# Reply to Anon Review #2

We thank the reviewer for their helpful and insightful comments. In particular, many of the papers suggested were new to the authors and have helped develop their understanding of the field. Inclusion of these references should improve the background, context and justification for the study. Please see responses to specific comments below.

***1. P6064, line 20. Regarding the use of coupled mesoscale model to study biomass burning aerosols, I recommend to include the following references. They are among the earliest on this topic and indeed developed a coupled model; in this model, the hourly smoke emission based up GOES fire product is also used. These information should be included in the manuscript as later, the authors did start to talk about diurnal variation of emissions.***

***Wang, J., and S.A. Christopher, 2006: Mesoscale modeling of central American smoke transport to the United States, 2: Smoke regional radiative impacts on surface energy budget and boundary layer evolution, J. Geophys. Res., 111, D14S92.***

***Wang, J., et al., 2006. Mesoscale modeling of Central American smoke transport to the United States, 1: "top-down" assessment of emission strength and diurnal variation impacts, J. Geophys. Res., 11, D05S17.***

These references have been added.

***2. P. 6065, :10-25. I recommend to include the following paper in the discussion of large difference in emission estimate. This reference can also provide a support later in the manuscript for increasing the emission by a factor of 5. Indeed, the reference below show the even on monthly and regional scale, the emission differences among current existing operational fire emission databases can be more than a factor of 10. But it does show smaller differences among those top-down estimate.***

***Zhang, F., et al., 2014, Sensitivity of mesoscale modeling of smoke direct radiative effect to the emission inventory: A case study in northern sub-Saharan African region, Environmental Research Letter, 9, 075002.***

Reference has been added. In addition, this paper has been referenced in section 2.2 in discussing the use and justification of the enhancement factor with the added line after “The need for this factor highlights the difficulties and uncertainties in estimating fire emissions using current observations and understanding.”:

“Zhang et al. (2014) have shown existing emission inventories can differ by a factor of 10 in some locations, although top-estimates tend to show less variation.”

***3. P. 6066 – 6067. It is important to recognize that there are many free parameters in the fire plume rise model that can not constrained by the observations, including heat flux and entrainment rate. While the plume rise model is physically based, several studies have shown that a simple fixed injection height approach may give very reasonable results in simulating the vertical profile of smoke aerosols. In other words, more sophisticated method may not yield good results in practice, although this should not prevent us from developing and improving plume rise model. So, some discussion on the “both sides of the coin” is needed here. Wang et al. (2006, reference above), Yang et al. (2013), and Wang et al. (2013) specified injection height at 1.2km, 0.8km, and 0.7km for fires in Central America, Sub-Sahara, and southeast Asia region, respectively, which yield consistent results when compared to either ground-based or CALIOP data. I recommend authors to include these references along with Colarco’s paper into the discussion on the importance of inejection height, and how it is now treated in other studies. These references also support the later part of the manuscript in which plume rise model injects too much aerosols into the free troposphere.***

***Yang, Z., et al., 2013, Mesoscale modeling and satellite observation of transport and mixing of smoke and dust particles over northern sub-Saharan African region, J. Geo- phys. Res. Atmos., 118, 12,139-12,157.***

***Wang, J., et al., 2013, Mesoscale modeling of smoke transport over the Southeast Asian Maritime Continent: interplay of sea breeze, trade wind, typhoon, and topography, Atmospheric Research , 122, 486-503.***

***Colarco, P. R., et al., 2004, Transport of smoke from Canadian forest fires to the surface near Washington, D.C.: Injection height, entrainment, and optical properties, J. Geophys. Res., 109, D06203.***

References to these papers have been added. Please see changes in the revised manuscript to paragraph beginning “The high temperatures of open vegetation fires produce flaming emissions with a lot of associated buoyancy…”

***4. P. 6075. L25. Peterson et al. (2014) has shown that the using FRP divided by the retrieved fire area can better interpret the MISR plume height, at least over the boreal forecast. Please add this in the***

***discussion.***

***Peterson et al., 2014. Quantifying the potential for high-altitude smoke injection in North American boreal forest using the standard MODIS fire products and sub-pixel- based methods, J. Geophys. Res. Atmos., 119, 3401-3419.***

The reference has been added with the addition of the following line after "As a consequence a number of modifications to the Dozier method have been proposed (e.g. Peterson and Wang, 2013; Peterson et al., 2013; Shimabukuro et al., 2013; Giglio and Schroeder, 2014).":

"Peterson et al. (2014) have developed a probabilistic method for estimating the emission injection height based on FRP and retrieved burned area products from MODIS for use over boreal forests."

