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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Quantifying pyroconvective injection heights using observations of fire energy: sensitivity of space-borne observations of carbon monoxide" *by* S. Gonzi et al.

Anonymous Referee #2

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The authors present a study on injection heights of biomass burning emission. They employ suitable observations, i.e. MODIS radiances over fires and MOPITT CO column, as well as suitable models, i.e. a newly adapted version of the 1d plume rise model of Freitas et al. 2006/2010 and the GEOS-Chem CTM. This subject is important, timely and highly suitable for publication in ACP. The abstract appears to give a good summary of the study and the conclusions that can be drawn. Unfortunately, the main text is frequently vague, some phrases are not written in a scientifically correct manner, various information is presented without explicitly stating the connections, and

some rather trivial statements are formulated as if they were new conclusions. This conveys the impression that the author do not really know what they are writing about. That is a pity because I am sure that they know better. Overall, the manuscript is in my opinion of such poor quality that I even cannot identify specific major modifications that would lead to acceptable quality for publication in ACP. Therefore, I recommend to reject this manuscript in its current form. At the same time, I would like to encourage the authors to carefully reconsider the arguments leading (hopefully) to the conclusions in the abstract and re-submit a more thoroughly prepared manuscript. Below, I am listing what I consider the major flaws of the manuscript, with exemplary justifications.

1. The scientific goal of the study is not clear.

The brevity of the Introduction is appreciated. But instead of motivating a research question from the existing literature, the authors only state what they are doing in the study. But answers to any of the following questions are missing: Why is it useful to do this? Which new insights are to be gained? Which scientific questions shall be addressed with this study? In which context are they important?

The only statement remotely related to the purpose of the study is already in the second line (towards the end would be more appropriate) of the Introduction: "We focus on the influence of fires on determining the atmospheric distribution of carbon monoxide". It remains unclear to me what "determining the atmospheric distribution of carbon monoxide" means.

2. All but one conclusions stated in the Concluding Remarks section are either self-evident or not proven in the study.

"We used MODIS FRP and fire size observations for 2006 to improve understanding their relationship and the resulting injection height": So, what is the improved understanding of the relationship between FRP and fire size observations? I did not find it in the manuscript.

"We did not find a robust relationship between FRP, fire size and injection height": This conclusion is very weak in itself, e.g. it does not answer the question whether it is possible to find such a relationship. Furthermore, simply parameterising injection height with fire parameters is not state of the art since Sofiev et al. 2012 included boundary layer height and Brunt-Väisälä frequency in the free troposphere in their own parameterisation of injection height. Therefore, I don't see what new insight this conclusion should represent.

"different prescriptions of injection height do have an impact on atmospheric CO concentrations over intense fires": It is quite trivial that changing the injection altitude of CO will change the resulting concentrations of CO at the different altitudes.

"MOPITT can differentiate between different prescriptions of vertical transport of CO emissions.": I agree that this is one conclusion of the study.

"As a consequence we cannot quantify the impact of injection heights on the inference of CO emissions from MOPITT CO profile data": I cannot understand why the inability of the authors to quantify this impact should be a consequence of the previous statement. In any case, it remains unclear throughout the paper what the authors actually mean by "impact on the interference of CO emissions from MOPITT CO". This would need to be defined with a physical quantity.

"The major implication from this result is that outside of detailed case studies, use of MOPITT to quantify biomass burning emissions is biased towards the very largest fires that can perturb substantial sections of the observed atmospheric column.": This conclusion/implication is something that can be expected a priori. But I did not see an argument for it in this study.

"Space borne observations of FRP, fire area and other land-surface properties together with atmospheric concentration measurement remain our best constraints for biomass burning emissions and associated vertical transport.": There is no investigation of alternative constraints, therefore the conclusion "best" cannot be drawn. Furthermore,

it is unclear what the authors mean by the "land-surface properties". This is far to speculative and unspecific to be of any scientific value.

