

## General comments

The manuscript describes black carbon and optical property measurements from a field site in the Tibetan Plateau, taken over a period of just under two weeks. They divide up their data into “clean days” and “pollution episode” and compare and contrast the BC mass loadings/properties, absorption and calculated radiative forcing, as well as attempting to identify source regions for the pollution episode. This is an important topic, and certainly deserves investigation.

Overall, the manuscript is well structured and reasonably well written, though it needs some thorough proofreading (including the title). The experimental data are high quality, and I enjoyed reading the paper. Having said that, there are some parts that I think need to be reworked, as some of the figures are not the best way of displaying the information the authors are trying to convey. In particular, the section linking mixing state to optical properties needs to be improved. I recommend this manuscript be published subject to the following revisions.

## Specific comments

Abstract- The main point I think you have shown, but need to highlight better is that the main factor that determines how much light is absorbed by BC is simply the concentration of BC. Comparing the pollution episode to the clean days, BC increased by 400% but  $MAC_{BC}$  only changed by ~15% according to figure 6. This point should be highlighted in the abstract and conclusions.

P14143 L4 metres above mean sea level

P14144 L2-6 for completeness you should also mention the semi-direct effect

P14146 L20&22 refractory BC mass

P14147 L7 Write a sentence saying what your calibration material was and what the absolute uncertainty is on the BC measurement (this is important for your  $MAC_{BC}$  calculation)

L18 give some examples of atmospheric processes that can cause BC to be internally-mixed

L9-14 You cite Wang et al. (2014) for details of the mixing state measurement. They say that only particles <275nm VED were used due to saturation. You should also use a lower diameter limit as well. For small particles the scattering measurement is likely to be too noisy to reliably measure the time delay. Taylor et al. (2015) discuss the instrument’s limitation when considering leading-edge scattering, but similar arguments apply when using the time delay technique.

L28 Please give more details on the calibration of the PAX. What concentration of NO<sub>2</sub> did you use? How well do you know the concentration? Did you do it in the dark to prevent photolysis? What absorption cross-section did you use? Specifically, you need to say the absolute accuracy of the measurement, as it is important for the  $MAC_{BC}$  calculation.

Figure 2 I can see what you are trying to do here but I’m not sure it’s appropriate in this case. This type of plot is useful when the diurnal variation is stronger than the day-to-day variation, and/or if you have a long dataset. It looks like there has been some artificial smoothing of the data- it should

just be a stripe for each day. I think you'd be much better off with just a time series of BC concentration and coated fraction. You could also add in  $MAC_{BC}$  and  $B_{Abs}$  as well. And then highlight the pollution episode.

P14148 L10 I think you are using too much precision when reporting some of your results. For example,  $59.3 \pm 6.9\%$  would be better as  $59 \pm 7$ . You've done this in many parts of the manuscript (e.g. table 1).

L11 Please rephrase this sentence

L12 I don't see the significance of 30% of the values being higher than the mean, that just means you have a non-gaussian distribution of BC concentration.

L13-14 I'm not sure how you get from a large variation coefficient to a large BC burden. Doesn't the measured BC concentration show that?

Figure 3 I know the y axis is arbitrary units but it would make it easier for the reader if you make the pollution episode larger than the "all days" distribution, as the concentrations were higher than average.

P14149 L1-8 You are correct that coagulation is slow and BC size distributions don't tend to change much. That means that the measured BC are likely to be from similar types of sources. Fossil fuel and biomass burning tend to have different size distributions. See (Kondo et al., 2011; Sahu et al., 2012; Taylor et al., 2014), it would be useful to compare to these studies and probably back up your assertion that the BC is from biomass burning.

L11 The HYSPLIT URL has a typo in it. Also you should see this page [http://www.arl.noaa.gov/HYSPLIT\\_pubs.php](http://www.arl.noaa.gov/HYSPLIT_pubs.php) for a better reference for HYSPLIT (i.e. one or more of the papers on that page). You should also state what meteorology data you used, and what method was used for vertical motion.

