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Interactive comment on "Cloud-top microphysics evolution in the Gamma phase space from a modeling perspective" *by* Lianet Hernández Pardo et al.

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(Comment) This is a review of the manuscript "Cloud-top microphysics evolution in the Gamma phase space from a modeling perspective" by Pardo et al., submitted to ACPD. The authors are interpreting gamma drop size distributions (DSDs) in terms of the parameters N_0, Λ, μ, in addition to the moments of the DSD. Measured DSD properties at the cloud top of a convective plume are compared to model simulations, using both bin and bulk microphysics schemes. According to the authors, the proposed method of interpretation yields additional insight into model results to provide a better understanding of cloud microphysical processes, including aerosol cloud interactions.

The outcome of the study is a modified parameterization of the cloud droplet shape parameter, as commonly used in two-moment bulk microphysical models.

(Answer) We would like to thank Anonymous Referee #2 for taking the time to analyze our work and suggest improvements. In this document we provide detailed responses to the issues raised.

Major points:

1. **(Comment)** Personally, after reading this manuscript (and parts of its precursor, Cecchini et al. 2017) I am still having a hard time to see the point of using the "gamma phase space" for interpretations of physical processes, or even just to compare differences between models and/or measurements. By recognizing the units of the parameters, it is obvious that the physical interpretation of these parameters is far less straightforward or intuitive than looking at the moments and the corresponding change rates – number, mass, and maybe surface or reflectivity if we want to add a third moment (thus constraining all three parameters N_0, Λ, μ). In the end, the authors seem to set aside their previously introduced method, and use the moments or a combination of moments instead, stating it yields additional physical insight. Large parts of the results section provide only short descriptions of what the parameters look like in the plots, while the actual interpretation is done based on the moments. I cannot see which conclusion of the paper would not have been possible by looking at the moments only. Also the outcome of their new parameterization is a function of bulk number and mass (eq. 11). So why should I make an additional effort in future and explicitly interpret the parameters, and why should I call a process rate pseudo force?

1. **(Answer)** Changes from one position to another in the Gamma phase space are directly linked to changes in the DSD, so the trajectories in this space correspond to effect of the different microphysical processes acting in the cloud (cloud top, in this case).

We believe both approaches, i.e. interpretation of moments or Gamma parameters, are incomplete when studied alone. Note that when 3 moments are known, while you can understand a lot about the bulk nature of the droplet population, there is a lack of information about the overall DSD shape and appearance. When you have the Gamma parameters, you know how the DSD will look like but have a hard time converting to bulk quantities without the use of computations. Each approach has its advantages and disadvantages depending on the application. For instance, DSD width (conventionally associated to μ in the Gamma case) is very important for collision-coalescence parameterization. On the other hand, precipitation retrievals by remote sensing mostly care about bulk quantities. As such, it is clear that both approaches complement one another by providing additional information about the nature of the droplet population.

While we agree that the values of the parameters have a non-trivial physical interpretation, we still have to study them as-is because a lot of applications rely on them. More important than their actual value, at least in our case, are their relative variations. For example, the isolated information of $\Lambda = 1\ \mu m^{-1}$ might not tell much. But if this value changes to $\Lambda = 3\ \mu m^{-1}$, then some interpretations are possible. The bigger-Λ-DSD is likely associated to higher number concentrations in the right tale of the DSD, because Λ controls the slope of the exponential part of the DSD, which dominates when $D \to \infty$. So the increase in Λ is a measure of the effect of growing processes on the DSD. This kind of analysis is likely useful for DSD physics theory, which must be brought to "reality" by the analysis of the corresponding moments.

What the Gamma phase space brings is a simple and direct way to analyze such theoretical DSD patterns – when they are widening/narrowing, when some averaged D is growing/shrinking, etc. What the Gamma phase space does not bring is a simple and direct way to study DSD moments – even though this can be done by coloring the trajectories according to any DSD variable that can be obtained from the Gamma parameters.

The manuscript was edited in order to highlight the complementary nature of both

approaches – the analysis of the gamma parameters and the DSD moments.

See next comment for our answer regarding the proposed new parameterization.

2. **(Comment)** A new parameterization of μ is proposed, and since the manuscript advertises the gamma phase space I was expecting something like μ being a function of other parameters - e.g., for raindrops it is common practice that $\mu = f(\Lambda)$. This is not even mentioned, instead the authors continue to rely on moments of the DSD. While I am not saying it is a bad idea, I cannot see how the gamma phase space has contributed here. Unfortunately, there is no indication of whether the new parameterization would be applicable to any other situations.

2. **(Answer)** To properly predict DSD moments, model parameterizations should emulate the underlying physics of the problem, which is seen as trajectories in the Gamma phase space, in our case. Therefore, one given parameterization should be able to produce similar trajectories to the benchmark reference chosen – be it a bin model or observations. That is the contribution of the gamma phase space here, independently of the means employed to adjust the trajectory of the bulk parameterization. Even though the new parameterization relies on DSD moments, it does not mean the Gamma phase space wasn't used. In fact, what we did was to benefit from noting that the addition of $1/q_c$ in Eq. (11) produced a gamma phase space trajectory much closer to the bin case as compared to the original parameterization. Therefore, the new parameterization better reproduces the DSD physics relying on the same q_c and N_d values. Note that while the Gamma phase space is not directly present in Eq. (11), it was essential to the development of the new parameterization. In this specific case, the gamma space was employed to understand the former μ parameterization, to test different hypothesis and to confirm the best μ adjustment.

