Comments on “Impacts of anthropogenic aerosols on a snowfall event – A case study in the Guanzhong Basin and its surrounding areas, China” by Yang et al.

General comments

This paper uses a cloud-resolving fully coupled WRF-Chem model to investigate the respective roles of aerosol-radiation interactions (ARIs) and aerosol-cloud interactions (ACIs) on a winter snowfall event in the Guanzhong Basin and its surrounding areas. The study systematically separates the contributions of ARIs and ACIs through a combination of anthropogenic emission scaling and ARIs on/off experiments. Overall, I think it is a solid work with a well-designed suite of sensitivity experiments. I suggest publishing it after addressing the following comments with a major revision.

Specific comments

1. The title and abstract emphasizes snowfall event , but the main text lacks a focused description of this event. It is recommended to either add a more detailed description of the event, or revise the abstract and title to reflect the broader scope.

2. Lines 129-131: The authors stated that F_ARI0 is setup by not considering ARI. Could you add more details about how do you remove ARI in the model?

3. **Section 2.1:**The 19 AESF values are not explicitly listed. If possible, please correct this and consider adding the full list of scale factors for clarity.

4. **Figure 2**: Only a brief comparison between the observations and simulations of $PM_{2.5}$ and SO_2 in Figure 2 was presented, without covering the comparison of O_3 and NO_2. Is there a particular reason why these two variables are emphasized here? Some differences can also be observed from Figures 2b and 2c.

5. **Section 3.2:** The simulation assessment section directly states that the simulations all underestimated air pollution, but does not provide the reasons for this. It is suggested to include a brief discussion on the possible causes of the simulation deviation.

6. **Figure 4:** It was pointed out that in Figure 4, five peaks were successfully simulated, but two of the peaks were significantly lower. Are there any studies that can explain the reasons for the low values of these two simulations? It was also found that there was a low-value simulation result that was much higher than the observation. Could this also be presented?

7. **Lines 184 and 203:** Where can we see the extent of the decline that "the PBLH is decreased by about 34% in GZB+GZBs"? It was later found that "PBLH over GZB+GZBs decreases from about 450 m to 300 m" in line 203. Are these two

statements related to each other? If so, it would be better to present the specific values alongside the percentage at the first mention to improve clarity. Moreover, the specific values (450 m and 300 m) are presented without a clear reference to a figure or table. It is recommended to either add a figure showing the PBLH variation with AESF, or explicitly indicate that this result is derived from model output statistics

8. **Figure 7:**The figure presents only the difference in vertical velocity between F_BASE and F_ARI0. The convergence phenomenon mentioned in the text cannot be directly derived from this graph. Additional diagnostics are needed to support this claim.

9. **Line236:**The statement that "decreasing RHW also explain the slow increase of the CND" is problematic because it uses one outcome of ACIs to explain another. A more straightforward explanation—such as how lower RHW makes it harder for CCN to activate, limiting the increase in CND—would be more logical and easier to follow.

10. **Line 268**:As can be seen from Figure 8a, CND does not decrease as R_{effc} does with the increase of AESF. Only when WIth ARI and it is greater than a certain value will it be reduced.

11. **Section 3.5:**Why was it specifically pointed out that Figure 11a does not take into account the ARI situation? This increase is too small, and it is even smaller compared to the increase trend when ARI is considered.

12. **Figure11**: It shows that the precipitation of GZB and GZBs has opposite responses to the increase in emissions. However, the article does not provide a comparison of the meteorological fields and cloud microphysics background of the two regions, which makes the explanation for "why they are opposite" somewhat weak. It is suggested to add an additional figure to illustrate the differences between GZB and GZBs in terms of the RH contour, the vertical distribution of cloud water/ice water, and the proportion of liquid water paths.

13. **Line260:** It would be better to add "As can be seen from Figure 9c, " at the beginning of this sentence to explicitly link the statement to the corresponding figure.