Response to Interactive comments on: "Long-term Brown Carbon and Smoke Tracer Observations in Bogotá, Colombia: Association to Medium-Range Transport of Biomass Burning Plumes" by - Juan Manuel Rincón-Riveros et al.

Anonymous Referee #1

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RC: Light-absorbing aerosols can affect both air quality and climate, so understanding their source and transport is important. This manuscript used a bunch of different observations to study the sources of light-absorbing aerosols over densely populated areas in the Central Andes of Northern South America. **It showed that these aerosols are closely related to medium-range transport of biomass burning plumes**. My comments are listed below.

Major comments

RC: I am concerned about the uncertainty associated with the BrC and BC measurements reported in this work. As mentioned in the work and reported by many other studies, there is large variability in reported mass absorption cross-section and Angstrom exponent values for absorbing aerosols. However, this study still used a single certain value for these variables (i.e. =7.77 g/m3; FF=1; BB = 2), without estimating the uncertainty due to the variation of these values. I expect that both eBC and BrC concentrations would change a lot if one assumes different values for these optical parameters. In addition, the authors should also estimate the uncertainties resulting from the process of measuring and analyzing the biomass burning tracers.

Authors Response: We have now addressed the issue of uncertainty by performing sensitivity analysis on the parameters used in the calculations. Regarding the use of a (mass absorption cross section) MAC 7.77 g/m2 for eBC (at 880 nm), we would like to clarify that, by definition, it is necessary to assume a specific MAC to convert b_{abs} into a "Equivalent Black Carbon" concentration. We strictly followed the recommendations of Petzold et al., 2013 (Atmos. Chem. Phys., 13, 8365–8379, 2013) by explicitly stating the MAC used, so the calculation is transparent and reproducible. We now explicitly mentioned this in the manuscript. Furthermore, we have now included b_{abs} in Figure 2 by adding a secondary axis.

One significant issue was the lack of sensitivity to parameters. **Figure R1** shows a sensitivity analysis performed on the parameters α_{FF} and α_{BB}. We computed the inferred BrC concentration for each set of parameters for high (DJF) and low (JJA) BB activity periods. Because our data is strongly influenced by urban emissions (dominated by traffic in Bogota) our observed Angstrom exponent is on average close to 1. Therefore, our deconvolution is much more sensitive to the assumed value of α_{FF} than it is to the much more uncertain α_{BB}. However, it should be noted that in all the parameter combinations the same trend remains, namely that during the high BB periods BrC is significantly higher than during JJA (i.e., has a strong seasonality). A discussion in this regard is now included in the manuscript and the sensitivity analysis included in the Supplementary Material.

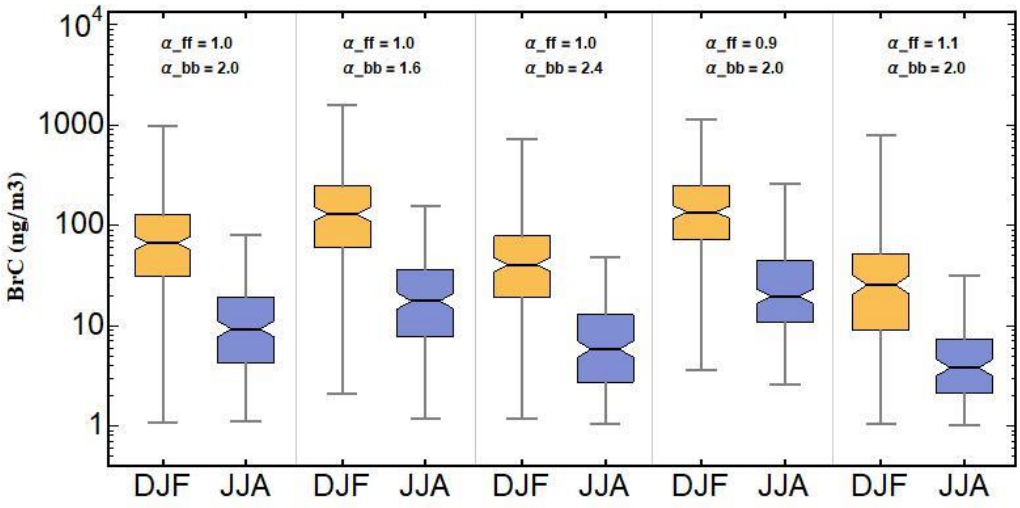

Figure R1 – Response to Reviewers – Parametric sensitivity

Minor comments

RC: Line 168: "The quartz filters were pre-baked at 550°C for 12 hours to reduce their organic background and later placed in." why is it needed to be heated? Wouldn't it reduce the biomass burning semi-volatile OA?