It is all about the choice of the reference system. We agree that we could have validated the cloud-top path in a bulk phase space instead, and then emulate it to conceive a new parameterization for μ. However, as stated in our response to major point 1, our point is that the gamma phase space approach is a more useful way to analyze the evolution of a DSD, even if it is not straightforward.

Of course it may be more convenient/accurate ways to reproduce observed gamma phase space characteristics in bulk models, and we continue to work into it. One of them could be the definition of additional relations between the gamma parameters (e.g. in the form of the mentioned $\mu = f(\Lambda)$ as in Zhang et al., 2003), which would imply a slightly large modification to current parameterizations, since we would have to change the method to solve the system of equations for the gamma parameters. It shouldn't be much difficult, but the objective of this paper is to bring up the gamma phase space utility from a modeling approach -for testing, evaluating and developing the parameterizations- rather than presenting a detailed implementation of those ideas. This study presents an initial modeling insight of the gamma phase space, analogously to the work of Cecchini et al. (2017), and exemplify how current parameterizations can benefit from it. It is already the subject of our current research, we are developing a new parameterization for the gamma parameters based in preferential directions of the pseudo-forces in the gamma space.

The manuscript was modified in order to clarify these aspects.

3. **(Comment)** Another example is how the manuscript addresses the effect of aerosols on cloud properties. Figure 3 shows the sensitivity of the parameters to aerosol concentrations. The main dependency seems to be represented by the magnitude of N_0 (while it is really challenging to see even qualitative dependencies in the 3D plots). N_0 is proportional to the bulk number (zeroth moment), but at the same time a complicated function of μ and Λ (which already span the other two dimensions). So the only effect of substituting bulk number by N_0 is that the analysis becomes more complicated or even meaningless - personally I cannot calculate $\frac{\Lambda^{\mu+1}}{\Gamma(\mu+1)}$ without using tools. On the other hand, the dependence of bulk number on aerosol concentration is well-established.

3. **(Answer)** We added projections on the three planes of Fig. 3 in the manuscript,

as reproduced in Fig. 1 here. The projections greatly facilitate the interpretation of the trajectories. Aside from the N_0 difference, we also highlight the differences in the $\Lambda - \mu$ plane. Note that the curves in this plane can be approximated by straight lines and the differences among them are mainly associated to their angular coefficient. Using the effective diameter $D_{eff} = \frac{\mu+3}{\Lambda}$ as an example, we observe that the angular coefficients are related to droplet growth with cloud height. The higher the coefficient is (in absolute terms) the faster the droplet growth will be. Given that the cleanest case is associated to the highest coefficient, it is also associated to the fastest growth rates. Therefore, aerosols affect not only droplet number concentrations but also their growth rates throughout the whole warm phase – which is already studied in the literature and is usually justified by the water vapor competition process. The point is that the trajectories provide a more complete view of the aerosol effect by showing the changes in all DSD properties at once – at least under the Gamma limitations.

This discussion was added to the manuscript for completeness.

4. **(Comment)** In the discussions about forces I do not understand why advection is considered one of them. When I imagine to be sitting within a parcel below cloud top that is being transported upward: Why would advection impose any changes on me, while I am moving along with the parcel and my direct environment as well? I am not resting at one level.

4. **(Answer)** In the manuscript, the discussion about the pseudo-forces refers to Fig. 4 and Fig. 5. Since this comment mentions a parcel below cloud-top that is being transported upward, we assumed it refers to the pseudo-forces analysis in Fig. 5. Note that in Fig. 4 we do stay in one constant level.

The no-advection assumption is only applicable in closed systems, such as in adiabatic parcel models. In our single-column simulations, what we actually have is an Eulerian framework, where following a parcel is non-trivial, if not impossible, which is pretty much what happens in the atmosphere. At every time step, in addition to the microphysics processes, the source and/or sink effect of the advection is calculated for each model grid-point, determining a continuous mixing between them. Therefore, when we follow the cloud-top, we are dealing with particles that arrived from inferior layers, as well as with those that were nucleated there. In other words, there is no mass conservation for a model grid-point. Also note that the pseudo-forces represented in Fig. 5 (in the manuscript) correspond to averages for the time-steps the cloud-top remained at the same height.

The explanation above was added to the manuscript for clarity.

5. **(Comment)** The main message I am taking away is that current parameterizations of the cloud droplet shape parameter, oftentimes a function of droplet number, are probably not in a final stage yet and there is room for improvements. This confirms what was recently described by Igel and van den Heever (2017). At the same time, the possibility of using 3-moment bulk schemes to explicitly predict the shape parameter based on the microphysical processes is hardly mentioned in the manuscript.

5. **(Answer)** Indeed, large uncertainties are still associated to the shape parameter characterization in bulk microphysics schemes, in addition to the assumption of a predefined functional relationship for droplets size distributions. That is one of the reasons why, even having its own uncertainties, bin schemes are considered more realistic and are often taken as a reference for improving bulk parameterizations. To notice the fact that triple-moments parameterization are already an alternative to overcome this problem, we added the next sentence to line 10, page 2 of the manuscript: "Although triple-moments schemes already allow to determine the three parameters of the gamma function without additional considerations (Milbrandt and Yau, 2005a,b; Szyrmer et al., 2005), they are still too computationally costly for many applications of practical interest, such as operational forecasts or even research activities."

