S. Gonzi. P. Palmer, R. Paugam, M. Wooster and M. N. Deeter
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We thank the two anonymous reviewers for their thorough comments and we respond (R) in the following to each reviewer (italics) individually. We also thank and respond to the comments of Ralf Kahn and Maria Val Martin that have helped to strengthen the paper. We mark changes to the text in the revised manuscript with boldface.

Response to Reviewer #1:

'Reviewer Comments', Anonymous Referee #1, 14 Oct 2014

Pg. 22550 – lines 15-16: In addition to Ichoku and Kaufman and Kaiser et al., Relevant work by Vermote et al. (2009, JGR) has outlined an approach for calculating smoke emissions (black and organic carbon) from FRP. You may consider referencing this recent work as well.

R: Good suggestion we now have referenced the relevant work of Vermote et al. 2009 in the revised version.

Pg. 22552 – line 12-13: You compare FRP and AF to previous work; was the comparison good? Inclusion of a sentence or two on how well the your analysis in this paper compared with your previous publication from 2011 (a correlation, figure, etc.) would strengthen your claim.

R: We have removed this sentence in the paper. Our work in 2011 did not make use of any FRP and fire area data.

Pg. 22552 – line 25-26: I think you mean to say "these MOPITT CO profiles are biased when compared to North American…

R: Thanks. We changed the text accordingly.

Pg. 22552 – line 28: how did you "thin" the data? Which data was selected for removal?

R: In the revised manuscript we have included an additional sentence that explains how we thin the data: "We use the first three profiles in a given time step that satisfy the following criteria: a) DOF >1.3, and b) CO profile concentrations at the 500 hPa pressure level >40 ppb".

Pg. 22553 – lines 5-6: Can you "prove" that it does not affect your final analysis? A figure or number may be helpful here.

R: We do not routinely store a complete set of model output, so we cannot provide a full analysis. However, we did a recalculation of parts that contribute to Figure 7 of the original text (Figure 7 and 9 in the revised manuscript) where we use the following criteria: (a) DOF > 0.8 (instead of >1.3) and (b) we use a maximum of 30 profile observations in the grid box (instead of 3). We have included the resulting new Figures in the Appendix of this response. As one can see by direct comparison to Figure 7 of the original text (or Figure 7 and 9 of the revised manuscript) the overall picture stills hold true, and although there are likely to be some smaller localized differences upon closer inspection these do not alter the primary results or our conclusions from them.

Pg. 22553-4 : The description of the calculation of 'heat flux' is a bit confusing to follow. You are asking the reader to make a leap from FRP to 'heat flux', but the description of this leap is scattered throughout sections 2 and 3. It may be worth considering moving this description to its own paragraph in the previous section (2.1) where FRP is discussed. Or, moving it to the beginning of section 3.1, i.e. line 23, right before "We drive the model…

R: We agree with the reviewer and we have moved the description of "heat flux" to its own subsection in Section 2.2.

Pg. 22555 – lines 23-28: In the control run –are total emissions the same in each of the 15 boundary layer levels or is there a gradient from the surface to the top of the BL? If the distribution is uniform, you should explain why you chose to distribute this way. In general, a bit more detail is necessary.

R: We have included one more sentence in the revised manuscript to explain the choice of injection height distribution. The reviewer is right, the emissions are distributed uniformly for each profile, but will give a non-uniform distribution if one injection height is in the PBL and the other one is in the free troposphere; we distribute mass from the PBL to the injection height if the injection height is above the PBL. However, in section 4.2 we briefly discuss the sensitivity by choosing a parabolic profile.

Pg. 22558-60: This is a very strong section (and Figure 6 is very strong as well) comparing your plume rise model to the Val Martin work (though Ralph Kahn's short comment should be considered for technical corrections to the description of Maria's work). You could also consider strengthening your argument by comparing your results with a similar paper- Tosca et al., 2011 (JGR)- that quantified plume injection heights over an entirely different region of the world – Indonesia.