Authors Response: The filters are pre-baked before being deployed for sampling. This is done exactly as the reviewer points out, to reduce semi-volatile OA from the filters, reducing this way any potential artifact during analysis post-sampling. We clarified this in the manuscript, and it reads "Previous to sampling, the quartz filters were pre-baked…."

RC: Line 174. What is LOD?

Authors Response: We intended LOD to stand for "Limit of Detection". We now explicitly define the term in the manuscript.

RC: Line 173-179. It seems OC and EC are measured in the same way? Then how does one differentiate OC from EC?

Authors Response: OC and EC are measured in the same instrument, with a technique called TOT (thermal-optical transmittance). However, they are not measured in the same way. The TOT measurement is based on the fact that the organic carbon contained in particles volatilizes at different temperatures. The organic carbon is defined in this technique as the carbon that becomes gas in a Helium atmosphere at temperatures below 580°C. Meanwhile, EC in this technique is defined as the fraction of carbon that does not volatilize after exposing it to 580°C, but that oxidizes when oxygen is added to the controlled atmosphere at temperatures above 580°C. The quantification of carbon in each case (either volatilized or oxidized) is done by converting it to CH4 to be detected with an FID.

We now expanded the explanation to avoid any potential confusion.

RC: Line 236. "The similarity between both datasets shows that eBC measurements at the site are overwhelmingly dominated by EC emissions from urban traffic and industrial emissions". No absorbing OC emissions from urban traffic and industrial emissions?

Authors Response: The phrasing was modified in this section. The phrase now reads ***"The strong correlation between both datasets suggests that eBC at the Monserrate site is closely associated to urban emissions. According to a recent emission inventory in Bogotá, mobile and industrial emissions are the dominant primary particle sources in the city. Furthermore, cargo and public transportation have the largest emissions share, and most of those vehicles are diesel powered (Pachón et al., 2018)."***

Regarding the question of -No absorbing OC from urban traffic and industrial emissions? - It is possible (as has been recently show in the literature) that fossil fuels contribute to UV absorbing carbon (i.e., BrC). We acknowledge this in the paper now (in the introduction). However, EC is known to be the main absorber at near IR wavelengths, while OC from fossil fuel combustion is not a particularly strong absorber of near-IR light.

RC: Line 250. I think the major reason for the seasonal pattern in PM2.5 is the different emission source/strength in different seasons.

Authors Response: Indeed, as the reviewer points out, this is exactly our working hypothesis in this paper, namely that biomass burning emissions in the region increase $PM_{2.5}$ concentration during the months of January-to-April, and we believe that we demonstrated that through measurements of biomass burning tracers in different seasons. In that specific paragraph we were merely pointing out that there are also meteorological conditions during those months (stronger surface inversions, stable conditions, lower mixing heights) that could concurrently have an impact of increasing $PM_{2.5}$ concentrations (this is explained in the reference Mendez-Espinosa et al., 2019). Furthermore, there is no clear annual pattern in either public transport, cargo transport, or industrial activities. There is no seasonal change in fuel composition as does occur in other countries.

RC: Line 263. I don't understand the reasoning here.

Authors Response: Point well taken. What we intended to say here was that the BrC we detected was likely aged biomass burning (because the sources are located hundreds of km away from our measurement site). The intended message is conveyed in the next section. Therefore, those lines were removed from the manuscript.

RC: Line 302. Not clear to me how the authors get these numbers.

Authors Response: These numbers were obtained by averaging WSOC for high and low BB activity seasons respectively (i.e, those represented by the open and filled circles in Figure 4b). This now reads: ***"The mean WSOC observed for low BB activity was 2.5µgCm^{-3} while for high-BB activity period was 4.2µgCm^{-3} reaching up to 8µgCm^{-3}."***