We would like to thank all three referees for taking the time to review our manuscript. All referees felt that the manuscript was confusing, and we have substantially revised the manuscript to address these concerns, including a modified methodology, new figures, and clearer discussion. The basic conclusions have not changed, but we feel that they are now better explained and better substantiated. Responses to specific comments are below.

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Disclaimer: This is a summary of a group peer review exercise in my senior undergraduate research class at the University of Oklahoma. 2 instructors and 36 students participated in this review, which we hope the authors find beneficial.
What an excellent class exercise! We thank you for your comments and hope that the class was able to benefit from the review as well. In our revisions, we have substantially modified the description of the methods, figures, discussion, and conclusions in a sincere effort to clarify the manuscript thus making it more accessible to all readers.

The authors present an analysis contrasting condensation rates predicted by two classes of microphysics parameterizations in a numerical model: bin and bulk. They argue that, even for objectively equivalent conditions, the condensation rates (which depend primarily on the size of a cloud particle) differ. It is suggested that the chosen shape parameter of the assumed drop size distribution in the bulk microphysics scheme accounts for the disparity.

Overall, we find the paper to often be difficult to read, the discussion to be misleading and/or vague, and the analysis to be incomplete. These findings are supported by numerous general and specific comments outlined below.

General comments:
1. Readability: Defining variables in a table rather than immediately following their introduction in the text negatively impacts readability. We recommend changing this throughout the paper. In addition, the text switches between tenses on several occasions, there are numerous lengthy sentences, and on multiple occasions conclusions are given without reasoning. Several of these instances are identified in the specific comments below.
We now define the variables immediately following the equations. Specific comments regarding readability that appear below have all been addressed. Special attention has been paid to tense in order to make it consistent throughout.

2. It might be good to test for a larger variety of aerosol concentrations (more than three) before reaching conclusions.
Since the three aerosol concentrations that we tested all behaved in approximately the same way, we don’t believe that testing of additional concentrations would provide more information. However, we do now include additional bulk simulations with different cloud droplet shape parameters. These tests have helped to strengthen our conclusions.

3. A more elaborate discussion/explanation of the differences between bulk and bin schemes in the introduction is needed to improve accessibility for readers with less cloud physics and/or modeling expertise.
Some additional explanation has been added. However, we recommend reading Khain et al. (2015) for a much more thorough description of the two types of schemes. This review article is also referenced in the manuscript.

4. Model design: There were several choices in the model design that were not well qualified (model resolution, Harrington radiation scheme, land surface model, vegetation type, etc.). What is the significance and/or reasoning for making these choices?
More reasoning is now provided. Fine horizontal and vertical spacing was used in order to well resolve the cumulus clouds and their microphysical structure. Land surface and vegetation choices were made in order to most closely resemble the ARM SGP site. A radiation scheme was necessary in order to allow the boundary layer to develop.

5. The value of the best-fit parameter could not be determined before condensation occurred. Why? If bin values are known (which they must be to proceed with the bin scheme) then it seems these could be easily output and used to compute a fit. If it is not expected to have large impacts, then what magnitude could be expected?
The values are certainly known by the model before condensation, but only the values after condensation were written to files and available for our analysis. We believe that this assumption has only small impacts on the results and conclusions.

6. The Discussion and Conclusions section (though somewhat confusing) claims that the cloud droplet size distribution shape is the most important factor for agreement in condensation rates between bin and bulk schemes, but it also states that current assumptions of the size distribution shape are adequate. What are the broader impacts of this study? Should parameterizations be changed or not?
We mean that assumptions of a gamma distribution function in general are adequate, but that in order for the gamma distribution to be useful, we need better knowledge of the shape parameter that appears in this distribution function. In order to obtain a better shape parameter, we need to either move to triple-moment schemes, or find better ways to parameterize it from observations. The discussion and conclusions have been substantially modified in an effort to clarify the points being made.

7. The differences found between the simulations with the bin and bulk schemes are argued to be related to the shape of the drop size distribution. However, a double moment bulk microphysics scheme with a constant shape parameter was chosen (after arguing for the importance but unknown relationship between cloud droplet concentration and shape parameter in the Introduction). Aren't the results shown here largely generated by this choice? Is it better (and possible) to use this analysis to determine which assumed relationship in previous parameterizations is appropriate?
Yes, additional analysis shows that the G98 relationship is the most appropriate of the three presented. This analysis appears in a separate paper, Igel et al. 2016a, which has been accepted pending revision. If we had used this relationship in our bulk simulations, then the comparison may indeed have been more favorable. We found however that while

the G98 relationship is the best, it is only appropriate for a small range of aerosol concentrations.

Specific Comments:
Line 1: Change to '. . .of the Gamma Function Shape Parameter. . ."
Done.

Line 15: Omit 'does'
This word is necessary for the sentence.