6. **(Comment)** As part of the discussion within ACPD, I also want to comment on the criticism of Reviewer 1. The paper of Xue et al. (2017) is cited in order to establish that

in practice, bin microphysical models may not be useful because they yield a spread in the results that is comparable to bulk models. I am going to explain why I cannot agree with the reviewer's opinion who claims that we cannot trust the bin model used here: Xue et al. (2017) present a model intercomparison of three bin models which simulate a squall line in an idealized setup. They find considerable differences which are solely attributable to the microphysical processes and their representation. However, the very point of the paper is that even bin models – whose primary advantage is a free evolution of the particle size distribution – are still relying on and suffering from a number of assumptions related to ice microphysics such as particle densities, shapes, conversion thresholds, treatment of liquid fractions, etc. On the other hand, liquid-only microphysics are way less ambiguous, even though we can think of slightly different relations for fall velocities, coalescence efficiencies and other details. Therefore the heavy criticism seems inappropriate.

6. **(Answer)** As we answered for Anonymous Referee #1, we also agree that the Xue et al. (2017) study should not be used to discourage the working with the TAU model. We understand the criticism from Anonymous Referee #1 because bin models may indeed produce high uncertainties even if they have better representation of physical processes. One example of the sources of those uncertainties in bin schemes is the spurious broadening that occurs due to numerical diffusion in physical space during condensational growth/evaporation (Morrison et al., 2018, JAS). But the point of using the bin model as reference in our study is because it is supposed to better represent the warm phase microphysical processes (namely nucleation, condensation and collision-coalescence). The 1D bin model specifically is possibly one of the best tools for such analysis because it isolates the microphysical processes from more complex and non-linear dynamical interactions. If we can emulate bin results in a bulk scheme, it should be a significant step forward – and that is one of the ideas behind the new parameterization introduced.

7. **(Comment)** While I regret I could not extract the essence of the manuscript regard-

ing the gamma phase space, I am going to provide a number of specific suggestions for improvements to the manuscript. Generally, I find a lot of vague sentences and I wished there were more information and more specific sentences in all parts of the manuscript.

7. **(Answer)** We appreciate the effort from Anonymous Referee #2 to suggest improvements for our manuscript. We are sure the manuscript is now more explanatory, clear and consistent.

Specific points:

1. **(Comment)** Page 1 Lines 22,23: What are practical applications? "Generally employed" is very vague or even wrong. Also references might help.

1. **(Answer)** The text was modified to "Although bin schemes are more accurate and flexible (Berry and Reinhardt, 1974; Enukashvily, 1980; Tzivion et al., 1987), their high computational cost makes them less useful for operational applications or for research activities that do not focus on the effects of microphysics processes. For most of those applications, bulk schemes are more frequently employed (Lin et al., 1983; Ferrier, 1994; Thompson et al., 2008; Morrison et al., 2009)"

2. **(Comment)** Line 5: N has units of cm^{-3} only when integrated over a finite size interval. There seem to be inconsistencies with units also in other places, see below.

2. **(Answer)** In page 2, line 5, we are not actually talking about N (the zeroth moment of the DSD), but of N(D), which, in fact, has units of $cm^{-3}\mu m^{-1}$ or, in other words, number of droplets with diameter D per cm^3 of air.

3. **(Comment)** Lines 6-10: vague formulation, what are "enough" moments. The impact of μ on cloud water path and condensation rates are described, but which μ are we talking about – cloud, rain, ice? Three-moment schemes have been introduced more than 10 years ago and it would be appropriate to mention at this point.

3. **(Answer)** The intended meaning for "enough moments" was "a number of moments

enough to obtain a fully determined system of equations". For clarity, the paragraph was edited: "To solve for the three parameters of the gamma function, three moments would be necessary. However, most bulk microphysical parameterizations – single- or double-moment schemes – do not predict enough moments of the DSD to properly describe their variability. As a closure, the μ parameter of the gamma DSD is commonly fixed or evaluated (Grabowski, 1998; Rotstayn and Liu, 2003; Morrison and Grabowski, 2007)." This modification also specify that we are referring to the μ parameter of droplet size distributions. As mentioned earlier in this document, a sentence was introduced to note the existence of triple-moment schemes.

4. **(Comment)** Lines 13-18: Very vague: What is very useful about it, what are the specific advantages? What are the new opportunities?

4. **(Answer)** As this information is used just to introduce the subject of the research described in the submitted manuscript, we don't believe it would be appropriate to specify many details about the paper of Cecchini et al. (2017). We would prefer to keep this paragraph in its current form, suggesting the reader to find more detailed information in the aforementioned publication.

5. **(Comment)** Page 3 Line 8: what is the sounding date, also there is a lack of information about the AC09 flight. It is cited further below but still it would be nice to have a quick overview including date and the cloud we are looking at.

