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Atmospheric Chemistry and Physics Discussions

Interactive comment on "The Contributions to the Explosive Growth of $PM_{2.5}$ Mass due to Aerosols-Radiation Feedback and Further Decrease in Turbulent Diffusion during a Red-alert Heavy Haze in JING-JIN-JI in China" *by* Hong Wang et al.

Anonymous Referee #2

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This paper investigated the impact of aerosol radiation feedback and decreased turbulent diffusion on PM2.5 during a heavy polluted episode in China. The objectives of this research might be interesting and potentially important; however, I have a number of concerns with the manuscript.

General comments

First, the lack of description about the GRPAES_CUACE model is troubling. What

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are the basic physical parameterizing schemes and chemical mechanism used in this study? How the model treat those crucial processes, such as SOA formation, two-way coupling, BC mixing states, aging processes. More important, how the model calculate the diffusion mixing? Any deficiency that can explain the supposed underestimation in diffusion coefficient, beside the lack of the aerosol radiative effect?

Second, I suggest the authors to provide additional validation of the model performance. How was the model performance in simulating the meteorological variables, PM chemical components and precursors? Does the underestimation apply to all PM components? It is also very important to exam that how the change in diffusion influence on the model performance in simulating species including both PM chemical components and precursor, since the mixing process is critical in determining the concentrations of all species.

Third, the description about scenario design need be elaborated. In EXP_td_af, how the dynamic field is updated by the aerosol feedback, and is there any nudging processed? In EXP_td20_af, how was the 80% reduction in turbulent diffusion implemented in the model. Did the change apply to all simulated domains? Is there any evidences or references which can support such modification? Based on the results (overestimation is found for clean days and areas outside JJJ), I don't think the DTD is applicable for all grid cells and days and can explain the underestimation of PM2.5.

Specific comments

Title: need provide some description about "Red-alert" in introduction section

Line 83: "GRAPES_CUACE", provide the full name and some references about the model.

Line 89: How to get the boundary conditions?

Line 92: "The model horizontal resolution is adopted as $0.15^\circ \times 0.15^\circ$". Is it high enough to capture the strong inversion during the episode? What about the vertical resolution?

Line 100: I would suggest the authors to elaborate the section 2.2. Is the emission data open to the public? What's the accuracy of the data? How does it compare to the others inventories, such as MEIC, EDGAR, etc? How was the spatial / temporal allocation processed?

Line 101: "human life", is it "domestic"?

Line 105-106: need provide full names for the VOC species

Line 121: "a further 80% decrease in turbulent diffusion (DTD) of chemical tracers based on EXP_td_af representing a compensation for the insufficient description of extremely weak turbulent diffusion by PBL scheme in atmospheric chemical model". how the 80% decreased DTD was determined? Was the overestimation of vertical mixing is due to the coarse resolution, or underestimation of aerosol feedback?

Line 134: in section 3.1, what about PM chemical component? The mixing basically can revolve the total PM mass. However, if the chemical profile doesn't agree well the observation, it still cannot solve the issue.

Line 155: "Some studies offline and online", is it "some offline/online modeling studies"?

Line 157: "AF of composite aerosols from black carbon, organic carbon, sulfate, nitrate, dust, ammonium, and sea salt aerosols had been online coupled into the in GRAPES_CAUCE model." how does the model treat mixing states and aging process? How is the model performance in simulating the PM components and AF?

Line 173: "the temperature inversion layer pre-existed during the haze event", it is not easy to see the temperature inversion in the plots.

Line 182: "Figure 4b shows that the observed temperature inversions were obvious stronger and the inversion depth thicker on 18 to 19 (during EGS of PM2.5) than those on 15 to 16 Dec (CS of PM2.5" But the PBL height seems opposite, lower on 18 to 19 but higher on 15 to 16 Dec.

Line 191: “The contributions to PM2.5 EG due to AF and DTD”. Since AF also contributes to DTD, how to separate these two effects.

Line 207: “Exp_bk under underestimated the PM2.5”, “under” should be deleted

Line 224: “the overestimation of turbulent DC”, is there any observation data to prove the overestimation of DC?

Figure 2: The PM2.5 in area outside JJJ seems all overestimated. The af/td cases make it even worse. Seems like it is not proper to apply the 80% DTD to all grid cells.

Figure 3: please clarify that the data is regional average in JJJ.

Figure 4: what about the days when PM reach peak for Dec 20-22 in Beijing.

Figure 5: PM2.5_td_af seems more reasonable than PM2.5_td20_af, in consideration of the possible missing heterogeneous chemistry. What’s the reason for the underestimation of the peak on Dec 21, even though the DC is already very low.

Figure 6: the figure is misleading. Since the reduced error in td20_af is because that the overestimation on Dec 18 compensates the underestimation on Dec 21 in Beijing.

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