R: In the revised manuscript we have carefully included the supportive suggestions and discussions, as pointed out by Ralf Kahn and Maria Val Martin.

Pg. 22563 – line 6: Do you mean Figure 8 instead of Figure 7?

R: We thank the reviewer for spotting this and we have included the correct Figure 8 reference in the revised manuscript.

<u>Figure 3:</u> Each panel in this figure needs to be labeled (e.g. "A", "B", etc.) Additionally - something seems "off" with the x-axis on the figure on the bottom right. In the panel directly above it, it seems hat A (ha) maxes out at 10,000, but the axes on the bottom right panel only maxes out at 1,000.

R: We thank the reviewer for spotting this. The reviewer is right that the last bar on the graph on the x-axis of Figure 3D) does not correspond to the range of active fire area as given in Figure 3B). This was a plotting error and does not affect the results of our analysis. The last bar in Figure 3D) represents the range of 1,000 – 20,000 (ha), where the upper value of this range is the maximum value in October in Figure 3 B). We have redrawn Figure 3D) and have included this information in the corresponding Figure 3 legend.

<u>Figure 4:</u> is very confusing to the reader. I think at a bare minimum the color labels need to be included in the actual figure and not just the caption. I also think it is confusing to have so many axes on a single figure. Consider breaking up each figure into separate figures: e.g. Potential temperature, Temperature and Humidity, and perhaps two columns: A "low ZTOP" and "high ZTOP" and instead of plotting the actual ZTOP, just list it as a number.

R: We agree with the reviewer that Figure 4 is rather busy. We followed the reviewer his suggestion and we broke up the Figure into 4 individual subplots A), B), C), and D).

Response to Ralph Kahn and Maria Val Martin

Notes on Gonzi et al., Quantifying pyroconvective injection heights using observations of fire energy, ACPD, September 2014.

<u>P 22550, lines 10-13.</u> Val Martin et al. [2012] vertically distribute the smoke emissions using the same 1-D physical plume-rise model of Freitas et al. [2007; 2010] that is used in the current study. The physics in this prognostic model includes the dynamical heat flux at the lower boundary, the atmospheric stability structure, parameterized entrainment, and latent heat. The point of the Val Martin et al. paper is to test the sensitivity of this leading plume-rise model to input parameters and to the underlying parameterizations, using MISR plume-height retrievals for validation.

Although we agree this model does not treat explicitly the role of storm systems in pyro-convection, we conclude in Val Martin et al. [2012] that there are fundamental uncertainties in the plume-rise model even when storm-related factors do not represent major energy sources for the plume. Specifically, uncertainties in available dynamical heat flux constraints, derived from the most widely used FRP and fire area methods, remain limiting factors in model predictive ability, and even more importantly, the entrainment parameterization itself might dominate the factors contributing to model indeterminacy.

R: We agree with that comment that a plume rise model, although prognostic in its nature, is limited by the input parameters. Even if all the input parameters were known with reasonable accuracy, the plume rise model itself is a simplification which relies on some assumptions and parameterizations.

R: We have appended a sentence to clarify our statement: "The nature and availability of input parameters and their relation to the progonostic model description often prohibits a better method for redistributing mass."

P22551, line 7 ff. You might consider referencing (1) Peterson et al. [Remt. Sens. Env. 129, 262-279, 2013], where they go beyond earlier studies in analyzing and refining the bi-spectral approach for deriving FRP from partly filled MODIS pixels and accounting for atmospheric transmittance, and/or (2) Peterson et al. [JGR 2014, doi: 10.1002/2013JD021067], where they apply this technique, and additionally demonstrate the role of upper-level moisture in fire energetics.

R: We thank the reviewer for suggesting some more up-to-date relevant references. We have included those two references in the revised manuscript.