Line 22-23: Suggest rewording. "since shape parameter can have a large impact. . ."
We were trying to avoid the term "shape parameter" in the abstract in order to make the abstract more understandable to a wide audience.

Line 22: Please specifically explain how the paper is important, rather than state that it 'may be' important.
'May be' has been changed to 'is'.

Line 40-41: The word 'plagued' implies a problem that should probably be identified specifically via reference to appropriate literature. In what sense do 'predefined ice habits' pose these issues?
More explanation and a reference to Khain et al. (2015) are now included. Predefined ice habits do not always appropriately describe real-world ice habits which smoothly transition between habit types.

Lines 44-46 and 53-55: Awkward sentence structure.
Thank you for the comment.

Line 54: Omit comma after 'is'
Done.

Line 61: Need to explain why this point is "clearly an outlier". The shape parameters are subject to the pitfalls of fitting a uni-modal, parametric function to a variety of histograms that don't necessarily conform to the shape of a gamma distribution. Furthermore, it isn't made clear that there exists some single distribution of which all these points should be considered 'realizations'.
It is unclear why the outlier exists. The value was calculated by Miles et al. (2000) and reported in their Table 1 based on Figure 3 in Korolev and Mazin (1993). It is possible that is an error in calculation. A value of 44.6 would indicate a rather narrow distribution, and visual inspection of Figure 3 does not suggest that the observed distributions were particularly narrow.

Line 64-65: Remove 'also' in consecutive statements.
Done.

Line 69-70: Change to 'to accurately model'

Done.

Line 81-85: Awkward, long sentence.
It has been split into two.

Line 89: Omit comma following reference.
Done.

Line 113: The differing formulations should be discussed and justified, even if only briefly.
We do not feel that the different formulations need to be justified as the formulations were not our choice, but rather the choice of the scheme developers.

Lines 126-127: "The wider range of thermodynamic conditions make the conclusions of this study more robust." How so?
The results are not specific to a narrow range of thermodynamic conditions and hence are more applicable for a wide range of meteorological situations.

Line 131: Define ARM SGP.
Done.

Line 133: Suggest revising "horizontally homogeneously" to "homogeneously in the horizontal dimension" here and similarly elsewhere.
Done.

Line 151-154: It would be good to give a reference to show that these values encompass a variety of continental and maritime regimes. Remove 'more'.
Thank you for the suggestions.

Line 162-164: Unclear. Also, single quotes around 'approximately proportional'.
We mean approximately linearly proportional.

Line 166-167: Suggest replacing 'nevertheless' with 'however' and italicizing 'can' in
Thank you for the suggestion.

Line 167. Suggest replacing 'doesn't' with 'does not.'
Done.

Line 186: Comma after 'therefore'
Done.

Line 191-193: Split into two sentences
Removed.

Line 192: Spelling error: "increases"
Done.

Line 197: Switch ‘easily’ and ‘compare’
This would result in a split infinitive.

Line 317-318: Why should conclusion hold for other hydrometeor types? Ice particles, for example, have more complicated vapor growth processes that ultimately depend on both particle shape and environmental characteristics.
This sentence has been removed.

Line 201: Clarify that one needs to focus on shape parameters from 0-5 to see the difference between RDB/SBM1600 results and the others. Also would be good to not that this is the same regime where previous assumptions for shape parameter behavior diverge (i.e., Figure 1).
No longer relevant given the broader revisions to the text.

Line 205: Should be ‘Fig. 4 d-f’
Thank you. This figure has been removed.

Line 206: Change ‘worst’ to ‘strongest’ or ‘largest’
Removed.

Line 208-209: This statement bares some explanation and maybe a citation. Also, if this is the most common case, why is it not shown in evaporation figures?
We are unsure what the reviewer is suggesting. This sentence is a statement of our results. Regardless, the figure and associated discussion has been removed.

Line 209: Comma after ‘Thus’
Removed.

Line 210: Change ‘between’ to ‘of’ and remove ‘do’
Removed.

Line 223: Omit comma after ‘distributions’
Thank you for the suggestion.

Line 229: missing period
Thank you.

Line 242-245: Why is that not expected? Seems ‘reasonable’ in most cases, but the a gamma distribution shape parameter fit to a very flat, broad distribution would seem subject to very rapid changes due to modest movements of probability left or right. It would be good to elaborate a bit more.
Yes, we agree that there may be some cases when the shape parameter does change rapidly in one second, particularly when the condensation or evaporation rate is particularly large and the distributions are broad (low shape parameters). Cloudy points with best-fit shape parameters less than 1 are not included in the analysis. This is

discussed in more detail now in the revised manuscript.

Line 244: Comma after ‘step’, omit ‘thus’.
The sentence has been split in two.

