

Review for “Anthropogenic aerosol influence on a mixed-phase cloud precipitation in early Meiyu season over Yangtze River Delta: simulated microphysical and thermodynamic effects” by Ruiyu Song, Bin Zhu, Lina Sha, Peng Qian, Fei Wang, Chunsong Lu, Yan Yin, Yuying Wang

Summary

This study discusses the effects of aerosols on precipitation formation using a model setup with varying aerosol concentrations. The authors show that a precipitation shift occurs with increasing aerosol loads. One driving mechanism is the cloud invigoration upon cloud droplet formation and / or ice crystal formation with its associated latent heat release. Aerosol–cloud interactions are often masked by various dynamical and thermodynamical effects, making it especially difficult to assess the impact of aerosols. The authors tried to alleviate that, however, I have several concerns regarding this study. First of all, the literature background needs to be drastically extended given that many (more recent) references are not even cited. The process of cloud invigoration is not discussed properly, but it is taken as a given that the latent heat release leads to cloud invigoration. Second, the methods are not clear enough such that the study could be independently repeated. The model setup is sparsely discussed, the microphysics schemes is not discussed at all, and how the aerosol perturbations are done is not discussed either. The authors say in their title that they talk about anthropogenic aerosols, but they never clearly state how they perturb the model. Throughout the manuscript, there are incomplete sentences and / or vague ones. I highly suggest the authors to be more quantitative in their arguments. In the current state of the manuscript, I recommend it to be rejected, such that the authors can improve their study, their analyses, and arguments. I hope my detailed comments below will help them in that regard.

Major comments

- **Literature background:** I believe the introduction is missing key references regarding aerosol–cloud interactions, as on Line 34 three references from 2008 are cited, but nothing newer. This is however not the current state of research and especially given the controversy regarding the Rosenfeld study, all possible aspects to aerosol–clouds interactions should be highlighted and clearly noted. This also goes on in Lines 37-39 and Lines 45-50. The authors strongly assume that the latent heat release upon cloud droplet formation or ice crystal nucleation must invigorate the cloud. However, there is more recent literature (Igel and van den Heever, 2021, GRL and Varble et al., 2023, ACP) illustrating the complexity behind aerosol–cloud interactions and calling the direct assumption of invigoration upon latent heat release in doubt. The authors should consider the full literature background of their study and accurately place their assumptions within that context.
- **Model setup and case study:** The overview Table 1 is missing many references to the schemes and the authors only state, that this is the best combination, but in terms of what? From the text I am wondering about the skill of the model to simulate deep convective clouds at resolutions of 27 km and 9 km. Is only the latter the point of analysis? Was the deep convection parameterization turned on or off for the 27 km domain? What variables were nudged? I am missing a contextualization of the case study, under what conditions they were simulated, and how the rather few grid points at a rather coarse resolution for deep convection can be justified? How was the spin-up time chosen? Is the nesting applied a one-way or two-way configuration? While the authors show a start of model validation with observations, this is not enough. In Figure 2, how was the precipitation rate averaged? I do not see the river in the map, so I am not clear on where the precipitation was anomalous high / low, and while the authors cite studies, here they need to show again how this relates to the climatology, especially in numbers and not only in high / low. The case study description should be illustrated by weather maps. The authors say they focus on the heavy precipitation areas: I would recommend to mark this region in the figure. Are there any gridded observations such as from CMORPH to have also a spatial comparison of model and observations? In 2b: what does 0 mean here? When the storm started? Or the model simulation? When was the model initialized then, based on the text, maybe >1 day before? What actually is the base concentration? This was never stated. There is no discussion on the microphysics and aerosol scheme. Especially it is not discussed what aerosols are perturbed (CCN or INP, species, size, ...).
- **Aerosol impacts:** Figure 3 shows (to me) randomly selected precipitation patterns, based on which the authors argue that the pattern more or less is widespread due to aerosol perturbation. However, I disagree with this, as I do not see a clear signal in Figure 3, and would expect a thorough quantification of the spread instead of a qualitative one. A difference plot could also already help, and ideally done for all sensitivity simulations. In Line 157: when the authors talk about the entire layer, what does this mean? Across what layer? PBL? Storm layer? There is no discussion on the significance of these results, which should be done. Are the changes in

precipitation really significant? The authors argue with insufficient water vapor supply to cloud droplet growth. What vertical velocities were in these clouds? In deep convective storms supersaturation with respect to water of up to 10 % can be found, locally in updrafts. Doing domain-averaging of updrafts (amounting to less than 1 m/s in Figure 5) usually dampens the signal, so I would opt for a different strategy for analyzing the connection between latent heat release and updraft, which may actually underscore the authors points even more. Their assumptions should be easily testable by checking the specific humidity tracer. A discussion on their R^2 should also be done, given that is is a non-linear function fitted to the data points for Figure 4. As the authors show averages across a “layer” at least the air density should be included in the analyses of the cloud microphysical properties. Are the patterns in Figure 6 significant? Or is maybe to some extent also internal model variability? In summary, the methods arrived at the results are not shown at all, thus making the comprehension of the presented arguments difficult.

- **Cloud invigoration:** This highly uncertain topic is handled very briefly by the authors in the results without providing much in-depth analysis regarding the microphysical processes. And again here, all novel literature on this topic is not discussed or cited at all. I recommend a more thorough analysis of this section, especially linking the latent heat release with increased updraft velocity. I do like the decomposition analysis in Figure 7 and 8, which shows a more in-depth analysis than what was done before, but I do not fully understand it. Over what time range were the tendencies accumulated? And how does this work with a moving system? It is very hard to read the numbers in Figure 7 for the latent heating. I do find it difficult to find differences Figure 8 for the cloud water tendencies. Can the authors please explain that? And how can they be sure to exclude any dynamical affect on the updraft changes?

Minor comments

- Line 14: WRF-Chem, in what resolution was this used? When is the Meiyu season?
- Line 16: can you be more specific what low to normal levels means?
- Line 21: how does the reduced cloud droplet collision efficiency lead to enhanced evaporative cooling and reduced vertical velocity?
- Line 22: how far downstream?
- Line 31 and 34: a comma is missing after interactions
- Line 58: what are high and low emissions, these are strongly region dependent values, and should be stated explicitly.
- Line 69: impacts → impacted
- Line 73: air pollution diffusion conditions: do the authors mean the transport of aerosol from other areas?
- Line 57 and 64: Why are these acronyms defined if they are never used in the manuscript?
- Line 96: YRD was already introduced.
- Line 98: the authors say this region is used in many studies, however, they do not give any references.
- Line 160: what does sharp intensification of CCN mean?
- Line 198: what is meant by material base?
- Line 228: I strongly disagree with this statement, and no significance was shown, and it is not obvious from Figure 3. Please see my comments above.
- Line 237: I do not understand this sentence. The authors say a different study showed the same effect? For the same area? In what setup?
- Line 255: A verb is missing for the sentence