

Atmos. Chem. Phys. Discuss., referee comment RC1  
<https://doi.org/10.5194/acp-2022-120-RC1>, 2022  
© Author(s) 2022. This work is distributed under  
the Creative Commons Attribution 4.0 License.

## Comment on acp-2022-120

Anonymous Referee #2

---

Referee comment on "Optimizing 4 years of CO<sub>2</sub> biospheric fluxes from OCO-2 and in situ data in TM5: fire emissions from GFED and inferred from MOPITT CO data" by H  l  ne Peiro et al., Atmos. Chem. Phys. Discuss., <https://doi.org/10.5194/acp-2022-120-RC1>, 2022

---

This study examines the impact of prescribed biomass burning emissions in CO<sub>2</sub> flux inversion analyses. They show that the prescribed biomass burning emissions have a minor impact on large scale net fluxes but a larger impact on regional scales (although these impacts are generally still small). They also find that differences between biomass burning emission estimates in the tropics are quite large, and thus have a large impact on inferred NEE estimates (to conserve NBE). Overall, I think this analysis is of interest to readers of ACP, but feel that the analysis could be expanded. In particular, the impact of including the CO-constrained biomass burning emissions on the performance of the CO<sub>2</sub> flux inversion could be more fully explored.

General comments:

- The biomass burning CO<sub>2</sub> emissions constrained by assimilating CO data seemed to just be used as an alternative fire CO<sub>2</sub> estimate, but it was unclear if this was an improved estimate and what implications it had on NEE. Firstly, I think that the posterior CO fields from the MOPITT flux inversion should be evaluated. I would recommend comparing the posterior fields to TCCON XCO in the same way that the posterior CO<sub>2</sub> fields were evaluated. Second, it would be useful to characterize whether employing this MOPITT-based biomass burning estimate improved the inversions in any way. In particular, were the inversions able to better fit to OCO-2 and IS measurements that were strongly impacted by biomass burning emissions? You could perform this comparison by running a tagged tracer experiment for biomass burning emissions and then look at the data-model mismatch for measurements that had a large biomass burning signal.
- The CO<sub>2</sub> flux inversion configuration is insufficiently described. Are the CO<sub>2</sub> flux inversions optimizing ocean and NEE fluxes? And what are the prior errors applied to these quantities? And of great relevance to the results, how do the prior errors vary between the different experiments? I would expect the posterior regional NBE fluxes to be very sensitive to the prior error statistics, particularly for the IS inversion.
- I find much of the text to be quite awkwardly worded, which can make the manuscript hard to follow. In addition, there are a number of rather sloppy mistakes in the

description of experiments, equations, and variable names. I have flagged several issues in my specific comments, but not all. I strongly recommend that the authors go through the manuscript carefully to fix these issues.

Specific comments:

L2: "used as a tracer of CO<sub>2</sub>" to "co-emitted with CO<sub>2</sub>"

L5-6: This statement is confusing: "These CO<sub>2</sub> fire emissions allow us, then, to estimate adjusted CO<sub>2</sub> Net Ecosystem Exchange (NEE) and respiration which are then used as priors for CO<sub>2</sub> inversions"

L20-22: This statement is confusing: "Evaluation with TCCON suggests that the re-balanced posterior simulated give biases and accuracy very close each other where biases have decreased and variability matches better the validation data than with the CASA-GFED3."

L48: "atmospheric measurements" to "atmospheric measurements of CO<sub>2</sub>"

L68: Define "terrestrial biosphere fluxes", to some this could include biomass burning.

L234: "optimal estimation"? I would assume TM5-4DVar uses 4DVar, correct?

Figure 2 caption: "Localisation" should be "Location"

L286: A Gaussian correlation length of 1000 km is also applied to CO? Hard to think of a physical reason for this?

Figure 3: Maybe try a different colorbar, it is hard to see the different regions.

L320: "a priori" to "from CO<sub>2</sub> data alone"

Equation 3: The notation "max(FIRE3 – FIREx,0)" is not typical notation. This appears to indicate that the max is taken down the zeroth dimension of the array, but the dimensions of the array have not been defined in the text. Please revise.

L354-358: This is a methods section not a results section.

L380-385: It is unclear what is meant by "bias satellite data due to cloud coverage". Please explain exactly how cloud coverage biases satellite data.

L380-391: This whole section is quite unclear. Consider re-writing.

L389-390; Fig 12 cap: There are not regions defined as "Tropical America". I think these should be Tropical South America.

I found figure 5 very hard to look at. The subset bar plots are far too small. I would recommend re-plotting similar to Figure A2.

L443: "meridional", should this be "zonal"?

Figure 8 and others: "OCOcms" – I did not see this defined anywhere.

L554: "the 2018 drop off sink". This is unclear.

L560: "Emissions estimated observed with OCO-2"?

L673: "fire emissions and plant respiration (and hence net fluxes)". These two quantities alone do not combine to the net flux.

L696-699: I do not understand the sentence "Over Southern Tropical and Northern Tropical Asia, the combination of the spatio-temporal variability of MOPITT CO fire and the GFED4.1s emissions information included in the prior fire emissions of the CO inversion might bring additional information in the emission ratio and hence in the fire prior used in CO2 inversions."

L727-739: This paragraph seems to be a description of the results rather than a discussion topic. The relevance to the main findings of the study also seem unclear, I would suggest removing.

L740: What findings?

L742-748: This is not an assertion, but a direct result of mass balance. Over a single year, there can be differences in the growth rate due to sampling. However, if averaging over a few years, the signal will be well mixed. It is unclear what is being referred to with these statements. Is it referring to the "Global" fluxes shown in Figure 10? If so, this is only showing the land flux, right? The difference between the IS and OCO-2 "Global" land fluxes should be compensated for by differences in the ocean fluxes. Please clarify.