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Comment on acp-2022-405

Anonymous Referee #2

Referee comment on "Aerosol–cloud interaction in the atmospheric chemistry model GRAPES_Meso5.1/CUACE and its impacts on mesoscale numerical weather prediction under haze pollution conditions in Jing–Jin–Ji in China" by Wenjie Zhang et al., Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2022-405-RC2, 2022

General comments:

This paper investigates the ACI in Jing-Jin-Ji region in China by specifically consider the ACI processes in GRAPES_Meso5.1/CUACE model. This topic is interesting and the results are potentially useful. However, major revision is required before this paper can be accepted by ACP.

Major comments:

- Some important details about the model is missing. Please see further comments below.
- The results are sometimes speculative, especially those in Section 3.4. Besides, in Section 3.4, two sub-regions are selected for analysis; while in Figure 4, the whole JJJ region is treated as a whole. This treatment makes the presentation repetitive, and also makes the logic of this paper quite difficult to follow.
- The English language is rough at some places, and needs improvement. Some places are mentioned in the minor comments, but other places are not.

Minor comments:

Line 38: 70% percent is probably near the higher end of global cloud cover reported in the literature, so it might be better to use a reference here.

Lines 50-51: “to be defined” to “to define”?

Figure 1: It is really difficult to see the automatic weather stations from this figure.

Line 142: There are 120 h from 4-8 January, while the prediction time plus the spin up time is 144 h. Is here an error?

Line 143: “achievement” to “implementation”?

Section 2.3. Some important details are not clear or missing here. For example, how droplet activation and ice nucleation are calculated? Which variables in Eq. (1-3) are prognostic by the model and which variables are specified? If specified, what are the values? What are the meaning of tracer number 1-49? Are scavenging processes considered? And some other details. I understand that some details might be included in the references, but a concise description might be helpful for the readers.

Figure 3. Specify which experiment is presented here. More importantly, why do the authors display the whole simulated domain instead of JJJ only, since almost all discussions focus on JJJ?

Section 3.1. Why do the authors compare E1 with the observations. If simulation E2 is the better one (which is the major point in this paper), it might be more appropriate to compare E2 with observations.

Line 204: “reasonably” to “reasonable”?

Figure 4. Why is the spin-up period also displayed?

Line 275: It may be inappropriate to present the maximum value of decrease, because it

may not be representative. For example, a slight shift of cloud position may cause a large change in SDSR.

Figure 9 and related discussions: What are the criterion of selecting the light-rain region and the moderate-rain region? In the northeast of the JJJ-region, there is also a contiguous precipitation region, which is this region not analyzed?

Line 333-335: Please clarify whether the absolute value or the relative values are compared. For example, the authors used “more significant” in line 334, however, the percentage change is “less significant”.

Line 339: Aerosol level might also refer to aerosol height. Change “level” to “concentration” might be better.

Line 343: A brief introduction of how supersaturation is calculated is helpful in interpreting the results here.

Line 345: What is “pre-calculated”?