Review of Basso et al.,

This study analyzes total carbon fluxes, NBE and fire fluxes for the Amazon basin over the period 2010-2018, using CO_2 vertical profiles and global flask measurements with a global atmospheric transport model. Their results show the Amazon as a small net source of carbon. These emissions mainly come from the eastern Amazon due to fire emissions, half of which are removed from forest uptake. This study is of interest to the global carbon community, not only for monitoring the Amazon basin, but also for recognizing the need for local measurements and the significance of fires in this region. The manuscript is generally well written and explained but it lacks some details.

General comments

Ln.21. What changes are you referring to?

Ln.47. What is the contribution of climate variability versus anthropogenic influences (fire and land use land change (LULC)) to the Amazon forest over the past 40 years? You mention that fire and LULC are the main disturbances for the Amazon forest, but what percentage are we talking about?

Figure 1. It would be interesting to have the outline of the Amazon forest on your map. It is difficult to estimate if, for example, the southeast site is on the border of the Amazon or in the middle of the forest. The 5 Amazon sites could be added in your Figure A14. Additionally, names of the sites could be included in the figure.

Ln. 146. Your simulations are run at almost 6x6 degree horizontal resolution. It is not specified wheter it is used globally as well as regionally. What would be the uncertainties/impacts associated with the coarse resolution of your inversion on your results?

Ln. 184. It is not clear if you have taken the average climatology 2003-2013 of CASA-GFED4 for your 2010-2018 period with a scale to adjust this climatology to your study period. More information should be provided wheter or not a scale was used, and the impact on your posterior fluxes for not having it scale. Additionally, why not using GEOS-Carb CASA-GFED v3, which has a temporal coverage from 2003 to December 2017 closer to your study period than what you used?

Ln. 206 and Ln. 492. It is not well explained how the MODIS burned fraction product is used here, regarding CARDAMON. MODIS burned fraction product has been shown to underestimate information about the burned area compared to the VIIRS product. How do you think this underestimation impacts and biases your results?

Ln. 209. What are the uncertainties associated with the forest biomass removal of the Global Forest Watch?

Figure 3. Posterior total flux estimates with Amazon vertical profile show sources of carbon in the western coastal part of the continent from October to March. This carbon source does not seem to be linked to the fires. What would be the cause? This carbon source is also observe in the annual mean with the posterior estimates and not with the prior estimates.

Ln.317. Positive NBE are observed between April and June for the western-central region, probably caused by decomposition process following years of a burning event. We should observe similar seasonality for the eastern Amazon, with positive NBE following years of burning event, or it does not seem to be the case. Could you explain what could be the differences between the eastern and western regions regarding the NBE and fires seasonality?

Ln. 330. No significant relationships between monthly posterior NBE fluxes and climate variables was observed. However, you mentioned earlier that NBE fluxes could be positive several months following a fire event which is correlated to climate variables. Have you looked at the correlation between NBE fluxes and climate variables with a time lag? Additionally, figures A5 and A6 could be performed for fire and non-fire seasons.

Figure A.11. It would be interesting to have a comparison similar to Figure 2 in order to see the differences between prior monthly mean CO_2 mole fractions for prior with CASA and with CARDAMON against respective posterior and observations.

Figure A.12. And Ln. 371. You mentioned having similar variability and flux magnitudes between your NBE and fire estimates compared to the estimates based on Naus et al (2022). It is important to notify some non-negligible differences between both estimates for the annual mean. The differences in fires between both estimates can be significant for the NBE estimates. For example, your NBE can show a

sink of carbon, like in 2010, while the NBE from GFAS and CO_opt show sources of carbon. In 2013, your NBE shows a source while NBE CO_opt shows a sink. Additionally, when both NBEs agree to have a carbon sink, the sink will be larger or not depending on the fires estimates. It is therefore important to note and conclude here that NBE estimates are not only constrained by observations but also by fire estimates, as observed in Peiro et al., (2022, https://doi.org/10.5194/acp-22-15817-2022). This seems to be particularly true for tropical regions where fires are a dominant component.

Details are missing on how you considered and separated deforestation, logging operations and ground-fires in your work and inversion. Could you elaborate?
As detailed in Andela et al., 2022 (DOI: 10.1126/sciadv.abd2713), fires in the Amazon basin can be classified into four different types (deforestation fires, propagating or forest fires, agricultural management, grassland fires occurring in pasture lands at the border of the forest.). In ln. 464 and 480, you mention agricultural and deforestation fires but without further details for your results. Knowing that these different types of fires have different characteristics, combustion, and may or may not be captured by satellite active fire detection, more explanation and detail should appear in the discussion of how these would affect your inversions and your results.

Technical comments

Ln. 58. Would suggest a comma after continues.

Ln. 75 and 78: these two sentences could be rearranged in one sentence. Additionally, NBE being a source of C to the atmosphere is repeated twice.

Ln 175. Could you develop what you mean by your posterior errors are likely to be “lower limits”?

Ln. 213. “To estimate the contribution of biomass burning emissions in Amazon” on what, can you precise?

Ln. 250. How was the 0.1 m/month evapotranspiration estimated for CWD?

Section 2.2.6. It has not be explained so far why solar radiation is introduced here and for which purpose.

Figure 2. Quality of the figure should be improved. Are the posterior with or without the Amazon observations?

Figure 5. Resolution of the figure should be improved, particularly Figure 5.a.

Ln. 325. Should be Figure A4 and A5.

Ln. 335. “Table 1”, parenthesis might be missing.