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Atmospheric Chemistry and Physics
Discussions

Interactive comment on "Elevated 3D structures of $PM_{2.5}$ and impact of complex terrain-forcing circulations on heavy haze pollution over Sichuan Basin, China" *by* Zhuozhi Shu et al.

Anonymous Referee #1

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Elevated 3D structures of PM2.5 and impact of complex terrain-forcing circulations on heavy haze pollution over Sichuan Basin, China

submitted for publication to Atmospheric Chemistry and Physics

General Comment

The paper analyzes an episode with high concentrations of PM2.5 in the Sichuan Basin (China), combining observations and numerical simulations. The paper is potentially interesting, in particular for the peculiar interaction between meso and local circulations

and pollutant emissions, which leads to the formation of an elevated pollutant layer. However, the discussion of the results should be improved before the paper can be accepted for publication.

Specific Comments

1) Meteorological conditions

I. a general meteorological overview of the event, including a synoptic characterization, is missing in the paper.

2) Model set-up

I. The Authors adopt a grid ratio of 1:4, while an odd grid ratio is recommended because for even values interpolation errors arise due to the nature of Arakawa C-grid staggering. The Authors should at least discuss this choice.

II. The Authors say that the "vertical turbulent diffusion coefficient of the boundary layer was reduced". This aspect should be better discussed, since it might significantly affect the results.

III. No information about the vertical discretization is given. An adequate vertical resolution is fundamental to evaluate the thermal stratification over complex terrain.

3) Model validation

I. The Authors propose a series of statistical indexes for evaluating model results, both for meteorological variables and PM2.5. From these statistical indexes it is difficult to judge the performance of the model, regarding in particular the time evolution of observed and simulated variables. I strongly suggest to show some representative time series to better evaluate the model performance at some representative location.

II. Figure 4 presents a comparison between the vertical profiles of potential temperature, wind speed and relative humidity from observations and model results. Also in this case it is difficult to evaluate model results, since only mean profiles and the variation range over the entire period are presented. I suggest to show also some representative profiles at some specific hours. In particular, it would be interesting to evaluate how the WRF model is able to capture the vertical temperature profile, since atmospheric stability is crucial for pollutant dispersion. In many points in the paper a temperature inversion is cited, but the simulation of this temperature inversion is never discussed. For example, at lines 243-250, “thermo-dynamical structures” and “stable stratification” are cited, but, without a representative figure, it is difficult to follow the discussion of the results.

4) Language

I. Although the paper is rather well written, a review by a native English speaker would be beneficial

Minor and technical remarks

Page 2, line 79: “Section 2 introduced. . .”. Here and in other parts of the paper I would use the present tense (when referring to tables, figures. . .).

Figures 7 and 8: the location of the cross sections should be indicated in Fig. 1.

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