5. **(Answer)** To include the suggested information, we updated the corresponding paragraph: "As initial conditions, vertical profiles of potential temperature and water vapor mixing ratio from an in situ atmospheric sounding were provided (Fig. 1a). We used the 12Z sounding, on September 11, 2014, from Boa Vista-RR, Brazil, for coherence with the atmospheric conditions where the data of the AC09 flight were collected (Wendisch et al., 2016), intending to use those measurements for comparisons here. This flight was performed by the High Altitude and Long Range Research Aircraft (HALO) on the same date of the aforementioned sounding, as part of the ACRIDICON-CHUVA

campaign (Machado et al., 2014). It sampled the top of growing convective cumulus, starting close to the local noon, over remote regions of the Amazon, where there is relatively homogeneous conditions, due to the characteristics of the surface, and low aerosol concentrations."

6. **(Comment)** Page 4 Line 2: What does the prognostic variable represent? What are the initialized aerosol properties in the model?

6. **(Answer)** The requested information was added to the text: "Aerosols are represented by a single prognostic variable, its bulk number concentration, that was initialized as 800 cm^{-3}. It is assumed to have a log-normal distribution, with a median radius of 0.05 μm and a geometric standard deviation of 1.5. The hygroscopicity of the aerosols was considered as 0.1, according to previous characterizations of the aerosol over the Amazon (Gunthe et al. 2009, Martin et al., 2010, Pöhlker et al., 2016)."

7. **(Comment)** Lines 5-10: The statements about ice properties seem to be irrelevant for this study. Since μ is a central topic here, what are the underlying observations/ cloud types/ reference other than Thompson?

7. **(Answer)** We intended to describe main aspects of both parameterizations, that is why we mentioned the species they include and the size distribution function used for them. However, we agree that this information is irrelevant, so we deleted it.

At this point, we just explained the scheme of Thompson et al. (2008), so we believe the introduction section would be more appropriate to include some content about observations of μ. According to that idea, we added a reference to Miles et al. (2000), who summarized several values of μ previously reported in the literature.

8. **(Comment)** Line 18: It seems worth noting that the exponential is a gamma distribution with $\mu = 0$.

8. **(Answer)** We now included this statement in the paragraph where the gamma distribution is presented, in the introduction.

9. **(Comment)** Line 26: The morrison scheme uses SI units. Also it does not use mass densitites, but mixing ratios.

9. **(Answer)** Yes, we are aware that the Morrison scheme uses mixing ratios and SI units instead, but it does not affect the expressions for μ, Λ and N_0 presented. They are equivalent in both schemes.

10. **(Comment)** Lines 29 and following: I do not understand: What is different in the approach of Morrison to estimate the parameters? Line 22 states that both schemes use the same expressions. I am also curious whether potential differences between the schemes in terms of units are considered correctly. Trying to understand: The Morrison scheme is not used to calculate any process rates, but only to diagnose DSD parameters? Are the expected differences due to the parameterization of μ or anything else? If so, why not simply replace the parameterization of μ within the Thompson scheme with the one used in the Morrison scheme? But in contrast to the Morrison scheme, the Thompson microphysics does calculate its own process rates? How can the comparison be fair when Morrison gets the moment input from TAU, but Thompson predicts the moments on its own? What are "the uncertainties introduced by the procedure…" – only the parameterization of μ?

10. **(Answer)** As explained in the manuscript, section 2.1, the only difference between the way these two bulk schemes estimate the gamma parameters lies on the calculation of μ, since they use the same expressions for Λ and N_0. To address the concern of Anonymous Referee #2 about the units in the scheme of Morrison, let's consider the expression for μ originally employed on it:

$$\mu_M = (0.0005714 N_M \rho_M \times 10^{-6} + 0.2714)^{-2} - 1 \quad (1)$$

where N_M is the droplet number concentration (kg^{-1}) and ρ_M is the density of the air (kg.m^{-3}). Note that $N_M \rho_M \times 10^{-6}$ becomes N_d (cm^{-3}), and Eq. 1 here equals Eq. 4 of the manuscript. Then, using the same N_d (cm^{-3}), we can obtain consistent values of μ for each scheme.

However, considering the schemes in its entirety, there are many more differences among them than just the calculation of μ. If we want to compare its gamma trajectories, it is useful to avoid the influence of other aspects of the schemes. That is what we do when we calculate the gamma parameters of the Morrison's scheme and recalculate the ones of the Thompson's from the moments predicted by TAU. Firstly, we run TAU and Thompson and compare the trajectories, they are obviously different, then we say "ok, let's test whether the differences in the gamma trajectories have a large influence of other features aside from the way they estimate μ, let's use the same base moments". And the answer is "No, even if somehow they could obtain the same moments, the estimation of the gamma parameters will be incorrect". Of course, obtaining different gamma parameters will influence conversion processes rates and mass and number concentration predictions.

We don't compare Morrison's scheme getting the moment input from TAU, with Thompson's scheme predicting the moments on its own, that wouldn't be fair, indeed. We compare Thompson, predicting the moments on its own with the results of TAU, and then Thompson and Morrison, getting the moments from TAU, with TAU.

11. **(Comment)** Page 5 Line 8: Please explain "process intensities", or otherwise it would be helpful to stick to established wording.

11. **(Answer)** "Process intensities" was edited to "rates of those processes".

