Review of "Simulated impacts of vertical distributions of black carbon aerosol on meteorology and PM2.5 concentrations in Beijing during severe haze events" by D. Chen et al.

This study presents the impacts of vertical distributions of black carbon aerosol on local PM2.5 concentration and meteorology using the Weather Research and Forecasting with Chemistry model (WRF-Chem) and airborne measurement of black carbon vertical profiles. The manuscript is well written and easy to follow, and the simulated impacts are well documented and reported quantitatively. However, my main concern is that the scientific wisdom gained from this research, contributing/adding to the current knowledge of the community, is not clearly conveyed in the current form of the manuscript. In other words, I found multiple places where the authors can discuss more on the implications of the reported impacts/results, as well as the physical reasonings behind them, instead of only reporting the changes from one simulation to another. Therefore, I suggest major revision.

Overall, I think this is a nicely designed and conducted modelling study, which can make valuable contribution to the field. Here I provided some specific comments and suggested changes regarding my main concern of the manuscript.

Major concerns/questions:

Abstract:
A general comment, the current form reads like a report summary, could the authors reconstruct the abstract in a way that scientific questions/goals of the study are clearly posed in the beginning, followed by a concise summary of the key findings (not only reporting the quantitative statistics, but also the logical flows behind these changes), and ended with implications of the study.

Section 2.4 Numerical experiments:
When VerBC_obs and VerBC_hs1-6 (RT) are compared with noBCrad (which is ran with the default BC profiles, except the optical properties are set to zero), it seems you're attributing the simulated differences between them solely to radiative effects, while assuming difference in vertical BC profiles between these simulations and noBCrad has no non-radiative effects (e.g. microphysical or chemical effects). Could you please justify this?

Section 3 Model evaluation:
L309-311: Indeed the model overestimate PM2.5 concentration if one compares the averages over the 9 day period, however, there is a lot more one can say about this model/obs comparison. For example, it seems the overall overestimation in the simulation is mostly coming from the clean days (DEC 13-15), whereas during the 2 haze events, the model seems to do fairly good job, comparing to obs, quantitatively, but there seems to be a timing difference, which could be due to discrepancies in advection between obs & model. Only reporting the mean biases doesn't help the reader understand the difference between model and obs that much.

Moreover, what is the meteorological conditions during the clean period, is there any precipitation event? Cloud formation? These can also help the reader understand these events better. I wonder

if a meteorological overview of this 9-day period can be added to the beginning of Section 3 or section 3.2?

L350-353: Again, more details are needed here. Even though model overestimate PBLH in the mean, daily maximum PBLH values from obs exceed that from the model, and the overestimation is mainly due to the fact that obs has '~0m' PBLH during most of the day, is this an artifact or obs mis-characterize PBL? More details here would be helpful.

Section 4:
Is there attempt to compare the simulations with obs-corrected/modified BC vertical distribution to observation? I wonder if getting the vertical structure of BC close to obs help improve the overall simulation relative to observations? And this could be an important result of this study, such that getting observationally constrained BC vertical distribution help (or does not help) improve the simulated local meteorology and PM2.5 concentration.

L405-411: I feel like Figure S4 is an important figure which can be moved to the main text, as it shows the role of BCrad on regional circulation/wind pattern, which further leads to changes in local PM2.5 concentration. However, how does BC DRE enhance the northerlies north of NCP and weakened the wind speed in central and southern Beijing is still not clear to me. I also think investigating the physical mechanisms behind this is critical to the whole study. I suggest more detailed discussion and further analyses here.

Related to Fig. 9: What are the implications of these results from the IPR analyses?

Section 5:
L500-509: Could you discuss more on how does larger delta_T_BC result in larger reduction in PBLH?

Related to Fig. 13: Again, what are the implications of these results? Are these results case/event dependent? Or they can be generalized, e.g. to make arguments on the role of BC vertical distribution on local accumulation and advection of pollutant?

Conclusions:
Again, the current conclusion section reads like a summary of the simulation results, which feels redundant and repetitive. I suggest reconstruction of this section, in a way that the repetitive results summary can be minimized or summarized in higher level languages, and the authors are encouraged to discuss more on the implications of the results and the study in general, e.g. whether these results are event specific or they can be generalized. Languages on the role of BC vertical distribution in affecting local meteorology and transport/accumulation of pollutant should be added.

L580-582: You mentioned results from this study highlights the importance of accurate representation of BC vertical profiles in models. I don't think this point has been made clear, as you haven't shown how simulations with obs-modified BC profiles compared to observations, is there improvement at all?

Minor comments:

L44, L477: is “sharper” the right word? To me, the sharpest decline is associated with hs=0.35 where BC drops from 13 to 4 in the first 500m, whereas when hs=0.96, BC drops from 6 to 4 (much slower decline).

Section 2.1 Model configuration:
How does the current model configuration deal with properties at domain top, are the top of the domain forced by free-tropospheric properties/motions from NCEP?
Is there nudging in the simulation?

Section 2.3 Observational data:
Regarding MODIS dataset, did you use Aqua or Terra, at what resolution (i.e. what level of the dataset). More details here could be useful.

L275-280: A bit hard to follow here, on L275, you meant ‘VerBC_hs1~6’ instead of ‘VerBC_hs1’, right? When I first read this part, it confused me, but when I saw figure 11, it started to make sense. I suggest some clarifications here.

L367: I don’t think “well simulate” is the right word to describe Fig. S3, even the horizontal distribution seems off between MODIS and simulation.

L414-418: repeated in the figure caption (Fig. 8), suggest reconstruction.

L498-499: not following this sentence, suggest rewording.

Figure 2: is the PM2.5 concentration shown here for near-surface or 850 hPa? Blue and red squares are hard to see on a printed copy. 2(i), could you also show BTH time series as well?

Figure 3: SO2 panel, is the NMB really 0.0% or a precision/rounding issue?

Figure 5: Wind speed and direction are critical to understanding the difference between obs and model, I wonder if daily or 6-hr (or even smaller intervals) averages of wind speed and direction can be shown to get a cleaner comparison between obs and model. It’s pretty hard to compare the two in the current form of Fig. 5, especially WD10.

Figure 6: suggest indicating the 1:1 line with a different color (other than red, as it thought it was a fitting line for the red dots at first).

Figures 3, 5, 7, 8, 11: needs x-label titles, e.g. day, time …