Reply to Anon Review #1

We thank the reviewer for their helpful and insightful comments. Please see responses to specific comments below.

1. Page 6071, line 13 – correct spelling 'cerrado'

Thank you for finding this error, we have corrected the spelling.

2. Page 6082, line 12 – here is the first time Table 5 is mentioned. However neither here nor in the Table caption there is an explanation about the column heading B731, . . .Later on the reader finds out that these are flight numbers. Please include explanation in table caption.

We agree that the meaning of the flight numbers could be made clearer. The opening of the caption now reads as follows:

"Table of instrumentation used on flight numbers B731 (14 September 2012), B734 (18 September 2012), B739 (23 September 2012) and B742 (27 September 2012)…"

3. Page 6084, lines 20-25 – If the meteorology of ECMWF forecast is identical to the one in the MACC-II run, this means that aerosol has no feedback on meteorology in the MACC-II run?

The aerosol does not feedback on to the meteorology of the MACC-II model. Its purpose is not to evaluate the impacts of aerosol directly, although data from MACC-II can be used to drive the boundary conditions of regional models which do (such as WRF-Chem). It should be noted that, while similar, the meteorology is not 100% identical due differences in resolution between the ECMWF and MACC-II model runs. To make this clearer, the authors suggest replacing the lines:

"The meteorology, including satellite data assimilation, of the MACC-II assimilation system is identical (except for its lower resolution) to the operational ECMWF dataset. This ensures equivalence between the meteorological and chemical boundary conditions."

with

"The MACC-II system is an extension of ECMWF's integrated forecasting system (IFS) used for operational forecasting, which is run at a lower resolution of T255 instead of T1279. Since feedback from aerosols on the meteorology is disabled, the meteorological fields are virtually identical to the operational meteorological forecasts, albeit with lower resolution. This ensures consistency between the chemical and meteorological boundary conditions in this study."

4. Page 6086, lines 1-2 – here and elsewhere the authors refer to the boundary

layer without actually defining it. If it is the mixed layer, then the numbers of 1.5-2 km for the height over forest are very much higher than the ones reported by Fisch et al (2004, Theor & Applied Climatology). The range here is similar to the mixed layer height over pasture.

As the reviewer states this refers to the mixed layer. This is defined by the YSU boundary layer height (h) parameterisation (Hong et al. 2006):

$$h = Ri_c \frac{\theta_{va}|U(h)|^2}{g[\theta_v(h) - \theta_S]} \quad (1)$$

where Ri_c is critical bulk Richardson number (=0.5), U(h) is the horizontal wind speed at h, θ_v is the virtual potential temperature, θ_{va} is the virtual potential temperature at the lowest model level and θ_S is the temperature at the surface. It is solved iteratively with θ_S, as described by (Hong et al. 2006).

If we assume the values from Fisch et al. (2004) are typical for the region, and the Cerrado/savannah biome has similar mixed-layer height to the pasture measurements, then the authors agree that the model (with YSU PBL scheme) appears to systematically overestimate the mixed layer height.

Table 1: Comparison of maximum peak mixed-layer height (h) at 17:00 local time, comparing data from Fisch et al. (2004) dry season data from Ji Parana site in 1999 with data from the WRF-Chem domain over the 2012 study period.

	Forest	Pasture/Savannah
Fisch et al. 2004	1094±385.	1641±595.
WRF-Chem with YSU	1873±541.	2912±301.

YSU is a non-local diffusion scheme. During the testing and development period for the work on this paper, the local 2.5 order MYJ PBL scheme was also tested. It was found to produce lower h, but produced instabilities in the model at mid-afternoon which resulted in anomalous precipitation fields. Other PBL schemes may potentially be more appropriate for capturing the PBL height over this period, but have not been tested for this study.

Improving the modeled PBL height will undoubtedly have an impact on the resultant fire emission injection height. For now, we suggest adding the definition of the mixed layer height in Equation 1 and a clarifier in the text conceding that the model is producing PBL heights too high compared to Fisch et al., and that further work is needed to improve this representation.

5. Page 6087, line 14 – Reference to Figure 3 which shows rainfall totals. Usually rainfall is shown with shading colors where blue means high amounts of accumulated rainfall and red small values, exactly the reverse of the shading used in the figure.

We have redrawn these figures using a more appropriate colour-table.

6. Page 6088, lines 18-19 – here the authors define the top of the modelled boundary layer as the height of the inversion in the temperature profile. However, as seen in Fig. 4d, there is another inversion at ~650 mb that is probably due to subsidence and is too high to be the top of the local mixed layer.

We acknowledge that for the case of figure 4d, there is an inversion at 650mb which is not the top of the mixed layer. We suggest resolving this by clarifying we are referring to the lowest inversions by changing lines 17-20 on page 6088:

"The top of the modelled boundary layer, inferred from the inversion in the temperature profile, is generally close to that observed in the measurements, but not as clearly defined or strong."

To:

"The top of the modelled boundary layer, inferred from the lowest inversion in the temperature profile, is generally close to that observed in the measurements, but not as clearly defined or strong."

7. Page 6089, line 17 – with reference to Figure 6 – apparently there is an error in the figure caption, in the line just before last, should be (a,c,e,g)?

Thank you for pointing out this error, it should read (a, c, e, g). This has been corrected.

8. Page 6098, lines 1-10 – The authors correctly state that modeling temperature inversions is very much dependent on the vertical resolution. However, imposing a PBL height into the plume rise model may be a solution if the model is run in hindcast mode where the height is known from sonde data, for example. The large variability of inversion heights in a day to day basis, and also geographically, means that imposing a value in forecast model run may be even worse than the weak inversion produced by the parent model that is used now.

We concede that there may be issues a priori assuming the magnitude of the temperature inversion. We suggest changing the last sentence of lines 1-10 on Page 6098:

“Forcing a small temperature inversion at the PBL top in the plume-rise parameterisation may be needed to improve its accuracy.”

To:

"Forcing a small temperature inversion at the PBL top in the plume-rise parameterisation may improve its accuracy, but day to day and geographical

variability could make such an intervention impossible without comparison of the hindcast with measured data.”