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Comment on acp-2022-83

Anonymous Referee #2

Referee comment on "Columnar and surface urban aerosol in the Moscow megacity according to measurements and simulations with the COSMO-ART model" by Natalia E. Chubarova et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-83-RC2, 2022

This work focuses on the aerosol properties on the ground and in the atmospheric column, and their relationship with meteorological parameters, in the urban environment of the Moscow megacity. Data include ground measurements of PM10, BC and gaseous precursors, columnar aerosol parameters retrieved by AERONET data, as well as modeled data obtained by the application of the COSMO-ART model. Additional data obtained in an upwind clean background site at Zvenigorod Scientific Station where used for the estimation of the urban component of the studied aerosol parameters. Overall, this is a very interesting work, based on sound experimental and numerical methods. To my knowledge, there are few works on the aerosol pollution in the Moscow megacity, rendering this information more valuable. I believe the manuscript merits publication, after some minor revisions are done, as indicated in the specific comments below.

A careful editing of the whole manuscript is needed in order to correct syntax errors (e.g. in lines 39-40, 59-61, 66-68, 70-75, among others).

Line 121: Please specify the time resolution of the eBC measurements, in accordance with the other online in situ measurements you mention above.

Figure 2: Is the red circle displayed in the maps the measurement site? Please clarify. Also, please comment with respect to how representative are the 2010 emissions for the measurement period (2018 and 2019).

Lines 198-199 “We consider that our BC measurements in Moscow provide the BCurb component, whereas the black carbon is mainly formed and emitted in the urban environment (see Fig. 2).”: The BC emissions are not depicted in Figure 2; please clarify

what you mean by citing Fig. 2. In any case, I agree that BC is mainly emitted in the urban environment.

Table 1: Please specify that PM10 and BC measurements correspond to surface observations and not the total column of the atmosphere, as noted in the Table caption. Also, please correct "Confident interval at 0.05" to "Confidence interval at 0.05".

Figure 4: Please note the units for all parameters presented. You may include this in the caption, if it is too complicated to include it on the axis.

Table 2 and related discussion: Most of the correlations discussed in Lines 260-287 are statistically significant but display a low correlation coefficient. In my opinion, a correlation coefficient below 0.5 does not really imply significant relationship between the two parameters. For example, the authors state "We obtained a statistically significant correlation of columnar AOD500 with surface PM10, and BC. A more pronounced dependence of both BC and PM10 with fine AOD500 mode could be explained by the fine mode BC composition and the predominant fraction of fine aerosol mode in PM10 in urban aerosol in Central and Northern Europe"; these correlations correspond to coefficients of 0.34 and 0.39 for BC with columnar AOD500 and fine AOD500 mode, respectively, which I think are too low to show real correlation. A better correlation is observed for PM10 with AOD500; nevertheless, I don't see a difference between AOD 500 and fine AOD500 mode (correlation coefficient of 0.57 versus 0.58). Similarly, no correlation can my claimed for the fine AOD500 mode and SO2 concentrations, while only low correlations are observed for the other gaseous precursors. I believe a more fruitful discussion may be based on the correlation analysis graphs (e.g. Figures 5 and 6), where interesting observations can be made (as in Lines 289-309). Please also consider revising the corresponding comments in the Conclusions section.

Line 318: Please correct "solar elevations" to "solar radiation".

Figure 8: I would suggest to include also the % of the urban component with respect to total variable. In my opinion, the absolute values of the urban component of the different parameters do not show clearly the impact of the city on local/regional air quality.

Figure 9 caption: "Figure 9: (a) - Time series of AOD at 550 nm simulated using direct observations of AOD 915 at 500nm and AEE at 440-870nm, and the AODurb component according to measurements and modelling in 2018 (left upper panel) and 2019 (right upper panel)": The caption is not so clear. The simulated AOD at 550 nm corresponds to the total AOD or maybe the AODurb component?

Table 3: I don't understand how the PM10 model values were obtained. According to the Methods section (and in particular Lines 176-178), only the anthropogenic component of

the surface mass concentrations of PM10 was simulated. The same question holds for BC model data presented in Table 3. In addition, how do the authors obtain the BC value used in the BCurb/BC ratio mentioned in Line 367?

Lines 371-375 "We analyzed if there is a relationship between urban aerosol component and the total aerosol content. Figure 10 presents the dependence of model and measured AODurb on total AOD according to the MSU MO measurements, and the dependence of PM10urb on PM10. There is a positive correlation of urban aerosol component for AOD and PM10 with total AOD and PM10.": I think there are some issues with respect to the discussion of Figure 10. For one, since the urban component is part of the total variable, a good positive correlation between these two does not clearly imply the simultaneous formation of natural and anthropogenic aerosol; It may be also related to meteorological conditions favouring the accumulation of pollutants. Also, a good correlation as the one displayed in Figure 10(c) suggests a constant PM10urb/PM10 ratio, so a constant % contribution of the urban component. In the case of AOD, I don't think the AODurb observations (Figure 10(a)) demonstrate a correlation with the total AOD. For the AODurb model (Figure 10(b)), there seems to be a positive correlation for higher AOD values (above 0.2). I think a better picture could be obtained if the authors plotted the ratio of PM10urb/PM10 (and AODurb/AOD) over the PM10 (and AOD). It would be interesting to comment if for higher AOD values, the anthropogenic component contributes more. For PM10, this does not seem to be the case.

Lines 376-377 "This may be also accompanied by higher concentrations of aerosol gas precursors both of urban and natural origin, which, in turn, have high correlations with PM10 and AOD according to Table 2.": According to Table 2, I don't think the authors can claim a high correlation between gas precursors and AOD (R between 0.17-0.34). For PM10, I agree that the correlation coefficients show high correlations (at least for NO2 and CHx).

Lines 395-397 "Both simulated and measured PM10urb values have a pronounced dependence on IPD with higher PM10urb at lower level of intensity of particle dispersion. Note, that the influence of the intensity of particle dispersion on AODurb is not observed.": I don't understand how we can observe the dependence of the PM10 urban components on IPD, based on Figure 11. The relationship between the data obtained from model and observations varies, depending on the IPD value; but the PM10urb values display a wide range in all 3 cases of IPD.

Figure 12: To my understanding, IPD will also vary during the day, so I am not sure how different diurnal cycles may be calculated for different IPD values. I would expect to see the diurnal cycle of the urban component (based on model and measurements), along with the emissions (when available) and the IPD value. The day-time and night-time meteorology and atmospheric conditions affect the IPD levels, so I am not sure what is the meaning of selecting the IPD values. Did the authors group the days by a daily average IPD value and then calculate the average diurnal cycle for each group (for mean 24hr IPD =1, 2 or 3)?