12. **(Comment)** Lines 10-14: What makes the phase space a projection? Is is something different from the 3d space that is used here?

12. **(Answer)** We substituted the word "projection" by "representation", to avoid ambiguities.

13. **(Comment)** Line 20: Please explain the restriction: Are there considerable amounts of drizzle/rain present which is just cut off from the DSD? Is the intention

to avoid having a second mode in the DSD, or are there other reasons? 50 micron appears pretty small indeed. Even though most of the bulk number will be contained at sizes below this threshold, considerable fractions of mass can be contained in the tail larger than that. This may be also a reason for the big values of diagnosed shape parameters.

13. **(Answer)** Please see response to major point 3 of Anonymous Referee #1. The 50 microns restriction intend to avoid raindrops, to be consistent with the analysis of Cecchini et al. (2017). It is a commonly used threshold in the literature to distinguish cloud droplets from raindrops. Nevertheless, there is no significant quantity of drops beyond that size interval in the simulated cloud-top (information added to the manuscript).

14. **(Comment)** Lines 28 – 31: The "bulk phase space" is another example when I feel that new content is created by new wording only. The authors interpret the moments of the DSD in order to get an idea about the physical processes, which has been done by the community for decades. Since the authors also see the need to do so, I am concluding that the "gamma phase space" as such is limited in being useful to interpret the physics.

14. **(Answer)** We call the $N_d - D_{eff}$ phase space as "bulk phase space" for simplicity. Its a phase space defined by bulk properties of the DSD, we are not saying that it is new content. Actually, because understanding physical processes in terms of DSD moments is a more common approach, we use it as a complement of the gamma phase space analysis, which is not so common. As we commented earlier in this document, we believe both approaches complement together and that, despite being more abstract, the information the gamma phase space provides can't be obtained from a single bulk phase space.

15. **(Comment)** Page 6: Line 9: There seems to be a hint that the real cloud contained ice, but the model does not? What does it mean that the cloud was limited to lower heights in the model?

15. **(Answer)** Indeed, since we are using a warm phase bin microphysics scheme as main tool, there is no way to simulate ice processes, any simulated variable above the freezing level would make no sense. Thus, the simulation didn't reach the highest levels of the troposphere, while real cumulus did. To facilitate the understanding, we edited this statement in the manuscript to: "The simulation did not reach the highest levels sampled in the observations because it includes only the warm-phase processes".

16. **(Comment)** Line 10: μ is commonly referred to as the width of the DSD – isn't it a sufficient criterion for a broadening DSD to find a decrease in μ? How are N_0 and Λ important in interpreting the broadening? Could we also think of opposite tendencies for N_0/Λ and still call it broadening?

16. **(Answer)** The habitual association between the DSD width and μ may come from the relative dispersion (ϵ) concept:

$$\epsilon = \frac{\sigma}{D_m} = \frac{1}{\sqrt{\mu + 1}} \tag{2}$$

where σ is the standard deviation and D_m is the mean diameter of the DSD. However, decreasing μ may not be associated to such an intuitive DSD broadening, even if the relative dispersion increases. Figure 2 here illustrates that when μ decreases and the other parameters remain constant, the DSD actually shrinks.

To address this question, let's analyze some characteristics of the gamma DSD, given by Eq. 1 in the manuscript. Taking the first derivative of Eq. 1,

$$\frac{dN}{dD} = N_0 D^{\mu-1} e^{-\Lambda D}(\mu - D\Lambda) \tag{3}$$

it can be determined that the maximum of the function is located at $D_{max} = \mu/\Lambda$ and the value of this maximum depends on the gamma parameters according to:

$$N_{max} = N_0 \left(\frac{\mu}{e\Lambda}\right)^{\mu} \tag{4}$$

Now, if we choose two points located at both sides of D_{max}, D_1 and D_2, such that:

$$N(D_1) = N(D_2) = \frac{1}{n} N_{max} \tag{5}$$

where $n \in \mathbb{N}$, we can find the values of D_1 and D_2 by solving:

$$D^{\mu} e^{-\Lambda D} = \frac{1}{n}\left(\frac{\mu}{\Lambda}\right)^{\mu} e^{-\mu} \tag{6}$$

Equation 6 has real solutions in the form of Lambert function branches 0 (W_0) and -1 (W_{-1}):

$$D_1 = -\frac{\mu}{\Lambda} W_0\left(-\frac{1}{n} e^{-1}\right) \tag{7}$$

$$D_2 = -\frac{\mu}{\Lambda} W_{-1}\left(-\frac{1}{n} e^{-1}\right) \tag{8}$$

where W_0 and W_{-1} are negative in the interval $(-e^{-1}; 0)$, so D_1 and D_2 are positive.

From the previous analyses, we can see that the relation μ/Λ determines D_{max} and N_{max}, as well as the difference $D_1 - D_2$. It means that decreasing μ, actually decreases the location of the maximum, its magnitude, and the distance between the ascending and descending branches of the gamma function. The interpretation depends on the definition of "broadening". While decreasing μ causes a relative broadening (expressed by the increase of ϵ), for an absolute broadening of the DSD, the decrease in μ must go along with a decrease in Λ, as illustrated in Fig. 2 here. An increment in the value of N_0 will act only as a scale factor, increasing the bulk number concentration of the DSD.

