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Comment on acp-2021-769

Anonymous Referee #2

Referee comment on "Interaction between aerosol and thermodynamic stability within the planetary boundary layer during wintertime over the North China Plain: aircraft observation and WRF-Chem simulation" by Hao Luo et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2021-769-RC2, 2021

This work investigates the roles of the synoptic pattern, PBLH, aerosol type and vertical distribution in aerosol-PBL interactions by using aircraft measurements, model simulation. Several parallel numerical experiments are conducted to investigate the radiative effects of scattering and absorbing aerosols under different aerosol vertical distributions. Moreover, the long-term variation in PBL stability from 1980 to 2020 over the NCP region is examined. However, the current method and model settings in this work cannot well support the conclusion proposed, and need to be reconsidered. In addition, I personally think that hardly the case study for 2 days with a flawed method can be beneficial in determining which pollutants to target and achieving precise controls of air pollution. Here list some major concerns that need to be addressed.

Major comment:

The present study focuses on the case on 3-4 Jan 2020. However, the WRF-Chem model simulation was started on 2 Jan with only 16 hours as model spin-up. As well acknowledged, the atmospheric lifetime of aerosol is more than one week. That is to say, such a short spin-up time cannot reflect the aerosol background, chemical environment (OH radical, VOC levels and etc) and regional transport at all. Thus, it is not possible that the secondary scattering aerosol like sulfate and nitrate aerosol was well reproduced. I

suggest that the authors either prolong the model time or use the other model output as the chemical initial condition.

Another issue concerning the model simulation is that the model adopted a 3-km grid resolution but used an emission inventory with ~30km grid, which is not very matched with each other in spatial. Please clarify. Besides, since that NCP has experienced significant emission reduction in past years, please specify the base year of the emission inventory that was used in this work.

According to the model settings, the Morrison double-moment microphysics was utilized, which means that the aerosol-cloud interaction has been included in all the simulations. Thus, the differences in these simulations reflect not only the changes in ARE but also perturbations in CCN due to different emission scenarios, for example, EXP_WFexBC, EXP_WF20BC and EXP_WF20Aer. I recommend the authors reconsider and reinterpret the model result and check if it can support the conclusion.

Since the respective contributions of the absorptive and scattering aerosol are compared, the validations of the aerosol components are suggested to be conducted, especially BC, rather than just evaluating PM2.5. It seems that the aircraft measurement and model simulation are totally isolated in terms of chemical profiles. How did the model represent the vertical profile of aerosols? It would be interesting and imperative to compare the model and the measurements.

As pointed out, the combination of aircraft and model simulation was not so much. I do not think that the simulation needs to confine to these two days. Hardly the case study for just two days can represent the general conditions in this small region. While investigating statistical and general characteristics of aerosol radiative effect, simulation with a longer time period is strongly suggested. Otherwise, the implication of this simulation is expected to be very limited.

Figure 12 is not a good way to show the impact of PBL and pollution. PBLH cannot well reflect the structure itself. And for CO, it is a relatively long-lived species in the atmosphere with a background concentration of around 100 ppb. The short-term perturbations of aerosol on PBL just for two days cannot substantially influence the concentration since the background concentration in the lower troposphere is way larger than the perturbation caused by ARE.

Minor issues:

Table2: NMB makes no sense when evaluating air temperature.

Correct the unit of mass concentration to μg in main figures.

In Figure 13, Label the correlation coefficient and specify the location of LR in the caption. Is b-c the correlations between ESWM and LR, SH and LR or their anomalies? The data needs to be double-checked.