

Reply to Reviewer #3

The Referee's comments were taken into consideration fully in the revised manuscript. Whereas we disagree with the overall assessment regarding the manuscript contributions (the cloud parcel model presented is the only publicly available parcel model that includes entrainment and can produce the vertical distribution of CDNC in a consistent numerical framework to addresses the multiscale time-space challenge; the sensitivity results presented for the competitive interference of entrainment strength and condensation coefficient are discussed for the first time; and much of the IPHEX data analysis and related modeling results has not been published previously), we do appreciate the points raised regarding the need to highlight the science goal and improve manuscript organization, and we made a conscientious effort to address all issues raised. Thank you.

Item-by-item replies are in blue. The Referee's comments are in black.

I think the authors did a lot of work for the study. They developed a new cloud parcel model, analyzed observational data, and did parcel model simulations to look at the sensitivity of cloud properties to aerosol and thermodynamic parameters. However, the current version of the paper does not have clear science goal and is not organized as a coherent research, and the results do not present much new understandings. These major comments are reflected by the specific comments below. I think quite a lot work is needed to have the paper of the scientific significance for ACP.

Substantial revisions were made in the Introduction Section to explicitly state the scientific objectives of this study, as well as throughout the manuscript to present a clear and coherent structure of the manuscript. The manuscript title was also slightly changed to reflect the modelling focus as follows:

“Understanding aerosol-cloud interactions through modelling the development of orographic cumulus congestus during IPHEX”

The main objective of this manuscript is to investigate the vertical evolution of cloud microphysical structure (e.g., number concentration and droplet spectra) by developing a new cloud parcel model (CPM) that explicitly solves the cloud microphysics of condensation, collision-coalescence, and lateral entrainment processes with temporal and vertical resolution consistent with the range of scales of the governing processes ($\mu\text{m-m}$; ms-s). The long-term science goal of this new CPM was explained in the revised manuscript (Pg. 4, Lines 1 - 9). The long-term goal for this new CPM is to be coupled to an existing stochastic rainfall column model, describing the dynamic evolution of raindrop microphysics (bounce, collision-coalescence, and breakup mechanisms) between cloud base and the ground surface (Prat and Barros, 2007;Prat et al., 2012). The purpose of this coupled system is to investigate low-level precipitation enhancement induced by seeder-feeder interactions (SFI) among multilayer clouds generally, and between locally initiated or propagating convective clouds and low-level boundary layer clouds in particular. Previous research (Wilson and Barros, 2014) showed that SFI can increase the intensity of rainfall by up to one order of magnitude in the SAM and explain the observed mid-day peak in rainfall. Thus, the ability to predict the evolution of cloud formation and its vertical structure of droplet

size distribution (DSD) in this region is of paramount interest. To maintain a coherent organization of the manuscript, the Section on multi-parcel simulations was removed from the revised manuscript and the discussion on the sensitivity to hygroscopicity was moved from Appendix to Sect. 4.2.4 in the revised manuscript. Replies to specific comments are addressed next.

1. Introduction, first paragraph, does not include discussion of any recent studies on aerosol-cloud interactions for orographic clouds (there are quite a lot studies after 2012 on this topic and significant understandings are gained). It is necessary to discuss the most recent progresses in this area and introduce what the major problem is.

The Referee's point is well taken. More discussion of recent studies from the peer-reviewed literature on aerosol-cloud interactions of orographic clouds were added in Sect. 1 (Pg. 2, Lines 4-16).

2. Page 2, Line 26-28, the statement is not correct.

We have clarified this statement in Section 1 (Pg. 2, Lines 22-25) in the revised manuscript. Although detailed 2-D and 3-D models that explicitly resolve cloud formation and microphysical evolution to varying degrees of completeness have been developed (Fan et al., 2009; Leroy et al., 2009; Muhlbauer et al., 2010), relatively large temporal and spatial resolutions, and coarse spectral resolution of aerosols and cloud droplets cannot explicitly solve the nonlinear stochastic coalescence-breakup dynamics that govern the vertical microstructure of clouds and rainfall. Even when high vertical resolution is used such as in Wilson and Barros (2017), where 100 vertical layers were used with at least 14 in the lowest 1 km. This presents a critical modeling challenge in capturing the variations in vertical velocity profiles of hydrometeors at high vertical resolution (~10 m, used in the rainfall column model), thus unable to explain low-level precipitation enhancement induced by SFI in the SAM.

