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.

Anonymous Referee #2
Received and published: 25 March 2016

Review of "The role of the size distribution shape in determining differences between condensation rates in bin and bulk microphysics schemes"

In this manuscript the authors argue that the shape parameter of bulk distributions is important in models to properly understand cloud properties as well and process rates. The problem is that the shape parameter is highly variable. They argue that the shape parameter accounts for much of the difference in condensation rates between bin an bulk models. Overall the manuscript needs more clarification of the results and better explanation of the impacts of the results.
We have substantially modified the manuscript in order to clarify the discussion and to provide better explanations.

Major comments: Condensation and evaporation will affect the dynamics of the simulation so why not use a kinematic framework similar to that used by Morrison and Grabowski, 2007 where microphysics does not feedback into the dynamics? Have variables such as updraft speed checked for the simulations to ensure that the dynamics are in fact similar between the two models?
We have checked, and the mean updraft speed is very similar amongst all of the simulations. However, it should not matter if the dynamics are different. The power of the method being used to compare the simulations is that we control for all of the quantities that impact the condensation and evaporation rates (microphysical properties and saturation ratio; temperature and water vapor will also impact the rates, but they are of secondary importance) in our binning approach. Changes in dynamics will impact the frequency at which specific combinations of these quantities occur, but should not impact the mean value of the condensation and evaporation rates for each combination (each of our joint bins). Even in a kinematic framework, it can be difficult to say, for example, that average condensation rates are higher with one scheme because that scheme inherently predicts higher condensation rates, or because feedbacks from other microphysical processes resulted in more frequent occurrences of high condensation rates. Our method removes the issues associated with changes in the frequency of occurrence of specific conditions and allows us to directly compare the behavior of microphysical processes predicted by the different schemes.

More explanation needs to be given in the discussion especially in explaining how condensation and evaporation work in both bin and bulk models and why the difference in results (Fig. 5) between condensation and evaporation. In general the conclusions are confusing (especially point 1 and 2) and need to be rewritten.

The analysis, results, and conclusions have been substantially rewritten in order to clarify our arguments and make the paper more accessible to all readers.

Only one value of the shape parameter was used for the bulk model. Do different values of the shape parameter provide better or worse comparison to bin condensation rates?
Thank you for this question. Different values of the shape parameter do change the comparison to the bin condensation and evaporation rates. Additional simulations are now included in the analysis in order to strengthen the conclusions in this regard

Does using a variable shape parameter as described in Fig. 1 lead to better results compared with bin?

The RAMS code is structured in such a way that we cannot try a variable shape parameter. However, we believe that using an appropriate diagnostic equation for the shape parameter could lead to an improved comparison.

Minor comments:
Line 27: suggest adding bulk model references
Khain et al. (2015) provide a comprehensive list of 37 bulk schemes and 22 bin schemes that have been developed, and the readers are referred to this paper for more information.

Line 28: should be "mass mixing ratio" and "total number mixing ratio"
Thank you, we have made the change.

Line 29: remove "typically"
It has been removed.

Line 31: what mixing ratio? Mass mixing ratio?
It can be either, but typically it is the mass mixing ratio. This has been specified now.

Line 37: remove "simulations with"
"Simulations with" is necessary for consistency with "benchmark simulation" earlier in the sentence.

Line 42: remove "both liquid- and ice-phase"
It has been removed.

Line 46: what do you mean by value? There is value in how computationally cheap bulk models are.
We mean predictive value and this is now explicit within the manuscript.

Line 66: explain why the third function is in total disagreement. What assumptions lead to this disagreement.
We mean that G98 shows an increase in the shape parameter as the number concentration increases whereas RL03 and MG07 show a decrease. All relationships are based on observational data. G98 bases their relationship on data from Simpson and Wiggert

(1969), MG07 bases their relationship on data from Martin et al. (1994), and RL03 bases their relationship on field campaign data compiled by Liu and Daum (2002). This is now clarified in the manuscript.

Line 79: suggest new word choice for "disagreement"
We have substituted "discrepancies".

Line 91: explain the liquid implementation here, get rid of the appendix and get rid of the ice implementation discussion.
We agree that the appendix includes some information that is irrelevant for the present study, but we include it in order to provide a complete description of the SBM implementation in RAMS, as this is the first time that this implementation has been described in the literature.