"A new space-borne mission that retrieves biomass burning trace gases and associated land-surface properties would be required to address some of the gaps in current understanding": This speculation is not supported in any way by the study. In particular, other satellite instruments that observe CO, i.e. IASI, are not discussed and not even mentioned.

"The ideal mission would have a vertical resolution < 1 km in the lower and free troposphere and a ground-pixel size of 1 km or less.": The resolution of CO observations from space is not discussed in any way in the paper. Therefore, this speculation, which even comes across as a conclusion, is completely unfounded.

3. The presentation of the study lacks consistency, careful preparation and scientific rigour throughout (except for the abstract). Here are just some examples:

The plots in Fig. 4 are inconsistent with the text and caption: In the left panel ZTOP is plotted at 0.25 and 4 km, while text, caption and text box under plot claim it to be 0.1 and 3.3 km. Analogous error in right panel.

The same quantity has multiple names, partly wrong ones, throughout the manuscript, e.g. "active fire area" (p.22551, l.26), "actual burnt area" (p.22557, l.22 & p.22579), "burnt areas" (p.22557, l.23), "active burnt area" (p.22557, l.24)

“2.2 MOPITT column observations of CO” (p.22552, l.14) appears to be contradicting the first sentence of Section 2.2: “We use MOPITT v5 CO profile retrievals”. Columns or profiles? It should be made clear that both columns and profiles are used. Additionally, in the latter text, it is not always clear which of the two are being discussed.

p.22551, l.20: What is "total amount of column water"? Such terminology should be exact, not just somehow similar.

"FRP and Active Fire (AF) area for each fire are computed with the dual-band approach" (p.22551, l.7) is a contradiction to "FRP is computed using the MIR band" (p.22551, l.27).

"NIR- and TIR-only products have DOFs peaking at 0.1–1.0 and 0.5–1.5, respectively" (p.22552): My understanding is that the degree of freedom of a satellite retrieval is just a scalar number. So, it cannot "peak" and the statement does not make sense.

"We assume a fuel moisture of 10 %, calculated from the colocated GEOS-5 relative humidity profile" (p.22554, l.3): This needs more explanation; why does fuel moisture end up to be constant globally when it is calculated from relative humidity profiles with large variability in time and space? Also, I would expect a strong dependence on the history of humidity. Finally, why is the whole profile needed?

"This supports the idea that above a certain threshold of fire energy released the buoyancy induced by the fire can overcome locally stable meteorological conditions." (p.22559, l.2): What is vaguely labelled as "idea" here is the basic understanding that when enough convective energy is released then convection occurs.

"one might expect Canada to have a larger number of high intensity, large active fire area fires compared to Russia" (p.22559, l.24): It would be interesting to see whether this study supports the earlier study by Wooster and Zhang (2004). But the presented results are not discussed in relation to this statement. So, why should it be in the text?

"injection height mean statistics" (p.22560, l.19): As far as I understand, the authors did not do any statistics on the mean values. Instead they simply compare mean values for five different cases.

"observed by MOPITT space with MOPITT data" (p.22561, l.26): What is "MOPITT space"?

"Previous work used the GEOS-Chem model to infer CO emissions from MOPITT v5 CO profiles between June and August 2006 (Jiang et al., 2012). They found that posterior emission estimates were sensitive to the pressure level used: GEOS-Chem

over(under)-estimates CO at lower (middle and upper) levels." (p.22562, l.1): Here the manuscript just lists related facts together, but if falls short of explicitly drawing the conclusions from them.

"We have reported that MOPITT averaging kernels are often broader than the vertical sensitivity necessary to distinguish between different prescribed vertical injection heights due to surface heating." (p.22563, l.9): This was not reported in this study. Even the word "sensitivity" does not appear on any of the previous pages. So, it is simply a false claim.

Figs. 1, 7: Some of the labelling is too small to be legible when printing the printer-friendly version.

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