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Atmospheric Chemistry and Physics Discussions

Interactive comment on "On what scales can GOSAT flux inversions constrain anomalies in terrestrial ecosystems?" *by* Brendan Byrne et al.

Abhishek Chatterjee (Referee)

abhishek.chatterjee@nasa.gov

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This is a highly relevant and high-quality study that makes use of the ever-increasing time record of satellite retrievals (in this case JAXA's GOSAT), and demonstrates their value in providing the community with new insights into carbon cycle science. While most inversion studies tend to diagnose carbon fluxes, this study makes a valuable attempt at attribution by relating the carbon flux anomalies with various auxiliary environmental variables (called proxies here). The manuscript is well-written and the figures and text are of high-quality. Before recommending final publication though, I would highly advise the authors and the ACP editorial team to make note of the following comments and suggestions:

Major Comments:

(1) In many ways, the manuscript reads like a dissertation chapter. There are too many details that are unnecessary for a manuscript as it outlines all the sensitivity tests and checks that were done. I would highly recommend that the authors think about what the main takeaway messages are, and accordingly cull the text from Sections 3, 4 and 5 to focus on those messages. There are some strong points made in the paper, for example - the need to use prior flux estimates that have a realistic seasonal cycle and IAV. But more often than not, these types of important messages are lost in the text. Even though the manuscript is well-written, it is cumbersome to read. Some of the sensitivity tests could be moved to Supplementary Information. Also, see comments #2 & #4 below on which text are most suited for this.

(2) Is SIF really needed? As the authors rightly point out in multiple places (Page 3, Lines 24-29, Page 11, discussion in Lines 17-30, Page 16, Lines 30-32), SIF is a good proxy for GPP. Its relationship to NEE is complicated by the role of R. If the authors were estimating GPP and R separately, then SIF would have been a valuable dataset to correlate against. But given that the authors estimate NEE, it is impractical to assume SIF will provide any new information or help constrain the scales at which GOSAT can inform NEE fluxes. Results presented in Figure 2 and Table 2 further show the redundancy of SIF for this paper. In fact, it is not surprising at all that SIF is well correlated with CASA GFED (a model based on light-use efficiency) and consequently CT-2016 (that uses CASA-GFED as its prior). Finally, Section 4.1.2 doesn't add any new information to the study at all. By taking out SIF, the authors could shorten the paper and make it more focused.

(3) Follow-on to the previous comment - if the authors were interested in using a vegetation index, they may want to look at something more direct like LAI. Or even indices like NDVI, EVI, fPAR x PAR may act as robust proxy capturing the interaction between vegetation and radiation.

(4) Should the OSSEs be moved to the Supplementary Material? While the scientific rationale for doing the OSSEs make sense, they add a new set of results that are frankly not necessary. The real data inversions already cover all the conclusions from the OSSEs. I understand that the OSSEs may have been done initially to ensure that the GEOS-Chem 4DVAR system works, before the authors move to a real-data inversion. But given the more interesting and thought provoking results with the real data, Section 3.3 seems superfluous.

(5) The attempt to isolate anomalies specific to each continent (Equation 2) is unclear. I do not follow how the standard deviation of the tropical and the continental anomaly are calculated in the first place. Do the authors do some type of Monte Carlo simulation (i.e., an ensemble of fluxes and anomalies) to calculate STD(ANOM).

(6) A critical component that is missing from the study is the calculation of time-lagged correlations. The impact of ENSO events or droughts are not immediate (i.e., within a month) on fluxes. There is a spatiotemporal cascade of this impact across different tropical and Northern extratropical regions. In fact the authors do acknowledge this briefly (Page 16, Lines 17-20). Also see Rayner et al. 1999 (GRL, Vol. 26(4)). My hypothesis is that both the R2 and the significance of several of the flux estimates will change when the authors use time-lagged correlation, especially with the Nino3.4 index. Can the authors comment further on why they didn't pursue? A figure or two in Supplementary Materials may help. (6) Finally, why isn't there a run at 2 x 2.5 with prescribed IAV in the prior flux? If anything, I would want to see how an inversion run at 2 x 2.5 with 200% uncertainty applied to prior fluxes and with prior NEE IAV performs. The results may really enforce the message in Section 4.3.3 about the realism of IAV in prior fluxes and the important role it plays on the posterior IAV that is recovered during the inversion.

Minor Comments:

(1) Page 5, Section 2.1 - What is the spatial and temporal resolution at which FLUX-

COM products are available? Are there potentially issues with scale mismatch that the reader should be aware of?

(2) Page 7, Lines 30-33 -The current nomenclature for the different runs is extremely complex. I do not know if there is a way of making the names more intuitive and/or easy to follow. But something the authors can think about a bit more.

(3) Page 8, Line 3 - Did GOSAT ACOS v3.5 had warn levels? Or just quality flags? To the best of my knowledge warn levels weren't implemented till v7.3. Kindly check.

(4) Page 9, Equation 1 - It is worth making the reader aware of the drawbacks of using a short period to define the climatology.

(5) Section 4 Discussion topics - what about the discussion for agreement between flux anomalies and Nino 3.4 index? That discussion seems to have gone missing.

- Abhishek Chatterjee, Global Modeling and Assimilation Office (GMAO), NASA Goddard Space Flight Center, Email: abhishek.chatterjee@nasa.gov, Url: http://gmao.gsfc.nasa.gov/bio600/abhishek.chatterjee.html

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