Line 248-254: The ‘theoretical’ ratio needs clarification. It is not clear what is meant by a bin scheme ‘predicting’ a gamma distribution. Evaporation and condensation rates can be predicted based on a histogram conforming to a gamma distribution of particular shape parameter. If this is what is implied, then rewording is needed.
The explanation of the theoretical ratio has been substantially expanded and is reproduced below. Note that in the revised paper, we group points by $S$, $N$, and $\bar{D}$ rather than $S$ and $N\bar{D}$.

Revised explanation:

The shape parameter term in Eq. (2) (hereafter $f_{NU}$), which is equal to $\nu\left(\frac{\Gamma(\nu)}{\Gamma(\nu+3)}\right)^{1/3}$, can be evaluated for each joint bin in the $S$, $N$, and $\bar{D}$ phase space for all simulations. In the case of each BULK simulations, the value of $f_{NU}$ is the same for every joint bin since the value of $f_{NU}$ is uniquely determined by the choice of the shape parameter value for each BULK simulation. For the BIN simulations, $f_{NU}$ can be calculated using the best-fit shape parameters. Unlike for the BULK simulations, the value of $f_{NU}$ for the BIN simulations will vary amongst the joint bins since the best-fit shape parameter is determined from the freely evolving cloud droplet distributions that are predicted by the BIN microphysics scheme. We can use the values of $f_{NU}$ in our comparison of the condensation and evaporation rates to account for the fact that the best-fit shape parameters in the BIN simulations will often be different from the single prescribed value in the BULK simulations. Specifically, in our analysis, we adjust the mean condensation and evaporation rates ($C$) for each joint bin from the BULK simulations in the following way:

$$C_{BULK,modified} = C_{BULK,original}\frac{f_{NU,BIN}}{f_{NU,BULK}}$$

By doing so, we find the condensation and evaporation rates that the BULK simulations would have had if they had used the same value of the shape parameter that best characterized the cloud droplet size distributions that were predicted by the BIN simulations.

Line 253: Omit ‘specifically’
Removed.

Line 264: Comma after ‘Therefore’
Removed.

Line 278: Move comma from after to before ‘removed’
Removed.

Line 286: Comma after ‘study’, suggest changing ‘conducted a comparison’ to ‘compared’

Thank you for the suggestion.

Line 300-302: Based on the preceding sections, the gamma distribution has not been rigorously shown to be ‘good’, in that there is no exact standard set forth with which to judge ‘goodness’. Also, nothing is offered with which to compare this estimator. There might be a better parametric form and certainly a semi-non-parametric form could be devised that would beat the max. likelihood fit of the gamma function in almost all cases. Not that one needs to test non-parametric forms in this paper, but the exact nature and limits of performance expectations needs to be defined in such a way that other options are reasonably set aside.

We now include a measure of “goodness” for the gamma distribution fits, and find that in general the gamma distribution performs quite well. We agree that there may often be a different PDF that may fit the bin model cloud droplet size distributions better, but as most bulk microphysics schemes use a gamma distribution, this is the distribution that we are interested in studying in the current manuscript. It may be of interest to look at other parametric and non-parametric forms in a separate study.

Line 313: Commas after ‘time step’ and ‘thus’
Removed.

Line 317: Suggest not starting with ‘And’. Also, the ‘them’ that has not been explored apparently refers to ‘other hydrometeors’, that doesn’t work well since one doesn’t really explore hydrometeors. Suggest rewording.
Removed.

Line 320: Reword. “presented a novel method. . .” instead of “presented here. . .”
Done.

Line 445: Figure 1. It is not clear that interpolation between data points is appropriate. See comments on Line 61.
No interpolation has been performed in Figure 1. Colored lines connect values that were reported from the same study.

Line 446: Number disagreement. If a clear reason to assume functionality is demonstrated, then it should read “Shape parameter as a function of. . .”, that is, omit “values”
We did not intend to imply that there is a definite functionality, only to indicate that shape parameter is on the y-axis and droplet concentration is on the x-axis. This is now clarified in the manuscript.

Line 455: Figure 2 caption. Should include date, time and station of the soundings from which profiles were adapted
This figure has been removed.

Line 459: Number disagreement. Should be “rates as functions. . .”

This figure has been removed.

Line 466: Figure 5. It would be interesting to see some 'quantile' brackets, R2 values, etc. to quantify 'closeness' of fit. It isn't clear from the figure (packed with dots) where the greatest concentration of dots is, other than the general shape of the opaque area. . . some areas may be 'more opaque' than others.
This figure has been removed. In the revised paper, we include standard deviation values in order to quantify the spread.


References:
Igel, A.L. and S.C. van den Heever, 2016a: The importance of the shape of cloud droplet size distributions in shallow cumulus clouds. Part I: Bin microphysics simulations. Accepted pending revision at *J. Atmos. Sci.*

Igel, A.L. and S.C. van den Heever, 2016b: The importance of the shape of cloud droplet size distributions in shallow cumulus clouds. Part I: Bulk microphysics simulations. Accepted pending revision at *J. Atmos. Sci.*