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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Estimation of fire-induced carbon emission from Equatorial Asia in 2015 by using in situ aircraft and ship observations" *by* Yosuke Niwa et al.

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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.

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.

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.

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.

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).

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

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