Reply to Julia Marshall (Reviewer #2)

The authors present a clear and well-structured inverse modelling study focusing on the application of in-situ measurements from aircraft and ships to target fluxes in Tropical Asia associated with the 2015 ENSO event. The focus is on biomass burning emissions, and the result is a reduction in the fire flux emissions of CO2 compared to established satellite-based emission products, which is in line with other studies examining the same period and region but based on completely different data streams.

The topic is interesting and relevant, and it presents a good application of aircraft- (and ship-)based measurements for fluxes, and not just model validation. The figures are clear and well-designed to support the story of the paper, and the references to related studies are fully appropriate. As such, I would consider it appropriate for publication in ACP after some minor concerns have been addressed.

We are grateful to Julia Marshall for taking valuable time to review our manuscript and giving positive comments. Described below are our replies to the reviewer's comments with page and line numbers of the revised manuscript. The supplementary manuscript shows changes from the previous one.

L159-164: I had some questions about the treatment of OH and CH4 here. Rather than assuming a constant value of CH4 everywhere, it might be more reasonable to use a fixed distribution, as the CH4 decreases with altitude. This decrease is most dramatic above the tropopause, which is not considered explicitly here, but is seen throughout the atmosphere, as the sources of CH4 are all located at the surface and the sink is (overwhelmingly) in the atmosphere. Because the WDCGG estimate is based on surface measurements, this will be an overestimation for an atmosphere- wide value. However, the amount of CO2 being created from the oxidation of CH4 is quite minor and as such it doesn't really matter much in this study – it was more a comment for future work. Regarding the OH: here I guess the Spivakovsky fields distributed within the TransCom CH4 project, reduced by 8%, are meant? If so, a reference to Spivakovsky et al. (1990) should be added.

We agree with the reviewer that the assumption of the globally constant CH4 value is optimistic. However, as also pointed out, its effect is very minor, especially near the surface. Furthermore, we consider such a simple treatment of CH4 reasonable, because we performed the CO2-only inversion and did not used CO data in the inversion.

Here, we apologize for incorrect descriptions of the BVOC data of VISIT caused by misunderstanding among co-authors. We now realized that we did not consider the oxidation of BVOC, but only considered the direct emissions of CO from vegetation. Although we recognize that the oxidation of BVOC is important for simulating atmospheric CO, we think it would not significantly affect our results and conclusions in this study, because we performed the CO2-only inversion and focused on biomass burnings.

In this regard, we modified the last part of Section 2.2.2 as follows.

"In the model, the contribution of oxidation from biogenic volatile organic compounds (BVOCs) to CO is not considered yet, but direct CO emissions from vegetation are given at the earth's surface. Although the oxidations of CH_4 and BVOCs are significant sources of atmospheric CO, we treated the former very simply and did not consider the latter. Therefore, we did not input CO observations to the inverse analysis. In the inversion, the biomass burning emissions of CO, which were predominant in Equatorial Asia, were modified along with those of CO_2, as described in the next section." **[Lines 167-172]**

For the reference of the OH data, we appreciate the reviewer's suggestion. Accordingly, we added Spivakovsky et al. (2000) as the reference **[Line 166]**. Please note that, rather than Spivakovsky et al. (1990), Spivakovsky et al. (2000) might be more appropriate, because the TransCom project used the latter dataset.

I guess that these signals are really small anyhow, but it would good to quantify this, also for BVOCs: approximately how large are the different components in the simulated (prior) signal of CO2? What is essentially background (from outside of your targeted region), how much is from local anthropogenic emissions, how much from oxidation of other species, how much from the biosphere (net), and how much from biomass burning? I would be really interested to see the contribution of the different signals to the total simulated signal, even just for e.g. one simulated flight.

According to the reviewer's suggestion, we made the same figure of Fig. 4, but for different CO2 components; they are from the fossil fuel emissions, the terrestrial biosphere and ocean fluxes, and from the oxidation of CO (Fig. R1). Note that this CO have all the contributions from the fossil fuel emissions, BVOC, and the fire emissions. From this figure, we can clearly see that contributions of the fluxes other than the fire emissions are marginal, indicating that fire signals are dominant in the observed mole fraction variations over Singapore. Of course, as the model resolution is limited, we cannot exclude possibilities of contributions from local fossil emissions or natural terrestrial biosphere fluxes. Based on this figure, we added a sentence as below.