3. Introduction, second paragraph on P.4, I am not clear of the purpose of paper. What is the purpose of replicating aircraft microphysical observations with a cloud parcel model? What is the big picture here in terms of goals? Do you want to gain something for better parameterization for realistic modeling or gain new understandings about important physical factors impacting aerosol-cloud interactions over the complex terrain? The paper needs further work in terms of organization toward either direction. In addition, cloud parcel models are not the tool meant for reproducing observed cloud properties, particularly for the clouds developed over the complex terrain. The dynamics of valley-mountain circulations, which cannot be simulated with parcel models, are important to determine cloud properties and aerosol-cloud interactions. Even if you reproduce some aspects of the observed clouds, it could just because of a wrong reason considering the simplicity of cloud parcel model in both aerosol setup and dynamics.

The Referee's point is well taken. Indeed, the authors recognized the complicated flow dynamics in complex terrain. In the manuscript, high-resolution (250 m horizontal grid size) model simulations were conducted to illustrate the ridge-valley circulations in this region and detailed discussion can be found in the supplementary material Sect. S3. Understanding the complex

dynamics of ridge-valley circulations in this region, cloud segments to perform the CPM study were carefully selected by screening the cloud droplet spectra obtained from the cloud droplet probe (CDP) during IPHEX, as stated in Sect. 3.3 (Pg. 10, Line 27-32). Criteria listed in Conant et al. (2004) were used to eliminate regions influenced by mixing and other unresolved mechanisms and only one in-cloud region (IC) was selected as it satisfies Conant et al.'s requirements.

To heed to the Referee's concerns regarding the limitations of a simple CPM, the purpose of this manuscript is on understanding the vertical development of clouds leading to the mid-day precipitation peak in the inner SAM region, and to investigate the quantitative impact of key aerosol-cloud interactions (ACI) modeling parameters and initial assumptions on early cloud development. Model results were compared to the airborne observations of cloud microphysics as good agreement was achieved between the two, thus it provides more confidence in the findings from the sensitivity tests to determine the controlling factors in the microphysical evolution of clouds at early stages in the SAM.

4. P15, Line 22-24, I do not think Fig. 11a shows that the simulated vertical velocity is consistent with the observations in either profile or magnitude.

As shown in Fig. 6a (Fig. 11a in the previous manuscript), the simulated updraft velocities at the observation levels are consistent with the general trend of airborne measurements, which decrease with height. The figure points out the negligible influence of large a_c values on the simulated profiles of vertical velocity. The purpose of this figure is not to reproduce the observed vertical velocity in the cloud.

5. P15-16, the different droplet concentration between above and below 1.6 km could be because of different aerosol composition (implying different condensation coefficients) and/or concentration in reality. So, what is the purpose of playing different condensation coefficients to have a good fit for below and above that altitude? In reality, what we need to understand if there are dynamics and thermodynamics difference first, then look at if there are differences in aerosol properties.

As stated in the manuscript (Pg. 13, Line 30- Pg. 14, Line 2), based on the drop spectra there are likely two clusters of air parcels at different levels above ground, but there is no clear distinction of observed vertical velocities between these two clusters. Thus, the hypothesis that the differences in droplet concentrations and spectra might be a result of different condensation coefficient values. More importantly, the purpose of varying the a_c values is to investigate the sensitivity of model results to condensation coefficient, not to calibrate the model result by fitting with the observations.

6. Figure 12a shows that the cloud parcel did not get the droplet spectral right at 1.5 km. Obviously, there are two modes in observations, a cloud droplet mode peaking at 11 μm and a raindrop model peaking at $\sim 14 \mu\text{m}$. The second mode indicates rain formation starts at this altitude. This indicates cloud parcel model only can get a single mode and has deficiency in getting complicated drop spectrum (might be due to simple updraft dynamics).