The previous discussion was added to the text of the manuscript for completeness.

17. **(Comment)** Figures: Is $log()$ referring to the natural logarithm? In particular to interpret the numerical values of μ, $log_{10}()$ scales will be much more intuitive. At the same time I wonder if the values considerably larger than, say 20-30, are pointing to problems in the diagnosis of μ. The 3D plots provide hardly any usable information, even qualitative judgements are difficult. It would make sense to show the data under discussion in a 2D plane or some other kind of restructuring.

17. **(Answer)** Yes, $log()$ referred to the natural logarithm in the figures. The intention was to be coherent with the analysis of Cecchini et al. (2017). However, there was a misunderstanding partially motivated by the terminology used. We noted that Cecchini et al. (2017) actually used $log_{10}()$, so we updated the corresponding figures in the manuscript. Figure 3 here is a reproduction of Fig. 2 in the manuscript, now $log()$ means $log_{10}()$. There we can see that the gamma phase space paths in the simulation and in the observation are actually in a better agreement than what was illustrated by the original figure.

We discussed the concern about the large values of μ in the answer to major point 4 of Anonymous Referee #1. In that document, we show that large values of μ corresponds to very incipient DSDs, when freshly activated droplets occupy only smaller-diameter intervals.

In the updated version of the manuscript we added projections to the 3D plots – as in Fig. 3 here–, it allows for interpreting the data both in 3D and 2D simultaneously.

References

Berry, E. X. and Reinhardt, R. L.: An analysis of cloud drop growth by collection: Part

I. Double distributions, Journal of the Atmospheric Sciences, 31, 1814–1824, 1974.

Cecchini, M. A., Machado, L. A. T., Wendisch, M., Costa, A., Krämer, M., Andreae, M. O., Afchine, A., Albrecht, R. I., Artaxo, P., Borrmann, S., Fütterer, D., Klimach, T., Mahnke, C., Martin, S. T., Minikin, A., Molleker, S., Pardo, L. H., Pöhlker, C., Pöhlker, M. L., Pöschl, U., Rosenfeld, D., and Weinzierl, B.: Illustration of microphysical processes in Amazonian deep convective clouds in the gamma phase space: introduction and potential applications, Atmospheric Chemistry and Physics, 17, 14 727–14 746, 2017.

Enukashvily, I. M.: A numerical method for integrating the kinetic equation of coalescence and breakup of cloud droplets, Journal of the Atmospheric Sciences, 37, 2521–2534, 1980.

Ferrier, B. S.: A double-moment multiple-phase four-class bulk ice scheme. Part I: Description, Journal of the Atmospheric Sciences, 51, 249–280, 1994.

Grabowski, W. W.: Toward Cloud Resolving Modeling of Large-Scale Tropical Circulations: A Simple Cloud Microphysics Parameterization, Journal of the Atmospheric Sciences, 55, 3283–3298, 1998.

Gunthe, S. S., King, S. M., Rose, D., Chen, Q., Roldin, P., Farmer, D. K., Jimenez, J. L., Artaxo, P., Andreae, M. O., Martin, S. T., and Pöschl, U.: Cloud condensation nuclei in pristine tropical rainforest air of Amazonia: size-resolved measurements and modeling of atmospheric aerosol composition and CCN activity, Atmospheric Chemistry and Physics, 9, 7551–7575, 2009.

Igel, A. L. and van den Heever, S. C.: The Importance of the Shape of Cloud Droplet Size Distributions in Shallow Cumulus Clouds. Part I: Bin Microphysics Simulations, Journal of the Atmospheric Sciences, 74, 249–258, 2017.

Lin, Y.-L., Farley, R. D., and Orville, H. D.: Bulk parameterization of the snow field in a cloud model, Journal of Climate and Applied Meteorology, 22, 1065–1092, 1983.

Machado, L. A. T., Dias, M. A. F. S., Morales, C., Fisch, G., Vila, D., Albrecht, R., Goodman, S. J., Calheiros, A. J. P., Biscaro, T., Kummerow, C., Cohen, J., Fitzjarrald, D., Nascimento, E. L., Sakamoto, M. S., Cunningham, C., Chaboureau, J.-P., Petersen, W. A., Adams, D. K., Baldini, L., Angelis, C. F., Sapucci, L. F., Salio, P., Barbosa, H. M. J., Landulfo, E., Souza, R. A. F., Blakeslee, R. J., Bailey, J., Freitas, S., Lima, W. F. A., and Tokay, A.: The Chuva Project: How Does Convection Vary across Brazil?, Bulletin of the American Meteorological Society, 95, 1365–1380, 2014.

Martin, S. T., Andreae, M. O., Artaxo, P., Baumgardner, D., Chen, Q., Goldstein, A. H., Guenther, A., Heald, C. L., Mayol-Bracero, O. L., McMurry, P. H., Pauliquevis, T., Pöschl, U., Prather, K. A., Roberts, G. C., Saleska, S. R., Dias, M. A. S., Spracklen, D. V., Swietlicki, E., and Trebs, I.: Sources and properties of Amazonian aerosol particles, Reviews of Geophysics, 48, 2010

Milbrandt, J. and Yau, M.: A multimoment bulk microphysics parameterization. Part I: Analysis of the role of the spectral shape parameter, Journal of the atmospheric sciences, 62, 3051–3064, 2005a.

