providers contacted, at the very least to let them know that an inversion study using their data was about to be submitted? If not, that is a significant oversight that needs to be corrected.

4. I have a problem with the terminology “flux signal”, even though the authors made the explicit caveat that this “signal” by definition depends on the inverse model and the prior. The term “flux signal” makes it sound like it's an inherent property of the observations, which it is not. I would recommend using a different term, such as “CO₂ adjustment” or “mole fraction update”.

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