P22556, lines 2-6. We find from MISR stereo-height observations that if smoke is injected above the boundary layer, it tends to accumulate in layers of relative stability in the free troposphere [Kahn et al., JGR 2007; Val Martin et al., ACP 2010].

R: In the current work, in the absence of any other information and additional constraints, we use the scheme as explained in section 3.2 were we inject emissions from fires as follows: A) from the surface to the injection height in case of a plume rise calculated injection height within the boundary layer, B) from the top of the atmosphere to the injection height in case of a plume rise derived injection height in the free troposphere. However, often there are more than one MODIS–FRP derived injection heights in a grid box - and the resulting injection height profile is then a weighted combination of cases A) and B).

P22559, lines 2-4. As you know, this depends quantitatively on the atmospheric stability structure as well as the heat flux.

R: We agree and we have included a sentence in the revised manuscript as not to give the reader the false impression that the stability structure itself is not important:

R: "Above a certain threshold of fire energy release rate and consumed active fire area, the buoyancy induced by the fire can overcome locally stable meteorological conditions, with resulting injection heights typically >3.5 km."

P22559, lines 6-18. The description of Val Martin et al. [2012] here is not quite accurate. The plume height climatology used in our paper was derived from MISR stereo height retrievals using the MINX algorithm [Nelson et al., Remt. Sens. 5, 4593- 4628, 2013], not from MODIS, covering the years 2002 and 2004-2007. The MISR- retrieved heights were used to test injection heights calculated by the Freitas et al. [2007; 2010] model, initialized with values from all combinations of four *widely used methods for deriving fire heat-flux, and four widely used methods for deriving fire area, most of which rely upon MODIS data one way or another.*

We found that the dynamic range of model-derived heights tends to under-predict the observations, i.e., the model sometimes over-predicts for low injections, and nearly always under-predicts for high injections [See Figure 2 of Val Martin et al., 2012], similar to the model result for CO in your Section 4.2. Our conclusion covers the range of injection heights over both the boundary layer and the free troposphere.

R: We thank the reviewer for pointing this out. We agree with Kahn and Val Martin here and apologise for our potentially misleading text (i.e. giving the improper impression the climatology was based on MODIS). We have changed the text and interpretation accordingly by including a reference to the MINX algorithm:

R: "Previous work derived a plume height climatology based on a compilation of derived MISR stereo height retrievals using the MINX algorithm (Nelson et al., 2013)."

To your statement: "... that finding a robust relationship with injection height may well be as uncertain as using the plume rise model itself," we agree that there is uncertainty in the measurement-derived statistics due to limited coverage of smoke injection height from stereo-imaging observations (and even more so from other techniques, such as lidar), especially diurnal sampling. However, we concluded that given current plume-rise model uncertainties, simply using a statistical summary of the relationship between satellite-observed fire properties and stereo-derived plume heights (which you refer to as a look-up table) would probably be preferable, for global climate model applications, compared to running an embedded, state-of- the-art plume-rise model with currently available constraints and parameterizations. And we provided the best statistical relationship we could from the five years of North American plume data in our study, with the understanding that similar relationships would need to be derived for other fire regimes.

R: This is an interesting comment. We agree with Kahn and Val Martin that implementing a plume rise model, even a simplified one, is no easy task. We also agree that compiling relevant injection height data (as presented in the work of Val Martin and Kahn) as a proxy for running a full featured plume rise model in a climate model is perhaps a more sensible approach to take, especially if one considers that current climate models do not routinely consider biomass burning injection heights; one could even argue that a climatological injection height value is most appropriate for a climate model. But as these (climate and plume rise) models improve and/or we gain access to better or wider ranging parameters, the parameter space that the compilation tables will have to describe soon becomes unwieldy so we argue that investment in embedding (and developing) a plume rise model within a larger-scale model is best in the longer-term.

