

Interactive comment on “Understanding aerosol–cloud interactions through modelling the development of orographic cumulus congestus during IPHEX” by Yajuan Duan et al.

Anonymous Referee #4

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This study introduces a cloud parcel model (CPM) which includes aerosol activation, droplet condensation, collision-coalescence, and entrainment to study the cloud-aerosol interaction in cumulus congestus clouds. A case study that used the aircraft measurement and WRF simulations as the input parameter and initial conditions was studied. Sensitivity studies on the parameters were conducted. The conclusion was that the cloud development was sensitive to the aerosol concentration, entrainment, condensation coefficient but not sensitive to the hygroscopicity. Finally, limitations of the study were analyzed.

General remarks (Page = P, Line = L):

1) Overall, the manuscript is not well-organized. First, the objectives of this study are not clearly present to readers. Second, the descriptions and analysis of the experiments were spread out in the manuscript, appendix, and the supplementary information. For example, part of the measurement data was introduced in the manuscript, and some others were described in the supplementary information. But the authors referred both back and forth (esp. when referring to the figures), which made the readers hard to follow coherently. I suggest the authors either merge the same content to the manuscript or to the supplementary information. Same suggestions go to the appendix.

2) The content in the “summary of sensitivity analysis” and “summary and discussion” are redundant and most of the contents can be found in both sections.

3) The author used “as expected” many times throughout the article (e.g., P 11 L 11, P17 L8, P19 L25), while the physical explanation behind the “expected” results was very obscure. I suggest the author remove such statements or bring out explicitly the explanation.

4) The scientific contributions of this study are not clear. I feel the paper mainly focused on building up the model, testing the sensitivity of the parameters and confirming the test results through comparison with previous studies. It is suggested that more effort should be put into giving physical explanations.

Specific remarks:

1) The author mentioned E_{coal} in equation (11), but what value was taken and what assumption was made? It was not mentioned in the paper.

2) The author mentioned cloud “core” several times, but what defines the core? Was it defined by the updraft speed exceeding a certain value?

3) On P11 L11, the author states that “droplet spectra in stronger updrafts at the core” have “narrower size range compared to the samples at the edge of the cloud”. This

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seems to contradict to the founding in the P3 L13 in the supplementary information that stronger updrafts lead to a wider spectrum. Does this suggest that both entrainment and strong updraft help broaden the size spectrum? If so, the comparison between strong in-cloud updraft region and weak updraft in the cloud edge was not sufficient to give any physical implication.

4) In section 4.2.2, the author stated implicitly that the entrainment strength was inversely proportional to the cloud parcel size R , but the accurate formulation of entrainment strength was not shown.

5) In P18 L20, the author relates cloud base height to the scale height, H_s . How was H_s calculated?

6) The last paragraph in P18 was mainly a review of the previous studies, this should be moved to the introduction, otherwise, the authors are suggested to make connections with the results of the present study.

Based on the comments above, I suggest the paper is not suitable for publication on ACP unless all the above concerns have been addressed.

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