***5. To highlight the novel of this study, some description about the model in section 2, and section 3 could be referred to some other paper in the literature or moved to supplementary material. For example, section 2.3 plume rise parameterization could be pointed to Freitas et al. (2007, 2010). Also Section 3.1 could be shorten and only make some points.***

Although section 2 only covers previously published work, it has been written to only cover aspects of the model that are directly relevant to later discussion. E.g. the equations in section 2.3 are needed to understand the relevance of changes made in Section 3.1. Section 2 is separate from 3 to make clear what is from previous work in the literature and what is novel in this study. However, on review the authors agree that the paper would benefit from being shorter and more concise in these sections. Please see the revised manuscript for changes.

***6. Line 18, 'Between 1 September 2012 and 11 September the model was run with meteorological nudging', so the nudging doesn't applied for the rest of simulation? Since the first phase of the campaign covers 6-22 September, what is the reason to set 1 September to 11 September as this special?***

The flight campaign ran from 14 September to the end of the month. Over the flight periods, plus three days before as spin-up, the model had its met reinitiated every two or three days. Running with nudging was avoided during this period so as not to interfere with radiative feedbacks from the aerosol (to be covered in more detail in future papers). The met was restarted regularly to stop the meteorology straying too far from reality, and was always restarted on the morning before each flight used in this study (B731, B734, B739 and B742).

A long spin-up period (1-11 September) was needed to minimise the influence of the initialisation from the (less detailed) global model on our modelled aerosol loadings. Because we had not been intending to make comparisons with flight data during this period, nor to assess the impacts of aerosol feedbacks, we used nudging instead of regular met restarts to keep the model meteorology on track. Only after finishing the model runs did we decide to compare the AOD and precipitation fields with remote sensing products for the early part of the campaign too. Although there is this inconsistency in model operation between the spin up period and the later sections of the model simulation, we don't believe that it is significant enough to influence our analysis of the model AOD and precipitation fields in this paper.

***7. The distribution of AODs in Figure 5 is displaced by the model when compared against satellite data. If we look at the profile in Figure 2, we can see the emission also show the peak area is around 65° W, so beside the wind caused transport, it is better to explain this from the emission part.***

Point taken – there is an emission hotspot at 65°W that does not show up as strongly in the satellite AOD measurements as the model. The following line has been added after:

"…particularly about a cluster of fires at 64°W and 10°S.":

to:

"This is location of greatest fire emissions in both emission products, as shown in Figure 3. As this does not show as strongly in the satellite data, emissions are presumably too large at this location."

***8. Figure 5. Are the modeled AOD sampled over the MODIS AOD's time and space when do the comparison? Be clear on this in the figure caption.***

Yes, the model was sampled at the same approximate times as the MODIS overpasses (to the nearest whole hour). This will be clarified in the text and caption.

***9. In Figure 6, the results from 2 scenario simulations could be plotted in one panel, so 8 panels could be replaced with 4 panels. Hence the differences between 2 simulations could be showed.***

Thank you for this good suggestion. Please see new version of figure 6.

***10. The title like 'summary and outlook' might be good for Section 6 instead of 'conclusion', and also section 6 could be elaborated more concisely.***

Whilst “Summary and Outlook” may be an appropriate heading, a “conclusions” section is required by the Copernicus class structure. Effort has however been made to make this section more concise. Please see the revised manuscript for changes.