At early stage of cloud development, unimodal spectra were produced due to the dominant role of condensation. As shown in Fig. 7b, the droplet growth from effective collision-coalescence process results in a secondary mode at larger sizes at later stages. Therefore, the CPM is able to produce bimodal droplet spectra as observed in clouds. At the targeted IC region, precipitation-sized drops were not present in concurrent cloud observations (highlighted in dark blue shade in Fig. S14d), thus the second mode is not indicative of rain formation. The lack of bimodal droplet spectra was discussed in Sect. 5. As introduced by Pinsky and Khain (2002), in-cloud nucleation of new droplets from interstitial aerosols can form a secondary mode at a smaller droplet size when the parcel supersaturation higher up in the cloud exceeds the cloud base maximum. However, this is not the case here by using the WRF sounding input.

7. P. 17 line 15-17, the statement “uncertainties of the assumed environmental thermodynamic conditions (e.g., temperature) impose significant constraints in the vertical development of clouds, thus posing as a significant challenge in cloud modeling study” only states the basic concept of convective cloud development. It should not be stated as a major conclusion of the research.

The statement (Pg. 15, Lines 7-9 in revised manuscript) was concluded from the sensitivity results by modifying the environmental conditions from WRF, as discussed in Appendix B1. The statement is not presented as a major conclusion of this study.

8. Section 4.2.2, need a transition about the purpose of looking at the impact of entrainment strength. In addition, the parcel size is only a parameter existing in the parcel model framework. Physically a parcel size is already determined by the environmental conditions and you should not have this freedom for a certain environment. To be useful for realistic 3-D model simulations, we need to know what this parameter means in 3-D model framework. Can it be translated to model resolution?

Each subsection in Sect. 4.2 discusses sensitivity results to a different key parameter in the model. The sensitivity experiments in Sect. 4.2.2 are to examine the role of entrainment strength in modifying the cloud microphysical properties, and they are not used for determining a parcel size under the given environment.

9. P.19, first paragraph, the statement “aerosols in the atmosphere exhibit a significant space-time variability especially in regions of complex terrain with heterogeneous mixing by different ventilation processes in addition to the possibility of remote transport, all of which can contribute to the diverse cloud droplet spectra observed across the cloud transect (see Figs. 8a-c). This cannot be captured by the current model simulations that assume a homogenous aerosol distribution at cloud base”. Exactly. It seems to me that research started from this problem and then concluded the same problem. Then what is the use of the study? Currently the results show droplet spectrum is sensitive to aerosol properties and thermodynamic properties. These are well-studied already in the field.

As stated in the reply to Comment #3, the objective of this manuscript is to investigate the vertical development of cloud microphysical properties in complex terrain by implementing a new

entraining CPM. Ultimately the vision is to track the evolution of local (within column) clouds from CCN activation through cloud formation, sustained cloud development, and rainfall. Sensitivity tests were conducted to assess the quantitative impact of key ACI modeling parameters and initial assumptions on cloud microphysical development at early stages. Model results of cloud droplet number concentration (CDNC), liquid water content, and droplet spectra show good agreement with airborne observations from IPHEX. Discrepancies between model results and observations were further discussed with respect to limitations of the CPM and challenges of its application in complex terrain. In particular, this study introduces a new entraining CPM and the model is successfully applied to explore the influence of entrainment strength on the cloud microphysical evolution, which are new contributions to the field.

10. P.19, Line 22-23, “Based on the sensitivity tests, the cloud spectra observed in the inner region of the SAM for early development of cumulus congestus on 12 June are reproduced better by a relatively low value of a_c (0.01).”, I do not think you can conclude it since your initial aerosol size distribution and composition around cloud base are not constrained by observations. You can easily achieve a similar droplet activation rate for different a_c by adjusting initial aerosol size distribution.