Milbrandt, J. and Yau, M.: A multimoment bulk microphysics parameterization. Part II: A proposed three-moment closure and scheme description, Journal of the atmospheric sciences, 62, 3065–3081, 2005b.

Miles, N. L., Verlinde, J., and Clothiaux, E. E.: Cloud droplet size distributions in low-level stratiform clouds, Journal of the atmospheric sciences, 57, 295–311, 2000.

Morrison, H. and Grabowski, W. W.: Comparison of Bulk and Bin Warm-Rain Microphysics Models Using a Kinematic Framework, Journal of the Atmospheric Sciences, 64, 2839–2861, 2007.

Morrison, H., Thompson, G., and Tatarskii, V.: Impact of Cloud Microphysics on the Development of Trailing Stratiform Precipitation in a Simulated Squall Line: Comparison of One- and Two-Moment Schemes, Monthly Weather Review, 137, 991–1007, 2009.

Morrison, H., Witte, M., Bryan, G. H., Harrington, J. Y., and Lebo, Z. J.: Spurious broadening of modeled cloud droplet spectra using bin microphysics in an Eulerian spatial domain, Journal of Atmospheric Sciences, in review, 2018.

Pöhlker, M. L., Pöhlker, C., Ditas, F., Klimach, T., Hrabe de Angelis, I., Araújo, A., Brito, J., Carbone, S., Cheng, Y., Chi, X., Ditz, R., Gunthe, S. S., Kesselmeier, J., Könemann, T., Lavric, J. V., Martin, S. T., Mikhailov, E., Moran-Zuloaga, D., Rose, D., Saturno, J., Su, H., Thalman, R., Walter, D., Wang, J., Wolff, S., Barbosa, H. M. J., Artaxo, P., Andreae, M. O., and Pöschl, U.: Long-term observations of cloud condensation nuclei in the Amazon rain forest – Part 1: Aerosol size distribution, hygroscopicity, and new model parametrizations for CCN prediction, Atmospheric Chemistry and Physics, 16, 15 709–15 740, 2016.

Rotstayn, L. D. and Liu, Y.: Sensitivity of the First Indirect Aerosol Effect to an Increase of Cloud Droplet Spectral Dispersion with Droplet Number Concentration, Journal of Climate, 16, 3476–3481, 2003.

Szyrmer, W., Laroche, S., and Zawadzki, I.: A Microphysical Bulk Formulation Based on Scaling Nor- malization of the Particle Size Distribution. Part I: Description, Journal of the Atmospheric Sciences, 62, 4206–4221, 2005.

Thompson, G., Field, P. R., Rasmussen, R. M., and Hall, W. D.: Explicit Forecasts of Winter Precipitation Using an Improved Bulk Microphysics Scheme. Part II: Implementation of a New Snow Parameteriza- tion, Monthly Weather Review, 136, 5095–5115, 2008.

Tzivion, S., Feingold, G., and Levin, Z.: An efficient numerical solution to the stochastic collection equation, Journal of the Atmospheric Sciences, 44, 3139–3149, 1987.

Wendisch, M., Pöschl, U., Andreae, M. O., Machado, L. A. T., Albrecht, R., Schlager, H., Rosenfeld, D., Martin, S. T., Abdelmonem, A., Afchine, A., Araùjo, A. C., Artaxo, P., Aufmhoff, H., Barbosa, H. M. J., Borrmann, S., Braga, R., Buchholz, B., Cecchini, M. A., Costa, A., Curtius, J., Dollner, M., Dorf, M., Dreiling, V., Ebert, V., Ehrlich, A., Ewald, F., Fisch, G., Fix, A., Frank, F., Fütterer, D., Heckl, C., Heidelberg, F., Hüneke, T., Jäkel, E., Järvinen, E., Jurkat, T., Kanter, S., Kästner, U., Kenntner, M., Kesselmeier, J., Klimach, T., Knecht, M., Kohl, R., Kölling, T., Krämer, M., Krüger, M., Krisna, T. C., Lavric, J. V., Longo, K., Mahnke, C., Manzi, A. O., Mayer, B., Mertes, S., Minikin, A., Molleker, S., Münch, S., Nillius, B., Pfeilsticker, K., Pöhlker, C., Roiger, A., Rose, D., Rosenow, D., Sauer, D., Schnaiter, M., Schneider, J., Schulz, C., de Souza, R. A. F., Spanu, A., Stock, P., Vila, D., Voigt, C., Walser, A., Walter, D., Weigel, R., Weinzierl, B., Werner, F., Yamasoe, M. A., Ziereis, H., Zinner, T., and Zöger, M.: The ACRIDICON-CHUVA campaign: Studying tropical deep convective clouds and precipitation over Amazonia using the new German research aircraft HALO, Bulletin of the American Meteorological Society, 2016.

Xue, L., Fan, J., Lebo, Z. J., Wu, W., Morrison, H., Grabowski, W. W., Chu, X., Geresdi, I., North, K., Stenz, R., Gao, Y., Lou, X., Bansemer, A., Heymsfield, A. J., McFarquhar, G. M., and Rasmussen, R. M.: Idealized Simulations of a Squall Line from the MC3E Field Campaign Applying Three Bin Microphysics Schemes: Dynamic and Thermodynamic Structure, Monthly Weather Review, 145, 4789–4812, 2017.

