Dear Editors and Reviewers:

Thank you very much for your careful review and constructive suggestions with regard to our manuscript "Impacts of synoptic forcing and cloud inhibition on aerosol radiative effect and boundary layer structure during winter pollution in Sichuan Basin, China" (Manuscript Number: EGUSPHERE-2023-1806). Those comments are valuable and helpful for revising and improving our paper. We have studied these comments carefully and made changes in the manuscript according the reviewers' comments. The responses to the reviewer' comments are listed as follows, and the main corrections are marked with blue in the PDF file named "revised manuscript egusphere-2023-1806.pdf".

General Comments:

The manuscript entitled "Impacts of synoptic forcing and cloud inhibition on aerosol radiative effect and boundary layer structure during winter pollution in Sichuan Basin, China" classifies the synoptic patterns influencing the SCB based on long–term data. Using WRF–CHEM simulation experiments, the impacts of synoptic forcing and inhibition of cloud radiation interaction (CRI) on ARI with the PBL in the SCB is analyzed. However, the authors are expected to carry out the following revisions as suggested below which may be helpful for the modification. In addition, the writing should be polished.

Response: We appreciate the reviewer for the valuable and constructive comments of our manuscript. We have carefully revised our manuscript based on the following comments and polished the writing throughout the whole manuscript.

Specific Comments:

1. The title "Impacts of synoptic forcing and cloud inhibition on aerosol radiative effect and boundary layer structure during winter pollution in Sichuan Basin, China" is conf Synoptic forcing refers to synergistic forcing of what and what? Winter pollution means the pollution of air or water or soil?

Response: Thanks for this rigorous comment. Synoptic forcing refers to synergistic forcing of synoptic atmospheric circulation patterns, and winter pollution here means winter air pollution. To make the expression more accurate, we have modified the title to be "Impacts of atmospheric circulation patterns and cloud inhibition on aerosol radiative effect and boundary layer structure during winter air pollution in Sichuan Basin, China”. Please see the title in the revised manuscript.

2. This author should be more careful. Extra spaces in the author's byline.

Response: We feel sorry for this formatting error and have made modification in the author’s byline.

3. Lines 28-29: Sentence “Numerical simulation experiments using WRF-Chem showed afternoon upper–level heating and morning surface cooling forced by ...”should be improved.

Response: We are sorry for not expressing it clearly and have improved the sentence as “The results of numerical simulation experiments utilizing WRF-Chem indicated that there was a upper-level heating during afternoon and surface cooling in the morning forced by the aerosol radiation interaction (ARI) under the EHWL and LT patterns. Additionally, strong surface cooling in the evening influenced by valley winds can be found.”. Please see Line 30-33 of the revised manuscript.

4. Lines 33: “thinner cloud liquid content” is confused? You mean low content? Improve it.

Response: Thank you for the careful comment. We have modified it as “lower cloud liquid content” in Line 36 of the revised manuscript.

5. Key words: Capitalize the first letter of each phrase.

Response: Sorry for the clerical errors. In the revised manuscript, we have capitalized the first letter of each phrase in Line 43-44.

6. Lines 33, 109, 179: Extra spaces in the

Response: Thanks for this careful comment. We have carefully checked the manuscript and deleted the extra spaces in the revised manuscript.

7. What are the boundary conditions, solver setup and governing equations.

Response: We are appreciated with the comment.

The initial and boundary conditions for the numerical model were obtained from the NCEP FNL reanalysis data with a horizontal resolution of $1° \times 1°$ and 6 h time interval. For chemical process simulations, anthropogenic emissions were sourced from the Multiresolution Emission Inventory for China (MEIC) in 2016, featuring a grid resolution of 0.25° × 0.25°. To address the empirically overestimated $PM_{2.5}$ emissions by the MEIC in the SCB (Zhan et al., 2023), the ensemble square root Kalman filter were implemented on the $PM_{2.5}$ emission during simulation (Wu et al., 2018; Lu et al., 2021). Biogenic emissions were calculated online using the Guenther scheme (Guenther et al., 2006). The corresponding description could be found in Line 209-218 of the manuscript.

