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Atmospheric Chemistry and Physics
Discussions

# *Interactive comment on* "The effects of cloud-aerosol-interaction complexity on simulations of presummer rainfall over southern China" *by* Kalli Furtado et al.

**Anonymous Referee #4**

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Review of "The effects of cloud-aerosol-interaction complexity on simulations of presummer rainfall over Southern China" by Furtado et al.

This manuscript investigates aerosol-cloud interaction in a squall line case in Southern China. Two types of simulations are performed: one is to use prescribed aerosol concentrations, and the other is to include aerosol recycling after cloud-processing. The purpose is to study if the cloud-processed aerosols (as there is a transition from accumulation mode aerosols to coarse mode aerosols after the collision-coalescence process) can change the precipitation of the squall line case. It is concluded that aerosol recycling after cloud-processing can affect the precipitation through the enhancement



of warm rain process.

I think it is very interesting to show that the cloud-processed aerosols can promote the warm rain process. It is also interesting to see the "polluted source - clean core" structure of the system. However, I think the manuscript needs major revision before it can be published in ACP.

Major changes:

1. A more detailed and logical discussion on the microphysical processes should be provided.

The manuscript concluded that the cloud-processed aerosols can change rain formation mainly through the warm rain process, not through the melting of ice phase particles. However, the fact that "differences in rain-drop numbers are largest close to the melting layer (page 9, line 5)" is an indication that changes in rain in the simulation with cloud-processed aerosol is somehow related to the melting of ice phase particles. The authors did point out in a later paragraph (page 9, line 20-22) that, if the melting of ice phase particles is dominant, then more rainfall should be associated with high cloud droplet number and increased ice particle production. This study did not find these characteristics, so it is concluded that the melting of ice phase particles is not important. I think the manuscript should be revised to explain these microphysical processes in a more detailed and organized way. The conclusion that the cloud-processed aerosols can change the precipitation through warm rain process instead of the melting of ice phase needs to be explained in a very clear way.

I can see from Figure 5b that rain rate is related to ice water path. But this study seems to think that the ice phase process has secondary importance.

The ice phase processes need more explanations. For example: page 6, line 14, 1s0dp has more rain, but less snow. Why? The manuscript should at least provide a short explanation why snow particles has lower concentration in this case. Note that all the explanations should be consistent with the results shown in Figure 4.

Some of the discussions related to ice phase processes might be wrong. For example: page 9, line 16-17, there is more graupel in the low aerosol experiment (because more rain is available for freezing). I would think the opposite: there is more graupel in the low aerosol experiment, so there is more rain from the melting of graupel. I strongly suggest the authors explain the mechanisms in a logical and clear way so that we can understand both the warm and cold cloud processes. Page 9, line 14-15, it is hard to understand this sentence.

Page 9, many characteristics of rain is discussed, including rain drop number concentration, the amount of rain, rate of rainfall, (line 29), and rain water (line 30). I suggest the authors explain these microphysical characteristics step by step. For example, I can easily understand that the cloud processing of aerosols can lead to changes in rain number concentration. But how the rain water and rain rate are affected should at least be explained.

Page 9, line 12: the “minimum” cloud droplet concentration. What does this minimum mean? Processes that can reduce cloud droplet concentration include collision-coalescence, entrainment and evaporation. Why would this minimum be so important? 1s0dp has cloud-droplet numbers that are orders of magnitude smaller than those in 5e7F. I think this is really a significant effect due to the cloud-processed aerosols. The manuscript should emphasize this point.

2. The setup of the model is not described very well.

Firstly, is the immersion freezing process affected by the different setup of aerosols? For example, it is said on page 4, line 26: a fraction of droplets are specified as ice. So for the 5 simulations in this study, cloud droplets have quite different number concentrations, then do they have quite different ice concentrations? Secondly, what is the use of the 5e5F and 5e8F simulations in this study? 5e5F has a really low aerosol concentration, which is probably unreal. In figure 8, if the 5e5F case is removed, then



the other simulations do not show much difference in precipitation amount. 5e8F is actually not quite “polluted”.

3. Some of the main findings are not reflected in the abstract.

Line 7: what kind of vertical structure? What kind of statistics of surface rainfall? I think the writing could be more specific. For example, a more frequent and less intense rainfall are associated with higher aerosol concentration. This kind of clear results should be mentioned in the abstract. In addition, “a modulation by aerosol of the time scale”, what kind of modulation? Are the evaluations of the simulations with observation data good? These should also be mentioned in the abstract.

Minor changes:

Figures 1-3 are too small. Especially the words and numbers in these figures are too small.

Figure 7, if the 20% criteria are changed, say, to 25%, are the results still robust?

Figure 8, (a) and (b) are missing in Figure 8.

Page 9ïijŇline 14, "eventual" should be changed to "eventually".

Page 13, notation 3, “of” should be changed to “or”.

Page 6, line 15, F5e7 should be 5e7F?

Page 8, line 9, F5e6 should be 5e6F?

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