P22559, lines 18-19. Looking case-by-case, we concluded that the parameterization of entrainment is also likely to make a leading contribution to uncertainty in the plume-rise model. (Also relevant here: P22562, last 2 lines + P22563, lines 1-3, where you introduce a parameterization for entrainment in the CO plume.)

R: We agree and we changed the text and include now this statement in the revised manuscript:

R: "We emphasize that we agree with the findings of Val Martin et al., 2012 that the uncertainty of detrainment and entrainment in the plume rise model could be a leading contribution to the overall error of the plume rise model description itself."

P22560, lines 7-13. We think Val Martin et al. [2012] instead of Val Martin et al. [2010] is the intended reference here.

R: We apologise for this egregious typo and we have placed the correct reference into the text in the revised manuscript.

Response to Reviewer #2:

'Review of Gonzi et al. "Quantifying pyroconvective injection heights using observations of fire energy: sensitivity of space-borne observations of carbon dioxide" ACPD 2014', Anonymous Referee #2, 14 Nov 2014

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.

R: We briefly describe in the Abstract what the reader can expect from our work and we give a more detailed explanation of our research motivation in the introduction and subsequent sections of the paper.

R: It has long been recognized in the scientific literature that biomass burning and subsequently pyroconvection is an important unknown in global models. Emissions from fires injected into the free troposphere are likely to travel longer distances and thus contribute to changes in atmospheric composition further downwind from its source. This is in contrast to emissions remaining in the planetary boundary layer, which are more likely to stay closer to the source fire. However, implementing the effect of pyroconvection in models is a difficult task and is currently done in an ad-hoc way. Satellite retrievals are capable of observing strong signals from biomass burning (e.g. fire radiative power) and map their variation over space and time, but are unable to vertically resolve pyroconvection effects except on a crude vertical resolution.

R: This study is the first in its nature that uses fire radiative power observations from MODIS to a) inform a plume rise model which is implemented in a global chemistry transport model to calculate the resulting injection height on a global scale and b) investigates the sensitivity of MOPITT observed CO (carbon monoxide) profile retrievals to the calculated injection heights, which as a consequence alters the posterior top-down observed CO emissions estimates.

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.

R: Our aim of this study was not to investigate the relationship between fire radiative power and active fire area, although they of course do relate to each other (i.e. a fire with a larger area of combustion is likely also to have a higher FRP). It is true we presented in Figure 1 and Figure 3 a compilation of heat flux (fire radiative power) and corresponding active fire area data. However, heat flux and active fire area are input parameters into our plume rise model.

R: We showed in Figure 4 and Figure 5 that injection height is not always a strong function of heat flux and active fire area, and that the atmospheric stability is often the limiting factor.

W e 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.

R: We agree with the reviewer that this conclusion will not hold true in a general sense. However, we did not state that finding a robust relationship is not possible. It is clear from our text that our statement is based on the available data and model being used. We were comparing our work to the work of Maria Val Martin and Ralf Kahn, who used similar data and models to those used in our study, although they came to different conclusions and did establish a (albeit weak) relationship between FRP and injection height.

R: In the revised manuscript we have changed the text to make it explicitly clear to the reader that we do not intend to draw conclusions in an absolute sense - rather we base our findings on the available data and methods we apply - and that there may well be a way to parameterize injection heights by sidestepping a plume rise model:

R: "Based on our data and models we did not find a strong relationship between FRP, active fire area and injection height. This is in contrast to other studies (Sofiev2012). We suggest based on our analysis a robust relationship may be as uncertain as using these data to determine scene specific initial conditions for a 1-D plume rise model."

"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.

R: We agree with the reviewer that changing the information how emissions are injected into the atmosphere will change the concentrations of trace gases (e.g. CO) at different altitude levels. But this obvious statement does not answer the questions: a) is the model sensitive to different prescriptions and b) is MOPITT or a satellite instrument sensitive to it. This is a non-trivial challenge.

