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Discussions

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

Anonymous Referee #2

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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 parametrs N0, lambda, mu, 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.

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 N0, lambda, mu). 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?

A new parameterization of mu is proposed, and since the manuscript advertises the gamma phase space I was expecting something like mu being a function of other parameters - e.g., for rain drops it is common practice that mu=fct(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.

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 N0 (while it is really challenging to see even qualitative dependencies in the 3d plots). N0 is proportional to the bulk number (zeroth moment), but at the same time a complicated function of mu and lambda (which already span the other two dimensions). So the only effect of substituting bulk number by N0 is that the analysis becomes more complicated or even meaningless - personally I cannot calculate (lambdaˆ(mu+1)/gamma(mu+1)) without using tools. On the other hand, the dependency of bulk number on aerosol concentration is well-established.

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.

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, DOI: 10.1175/JAS-D-15-0382.1). 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.

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.

While I regret I could not extract the essence of the manuscript regarding 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.

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

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.

Lines 6-10: vague formulation, what are "enough" moments. The impact of mu on cloud water path and condensation rates are described, but which mu 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.

Lines 13-18: Very vague: What is very useful about it, what are the specific advantages? What are the new opportunities?

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.

Page 4 Line 2: What does the prognostic variable represent? What are the initialized aerosol properties in the model?

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

Line 18: It seems worth noting that the exponential is a gamma distribution with mu=0.

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

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 mu or anything else? If so, why not simply replace the parameterization of mu 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 mu?

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

Lines 10-14: What makes the phase space a projection? Is is something different from the 3d space that is used here?

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.

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.

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?

Line 10: Mu 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 mu? How are N0 and lambda important in interpreting the broadening? Could we also think of opposite tendencies for N0/lambda and still call it broadening?

Figures: Is log() referring to the natural logarithm? In particular to interpret the numerical values of mu, log10() 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 mu. 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.

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