The main solver setup for WRF-Chem is presented in Table 1 in the manuscript as below:

Table 1 The main options of WRF–Chem

Items	**Contents**
Domains (x, y)	(155, 110), (184, 160), (320, 250)
Grid spacing (km)	27, 9, 3
Center	(29.1° N, 106.2° E)
Time step (s)	60
Microphysics	WRF Single–Moment 5 class (WSM5) scheme
Longwave radiation	RRTMG scheme (Iacono et al., 2008)
Shortwave radiation	RRTMG scheme (Iacono et al., 2008)
Planetary boundary layer	Younsei University scheme (Hong et al., 2006)
Land surface	United Noah land surface model (Tewari et al., 2004)
Cumulus parameterization	Grell–Freitas ensemble scheme (Grell et al., 2013)
Advection	fifth- and third-order differencing for horizontal and vertical advection respectively
Photolysis scheme	Fast-J photolysis (Fast et al., 2006)
Gas–phase chemistry	RADM2 (Stockwell et al., 1990)
Aerosol module	MADE/SORGAM (Schell et al., 2001)

The Advanced Research WRF (ARW) dynamics solver integrates the compressible, nonhydrostatic Euler equations, for example, the momentum equation, the continuity equation, the thermodynamic equation, the moisture equation and the ideal-gas equation of state. The details can be found in Skamarock et al. (2008).

We have added related descriptions in the revised manuscript. Please see Line 204-207 of the revised manuscript.

8. “The baseline experiment (BASE) considered both CRI and ARI, while the three sensitivity

experiments excluded ARI or CRI. Experiment 1 (EXP1) did not include ARI, Experiment 2 (EXP2) did not consider CRI, and Experiment 3 (EXP3) did not consider ARI when CRI was not excluded. The differences between BASE and EXP1 represented the disturbances caused by ARI, while EXP2 and EXP3 represented the influences of ARI without CRI inhibition.”. Description on the case setup is a bit unclear. It is suggested to represent it in a table that illustrates the differences between experiments.

Response: We are appreciated with this suggestion, and have followed it by adding Table 2 in the revised manuscript.

9. “Figure 2. Time series of $PM_{2.5}$ and potential temperature derived from the sounding data during 2015-2021 winter months. The $PM_{2.5}$ pollution episodes are marked with black dotted ” (1) The legend description should give the specific data time resolution in this graph. hourly or daily or monthly? (2) Try to give as much complete information as possible in the diagrams and don't use unofficial abbreviations, e.g. potential temperature abbreviations here.

Response: Thanks for the comments regarding Figure 2. In the revised manuscript, (1) The figure captions of Figure 2 and 3 have been modified with explicit time resolution to be “Time series of daily mean ...”; (2) The title of colorbar has included the full name of “Potential temperature” to avoid the use of unofficial abbreviations. Please see Figure 2 and Figure S2 in the revised manuscript and supplement.

10. Figures 4, 10, 11, 12. Mark the Sichuan Basin in this figure.

Response: We are appreciated with the suggestion and have marked the Sichuan Basin outline with an altitude contour of 750 m terrain height in the corresponding figures. To streamline the manuscript, we have moved some figures to the supplement and merged Figure 10 and 11. As a result, the orders of the figures in the revised manuscript have been changed. Please see Figure 3, 7 and 8 in the revised manuscript.

11. Figure 6: Relationship of $PM_{2.5}$ concentrations and the day to day 850 hPa synoptic patterns is not pictured clearly and should be improved.

Response: Thanks for the suggestion. We have added some arrows in Figure 6 to make the relationship of daily mean $PM_{2.5}$ and day to day 850 hPa synoptic patterns clearer. It is obvious that pollution episodes always begin with Pattern 2 and ended with Pattern 1, suggesting that Pattern 2 is the key synoptic pattern for pollution initiation. Besides, Figure 5 provides a statistically and quantitative clearer representation of the relationship between daily mean $PM_{2.5}$ and day to day 850 hPa synoptic patterns. Please see Figure S3 in the revised supplement.

12. Lines 308-309: Figure 8: The section on model validation is not sufficiently analyzed, especially the lack of quantification of the correlation between the model and the observations, the assessment of the consistency, and the explanation of the reasons for the large differences, such as wind speeds.

Response: Thank you for the valuable comment regarding the evaluation of model performance. We have provided the statistical metrics between the simulation and observation of $PM_{2.5}$ and the meteorological factors in the revised Figure 6 and S4. The MB of the simulated and observed $PM_{2.5}$ concentrations were -15.59, -13.42, 2.10 and -13.11 $\mu g/m^3$, respectively, with NMB values of -4.12%, -4.22%, 6.01% and -0.68%, respectively, which are within the acceptable standards (NMB < ± 15%). The R of $PM_{2.5}$ are 78.91%, 57.23%, 61.15% and 62.86% for 4 representative cities, respectively. The statistical metrics for $PM_{2.5}$ are consistent with previous studies (Wang et al., 2020; Shu et al., 2021; Zhan et al., 2023), indicating that our model results for $PM_{2.5}$ are reasonable and acceptable. Regarding to the surface meteorological factors, low MB and high R for both temperature and dew point temperature suggested good simulation performances for these variables. However, the simulation results for wind speed were poor, which was expected under conditions of low wind and complex terrain. Due to the starting speed of the anemometer and high calm wind frequency in the SCB, the disturbances of observed wind speeds were usually smaller than simulation. This led to a significant deviation in the simulation results (Shu et al., 2021; Zhan et al., 2023). Additionally, it could be argued that unresolved topographic features introduce additional drag, beyond that generated by vegetation, which was not considered in the WRF model (Jimenez and Dudhia, 2021). These findings indicate that the simulation of $PM_{2.5}$ and meteorological factors is reasonable in the SCB; thus, the simulations can be used for subsequent analysis. Please see Figure 6 and S4 of the revised manuscript and supplement. The corresponding description can be found in Line 381-396 of the revised manuscript.