Line 96: Walko (2000a) or Walko (2000b)?
Walko et al (2000b)

Line 96: Eq. 6 is not in Walko 2000
We are confused by the reviewer's comment. We have double-checked and Eq. 6 in Walko et al. (2000b) is indeed the equation we are referencing.

Eq. 2: What are the units of G? Is r_c a mixing ratio or mass concentration?
Units of G are kg m^{-1} s^{-1} and it is a mass mixing ratio. These details are specified in Table 1.

Line 113: The ventilation coefficients could be set to 1 in both models to see their impact.
Yes, true, but we believe that the difference in ventilation coefficients is of secondary importance and we do not wish to investigate this level of detail here.

Line 129: what model time period are the results from? And how long does it take for the clouds to spin up?
Clouds appear after about 4.5 hours of simulation and clouds existing at any time in the simulation are used for analysis.

Line 133: suggest "homogeneously in the horizontal direction."
This has been changed.

Line 141: define relative humidity
This is now included.

Line 169: suggest "in order to better compare..."
This has been changed.

Line 173: do you mean S-ND bins or bin-model bins
This is no longer relevant within the revised text.

Line 200: why does the RDB scheme predict higher condensation rates for low integrated diameter values? I suggest showing some bin and bulk distributions to explain the discussion from lines 199-203

This part of the discussion has been removed in the revised text.

Line 210: can you explain what it is about evaporation versus condensation that leads to the better evaporation rate comparison between the two schemes? How does the bin distribution change during evaporation versus condensation?

It is simply that the shape parameter value chosen for use in the RDB simulations (4) more closely matches the mean value of the best-fit shape parameter from evaporating cloudy points (~4) in the SBM simulations than the best-fit shape parameter from the condensing cloudy points (~7). If we instead run the RDB simulation with the shape parameter set to 7 (instead of 4), then the comparison becomes better for condensation. Physically, we expect a lower shape parameter (wider distribution) for evaporating size distributions. During condensation, the large droplets increase in diameter slowly whereas the small droplets increase in diameter quickly and thus the size distribution narrows (droplets become more similar in size). During evaporation, the same differences in diameter growth rates lead to a widening of the size distribution.

Line 221: suggest "larger shape parameter"

No longer relevant.

Line 232: why use the first 15 bins? What are the other bins used for and how many bins are there?

There are 18 additional bins with water drops having raindrop-sized diameters. We are only interested in the cloud droplets, so these additional 18 bins are not used for the analysis.

Fig. 4: suggest doing fits of the data points for better analysis

Yes, this is a good suggestion. However, we do not include these type of plots in the revised manuscript.

Line 262: The 1600 simulations cover a larger area in integrated diameter space but not supersaturation space. This should be pointed out.

In our new analysis, we group data by number mixing ratio and diameter separately. The same comment that the reviewer makes is applicable to the number mixing ratio, and this point has been made clear.

Line 268: suggest changing the word "startling"

No longer relevant.

Line 298: The rates are similar, but there is a lot more spread in the data. Statistics on the data would help here.

Standard deviation values are now included in the analysis in order to quantify the spread.

Line 300: What are you using to base the fact that a gamma distribution is a good assumption for cloud droplets? Is it because the bulk model with an assumed gamma distribution predicts condensation rates fairly well compared to a bin model? If so this should be explained.
Yes, this is the reason. This is hopefully better explained in the manuscript now.

Conclusion point 2: Just state the most important variables that determine differences between bin and bulk condensation rates. Don't worry about stating what is not important (f and G) unless it is surprising.
This point has been removed.

Conclusion point 4: There are other reasons to use sub-stepping in bin models. Suggest removing point 4.
We agree that this point should be removed.

Line 318: condensation rates become less important when riming rates are large. Also ventilation can be large for hail. This may not matter or be relevant for certain other hydrometeor types.
This sentence has been removed.

Table 1: G_RDB should read "Terms to account..." This term also accounts for vapor diffusion.
Agreed. This has been modified appropriately.

r_c should be mass mixing ratio; saturation ratio should be defined
Yes, thank you.

Fig. 5 suggest putting a line through condensation rate ratio = 1
This figure has been removed.

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.*