"Nevertheless, a model simulation that separately calculated CO_2 mole fractions from other different sources (fossil fuel emissions, terrestrial biosphere and ocean fluxes, and oxidation of CO) indicated that these fire contributions are dominant in the CO_2 mole fraction variations over Singapore for this period." **[Lines 295-297]**

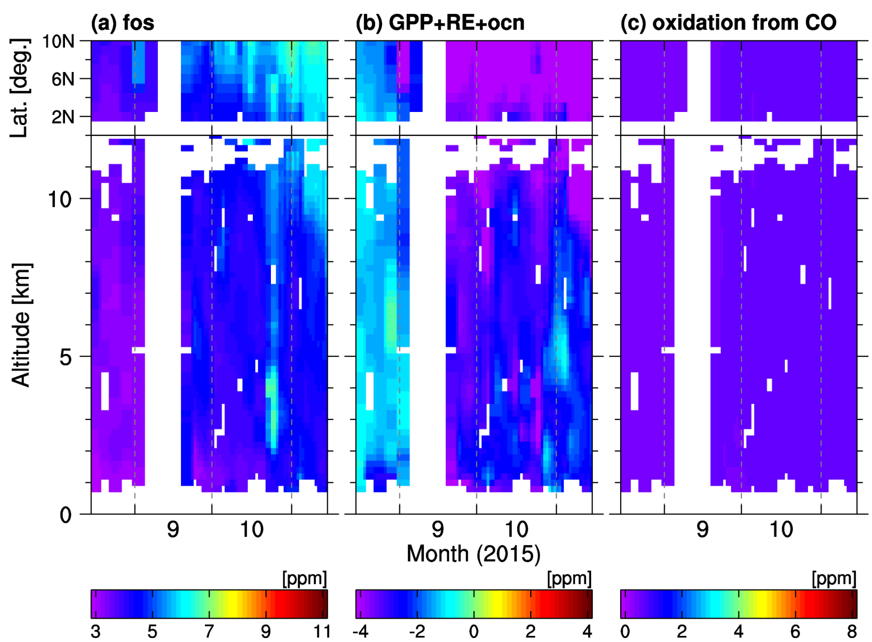

Fig. R1. The same as Fig. 4 of the manuscript but for different CO2 components: from the fossil fuel emissions (a), the terrestrial biosphere and ocean fluxes (i.e., GPP+RE+ocn) (b), and from oxidation of CO (c).

I also had a question about Figure 8 and 9 that I would like to see addressed in the text: what do you think is the reason for the large positive adjustment over the Indochinese Peninsula (Vietnam, Cambodia, Laos. . .) for this period? This is not seen in the fire prior at all, and I presume that it is a biosphere signal, but the adjustment is really large for both months, but in opposite directions. Indeed, it almost looks as if the prior for October better fits the posterior for September, and the prior for September the posterior for October. Is this a shifting in the seasonality of the biospheric fluxes from the climatological norm (or the 2003-2005 mean) due to El Nino? Some discussion of this would be welcome.

We appreciate the reviewer for pointing out the interesting feature in the posterior flux patterns. Indeed, it would be worth investigating that flux pattern change, because the CONTRAIL aircraft flew to Bangkok frequently in 2015 and the observations may have constrained the fluxes. However, in this paper, we focused only on Equatorial Asia, which does not include Indochinese Peninsula, and we also focused on fire emissions. As the fire season of Indochinese Peninsula is completely different from that of Equatorial Asia, those flux changes might be contributed by biospheric fluxes; however, we need a further analysis, which is left for a future study.

Finally, no estimate of the uncertainty of the resultant fluxes is presented, unless one considers the spread related to the different priors. Is such an estimate feasible with the inversions system presented here? Even if such a calculation is not technically feasible, some discussion of this shortcoming should be included. And one last minor point: I think it would be beneficial to explain earlier in the text (in the methods section) how the emission ratio of CO/CO2 is set. References to Akagi et al. only come much later (in the discussion).

In fact, estimating posterior uncertainties is feasible with our inversion system and we have described its algorithm in a recently published paper (Niwa and Fujii, 2020). However, it is a little bit computationally demanding, especially for the resolution we used here (dx~112km). In the last part of the Discussion section, we added some discussion as below.

“In this inversion, we did not calculate the posterior errors, which could give implications of the estimated flux uncertainties. Instead, the spread of the sensitivity tests shows a reasonable range of conceivable flux estimates. In fact, an algorithm for estimating posterior errors was developed by Niwa and Fujii (2020) and it could be applicable to the inverse calculation. However, we left it for a future comprehensive inversion because the algorithm is computationally demanding, especially for the model resolution we used here.” **[Lines 568-562]**

For the emission ratio, accordingly, we added a description of the emission factors of CO2 and CO in Section 2.2.5 as follows.

“Note that both the datasets use similar emission factors of CO_2 and CO based on Akagi et al (2011); particularly, the same emission factor from Christian et al. (2003) was applied to peatland, from which a large part of fires arises in Equatorial Asia (van der Werf et al., 2017).” **[Line 245-247]**

Besides this, I made some language corrections/suggestions in the document itself, which I have uploaded. I hope that some of these may be helpful.

Please also note the supplement to this comment: https://acp.copernicus.org/preprints/acp-2020-1239/acp-2020-1239-RC2- supplement.pdf

We really appreciate the corrections/suggestions the reviewer made. All of them are incorporated in the revised manuscript.