13. Lines 312-313: “The simulation aligned well with the sounding observations, reflecting upper air warming and PBL humidification during the accumulation process of PM2.5 ...”is not appropriate. With regard to the comparison between simulated and observed vertical profiles, the results are not very good. Even the temperature, which is best simulated accurately, shows a maximum difference of up to 5 ℃, mainly within the boundary layer (below 1.5km), so is the choice of parameterization scheme open to question? Not to mention the relative humidity and wind speed, which are numerically very different, but the trending is okay.

Response: Thanks for indicating this. Regarding to this issue, we have checked the drawing code and found that we have not unified the color scale when drawing the vertical distribution figures, which caused significant differences in temperature values within the boundary layer in the figure. However, based on the variations of surface temperature (Figure 6 and S4), it can be seen that there are no so many differences in surface temperature. We have unified the color scale in the revised figure. In terms of the choice of parameterization schemes, the schemes employed in this study is the one used by the Chongqing Meteorological Bureau in the daily operational activities. The schemes have been obtained through multiple sets of control experiments and are considered suitable for the simulation in the SCB. Additionally, we have also attempted to conduct the simulation experiment with parameterization schemes proposed by previous studies relating to the SCB simulation. The figure below illustrates the comparative results among simulations with parameterization schemes of this study, Wang et al. (2020) and Zhan et al., (2023). It can be seen that these schemes do not yield better simulation results than our chosen schemes when compared with the sounding data. The SCB is characterized with cloudy and foggy conditions, which result in abundant water vapor and near 100% relative humidity above the nocturnal boundary layer. Models often underestimated the humidity above the boundary layer during night in the SCB (Shu et al., 2021). Furthermore, due to complex terrain and measurement bias of the anemometer for weak winds, the evaluation of simulation results for wind speed often exhibited certain deviations (Jimenez and Dudhia, 2021; Shu et al., 2021; Zhan et al., 2023). Overall, the simulation results can capture the meteorological variation trends. According to the simulation evaluation standards for the SCB in previous studies (Wang et al., 2020; Zhan et al., 2023), the simulation results is acceptable and can be used for subsequent analysis and discussion.

In the revised manuscript, we have updated Figure 6 and S5. The description of simulation results has also been refined to make it more consistent with the figures. Please see Figure 6 and S5, and Line 397-413 of the revised manuscript.

