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Atmospheric Chemistry and Physics Discussions

Interactive comment on "Understanding aerosol-cloud interactions in the development of orographic cumulus congestus during IPHEx" *by* Yajuan Duan et al.

Anonymous Referee #1

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This paper shows some very interesting results by applying a modified cloud parcel model (CPM) to study the aerosol-cloud interactions (ACI). The authors first describe the processes included in the CPM, such as collision-coalescence, and lateral entrainment. Then the authors show the observations from the IPHEx field campaign, which include ground and aircraft measurements. A series of parameter sensitivity analyses are carried out using the CPM and the model outputs are compared with the observations. This paper is certainly of great interest and well within the scope of ACP, but it can also be significantly improved. I have several comments:

1. Page 2, Line 30-34: The authors try to point out the shortcomings of parameteri-

zation in the model. Instead of using "inadequate to capture the spatial and temporal resolution", it would be better to list some detailed discrepancies between model and observations from the literature.

2. Page 8, Line 10-13: Euler method is used as the integration method for the collision-coalescence processes. The reason is "to examine its role individually in cloud formation". Does this mean the collision-coalescence processes do not suffer from stiffness? How would you justify the benefit of using the Euler method while it may potentially cause numerical instability in the model?

3. Page 11, Line 10: in Fig. 5b, when CDP LWC value is close to zero, there is a clear intercept of $\sim$0.05 g m-3 in King LWC. As such, including an intercept value in the linear regression would produce a better fit (i.e., fit to the equation y = a x + b instead of y = a x). Please explain why the intercept is not included in the linear regression.

4. Page 15, Line 6-8: The underestimation of supersaturation by model is argued to be due to the uncertainties of temperature and humidity in WRF simulation. However, in the sensitivity test discussed in Appendix B1, adjusting the temperature and humidity increase the supersaturation to $\sim$0.5% (Fig. B1(a)), which is still significantly smaller than the observations. This indicates that the temperature and humidity in WRF simulation do not have a strong influence on the supersaturation profile. Could the authors list other factors that affect the supersaturation profile?

Minor comments:

1. Page 2, Line 25-26: the scale gap should be 5 to 9 orders of magnitude when comparing μm, cm with km.

2. Page 5, Line 11: "Fig. 2" appears earlier in the text than "Fig. 1" (Page 8, Line 17), thus the order of Fig.1 and Fig. 2 should be switched.

3. Page 8, Line 29: "Aerosol observations were collected" should be "Aerosol observations were carried out".

4. Page 8, Line 30: first time “MSL” appears, give full name.

5. Page 9, Line 1: “scanning mobility particle counter system (SMPS)”. Please provide the manufacturer of the instrument. This applies to other instruments listed thereafter.

6. Page 9, Line 7: “shows very close agreement with the SMPS measurements”. Maybe the authors could provide some data (e.g., correlation coefficient) to show the degree of agreement.

7. Page 9, Line 11: “8 mins” should be “8 min”.

8. Page 14, Line 12: “range [0.001–1.0]” should be “range [0.001, 1.0]” or “range 0.001–1.0”.

9. Page 15, Line 7: “obtained the WRF simulation” should be “obtained from the WRF simulation”.

10. Page 34, Table 2: please make the significant figures consistent within each parameter.

11. Page 42, Fig 7c and 7d. It is difficult to differentiate lines in same color from each other. Please consider using different colors for each line if possible.

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