***11. To make the Table 4 more informative, the resolution information could be included.***

The following temporal resolution information of the instruments used has been added to this table:

| **Instrument** | **Temporal Resolution** |
|---|---|
| SP2 | 1 s |
| cToF-AMS | approx. 30 s during Straight and Level Runs<br>approx. 10 s during Profiles |
| CO analyser | 1 s |
| Nephelometer | 1 s |
| PSAP | 25 -30 s |
| SMPS | approx. 60 s |
| GRIMM | aprprox. 6 s |
| CCNc | 1s |

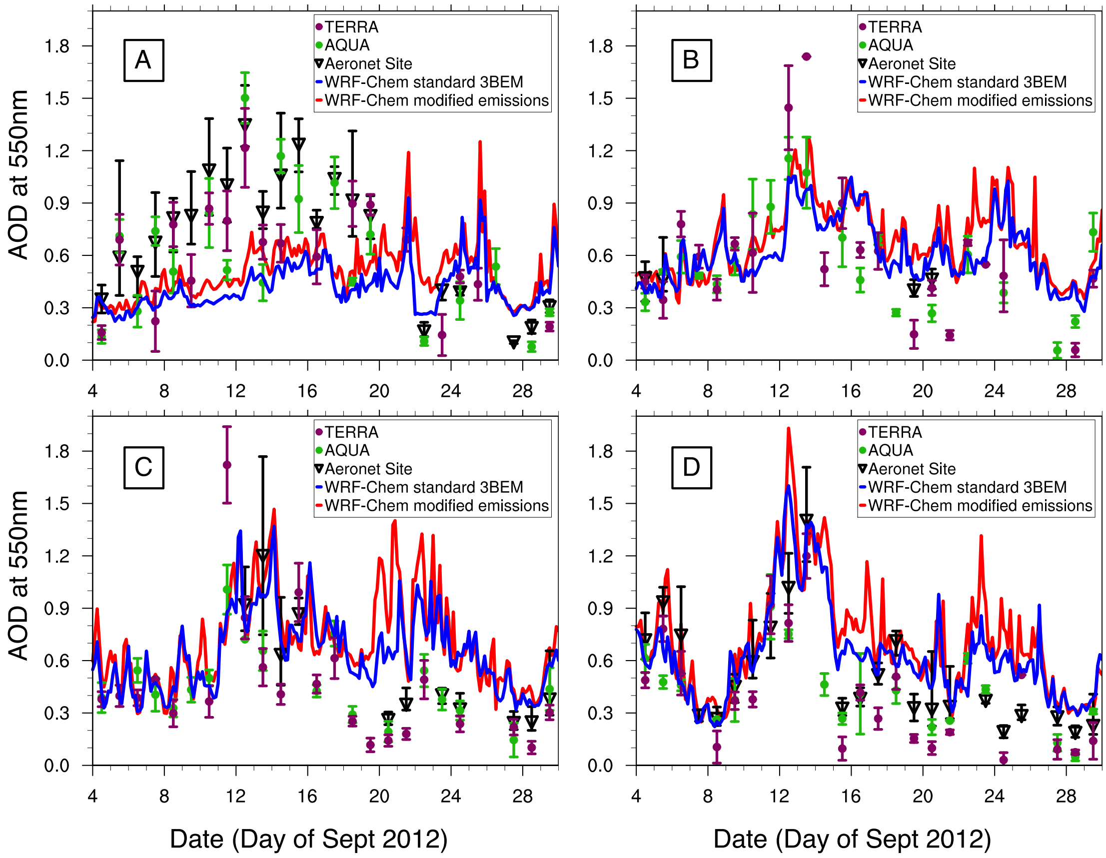


**Figure 1: Timeseries of aerosol optical depth at 550 nm at four Aeronet sites between 4 September and 1 October 2012. (a at Cuiaba, (b at Ji Parana, c) at Porto Vehlo and (d at Rio Branco. Blue triangles show Aeronet Site daily measurements, with bars indicating range in values over the day. Purple and green circles indicate measurements from overpasses of TERRA and AQUA satellites respectively, with bars indicating error range. Blue lines show data from WRF-Chem model simulations using standard 3BEM emissions. Red lines show data from WRF-Chem model using the modified emissions.**