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Reply on RC2

Jessica Slater et al.

Author comment on "The effect of BC on aerosol–boundary layer feedback: potential implications for urban pollution episodes" by Jessica Slater et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-139-AC2, 2021

We thank the reviewer for their time in reviewing the manuscript and for their insightful comments highlighted below. Specifically, we thank the reviewer for highlighting the recent manuscript by Ma et. al which is very insightful and works well to support the study presented here. Below we have responded to specific comments, technical comments have been addressed or corrected within the manuscript directly. Reviewer comments are highlighted in bold, responses in plain and any responses which are now additions or edits to the manuscript are in italics.

There have been researches emphasized the sensitivity of BC impact on PBL due to the BC positions relative to PBLH, which have not been viewed by this study, eg. Yongjing Ma; Jianhuai Ye; Jinyuan Xin*; et. al (2020). The Stove, Dome, and Umbrella Effects of Atmospheric Aerosol on the Development of the Planetary Boundary Layer in Hazy Region. Geophysical Research Letters, 47, e2020GL087373. I wonder that any new findings in this study compare to previous studies.

There are numerous findings in this study which relate to the work presented here. We have added the following text to the discussion section to highlight this.
A recent study by Ma et al. (2020) utilises an LES model to investigate the impacts of the altitude of absorbing and scattering aerosol layers on planetary boundary layer height. Although, in their work they do not specifically treat the aerosol population but focus on changing the aerosol optical depth and aerosol optical properties directly. Their work agrees with the work published here, to show that absorbing aerosols above the PBL will suppress turbulence and PBL growth due to the so-called "the dome effect". Their results also show an increase in PBL height due to absorbing aerosols at the surface promoting turbulent motion, which they term "the stove effect". In contrast to the work presented here, their work shows a much greater increase in PBL height due to absorbing aerosols within the PBL, however, the PM2.5 concentrations over the period investigated by Ma et al. are much lower than in this work. Similarly, the maximum PBL height in that work (around 1000 m) is much higher than in our work. Consequently, it is likely that the specific meteorological conditions are different in Ma et al. and this study and that, as highlighted in section 3.2 of this paper, this has a significant impact on the ability of absorbing aerosols to increase PBL height or the magnitude of "the stove effect".

(LES)-aerosol-radiation model has been seldom used in this kind of 3-days long lasting events. I strongly suggest authors to present the detailed results of the LES simulations and the comparisons to observational meteorological factors of PBL and BC and BC related air pollutants profiles in PBL. It is the base and could be the advantage of this study. In the title and content of the paper, I feel the "Potential implications for Beijing haze episodes" could not give a direct and effective cognition to reader. If the mechanism is correct, it should be implicated to the BC related air pollution all over world. And this study did not show the distributions and profiles of BC and air pollutants in Beijing.

The focus of this work was not to compare specifically to pollution episodes in Beijing but rather to examine various sensitivities relating to black carbon and its influence on planetary boundary layer dynamics. The model setup that was used here has previously been tested and compared to observations (including meteorology) and turbulent metrics (SHF and PBL height) in the previous papers published by these authors (https://doi.org/10.5194/acp-20-11893-2020 and https://doi.org/10.1039/d0fd00085j). As highlighted in the response to the other reviewer, we feel that there may have been some confusion about the aim and key features of this work, likely due to these points not being highlighted by the authors in the manuscript. We have therefore added a section in both the introduction and conclusions section of the manuscript to identify this. A key point to highlight is that this study utilises an idealised model framework, with no surface heterogeneity or synoptic drivers but rather examines various processes which are believed to be important for Beijing pollution episodes. Overall, the point of this manuscript is to examine the effects of black carbon on atmospheric dynamics and its feedback on pollution via absorption of radiation using an idealised model framework that can resolve turbulence and has two coupling between aerosols and radiation. We have used measurement data from Beijing to provide input constraint to the model but our findings are generalizable to other polluted environments. We therefore recognise that the title of the manuscript is confusing. This has now been changed to read 'Potential implications for urban pollution episodes'.

I don't understand the setting of the sensitivity experiments. In section 2, it looks like that the settings were a serial of ideal sensitivity experiments of BC profiles, while in section 3, there were PBL changes in the specific days. When there were no observed BC profiles, we cannot know which setting was close to real profiles of PBL structures. If they are ideal experiments, why did you indicate the results were the specific days in Beijing?

The settings in terms of the variations in aerosol compositions, vertical profiles and concentrations are varied in these various sensitivity experiments. However, initial meteorological conditions (such as surface temperature, vertical profiles of potential temperature, humidity and wind, which greatly influence the development of the PBL and various moisture and heat fluxes in the model are taken from observations on these specific days in Beijing where a heavy pollution episode took place. LES models, such as the one used in this work are not able to initiate pollution events and no emissions were included in these simulations. Rather, the point of this work is to examine the feedbacks which occur during the pollution episode. Hence, we have used initial meteorological and thermodynamic profiles from observations made during a pollution event to represent typical conditions when high pollution is observed.

In figure 6, what are the reasons of the BC fluctuations in vertical direction and peak at around 300-500 in figure 6b-d ?

Figure 6 relates to potential temperature lapse rates not BC concentrations.

In line 305, "this can further increase surface concentrations if the aerosols at PBL top mix down to the surface." correspond to which figure?

This was more of a hypothetical statement – figure 5 shows the aerosol layer moving to lower layers around the top of the PBL. This could potentially result in downward mixing of the aerosol layer to the surface but as the simulations presented in this work don't directly showcase this, this sentence has been deleted to avoid confusion.

In line 381, "lead to a saddle type pressure field over the region which leads to a temperature inversion" correspond to which figure? If did not discuss in the manuscript, you cannot obtain the conclusion.
Again, we thank the reviewer for recognising this discrepancy, as this hasn't been previously mentioned in this manuscript (it is well described in the paper by Slater et. al (2021) and Wang et. al (2019)) it has been deleted here to avoid confusion.

Line 171, " the same as BC surface", what is it mean?
This is related to the previous case but has now been better clarified in the text through the use of specific case names for each simulation
In equation 1, what is the interval of t+1 and t? Is the eq. 1 right?
Here t is related to the time (t) at which the SW heating rate is calculated and t+1 is the timestep (2 minutes in these simulations) this has now been clarified in the text.

Line 217 "There is slight subsidence of the aerosol layers", ??
Figure 5 shows that there is a slight sinking of the aerosol layers which exist only aloft. This is due to the model setup which accounts for large scale subsidence. This has been deleted to avoid confusion.