

Interactive comment on “The Role of the Size Distribution Shape in Determining Differences between Condensation Rates in Bin and Bulk Microphysics Schemes” by A. L. Igel and S. C. van den Heever

C. R. Homeyer

chomeyer@ou.edu

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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.

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 cho-

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sen 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.
2. It might be good to test for a larger variety of aerosol concentrations (more than three) before reaching 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.
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?
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?
6. The Discussion and Conclusions section (though somewhat confusing) claims that the cloud droplet size distribution shape is the most important factor for agreement

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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?

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?

Specific Comments:

Line 1: Change to '...of the Gamma Function Shape Parameter...'

Line 15: Omit 'does'

Line 22-23: Suggest rewording. "since shape parameter can have a large impact..."

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

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?

Lines 44-46 and 53-55: Awkward sentence structure.

Line 54: Omit comma after 'is'

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

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should be considered ‘realizations’.

Line 64-65: Remove ‘also’ in consecutive statements.

Line 69-70: Change to ‘to accurately model’

Line 81-85: Awkward, long sentence.

Line 89: Omit comma following reference.

Line 113: The differing formulations should be discussed and justified, even if only briefly.

Lines 126-127: “The wider range of thermodynamic conditions make the conclusions of this study more robust.” How so?

Line 131: Define ARM SGP.

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

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’.

Line 162-164: Unclear. Also, single quotes around ‘approximately proportional’.

Line 166-167: Suggest replacing ‘nevertheless’ with ‘however’ and italicizing ‘can’ in line 167. Suggest replacing ‘doesn’t’ with ‘does not.’

Line 186: Comma after ‘therefore’

Line 191-193: Split into two sentences

Line 192: Spelling error: “increases”

Line 197: Switch ‘easily’ and ‘compare’

Line 317-218: Why should conclusion hold for other hydrometeor types? Ice particles,

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for example, have more complicated vapor growth processes that ultimately depend on both particle shape and environmental characteristics.

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).

Line 205: Should be 'Fig. 4 d-f'

Line 206: Change 'worst' to 'strongest' or 'largest'

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?

Line 209: Comma after 'Thus'

Line 210: Change 'between' to 'of' and remove 'do'

Line 223: Omit comma after 'distributions'

Line 229: missing period

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.

Line 244: Comma after 'step', omit 'thus'.

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.

Line 253: Omit 'specifically'

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Line 264: Comma after 'Therefore'

Line 278: Move comma from after to before 'removed'

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

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.

Line 313: Commas after 'time step' and 'thus'

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.

Line 320: Reword. "presented a novel method. . ." instead of "presented here. . ."

Line 445: Figure 1. It is not clear that interpolation between data points is appropriate. See comments on Line 61.

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"

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

Line 459: Number disagreement. Should be "rates as functions. . ."

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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.

[Interactive comment on Atmos. Chem. Phys. Discuss.](#), doi:10.5194/acp-2016-64, 2016.

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