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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.

Anonymous Referee #1

Received and published: 25 February 2021

This paper analyses CO2 (and CO) flux anomalies from the Southeast Asia region, using a data assimilation method for estimation of CO2 fluxes at transport model grid resolution. They have used unique observational datasets and a model framework. The region has already been widely studied by various groups and this paper would be a nice addition to the growing list of literature. My judgement is that the authors demonstrate a capacity that would be utilised in near-real-time in the future or on regular basis. The paper is generally well written and can be accepted for publication after a major revision, in my opinion. Please see my specific comments below for revisions, which I hope are useful for you to consider.

line 33: should you say 2nd strongest - please check MEI or something like that?

Section 2.1: Just to make sure, you haven't use any other observations apart from the CONTRAIL and VOS in this study. Could you be explicit please.

line 187: a bit of details or a reference is needed for the BVOC model here, which ones are considered etc. and how CO prodection is modelled.

Table 1: GPP, RE errors are quite large, which is nice. why is the error for fire emission is set to greater over the rest, compared to eq. asia?

line 221: CDIAC data are used for what time period ?

line 241: how were they derived ? did you model BVOC oxidation ?

line 258: should work in principle because you are starting with an inversion flux, but not sure 2-month of spin-up is sufficient

Figure 3: Are these plots better shown by "Model - Observation" for the lower 2 panels?

Figure 4: I wonder if you really need Figure 4

Figure 6: Why not show the CO2 time series as well, as that is the main focus of the paper?

line 343: Delete "We do not consider CH4...". I think, none will as ask for CH4 at this point.

line 352: since you are putting less prior flux uncertainties outside the domain of this analysis, this is bound to happen. do you have another simulation with uniform treatment of prior flux uncertainty ?

line 365: One of the problems here is the over interpretations of the flux differences, without giving a range of uncertainty e.g., how significant are 31 Tg vs 2.7 Tg mentioned

Figure 8 & 9: please mention in the caption if the prior flux map is same for both the inversions (C_CG & C_NO). and also show the posterior - prior for the C_NO case. hopefully you will find a place to show the colour bar (on the right?)

line 378: Is this statement true? see over the Papua New Guinea or the southern part of Borneo, where are there are large differences in patterns

Figure 11: Nicer to have a common x-axis for these plots for easier reading.

line 410: not clear what you mean by "its degree"?

Figure 12: A bit surprised to see posterior RMSDs are quite often exceeding the prior values for October

Figure 13 and associated text: I am not very sure of usefulness of this discussion. You need to take in to account the ageing of the air mass, i.e., the influence of near and far fields in constructing the emission ratios. It is possible to implement such an analysis using trajectory model for instance.

Section 4: not sure if you need these detailed discussions, a table of fire emission estimation can be given in the results section or here for improving readability. Then some of the text, say on the modelling limitations, could be integrated with the conclusions or new subsection in Results

Section 5: Conclusions can focus on the outcome of this paper, with minimal or no referencing to old papers. Some can be kept for the significance of the paper, e.g., the ENSO related flux variability, which has been discussed in the results section.

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