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Atmospheric Chemistry and Physics Discussions

Interactive comment on "On the ability of a global atmospheric inversion to constrain variations of CO_2 fluxes over Amazonia" *by* L. Molina et al.

Anonymous Referee #3

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Tropical South America is a geographical region where we know very little about the carbon balance on a large scale, with implications for quantifying the carbon balance over other regions. This paper examines the ability of using CO2 mole fraction measurements from four additional sites on the eastern coast of South America, relative to a control calculation that has used all other available mole fraction data. I have a few comments but none of them are sufficiently negative to prevent this work being published – they can be addressed quickly.

To some extent this is (yet) another paper that highlights the many difficulties using measurements that represent constraints on spatial scales and temporal scales that are not described well by current models. In this experiment, the model resolution is very coarse that could easily compromise its ability to capture reliably observed

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variations on certain time scales. It would be good to learn a bit more about the model error that takes this into account because it plays an important role in determining the results.

The new sites looks great but there is precious little information to judge whether they are actual useful. I assume they have been calibrated on a scale that is common to the data assimilated as part of the MACC project, but this point needs to be confirmed. More details would be helpful for this reader. I appreciate that these measurements are difficult to sustain over long periods but I am left concerned about the role of sampling frequency on the results. A simple simulation could be used to determine the ability of each site to constrain estimates of NEE and ocean fluxes. This would strengthen the ultimate message of the paper.

Incidentally, what about the ocean fluxes?

Regarding the footprints that are shown for a day in February 2009. Are these representative of the season, year? Either a more comprehensive discussion of the site footprints or a climatology of wind fields would help to explain to the reader why these sites were chosen and potentially why that can add to what we know about NEE over the geographical region.

The authors mention a comment on page 1928 line 20: "...results at ABP may reveal some local issues." What are they?

Perhaps my most serious concern is the absence of a discussion about uncertainties. How well did the model fit these new data? Can you give the reader a sense of the ratio of posterior and prior uncertainties associated with the NEE and ocean fluxes? What about the spatial correlated associated with the posterior NEE fluxes shown in Figure 8? For some of the estimates how does this reader know whether these new data have improved our knowledge of NEE? I expect the authors will respond by saying that the assimilation approach does not easily provide posterior uncertainties but I would argue that these results are difficult to interpret without this information.

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