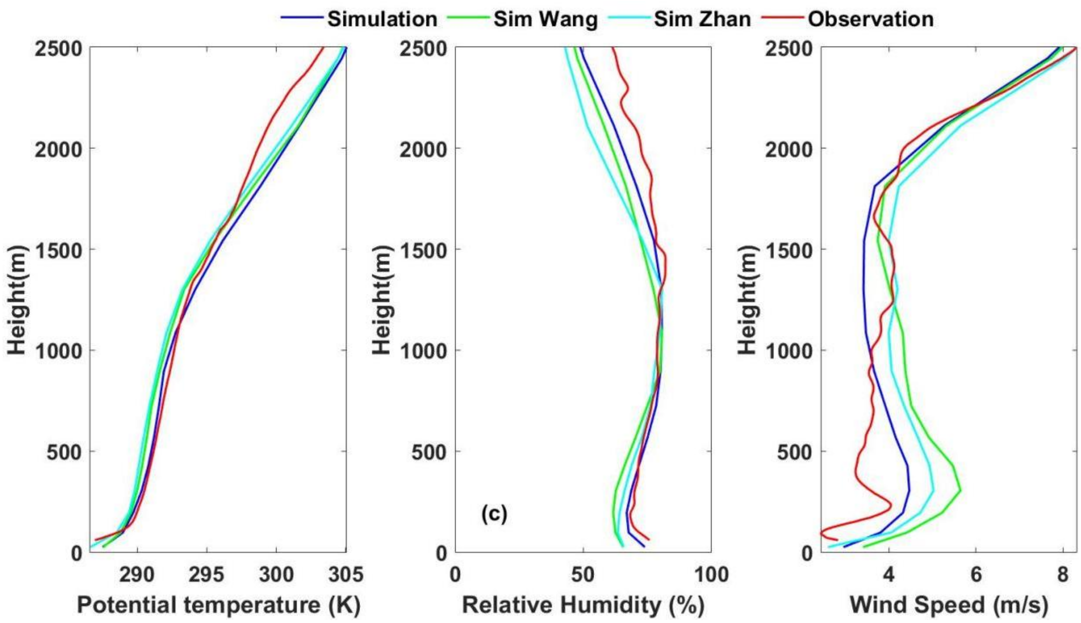

Figure 1 Comparison between observations and simulation results using different parameterization schemes in this study, Wang et al. (2020) and Zhan et al. (2023).

14. Figure 9. Regarding the boundary layer height, illustrate the determination of BLH from simulations. From what seen in Figure 9, I think your BLH is not consistent with the potential temperature profiles and wind profiles. And according to the literature below, BLH determination method is relative to boundary layer structure. 1016/j.atmosres.2020.105179

Response: We are appreciated with the comment and feel sorry for not clarifying clearly about the method and data to calculate the PBLH.

There are various methods to determine the PBLH, as Jiang et al. (2021) mentioned by the reviewer, who estimated the thermodynamic and material PBLH based on MWR with the parcel method and ceilometer, respectively. The dynamic PBLH can be calculated by the the bulk Richardson number (*Ri)* method or some other methods, like derived from the TKE profiles. The differences in methods, data or threshold values may yield quite different PBLH results (Seibert et al., 2000; Eresmaa et al., 2006; Jiang et al., 2021).

The hourly PBLH variations (red line) in Figure 9(a)-(d) are obtained from ERA5 data. In the revised figure, PBLH calculated based on sounding data (green dots) is also included in Figure 6(h) and S5(a)-(d), despite its low temporal resolution and inability to capture the peak values of diurnal PBLH. The hourly PBLH variations (red line) in Figure 6(k) and S5(e)-(h) are derived from simulation results. Both the EAR5 and YSU schemes use the *Ri* method to calculate the PBLH, with a threshold value of 0.25 (Hong et al., 2006; ECMWF, 2017). Therefore, the PBLH calculation based on sounding data also employs the *Ri* method and the same threshold value.

Theoretically, the variation of PBLH should be consistent with the variation of potential temperature or wind speed. However, the PBLH calculated using the *Ri* method may not perfectly align with the isentropes or wind speed contours, but just exhibits the similar trends. Furthermore, the potential temperature and wind speed in Figure 6 and S5 are obtained at 12-h intervals (00 and 12 UTC), with the same temporal resolution as the sounding data, which may not fully capture the diurnal peak and finer variations. But the PBLH derived from ERA5 and simulation results are provided at hourly intervals. Differences in temporal resolutions may also result in some discrepancies between variations in the PBLH and the potential temperature or wind speed.

We have added details of the method and data to calculate the PBLH in the revised manuscript. Please see Figure 6 and S5 of the revised manuscript and supplement. Corresponding description can be found in Line 167-177 of the revised manuscript.

15. Figure 10: The magnitude of the wind speed represented by the arrow vectors is not specified.

Response: We feel sorry for the neglect, and have added the specific magnitude of the wind speed in the legend of Figure 10. Please see Figure 7 of the revised manuscript.

16. Too many pictures in the article, Figures 10 and 11 are suggested to be merged.

Response: Thank you for the suggestion. We have merged Figure 10 and 11 to reduce the number of pictures and improve the overall flow and readability. Please see Figure 7 of the revised manuscript.

17. For all images, both in the picture and in the title, lack of space between brackets and words.

Response: Thanks for pointing this out. We have modified all figures and their titles concerning this issues. Please see figures and their titles in the revised manuscript.

18. Figure 15. This abstract figure is good. But the title "The Synergetic interactions of cloud, aerosol and radiation under the influence of cloudy pollution synoptic forcing." is a little confused. And the second word should not be capitalized.

Response: We are sorry for any confusion caused by the captain of Figure 15, and have rephrased the sentence. Please see captain for Figure 12 of the revised manuscript.

19. The article is overloaded with graphs, some graphs such as Figures 5 and 6 express similar results and meanings, and can make trade-offs. The focus of the article's diagrams is unclear; some of the diagrams could be placed in supplemental material.

Response: We are appreciated with the valuable comment and have adjusted with diagrams in the manuscript. Figure 3 and 6 have been moved into the supplement, while Figure 10 and 11 have been merged. Besides, Figure 8 and 9 have been moved to the supplement, and information in CD have been merged to be Figure 6 as an example. Please see figures in the revised manuscript and the supplement file.