R: We have clarified the text by the following statement: "We demonstrated using a plume rise model that different prescriptions of injection height do have an impact on the distribution and concentration of model CO over intense fires. However, transformation of model CO concentration into MOPITT measurement space using scene dependent averaging kernels greatly reduces this impact. This is largely due to the vertical broadness of averaging kernels. Therefore, it cannot a priori be assumed that MOPITT is sensitive to different prescriptions of biomass burning injection height."

"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.

R: As we have demonstrated and emphasized in this paper MOPITT may be able to see the impact of injection height, but the fire has to be powerful enough to overcome the atmospheric stability barrier. The "impact" would be for example a substantial change in a posteriori inferred top-down observed CO surface emission estimates (e.g. Tg CO/yr).

R: The reviewer is right and raises a good point, and we agree that the text as it was being presented could be misleading. We therefore have changed it to:

R: “We have shown examples over large fires where MOPITT measurements can differentiate between different prescriptions of the vertical transport of CO coming from fires. But those instances are relatively rare, and for most fires MOPITT measurements of CO are largely insensitive to the injection height. As a consequence injection height does not significantly affect CO emission estimates inferred from MOPITT data.”

"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.

R: With all due respect, we disagree with the reviewer here. It may be true that one can assume that large fires will have a substantial impact on the atmospheric composition and vertical structure of the atmosphere. But it is not a priori clear to what extent MOPITT is able to see those disturbances.

R: As we have shown, e.g. Figure 6, the majority of biomass burning injections are in the PBL (planetary boundary layer). MOPITT's broad averaging kernels meant that, whilst it can detect the presence of CO in an atmospheric column (including down to the PBL), it is not sensitive to the location of the CO within that column unless the injection height and mixing ratios are unusually high , which only occurs occasionally and always over the larger fire events rather than the numerous more standard types of fires. We also need to mention that a “large fire” does not always mean rapid injections into the free troposphere as intense fires with large active fire area, although with substantial biomass burning emissions, will not always have enough updraft into the free troposphere. Consequently this will affect the interpretation of data as observed by MOPITT.

"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.

R: While this statement is unsubstantiated it is obvious that the more and different data we apply to this challenging problem the better chance we have at understanding and describing the rapid atmospheric vertical mixing associated with biomass burning. We do however appreciate the concern of this reviewer and have removed “best” from the statement in the revised manuscript:

R: “Space borne retrievals of FRP and active fire area, together with atmospheric concentration measurements of fire-emitted species such as CO, are more effective together than individually when used as constraints for biomass burning emissions and their associated vertical transport. More thorough use of these types of data may, however, require assimilation within a model that explicitly includes these observed parameters."

"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.

R: IASI is mostly sensitive to mid tropospheric/lower stratospheric regions. We do not rule out that we will use IASI data in a future study to complement our data.

"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.

R: With this statement we wish only to stimulate further discussion about a future mission concept focused on biomass burning and its technology. This statement follows on naturally from the main conclusions of the paper.

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.

R: We thank the reviewer for spotting this, and for the valid points made. The analysis is unaffected by our mistake in plotting the 4 injection heights slightly higher than they should be (e.g. 3.9 instead of 3.3 km). We have included a new Figure 4 in the revised manuscript which shows the right vertical lines.

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).

R: The reviewer is right and we agree that it is not good form to switch between different designations of the same parameter, for which we apologise. We have changed every occurrence of fire "area" to one common name "active fire area".

"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.

R: We agree with the reviewer. MOPITT retrieves profiles and columns. Throughout the text we make use of MOPITT profile concentrations [ppb] and not columns [molecules CO cm^{-2}]. We have changed the title of subsection 2.3 accordingly to: "MOPITT Profile Observations of CO"

R: However, in Figure 7 (Figure 7 and 9 in the revised manuscript) we make use of total columns of CO (molecules CO/cm2) which are being calculated from the MOPITT profiles [ppb] and model profiles [ppb], respectively.