L12-14 I don't agree with this as the reason for using 5-day trajectories. It's more that 5 days (or less) is about as long as you might typically trust a trajectory. BC lifetime varies- on average it's around a week but some plumes last longer and some not as long. It will depend on the meteorology.

Some of this section (e.g. the AOD and MODIS data) reads as if you are saying that if you follow back a trajectory 5 days then you will find the source of the BC. This is possible, but you might also pass through a strong source region 1 day back, in which case most of the BC would have come from there. I think you are probably right that the pollution episode came from fires in North India but that really comes from the PSCF part of this section rather than the trajectories alone. So, I think you should move the part about AOD and MODIS after the discussion of the PSCF model. You ran some trajectories, you used the PSCF model to try work out where/when the sources might be, then you used AOD and MODIS data to back up your findings.

P14150 L10-20 You should clarify how you did your data averaging- you ran trajectories every 6 hours (with a timestep of 1 hour?) but get 1s data from the SP2- did you average to the timestep of the trajectories?

My other main comment about this section is that it's useful to identify where the pollution episode originated from, but most of the BC you measured was during the clean periods as the pollution episode only lasted 2 days. If you use a lower threshold with the PSCF, can you say anything about the BC sources during the clean days? These might be more important in the long-run.

Section 3.3 and Figures 6 and 7

In this section you have tried to address two questions. 1- what is the  $MAC_{BC}$  of the BC during the clean and pollution days and how much is this linked to mixing state? 2- What is the main factor that determines absorption (i.e. BC concentration or mixing state)?

You have focused on the first and touched upon the second in lines 16-23 on P14152, but the second question is arguably the most important.

The way you've plotted figure 6, the slopes of the lines are determined mostly by the points in the top/right of the plot. For the pollution episode the points follow a straight line reasonably well but for the clean days some of the lower down points are quite far away from the line. If you calculated  $MAC_{BC}$  as a function of time, I think you would have some periods during the clean days where the calculated values would be quite different to the straight line fit. In fact, if you take the mean or median values of BC and  $B_{Abs}$  from table 1 and use them to calculate  $MAC_{BC}$ , then you would come to the conclusion that  $MAC_{BC}$  was higher during the clean periods, which is opposite to what you have found using the straight line fits!

Figure 7 looks quite strange- why is there a neat straight line cutting off the bottom right of the graph? Was there really no data in that section? Without the data in the bottom right corner it's difficult from this plot to work out whether BC concentration or mixing state are more important for determining absorption.

I think using a different approach would answer these questions much more clearly. Based on figure 7 I think you should have enough signal/noise to calculate  $MAC_{BC}$  as a function of time. You could add it to Figure 1 as well. Then instead of figure 6, you could add the mean/quartiles of  $MAC_{BC}$  to table 1, and replace figure 6 with histograms showing the variation of  $MAC_{BC}$  during the clean and polluted periods. That will tell you if  $MAC_{BC}$  is different in each time period, and show the variation.

I think the key plot to work out if  $MAC_{BC}$  is linked to mixing state is make a plot like figure 2 in Lan et al. (2013). Divide up  $MAC_{BC}$  into bins of coated number fraction and see if there is any relationship. That will tell you how much any changes in  $MAC_{BC}$  are linked (qualitatively) to mixing state. Then also say how much brown carbon could have affected your result.

Finally, to answer whether BC concentration or  $MAC_{BC}$  is more important for determining absorption, just look at the numbers in table 1 and see which changes most. I don't think this should be too much effort, and I think it would make the conclusions much clearer and more robust.

P14152 L25 I'm not sure how much the 7.5 m<sup>2</sup>/g from Bond and Bergstrom (2006) is set in stone. For example, Lan et al. (2013) measured values lower than this value. Also, it is critical that you calculate the absolute calibration error on the BC and  $B_{Abs}$  measurements in order to quantitatively compare your calculated  $MAC_{BC}$  to any other value.

P14153 A point to mention is that although the BC concentration was 4x higher in the polluted days, the radiative forcing was only about 2x higher, meaning other aerosol components are also important.

## References

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