The Referee’s point regarding the lack of observation constraints due to limited data in our case is well taken. Because of IPHEX’s emphasis on precipitation microphysics and specifically the vertical distribution of hydrometeors, aerosol measurements were limited compared to other campaigns when the focus has been strictly on aerosols, in particular near and at cloud base. In this study, there are no measurements at cloud base or near cloud base proper as the focus was on precipitation microphysics during the field campaign. As noted in Sect. S3 (Pg. 4, Line 5 - Pg. 5, Line 2), high-resolution model results show there is significant horizontal wind shear with 3D circulations including valley winds, thermal winds, ridge-valley circulations, and well defined southerly mesoscale mid-level transport patterns that result in complex horizontal and vertical wind structure very different therefore from the classical convective boundary layers in flat terrain. Given the complexity of the 3D circulations, especially the horizontal winds, the regional convergence patterns and the potential role of advective venting, and the nearly self-similar regional distribution of vertical velocities at the mesoscale, initial aerosol concentration at cloud base is extrapolated vertically from the surface aerosol number concentrations at MV by assuming an exponential decay with a scale height (H_S). This assumption is in keeping with previous studies using mesoscale models (Iguchi et al., 2008; Muhlbauer and Lohmann, 2008) and cloud parcel models (e.g., Eichel et al., 1996) and consistent with differences in total aerosol numbers between the two horizontal flight legs at different heights over IC (not shown here). The reference scale height ($H_S = 1000$ m) in this study is within the typical range (H_S : 550–1,100 m) for remote continental aerosol type (Jaenicke, 1993), which is a reasonable assumption.

As discussed in Sect. 1 (Pg. 3, Line 16 - 32) and Sect. 4.2.5 (Pg. 17, Line 30 - Pg. 18, Line 4), a relatively low value of a_c (0.01) is consistent with previous findings from laboratory experiments with background air flows, thus not adiabatic conditions. Recognizing that limited measurements were available at cloud base, the conclusion (Pg. 17, Lines 30-32) regarding a_c has been revised

to read “Based on the sensitivity tests, model simulations using a relatively low value of a_c (0.01) exhibit CDNC and spectra consistent with the cloud spectra observed in the inner region of the SAM for early development of cumulus congestus on 12 June.” This further highlights the need to have a constraining set of observational inputs in order to validate our findings over the SAM. Additional simulations were conducted assuming well-mixed conditions, and the results are included in the Supplementary Material Sect. S4. Thus, surface aerosol concentrations were used as model input, assuming a well-mixed well-developed convective boundary layer. The results are presented and discussed in Sect. S4 (see the Supplementary Material). Although agreement in total CDNC can be obtained between simulations with larger a_c values and the airborne observations, large discrepancies exist between the predicted and observed activated spectra with implications for precipitation processes. Detailed discussion can be found in Sect. 4.2.3 (Pg. 16, Lines 17 - 29).

11. Section 4.3, I do not get the point of multi-parcel simulation discussion based on the text here. The text only describes the techniques and what the first, second, and third parcel look like. It is not connected with any points presented before.

The Referee’s point is well taken. The Section of multi-parcel simulation was removed from the revised manuscript.

12. Section 5, since the point “simulated CDNC also exhibits high sensitivity to variations in initial aerosol concentration at cloud base, but weak sensitivity to aerosol hygroscopicity” is a key conclusion shown in the abstract, the results for weak sensitivity to aerosol hygroscopicity should be presented in the main figure instead of putting in the Appendix. This indicates the results are not well organized. Another evidence to support this point is the authors used 6 figures (Figures 3-8) to just present observational data that they have from the field campaign, while do not make any important points to the paper. The results of different sensitivity tests need to be organized in a coherent and logic way (right now it seems that a bunch of tests are put together and the results are discussed for each set of the tests).

The Referee’s point is well taken. The discussion of sensitivity results to aerosol hygroscopicity was moved to Section 4.2.4 in the revised manuscript. Figures 3-5 and 7-8 in the previous manuscript were discussed in the supplementary section (see Sect. S2) and are now Figures S7-S8, S11, and S14-S15 in the revised manuscript.

Thank you

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