Authors Comment (AC)

On the Referee Comments (RC) #1

We thank the Anonymous Referee #1 for the positive comments and constructive suggestions, which helped improve the manuscript. Item-by-item replies are inserted in blue, whereas the Referee comments are in black.

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

The Referee's comment is well taken. The following sentences were added in the revised manuscript (Pg. 2, Line 31 - Pg. 3, Line 5) "In particular, six microphysical parameterization schemes available in WRF were examined to investigate the spatiotemporal evolution of low level moisture fields in the SAM under weak and strong synoptic conditions. However, the simulations could not capture persistent low-level clouds and fog (LLCF) and in particular the mid-day peak observed in this region (Duan and Barros, 2017;Wilson and Barros, 2015). Furthermore, simulations exploring the use of different planetary boundary layer (PBL) parameterizations in WRF could not replicate the observed vertical structure of LLCF, thus failing to reproduce the reverse orographic enhancement linked to seeder-feeder interactions (Duan and Barros, 2017;Wilson and Barros, 2014, 2015), and consequently resulting in significantly lower rainfall intensities as compared to the surface disdrometer observations."

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?

The collision-coalescence is solved separately from condensation. The condensation process is described by a system of non-linear, first-order ordinary differential equations with variables of different orders of magnitude (see Sect. 2.1). The stiffness in this case is addressed numerically by implementing a fifth-order Runge-Kutta method with adaptive time steps. Collision-coalescence is described by the stochastic collection equation (SCE, see Eq. 10 in Sect. 2.2). In the present study, the discretized SCE is solved by a linear flux method (Bott, 1998), which does not suffer stiffness. Bott (1998) demonstrated that the flux method is numerically stable for various grid structures and integration time steps when the positive definiteness is maintained. Thus, a time step of 0.2 s was chosen to assure that the available mass in each bin at one time step is much larger than the change of mass in the bin due to the redistribution of the mass. We have clarified this in Section 2.3 (Pg. 8, Lines 5-8) in the revised manuscript: "The flux method for solving the discrete SCE was demonstrated to be numerically stable for various grid structures and integration time steps when the positive definiteness is maintained (Bott, 1998). Thus, a time increment of 0.2 s is chosen to assure that the available mass in each bin is much larger than the change of mass in the bin during the redistribution of the mass at one time step."

3) Page 11, Line 10: in Fig. 5b, when CDP LWC value is close to zero, there is a clear intercept of ~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.

The Referee's point is well taken. A further examination of droplet measurements from the two-dimensional cloud (2D-C) probe abroad the UND Citation indicates that the LWC data with values ~ 0.05 g m^{-3} observed by the King probe and near zero reported by the CDP (intercept along the x-axis) are associated with particles larger than 50 μm, which is beyond the upper sizing threshold of the CDP. In the revised manuscript, bulk LWC data with particles above 50 μm are removed from the analysis in Sect. 3.2 and a new linear regression was fitted with a slope of 1.36. Fig. 5 was modified accordingly as well as later figures with the CDP droplet spectra observations (slightly shift the spectra to smaller drop sizes).

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 ~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?

After further communication with the UND Citation scientists, we removed the supersaturation observations in the figures and revised the relevant discussion because large uncertainties are associated with the airborne temperature measurements as brought up by Reviewer #2, thus resulting in significant ambiguities in the derived supersaturation. We also added a note about the supersaturation in the caption of Fig. 11b (Pg. 48, Lines 4 - 5) in the revised manuscript. As shown in Eq. (7) in Sect. 2.1, the supersaturation strongly depends on the updraft velocity, condensation rates, and entrainment strength. In the present cloud parcel model, stronger updraft indicates faster cooling of the parcel, thus greatly increasing supersaturation. Efficient condensation process can accelerate the depletion of water vapor available in the parcel, and hence lead to reduced supersaturation. Entrainment mixes in dry ambient air, resulting in decreased supersaturation.

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.

Thank you for pointing this out. This was corrected in the revised manuscript.

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.

In the manuscript, Fig. 1 was first mentioned in Pg. 2, Line 14.

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

This was changed in the revised manuscript (Pg. 8, Line 28). Thank you.

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

The full name of MSL was added in the revised manuscript (Pg. 8, Line 29). The sentences were changed to: "Aerosol observations were carried out at the MV supersite (marked as the yellow star in Fig. 1b) in the inner mountain region during the IPHEx IOP. The elevation of the MV site is 925 m mean sea level (MSL)."

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.

The manufacturers of the instruments in the SMPS are denoted in the parentheses after each instrument in Pg. 9, Line 3. The manufacturer of the passive cavity aerosol spectrometer (PCASP) was added after its name in the revised manuscript (Pg. 9, Line 2).

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.

The aerosol size distributions recorded by the SMPS every 8 min were used to calculate the integrated aerosol number concentrations. The CPC reports total aerosol concentrations at 1-s interval. As different collection intervals are used by the SMPS and the CPC, the integrated aerosol concentrations from the SMPS are compared to the CPC measured aerosol concentrations which are closest to the SMPS sampling time and the corresponding correlation coefficient is 0.64.

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

This was changed in the revised manuscript (Pg. 9, Line 11). Thank you.

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

This was changed in the revised manuscript (Pg. 15, Line 12). Thank you.

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

That sentence was removed from the revised manuscript due to the unreliable temperature measurements by the aircraft (see detailed explanation in Point 4). Thank you.

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

This was corrected in the revised manuscript (Pg. 36). Thank you.

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.

Previous Fig. 7 was changed to Fig. 6 in the revised manuscript (Pg. 43). In Fig. 6c and d, different colored lines are used to represent observed cloud droplet spectra. Thank you.

References

Bott, A.: A flux method for the numerical solution of the stochastic collection equation, Journal of the atmospheric sciences, 55, 2284-2293, 1998.

Duan, Y., and Barros, A. P.: Understanding how low-level clouds and fog modify the diurnal cycle of orographic precipitation using in situ and satellite observations, Remote Sensing, In press, 2017.

Wilson, A. M., and Barros, A. P.: An Investigation of Warm Rainfall Microphysics in the Southern Appalachians: Orographic Enhancement via Low-Level Seeder–Feeder Interactions, Journal of the Atmospheric Sciences, 71, 1783-1805, 10.1175/jas-d-13-0228.1, 2014.

Wilson, A. M., and Barros, A. P.: Landform controls on low level moisture convergence and the diurnal cycle of warm season orographic rainfall in the Southern Appalachians, Journal of Hydrology, 531, 475-493, 10.1016/j.jhydrol.2015.10.068, 2015.