Zhang, G., Vivekanandan, J., Brandes, E. A., Meneghini, R., and Kozu, T.: The shape–slope relation in observed gamma raindrop size distributions: Statistical error or useful information?, Journal of Atmospheric and Oceanic Technology, 20, 1106–11

Interactive comment on Atmos. Chem. Phys. Discuss., https://doi.org/10.5194/acp-2018-190, 2018.

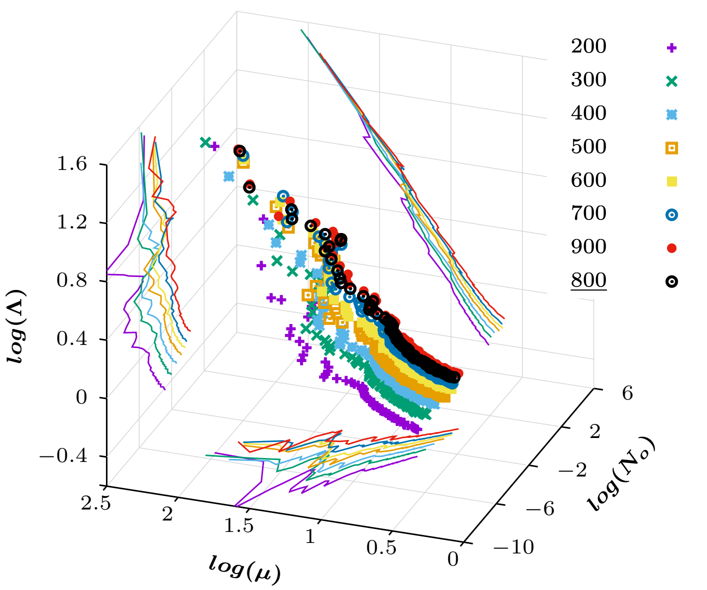

Fig. 1. Updated version of Figure 3 in the submitted manuscript

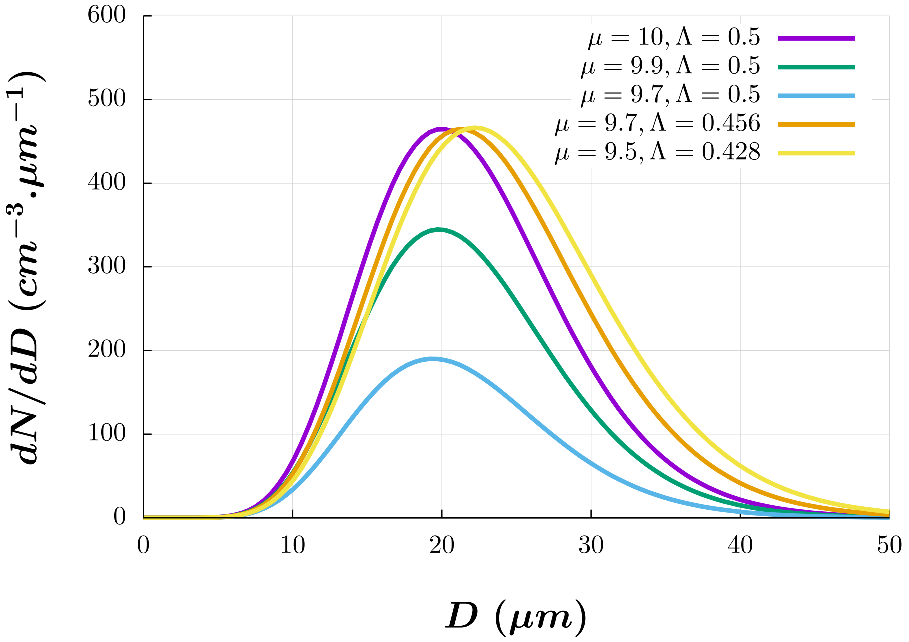

Fig. 2. Gamma size distributions plots for different combinations of μ and Λ

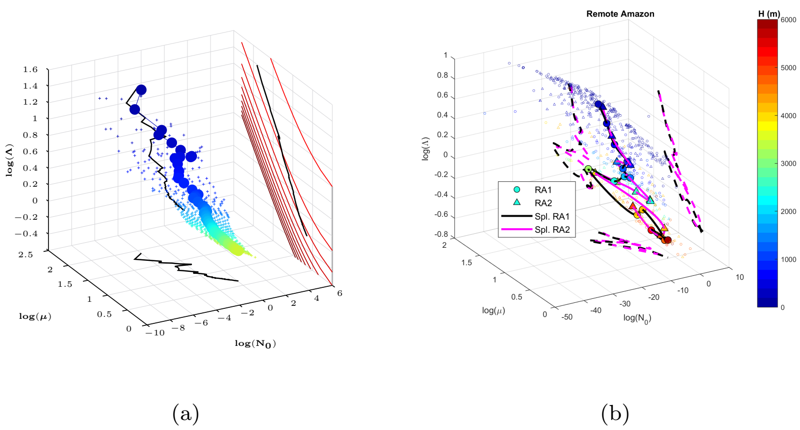

Fig. 3. Updated version of Fig. 2 in the manuscript