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

R: The fully correct terminology is "total column water vapour" and the unit is [kg m^{-2}]. We made appropriate changes in the revised text.

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).

R: We apologize for the perhaps unclear text. In the revised manuscript we have rewritten parts of section 2.1, though the original meaning still remains true.

"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.

R: The reviewer is right the DOF cannot peak for one single profile but the DOF is not a constant number. But we use it in "plural" and given an ensemble of profile retrievals there will be profiles with low and high degree of freedoms, respectively.

"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?

R: Fuel moisture is assumed to be constant as a first initial guess. However, environmental humidity is variable in space and time (which we describe using GEOS-5 meteorology). We agree with the reviewer this sentence was misleading we have reformulated it to:

R: "We assume a vegetation fuel moisture of 10%, which we add to the existing atmospheric levels of calculated colocated GEOS-5 relative humidity profile

"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.

R: The reviewer has a valid point and we also think it is better to remove 'idea' from the sentence. We have reformulated the revised text to:

R: Above a certain threshold of fire energy release rate and consumed active fire area, the buoyancy induced by the fire can overcome locally stable meteorological conditions, with resulting injection heights typically >3.5 km.

"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?

R: The reviewer touched upon a valid point and we concur with him that we did not discuss our results in relation to the Wooster and Zhang 2004 paper in enough detail. We have removed this statement from the revised manuscript so as not to complicate things.

"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.

R: For the revised text we rewrote the text to: "The corresponding injection height means are similar for all vegetation types, with the exception of agricultural vegetation for which the mean height is < 5 km."

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

R: Output from a model, in our case GEOS-Chem (MODEL), cannot easily be compared to a profile retrieved from a satellite instrument measure (e.g. from MOPITT; which records data at N pressure levels). However, every MOPITT profile (and satellite retrieved profile in general) comes with a so called averaging kernel, which is a NxN matrix. This averaging kernel (AVK) can be used to transform model output (interpolated onto the MOPITT pressure grid) into the so called "MOPITT space (MS)" by the following relation:

Profile_MS = MOPITT_apriori + AVK *(MODEL – MOPITT_apriori)

where MOPITT_apriori is the a priori MOPITT profile and Profile_MS is the model profile (MODEL) in MOPITT space. Profile_MS can then be used to compare it with the retrieved MOPITT profile.

"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.

R: We think the paper of Jiang et al 2006 is relevant to our work and we have augmented the text by referencing our Figure 8 to the work of Jiang et al 2006. We have included the following text in the revised manuscript:

R: "Previous work used the GEOS-Chem model to infer CO emissions from MOPITT v5 CO profiles collected between June and August 2006 (Jiang et al. 2012). This work found that posterior emission estimates were sensitive to the pressure level used: GEOS-Chem over(under)-estimated CO at lower (middle and upper) levels. The authors did not account for injection height however, and as Figure 8 shows, accounting for injection height will not necessarily reduce the CO concentrations within the boundary layer. Figure 8 shows, for the daily FRP cycle, that accounting for injection height will increase the CO concentrations (bias >0%) in the PBL between the latitude cross section -10 and -20 degrees, but will decrease CO concentrations between 0 degrees and -10 degrees, respectively. The decrease of CO concentrations is a consequence of the injection height and model transport and corresponds to the location of maximum injection heights (see Figure 1 F) and G)) in Africa. Emissions injected into the free troposphere are quickly advected, hence the positive bias (control run > model with injection height).

"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.

R: We reworded it to: "We argue that MOPITT averaging kernels are too broad to distinguish between different prescribed vertical injection heights due to surface heating."

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

R: This seems to be mostly an editing issue. For the revised manuscript we have replotted Figure 1 and we split up Figure 7 into 3 individual larger sized Figures 7, 8, and 9.

APPENDIX: Response to reviewer #1. The 3 plots correspond to Figure 7 of the original version (and Figure 7 and 9 of the revised one).